Chapter 6
Atomism, Experiment and the Mechanical Philosophy: The Work of Robert Boyle

Abstract Boyle articulated and defended a strict version of the mechanical philosophy, a theory about the ultimate structure of matter. According to that philosophy, the material world is made up of corpuscles of the one impenetrable matter possessing a definite shape and size and capable of motion. Boyle was also a pioneer of experimental science, best exemplified in his pneumatics. Boyle himself distinguished between the two forms of knowledge, arguing that the 'intermediate causes' involved in his experimental science, such as the weight and spring of air, were empirically accessible in a way that the ultimate mechanical corpuscles were not. As a consequence, Boyle's experimental science could not be fruitfully guided by the mechanical philosophy and the success of his experimentation did not constitute significant support for it. This is at least implicit in some of Boyle's own remarks. What was scientific about the scientific revolution, in my view, was the emergence of experimental science as distinct from philosophical theories about the ultimate structure of matter.

6.1 What Was Scientific About the Scientific Revolution?

Our discussion of Sennert's atomism in the previous chapter gives a hint of one aspect of the scientific revolution that took place in the seventeenth century, the increased emphasis on experiment as the source of and grounds for scientific knowledge. The utilisation of artificial experiments to throw light on nature that had been marginalised in the work of the Aristotelian alchemists became a new focus of attention. The writings of Francis Bacon were emblematic of the new move.

Another aspect of that revolution, which was already making its presence felt in the latter part of Sennert's lifetime in academic circles, was the replacement of the Aristotelian world-view by the mechanical one. Those mechanical philosophers who were atomists recast atomism in a form that reconciled it with the new philosophy and, in doing so, constructed a version of atomism that had marked similarities with that of Democritus and Epicurus and which, consequently, but as we shall see, largely mistakenly, can easily be construed as a revival of the ancient theories.

This chapter involves an investigation of the relationship between the switch from an Aristotelian to a mechanical matter theory, on the one hand, and the rise

A. Chalmers, *The Scientist's Atom and the Philosopher's Stone*, Boston Studies in the Philosophy of Science 279, DOI 10.1007/978-90-481-2362-9_6,

of experimental science on the other. I argue that these two changes were not as closely connected as is typically supposed. What was scientific about the scientific revolution, in my view, was the emergence of experimental science as distinct from deep theories about the ultimate structure of matter, mechanical or otherwise. What was scientific about the scientific revolution was the emergence of science as distinct from philosophy. In the seventeenth century, atomism retained the speculative, philosophical status that it had possessed in the hands of Democritus and Epicurus. It did not feed productively into the new experimental science.

René Descartes constructed the first systematic version of the view that came to be known as the mechanical philosophy.[1] For Descartes the material world is full of inert matter (which he identified with extension) and the only cause of changes in motion is the pushing of matter against matter. Descartes was not an atomist, insofar as he regarded the world to be a plenum and he regarded matter to be indefinitely divisible. Nevertheless, he did appeal to particles of matter in his construction of mechanical explanations of a range of chemical, magnetic, optical and other phenomena. Already present in Descartes is the division between the material realm, composed of inert matter, and the spiritual realm of souls, minds and angels. This division was adopted in some form or other by all the seventeenth-century mechanical philosophers and constituted the main difference between their world-view and that of Democritus and Epicurus.

Descartes' French compatriot, Pierre Gassendi, was a mechanical philosopher who was an atomist who explicitly presented his view as a revised version of that of Epicurus. However, my main focus will be on the version of atomism developed in the context of the mechanical philosophy by Robert Boyle. There are several reasons for this. Boyle was one of the most articulate expositors and defenders of the new philosophy and did much to popularise it.[2] However, articulation of the mechanical philosophy is by no means Boyle's only claim to fame. He is also famous as an experimentalist. His voluminous and wide-ranging writings are full of detailed descriptions of many experiments, most notably in chemistry (and alchemy) and in pneumatics. Indeed, he is known mostly through his experimental discovery of the law that bears his name. If we are to find a version of the mechanical philosophy in the seventeenth century that was intimately linked with and borne out by experiment, then it is in the works of Boyle that we are most likely to find it.

Boyle articulated an atomistic version of the mechanical philosophy. It involved the total elimination of Aristotelian forms and sought to reduce the material world to the arrangements and motions of particles of universal matter possessing an unchanging shape and size. He also conducted experiments in pneumatics, many of them involving his newly-devised air pumps. The claims supported by those experiments involved appeal to the weight, spring and pressure of air. Consequently, they did not qualify as 'mechanical' in the strict sense, as Boyle openly acknowledged. Implicit in Boyle's practice, and also made quite explicit by him, is a distinction between 'matters of fact' established by experiment and the fundamental matter-theory called the mechanical philosophy.

The mechanical philosophy was 'philosophical' insofar as it offered an ultimate theory of matter in general. By contrast, Boyle's pneumatics was subject-specific and the explanations it made possible were not ultimate insofar as weight, spring and pressure remained unexplained. I urge that the emergence of the latter kind of knowledge as distinct from the former is what renders the name 'scientific revolution' appropriate. In the remainder of this chapter I aim to give a detailed articulation and defence of this view in the context of Boyle's work.[3] I am particularly concerned to defend myself against the charge of anachronism, along the lines that I am illegitimately imposing a modern distinction between science and philosophy on the seventeenth century. Boyle was intent on defending his mechanical philosophy as well as his experimentation and he explored and insisted on a detailed relationship between the two in a way that does not mirror the contemporary separation of science and philosophy as distinct practices assigned to different university faculties. Nevertheless, a distinction was there in practice in Boyle's work and sometimes made explicit by him.

Newman (2006, p. 2) sees the scientific revolution to which Boyle contributed as 'the great disjunction between the common view of matter-theory before and after the mid-seventeenth century', the former involving immaterial forms and the latter arrangements of minute, robust corpuscles. I, by contrast, construe the revolution in terms of the emergence of experimental science as distinct from philosophical matter theories. In my view, Boyle was one of the important pioneers of experimental science as opposed to philosophy but not a successful defender of an atomistic version of the mechanical philosophy that was experimentally based.

6.2 Boyle's Version of the Mechanical Philosophy

In the early 1650s Boyle was already an advocate of and participant in 'Baconian science', that is, science based on experimentation. He became a participant in this tradition through his association with the German expatriate Samuel Hartlib and his circle. We know Boyle was knowledgeable about experimental chemistry as practiced on the continent by the likes of Rudolph Glauber and that he was tutored in the practice of alchemy by the American émigré, George Starkey.[4] Boyle moved to Oxford late in 1655 and there began extensive experimentation, especially in chemistry and pneumatics. It was in that period that Boyle constructed his air pump and thereby revolutionised pneumatics. It was also in that period that he began a careful formulation of his mechanical philosophy. In this section I am concerned with the latter.

The key sources for Boyle's articulation of the mechanical philosophy are 'The origin of forms and qualities according to the corpuscular philosophy', written in the late 1650s and published in 1666 and 'About the excellency and grounds of the mechanical hypothesis' published in 1674.[5] According to Boyle there is one universal matter characterised by its impenetrability. The world is composed of particles of this matter that are too small to be detected by the senses and which remain undivided in physical processes, although they are divisible mentally or by divine omnipotence. Boyle refers to these as *minima naturalia* or *prima naturalia*.[6] As portions of matter, the natural minima have a distinctive and permanent shape and size and they can move. These are the only properties they possess in addition to the impenetrability characteristic of all pieces of matter. They are the ‘primary affections’. Natural minima can combine into relatively stable clusters, which clusters will themselves have distinctive shapes, sizes and motions, and which may well remain undivided through various kinds of change. The main tenet of the mechanical philosophy is that all the phenomena of the material world are to be explained in terms of, traced back to, or reduced to, the motions and arrangements of portions of matter characterised in terms of their primary affections only. Boyle referred to the arrangements and motions of the invisible particles assumed to be responsible for a body’s observable properties as its ‘texture’.

The reductionist character of Boyle’s mechanical characterisation of the world required that he explain how observable bodies come to have a range of properties, some detectable by the senses, such as colours and smells, and others, such as temperature or degree of elasticity, determining how bodies interact with each other. Perceptible properties (‘sensible qualities’ in Boyle’s terminology) are explained as responses in us due to the impact of mechanical particles (that is, particles characterised solely in terms of their shape, size and motion) on our sense organs. Visible objects have the colours that they have in given circumstances as a result of the interaction of their corpuscular structures with the structures that constitute light and the interaction of that light with our eyes (themselves made up of a characteristic arrangement of mechanical particles) in those circumstances. Properties such as temperature or chemical properties, which, as Boyle recognises, are possessed by bodies independently of whether they are perceived by humans or not, are to be explained in terms of mechanical particles and their motions. Temperature of a body arises from the relative vigour of the motions of the particles composing it whilst the ability of gold to be dissolved by nitric acid is to be attributed to the relationship between and interaction of the shapes and motions of the mechanical particles making up those two substances.

So far, a marked similarity between Boyle’s mechanical philosophy and ancient atomism should be obvious. Major differences enter in when it comes to the non-material world of souls and minds and the related issue of the role of God. Unlike the Ancient Greeks, Boyle and other mechanical atomists (as I shall refer to those mechanical philosophers who were atomists) restricted their mechanical, atomistic explanations to the material realm. Boyle (2000, Vol. 5, p. 300) made it quite clear that he rejected the atheistic implications of Ancient atomism and insisted that the ‘Reasonable Soule, that is said to inform the humane Body’ is immune from mechanical reduction and he explicitly invoked God as actively involved in the construction and operation of the material world in a number of ways. Firstly, Boyle saw God as the author of both matter and any motion that it possesses. Secondly, he regarded it as necessary that God break matter into pieces and arrange those pieces into the interconnected structure that constitutes our world. Thirdly, God is the author of the laws of motion that govern the regular behaviour, that is, motion and collision, of particles of matter and is constantly at work in the world to ensure that those laws are obeyed. Here is how Boyle (2000, Vol. 5, p. 306) summarised the role of God in the mechanical world in the ‘Origin of forms and qualities’:

> I shall not scruple to say ... That the Origine of Motion in Matter is from God; and not onley so, but that thinking it very unfit to be believ'd that Matter, barely put into Motion and then left to itself, should Casually constitute this beautiful and orderly World: I think also further that the wise Author of Things did, by establishing the laws of Motion among Bodies, and by guiding the first Motions of the small parts of Matter, bring them to convene after the manner requisite to compose the World, and especially did contrive those curious and elaborate Engines, the bodies of living Creatures, endowing most of them with the power of propagating their Species.

I will refer to the mechanical philosophy as I have summarised it in this section as the 'mechanical philosophy in the strict sense'. Boyle and other mechanical philosophers often used the term 'mechanical' in senses weaker than the strict sense. These weaker senses will be characterised and their applicability discussed in later sections of this chapter. Given the range of usages, it is important to be clear about which sense of mechanical is at issue when discussing the character and status of the 'mechanical philosophy' as critics of earlier work of mine have been quick to point out.[7]

There is one aspect of Boyle's deliberations that do not fit into my characterisation of the strict version of his philosophy. I refer to his appeal to 'seminal principles' to explain phenomena, especially biological phenomena such as reproduction. Clericuzio (1990) gives considerable emphasis to this aspect of Boyle's philosophy but I am more inclined to see his resort to seminal principles as what Newman (2006, p. 215) describes as 'a sort of rearguard action intended to evade certain explanatory difficulties resulting inevitably from the postulation of a purely mechanical universe'.[8] Since my focus in this book is the introduction of the atom into the physical sciences rather than biology I bypass this debate.

6.3 Boyle's Case for the Mechanical Philosophy

The main general arguments for the mechanical philosophy offered by Boyle appealed to its clarity, intelligibility and simplicity. The notions of matter and the sizes, shapes and motions of portions of it are clear, easily understood and clearly not in need of any further explanation at a deeper level. Further, explanations that appeal to the motions and interactions of portions of matter are unproblematic and accepted, where they can be upheld, even by opponents of the mechanical philosophy. Boyle contrasted the clarity and intelligibility of mechanical explanations with the obscurities and ambiguities of explanations offered by his opponents, especially the alchemists and the scholastics. There is no doubt that there were plenty of obscure if not unintelligible texts for Boyle to target in this respect and Boyle's writing is clear (if somewhat prolix). But not all of Boyle's opponents can be accused of lack of clarity, and it is worth pointing out that Boyle found arguments of Daniel Sennert, who accepted versions of Aristotelianism and alchemy, sufficiently clear to virtually reproduce them in his own work, albeit without acknowledgement.[9]

Boyle, along with other mechanical philosophers, saw a key merit of his scheme to be an avoidance of the obscure and empty nature of the scholastic appeal to the

Aristotelian notion of form. The title of his key work 'The origin of forms and qualities according to the corpuscular philosophy' signals his intent to give an account of form and qualities as an alternative to the Aristotelian one. One of Boyle's targets was the notion of substantial form. As we have seen, these forms were claimed to inhere in complex bodies of a particular kind and to confer on those bodies the properties of the kind. A sample of gold is what it is by virtue of possessing the substantial form of gold, just as a horse is a horse through possessing the substantial form responsible for horseness. Boyle's response was to argue that properties of wholes could be explained in terms of the properties and mode of combination of their parts without the need for the addition of anything over and above those parts. He exemplified his position by reference to the workings of a watch understood in terms of the inter-relationships between its component parts. He also pointed out that specific knowledge of the substantial forms and their mode of operation was beyond our access, which rendered appeal to them empty. One of the strongest points in favour of the Aristotelian position involved the difference between a live human and a recently dead one, where it could plausibly be argued that the two were composed alike of the same parts. The substantial form in the living being was to account for the difference. But here Boyle (2000, Vol. 5, p. 300) did not disagree, insofar as he accepted the crucial presence of the soul in the living body and himself likened it to a substantial form.

Another argument of Boyle's against a scholastic view concerning forms built on what we have located in the work of Sennert, although, again, there is no acknowledgement of Sennert by Boyle in this respect. A common scholastic assumption was that substantial forms exist in natural bodies but not in artefacts (so that these scholastics would have been unimpressed by Boyle's analogy with watches). The case against this involved arguing for the identity of chemical substances formed either naturally in the earth or artificially in the laboratory, with the latter often being deliberately composed from parts, that is, from component substances, in the laboratory.

As well as appealing to substantial forms to account for the being of a body as a whole, scholastics invoked forms that they called 'real qualities' to account for individual properties of a body, such as the whiteness of snow or the fusibility of a metal. Boyle's response was that little is gained by way of understanding by attributing properties such as whiteness or fusibility to the presence of otherwise unspecified real qualities of whiteness and fusibility. In any case, argued Boyle, it is very unclear what the mode of existence of this quality is that is added to snow to make it white, or to metals to make them fusible, since qualities were not considered by the scholastics to be a material part of the subjects they were qualities of. 'Nor could I ever find it intelligibly made out', Boyle (2000, Vol. 5, p. 309) wrote, 'what these real Qualities may be, that they [the scholastics] deny to be either Matter, or modes of Matter, or immaterial Substances.' Boyle advanced his case by showing how properties could be changed or accounted for mechanically, removing a need to invoke real qualities. For instance, glass can be changed from transparent to white merely by grinding it, where the ensuing whiteness can be explained in terms of the multiple reflections from the many surfaces of the resulting pieces of glass and the resulting interaction of the reflected light with our eyes, with no need to attribute the whiteness to some form or quality added to the glass (Boyle, 2000, Vol 5, p. 320). Likewise, the capacity of a polished sphere to form images can be explained by appeal to the reflecting power of its smooth surface and the poisoning effect of food containing ground glass can be explained by the cutting action of the pieces of glass (Boyle, 2000, Vol. 5, pp. 311–312). No 'real qualities' are needed.

The only properties that matter can have primitively are those of shape, size and motion, the primary affections, together with the impenetrability characteristic of all matter. These are the properties that must be possessed by any portion of matter by virtue of being such. Other properties need to be explained away as arising from the primary affections. The idea that a portion of matter should possess primitively some property other than the primary ones was considered by Boyle to be unintelligible.

6.4 Boyle's Use of the Macroscopic/Microscopic Analogy

Boyle's mechanical explanations, insofar as they appeal to the shapes, sizes and motions of unobservable particles, necessarily go beyond what can be justified by direct appeal to the observable. The problem of how to gain knowledge of the unobservable micro-realm by appeal to observations of the macro-realm has been highlighted by Maurice Mandelbaum (1964, pp. 88–112) and called by him 'the problem of transdiction'. Mandelbaum (1964, pp. 107 and 110–111) attributes to Boyle a response to this problem which he labels 'the extension of sense knowledge by analogy' and 'the translation of explanatory principles from the observed to the unobserved'. Madelbaum's position has recently been endorsed and amplified by William Newman (2006, pp. 203–208). I will first outline the position Mandelbaum and Newman attribute to Boyle. Then I will argue that the arguments are grossly inadequate as a way of defending Boyle's mechanical atomism and have been given more credibility than they warrant.

Boyle, in a variety of places, attempted to render plausible claims about the micro-realm by drawing analogies with the observable macro-realm. The observed compression of snow into a snowball is an effect that can be assumed to apply at the corpuscular level also, and so account for the effect of pressure on firmness of materials (Mandelbaum, 1964, p. 107). Objects of various sizes, such as collections of apples, walnuts, filberts, wheat, sand and flour, when poured from a sack more closely resemble fluids the smaller the individual objects are. This makes it reasonable to suppose that the fluid-like behaviour of a molten metal is due to the fact that its minute, component particles have been separated by the heat (Newman, 2006, pp. 204–205). The way in which two highly polished sheets of glass cohere suggests that the corpuscles composing a solid cohere in the same way (Mandelbaum, 1964, p. 111).

Another move by Boyle that certainly has a superficial plausibility at least, and which Mandelbaum and Newman invoke, is the idea that laws that apply at the observational level and which do so independently of size can be assumed to apply at the micro-level also. Boyle (2000, Vol. 8, pp. 107–108) put the case in 'Excellency of the mechanical hypothesis' as follows:

> [F]or both the Mechanical affections of Matter are to be found, and the Laws of Motion take place, not only in the great masses and the middle-siz'd Lumps, but in the smallest Fragments of Matter; and a lesser portion of it, being as well a Body as a greater, must, as necessarily as it, have its determinate Bulk and Figure. And he that looks upon Sand in a good Microscope will easily perceive, that each minute Grain of it has as well its own size and shape, as a Rock or a Mountain. And when we let fall a great stone and a pebble from the top of a high Building, we find not but that the latter as well as the former moves conformably to the Laws of acceleration in heavy Bodies descending. And the Rules of Motion are observ'd not only in Cannon Bullets, but in Small Shot; and the one strikes down a Bird according to the same Laws that the other batters down a Wall. ... And therefore, to say that, though in Natural Bodies whose bulk is manifest and their structure visible the Mechanical Principles may be usefully admitted, they are not to be extended to such portions of matter whose parts and Texture are invisible, may perhaps look to some as if a man should allow that the Laws of Mechanism may take place in a town clock, but cannot in a Pocket-Watch.

What are we to make of these extrapolations from the observable to the unobservable? I acknowledge that they have some force. In many respects bodies do behave independently of their size, and, in such cases, it is reasonable to conjecture that observed behaviour carries over into the unobservable micro-realm if there is no evidence to the contrary. However, whilst I can tentatively accept such arguments, a mechanical atomist such as Boyle cannot afford to give too much scope to them because *they run counter to and undermine the main tenets of that philosophy*. If the mechanical philosophy is true then the world of interacting minima is qualitatively different from the observable world. In the latter, bodies have a range of properties. They have shape and size and a degree of motion or rest, to be sure, but they also have colours, are rigid and elastic to some degree, are hot or cold, have a taste and so on. Boyle's atoms, his natural minima, are quite unlike this insofar as they lack all such properties. 'And if we should conceive that all the rest of the Universe were annihilated', wrote Boyle (2000, Vol. 5, p. 315) in the 'Origin of forms and qualities', 'it is hard to say what could be attributed to it besides Matter, Motion (or Rest), Bulk, and Shape' Whatever the case for this claim is, it cannot be based directly on observation because no such body has ever been observed. Insofar as Boyle's natural minima are perfectly rigid and impenetrable and lack all properties but the primitive affections, they are quite unlike the bodies of our experience.

With this general point in mind, we can see the problematic character of the arguments from Boyle invoked by Mandlebaum and Newman as giving support to mechanical atomism. The projection of the compressibility of snowballs onto small unobservable portions of it cannot proceed as far as natural minima because they are incompressible. If the adhesion between two polished surfaces is attributed to air pressure, as Boyle came to do, then it becomes a problematic explanation of the alleged cohesion of the minima of a solid. A mechanical philosopher cannot afford to assume that the fluidity of water can be extended, by analogy, to the corpuscles composing it. As for the law of fall, whilst it is true that this law is scale invariant as far as experiments on heavy bodies are concerned, it cannot be assumed to apply to Boyle's minima because they lack weight. For the mechanical philosophers, the latter property is one that required explanation by reference to the motions and impacts of corpuscles characterised solely in terms of the primary affections as Boyle himself openly admitted on more than one occasion. For a mechanical philosopher, no law involving properties other than the primary affections can be carried over to the level of minima however scale-invariant they may appear at the level of observation. As for the approach of the behaviour of poured powders to that of liquids the smaller the size of the particle, taken as an argument for the particulate character of liquids, this argument flounders because of crucial ways in which the analogy breaks down The powders form a pile in a way that poured liquids do not, while pressure applied to liquids is transmitted isotropically through them in a way that is not the case for powders.

I do not here wish to cast doubt on some efficacy for arguments from analogy, nor do I wish to discredit the idea that laws apparently scale-invariant should be assumed to hold generally until there is evidence to the contrary. What I do claim is that these arguments cannot help Boyle defend his mechanical atomism, for the simple reason that that philosophy implies a radical lack of analogy between the observable world and the world of atoms.

Faced with the task of defending the mechanical philosophy in the strict sense, Boyle was in a position similar to that of the Ancient Greek philosophers. He wished to give an account of the ultimate nature of material reality, the reality behind the appearances. Like the Ancients, he drew on the knowledge of the observable world available to him, abstracted aspects of it and turned them into fundamental principles. He argued for the intelligibility of those principles and used analogies with known phenomena to render their general applicability plausible. But there was plenty of opportunity for philosophers to disagree with Boyle's selection of principles. It could be assumed that there is more than one kind of matter, contrary to the fundamental assumption of the Ancient atomists shared by Boyle and the mechanical philosophers generally, or it could be argued that Boyle's 'primary affections' were insufficient to capture the degree of variety and activity evident in the world. If a reality behind the appearances is to be specified then there is a fundamental question of whether properties operative at that level are a subset of observable properties and if so, which subset. Arguments involving some macroscopic-microscopic analogy of the kind invoked by Boyle were not up to the task and I believe that champions of them such as Mandlebaum and Newman have underestimated their problematic character.

There is more to be said about the way in which Boyle marshalled knowledge of the phenomena to defend his mechanical matter theory. As Newman rightly insists, the main area in which Boyle looked for empirical support for his mechanical philosophy was chemistry. I delay most of my discussion of that particular topic to Chapter 8, which is devoted to the emergence of modern chemistry. Empirical support from areas other than chemistry is discussed later in this chapter. Before exploring that issue further, it is necessary for me to investigate in some detail the character of the knowledge of the phenomena that Boyle was able to invoke. That knowledge typically took the form of experimental knowledge, much of it of Boyle's own devising.

6.5 Boyle's Experimental Science as Distinct from the Mechanical Philosophy

From the late 1650s onwards Boyle conducted experiments extensively and published the results of his efforts in tracts that he called 'histories', 'experimental essays' and 'new experiments'. He referred to the knowledge acquired by means of his experiments as 'matters of fact', 'physiology' and as knowledge of the phenomena.[10] I will refer to this body of work as Boyle's experimental science. In his articulation, appraisal and defence of that work Boyle distinguished between experimental science and the mechanical philosophy. In this section I focus on the character and status of the body of knowledge that Boyle considered to have established by experiments as Boyle himself construed them.

In 1661 Boyle published a collection of 'experimental essays' based on experimental work he had conducted at Oxford during the previous 5 years. In the first of these, 'A proemial essay . . . with some considerations touching experimental essays in general', Boyle (2000, Vol. 2, pp. 9–34) spelt out the character of the knowledge he believed himself to have produced through his experimental work. There are also scattered references bearing on the issue in his unpublished papers. Especially significant is an unfinished 'Essay of various degrees or kinds of ye knowledge of natural things'.[11] From these sources and others it is possible to construct a picture of Boyle's view on the status of knowledge established by experiment as distinct from the matter theory codified in his mechanical philosophy.

According to Boyle (2000, Vol. 2, p. 21) there is a 'scale, or series of causes' and corresponding 'degrees of explication'.[12] Highest in the scale of natural causes are the truly mechanical causes stemming from the motions of minima or atoms of matter, characterised in terms of the primary affections. The most fundamental explanations are those that invoke such causes. Lowest in the scale of causes are the most readily accessible causes, such as the weight of a stone invoked to explain its fall. An exemplification of Boyle's scale of causes is the following. We can explain the mercury level in a barometer by appealing to air pressure. We then move up the scale to explain pressure by invoking the elasticity (the 'spring') and weight of air. Perhaps the elasticity of air can be explained by appeal to the elasticity of its component particles and so on until we reach the highest level, where elasticity of the particles is explained by appeal to the shapes, sizes and motions of portions of universal matter.

Boyle acknowledged that the highest level of causes and the fundamental explanations were difficult, if not impossible, to arrive at, so that 'we may aspire to, but must not always require or expect, such a knowledge of things as is immediately derived from first principles'.[13] Consequently, explanations in experimental science typically involve appeal to 'subordinate principles' and 'intermediate causes'.

> And, indeed, there are oftentimes so many subordinate Causes between particular Effects and the most General Causes of things that there is left a large field, wherein to exercise Men's Industry and Reason, if they will but solidly enough deduce the Properties of things from more general and familiar Qualities, and also intermediate Causes (if I may so call them) from one another. (Boyle, 2000, Vol. 2, p. 23)

Boyle listed gravity, fermentation, springiness and magnetism amongst the subordinate principles. Indeed, he made it plain that most of the science of his day, including his own chemistry and pneumatics, was to be regarded as offering knowledge of intermediate rather than first, that is mechanical, causes.

> Of the subordinate or intermediate causes or theories of natural things, there may be many: some more and some less remote from the First Principles and yet each of them capable to afford a just delight and useful instruction to the mind. And these we may call for distinction sake the Cosmographical, the Hydrostatical, the Anatomical, the Magnetical, the Chymical and other causes or reasons of Phenomena as those which are more immediate (in our way of estimating things) than ye general and Primordial causes of natural effects.[14]

Boyle acknowledged that it is 'very fit and highly useful, that some speculative wits, well versed in mathematical principles and mechanical contrivance' should speculate about fundamental mechanical explanations of phenomena, but he also expressed his reservations about those involved in such an enterprise since 'they oftentimes give forced and unnatural accounts of things rather than not to be thought to have deriv'd them immediately from the highest principles'. What is worse, 'they despise and perhaps too condemn or censure all yt knowledge of the works of nature yt Physicians, Chymists, and others pretend to, because they cannot be clearly and easily deduc'd from ye doctrines of Atoms, or ye Catholick Laws of motion'.[15] In Boyle's view, explanations that appealed to intermediate causes and were substantiated by experiment were not to be despised. They might fall short of fundamental or ultimate explanations, but they were genuine and useful explanations for all that.

> He gives some Reason, why Stones and Iron, and all other heavy bodies, will swim in Quick-silver, except Gold, which will sink in it; that teaches that all those other bodies are *in specie* (as they speak) or bulk for bulk, lighter than Quick-silver, whereas Gold is heavier. He, I say, may be allow'd to have render'd a Reason of a thing proposed, that thus refers the Phaenomenon to that known Affection of almost all Bodies here below, which we call Gravity, though he does not deduce the Phaenomenon from Atoms, nor give us the cause of Gravity; as indeed scarce any Philosopher has yet given us a satisfactory Account of it. So if it is demanded, why, if the sides of a blown Bladder be somewhat squeez'd betwixt one's hands, they will, upon the removal of that which compress'd them, fly our again, and restore the Bladder to its former figure and dimensions; it is not saying nothing to the purpose, to say, that this happens from the spring of those Aerial Particles, wherewith the Bladder is fill'd, though he, that says this, be not perhaps able to declare, whence proceeds the Motion of Restitution, either in a Particle of compress'd Air, or any other bent spring. (Boyle, 2000, Vol. 2, p. 22)

The aim to establish intermediate explanations by way of experiment, then, is legitimate useful and productive. What is more, the strong implication is that the search for fundamental atomic explanations is typically futile and unproductive. When Boyle wrote that 'the most useful notions we have, both in Physics, mechanics, Chymistry, and ye medicinal art, are not deriv'd from ye first principles, but from intermediate Theories, notions and rules' he was in effect saying that experimental science is able to proceed productively independently of the dictates of the mechanical philosophy.[16] He insisted that it would be 'backward to reject or despise all explications that are not immediately deduced from the shape, bigness and motion of atoms or other insensible particles of matter' and urged that those that persist with that mechanical programme 'undertake a harder task than they imagine'.[17]

Boyle employed experiments to support matters of fact and was quite explicit about what could be achieved by doing so. I cannot improve on the account of Boyle's reflections on the capabilities of experiment for establishing matters of fact given by Rose-Mary Sargent (1995, pp. 159–180). I summarise key points she attributes to Boyle as follows: The normal course of nature involves a complicated combination of multiple causes producing their effects in ways that are difficult to fathom. The artificial conditions of an experiment enable the situation to be simplified, individual causes isolated and their effects investigated.[18] Chemical changes, for example, are best investigated by preparing pure substances and engineering their combination in simple, controlled conditions. The purity of chemicals is best accomplished by a range of tests rather than a single one. Indeed, it is always advisable to support matters of fact by a range of tests preferably involving a variety of instruments. The ability to purposefully reproduce an experimental effect is important, but of great significance is the ability to vary the conditions under which causes can be made to yield their effects. An experiment presupposes knowledge of the experimental situation. The purification of a chemical substance requires knowledge of chemistry sufficient to dictate the procedures to be carried out and the tests to be administered. Just because experimental inferences presuppose knowledge they are fallible. Matters of fact are never final, although a sound experimental case is necessary to revoke a claim that has been supported by a range of independent tests.[19] An experimental claim is supported by a range of independent evidence in a way similar to that in which a claim in a court of law is substantiated by evidence from independent witnesses. In either case, to the deny the claim supported is to accept unexplained coincidences

Boyle's pneumatics had good claims to the status of matters of fact established by experiment. Boyle claimed that air has a pressure arising from its weight and its 'spring' and appealed to it to explain a range of phenomena involving barometers, syringes and the like. Boyle supported his claims by a range of evidence, the most novel and striking involving use of his air pump. Over a decade or more Boyle improved and modified his air-pump experiments and augmented them with other experiments in response to alternative explanations of his results offered by critics. Acknowledging the challenge to his 'grand hypotheses' concerning the weight and spring of the air Boyle proceeded to defend them, and the case he made was a detailed experimental one whose status he compared to the case made by Harvey for the circulation of the blood.[20]

The status of Boyle's pneumatics as an example of experimental knowledge, as distinct from the kind of knowledge that the mechanical philosophy involved and also from what Boyle termed metaphysics, is brought out in Boyle's dispute with Hobbes. The latter insisted that the receiver evacuated by Boyle's air pump was occupied by some subtle matter rather than coarse air, and attributed pressure to the circulation of that subtle matter. He also criticised Boyle for attributing elasticity to air without being able to explain it. According to Hobbes, the mechanical philosopher, attributing elasticity to air was tantamount to admitting that it could move itself.[21] Boyle responded in several ways, all of which involved retreating to what he regarded as 'matters of fact' that could be established by experiment. He refrained from taking a stand on the possibility of a vacuum and on whether his evacuated receiver or the space above the mercury in a barometer constituted one. He regarded such an issue as 'metaphysical' because not susceptible to experimental investigation.[22] He did claim that his evacuated receiver was relatively free of air and was able to give a range of experimental evidence for that claim. He freely admitted he had not given a mechanical explanation of the 'spring' of the air but insisted that he had shown experimentally that air has a spring and that it can be appealed to in order to explain the behaviour of barometers and various phenomena revealed by use of his air pump and otherwise.[23]

Another instructive dispute involved Spinoza's challenge to Boyle's interpretation of his experiments on nitre (potassium nitrate). The dispute was carried on in correspondence between Spinoza and Oldenburg, with the latter's exposition of Boyle's view constructed in consultation with Boyle.[24] In those experiments Boyle transformed nitre into 'fixed nitre' (potassium carbonate) by plunging red-hot carbon into it and then recovered the nitre by adding spirit of nitre (nitric acid). Spinoza criticised Boyle for not substantiating a truly mechanical account of the process and attempted to repair the deficiency. He claimed that nitre and spirit of nitre are in fact composed of the same matter, the difference lying in the rapid motion of the underlying matter in the case of spirit of nitre. Boyle declined to speculate about a precise mechanism. He claimed to have shown that nitre can be broken into and built up from fixed nitre and spirit of nitre and urged that this posed problems for the appeal to substantial forms. (The latter point did not impress Spinoza, who already took the dismissal of substantial forms for granted.)

Boyle's understanding of experimental knowledge makes it quite different from what would nowadays be described as some extreme positivist or empiricist ideal, according to which facts are given and able to speak for themselves in some straightforward way. Establishing experimental facts involves purposeful activity, guided by knowledge of the situation investigated. Boyle (2000, Vol. 2, p. 14) made it clear in his 'Proemial essay' that such work often needed to be guided by hypotheses and that appropriate concepts (which Boyle referred to as 'notions') necessary for the framing of such hypotheses needed to be fashioned (2000, Vol. 2, p. 20). He referred to the spring and weight he attributed to air as 'grand hypotheses' but by the end of the series of experiments involved in resolving disputes with critic such as Hobbes he was claiming them as 'matters of fact'.

Boyle advocated and put into practice means for establishing experimental matters of fact by putting them to a range of experimental tests and by ruling out alternatives and possible sources of error. He put the idea into practice to great effect in his pneumatics. Experimental knowledge is not established infallibly because there may remain sources of error unknown and hence untested for. However, from the new perspective, an objection to a piece of experimental knowledge should take the form of one that opens up the possibility of some new experimental test. A generalised point about the fallibility of experimental reasoning will not do, nor will an appeal to untestable possibilities (such as positing an ethereal medium in the receiver of Boyle's pump with no testable consequences). In evaluating experimental claims, the results of tests are primary and the consent of the community is secondary. The aim is to produce knowledge rather than consensus about knowledge.[25]

Experimental knowledge can be strongly established, although not infallibly, as matter of fact. At the other extreme Boyle placed metaphysics that could not be tested by experiment at all. This leaves the question of the status of the mechanical philosophy. I have given ample evidence that Boyle distinguished it from experimental knowledge. Nevertheless, Boyle did not leave room for doubt that he considered the mechanical philosophy to be empirically supported in some way. He distinguished between it and metaphysics. Boyle's view on the empirical status of the mechanical philosophy is the issue discussed in the next section.

6.6 Empirical Support for the Mechanical Philosophy

The characterisation I have offered of Boyle's mechanical philosophy and his experimental science suggests a distinct lack of fit between the two. Mechanical atomism was remote from what could be tested experimentally whilst Boyle's experimental science was required to pass stringent experimental tests. Boyle acknowledged and exploited a distinction between untestable metaphysics and his experimental science but did not conclude that his mechanical philosophy constituted metaphysics. Rather, he claimed that his mechanical atomism was susceptible to and in need of experimental support. This is reflected in the fact that he referred to the mechanical philosophy as a hypothesis. Early in the 'Origin of forms and qualities' Boyle (2000, Vol. 5, p. 296) explicitly made the point that 'by the Lovers of real Learning it is very much wish'd, that the Doctrines of the new Philosophy (as 'tis called) were back'd by particular Experiments, the want of which I have endeavour'd to supply'.

Not only did Boyle construe the mechanical philosophy as in need of experimental support, but also he claimed to have supplied a good deal of such support. He repeatedly claimed that the experiments that he appealed to in his chemistry, such as his experiments on nitre, lent support to his mechanical philosophy. Towards the end of his 'Excellency of the mechanical hypothesis' Boyle (2000, Vol. 8, p. 114) clearly expressed the view that experimental support for the mechanical philosophy was on the increase.

> [T]he Sagacity and Industry of modern Naturalists and Mathematicians, having happily applied them [mechanical principles and explications] on several of those difficult *Phenomena* (in *Hydrostaticks*, the practical part of *Opticks, Gunnery, &c.*) then before were referr'd to occult Qualities, 'tis probable that, when this Philosophy is deeplier searched into and further improv'd, it will be found applicable to the solution of more and more of the *Phenomena of Nature*.

> And therefore, if the Mechanical Philosophy go on to explicate things Corporeal at the rate it has of recent years proceeded at, 'tis scarce to be doubted, but that in time unprejudic'd persons will think it sufficiently recommended by its consistency with it self, and its applicableness to so many Phenomena of Nature.

The general problem with claims such as these, implicit in the discussion of the previous section, is that the scientific successes that Boyle invokes in support of the mechanical philosophy do not involve mechanical explanations in the strict sense because they involve appeal to such things as weight, elasticity, the reflecting and refracting properties of materials and so on rather than involving reductions to the primary affections only. This is precisely the point Boyle makes when he invokes the scale of causes, with experimentally accessible ones near the bottom of the scale and remote mechanical ones at the top. It is exemplified in Boyle's response to criticisms from Hobbes and Spinoza that we have described above. If Boyle's pneumatics and his experiments on the analysis and synthesis of nitre do not provide strict mechanical explanations then how can those examples of experimental knowledge be invoked to provide empirical support for the mechanical philosophy in the strict sense?

I mention one more instance of the problematic character of Boyle's insistence that his experimental matters of fact provided support for the mechanical philosophy before offering a solution to the puzzle. In an essay 'Of the imperfections of the chymist's doctrine of qualities' Boyle criticises attempts by chemists to reduce the object of their study to the action of the three principles, mercury, salt and sulphur. One of the key objections raised by Boyle (2000, Vol. 8, p. 166) is to the effect that, even if their attempts were in some sense empirically successful, the theory of the chemists would be inadequate because it leaves the properties of the three principles themselves, such as the fusibility and inflammability of sulphur or the weight and solidity of salt, unexplained. 'For even when the explications seem to come home to the phenomena, they are not primary and, if I may so speak, *fontal* enough'. This stand of Boyle seems to be in conflict, for example, with his stand on the status of his pneumatics, where he insists that his explanations have merit in spite of the fact that weight, spring and pressure of air remain unexplained. The properties invoked by Boyle in his experimental science are no more 'fontal' than the principles of the chemists.

I suggest the way to dissolve these apparent tensions in Boyle's writings is to respect his distinction between the mechanical philosophy and his experimental science and to recognise that, whilst he sought empirical support for the mechanical philosophy, it was of a different, and weaker, kind than the stringent kind of support he demanded of experimental knowledge. In the previous section I documented Boyle's distinction between the mechanical philosophy implicated at the top of the scale of causes and experimental knowledge of causes lower down the scale. It remains for me to locate in Boyle's writings the idea that the kind of empirical support sought for the mechanical philosophy was different in kind from that demanded of experimental knowledge.

Boyle did not claim that his mechanical philosophy qualified as a matter of fact. In the context of his pneumatics, for instance, he was quite explicit on the point that to show that air has weight and a spring and to explain a range of phenomena by appeal to it was one thing, whereas to give an explanation of the weight and spring of air by appeal to the configurations and motions of underlying corpuscles was another. He claimed to have done the former while acknowledging his inability to do the latter. The whole tenor of his 'Proemial essay' in which Boyle (2000, Vol. 2, p. 14) introduced and explained the purpose of his early empirical work is that there is a distinction to be drawn between matters of fact and philosophical systems such as those articulated by Gassendi, Descartes and Aristotle. Some 15 years later, in a tract on the 'mechanical origin or production of divers particular qualities' Boyle commented specifically on the relation between matters of fact and his own mechanical philosophy. In his view the 'corpuscular doctrine' could be backed up by appeal to experiment to the extent that possible corpuscular mechanisms could be proposed that served to explain the phenomena or render them compatible with the mechanical philosophy. The case could be further strengthened by pointing to the difficulty of reconciling the phenomena with alternative hypotheses such as those involving appeal to Aristotelian substantial forms or real qualities. The more matters of fact that could be accommodated by the mechanical philosophy, and the greater the problems posed by them for alternative hypotheses, the stronger the case for it. However, Boyle did not consider the case to be strong enough to qualify that philosophy as a matter of fact. Showing that corpuscular mechanisms could be contrived capable of explaining the phenomena was not sufficient to establish that the contrived mechanisms corresponded to the true ones.[26] I quote Boyle's own words on the distinction, central to my argument, between the status of matters of fact and the mechanical philosophy.

> There is yet another way of arguing in favour of the *Corpuscular* Doctrine of Qualities, which, though it do not afford direct proofs of its being the best *Hypothesis*, yet it may much strengthen the Arguments drawn from other Topicks, and thereby serve to recommend the Doctrine it self. For, the use of an *Hypothesis* being to render an intelligible account of the Causes of the Effects or Phaenomena propos'd, without crossing the Laws of Nature or other Phaenomena, the more numerous and the more various the Particulars are, whereof some are *explicable* by the assign'd Hypothesis, and some are *agreeable* to it, or at least are nor dissonant from it, the more valuable is the Hypothesis, and the more likely to be true. For 'tis much more difficult, to find an *Hypothesis* that is not true which will suit with *many* Phaenomena, especially if they be of various kinds, than but with *few*. And for this Reason, I have set down among the Instances belonging to particular Qualities some such experiments and observations, as we are now speaking of, since, although they may be not direct proofs of the preferableness of our Doctrine, yet they may serve for Confirmation of it. (Boyle, 2000, Vol. 8, p. 325)

Boyle proceeds to illustrate his point drawing an analogy with clocks. Boyle raises the (no doubt apocryphal) story of the Chinese who, when first confronted with a working clock, presumed it to be a kind of animal. Boyle explains that offering an explanation of the clock's behaviour by contriving some possible mechanism involving wheels, springs and so on is sufficient to dispel the belief that the clock must be animated, even though the postulated mechanism may not correspond to that in place in the clock in question. In like manner, contriving a possible mechanism that serves to explain some phenomenon provides some grounds that the phenomenon is at bottom mechanical. The proposed mechanism may not be the true one, but the fact that it is a possible one at least shows that invoking substantial forms is not necessary.[27]

Boyle is here responding to the claim that the mechanical explanations, whilst they can be effectively applied to mechanisms such as clocks and watches, cannot possibly be extended to explanations of all the phenomena of nature. 'To remove therefore this grand Prejudice and Objection', Boyle (2000, Vol. 8, pp. 326–327) writes, 'which seems to be the chief thing that has kept off Rational Inquirers from closing with the Mechanical Philosophy, it may be very conducive, if not sufficient, to propose some Mechanical accounts of Particular Qualities themselves as are intelligible and possible, and are agreeable to the *Phenomena* whereto they are applied'. Given the phenomena, the mechanical philosophy can be made plausible and even probable to the extent that hypothetical mechanisms can be devised that are sufficient to account for those phenomena. Such a mode of argument does not constitute 'direct proof' of the mechanical philosophy, but it does support that hypothesis to some degree and at least makes appeal to such things as sympathies, antipathies and substantial forms unnecessary.

When Boyle raised the issue of empirical support for the mechanical philosophy in 'Excellency of the mechanical hypothesis' he drew an analogy with code-breaking. Just as one can claim to have found the correct key to a cipher by showing how application of that key makes sense of a variety of encrypted messages, so is the mechanical philosophy supported by being rendered compatible with a variety of phenomena. Immediately following this analogy comes the passages referred to above in which Boyle claims empirical support for the mechanical philosophy.

The distinction between the strong mode of support implicit in Boyle's experimental science and the weaker notion he invoked in the context of his mechanical philosophy can be summed up by recognising that the former involves confirmation by empirical evidence whereas the latter involves mere accommodation to evidence. Arguments from coincidence work for the former in a way they do not for the latter. An experimentalist can hope to provide a range of evidence for a claim to the extent that denying that claim involves admitting a remarkable coincidence. Such arguments lose their force in the case of mechanisms that have been contrived to fit phenomena established by other means. It is no coincidence that a range of mechanisms fit the phenomena if they have been contrived to do so. Here, Boyle's analogy with code-breaking is deceptive. The case that the correct key to a cipher has been found rests on the fact that that one key naturally applied to a range of cases yields intelligible messages. The case would lose its force if the key needed adjusting or amplifying to meet the needs of each individual case.

Another mark of the distinction between accommodation and confirmation that can be discerned in Boyle's work is the fact that he is content to offer more than one, mutually inconsistent, mechanical accommodations of a phenomenon. For instance, as we shall have occasion to discuss in more detail in Chapter 8, Boyle (2000, Vol. 8, p. 470) suggests that the transformation in the properties of mercury sublimate when it is combined with more mercury can be explained, either by the sharp edges he attributes to particles of sublimate be sheathed as a result of the combination with mercury or that the corpuscles of sublimate be arranged in bundles with their sharp edges pointing inwards. This contrasts with Boyle's attitude to alternative explanations in the context of his experimental science. In his pneumatics, for example, he goes to great pains to conduct experiments to counter alternative explanations to his own. Two accommodations are better than one so long as mere accommodation is the aim. A stronger sense of empirical support is required of experimental knowledge.

With the distinction between the confirmation of and accommodation to the evidence in hand, there is scope for removing some of the apparent tensions in Boyle's writings that I have pinpointed earlier in this section. When Boyle criticises the chymists for employing their principles in a way that is not sufficiently 'fontal' he is speaking as a mechanical philosopher who is seeking ultimate explanations at the top of the series of causes. When he defends his appeal to weight, spring and pressure in his pneumatics he is speaking as an experimental scientist. When Boyle, in the 'Excellency of the mechanical hypothesis' claims that that hypothesis had empirical support that was increasing, his claim makes most sense if the support in question is of the weak kind appropriate for supporting ultimate claims about the structure of matter.

I have done my best to free Boyle's remarks about empirical support for the mechanical philosophy from inconsistency by highlighting the distinction Boyle himself draws between that philosophy and experimental knowledge and the mode of support appropriate in the two domains. However, I insist that, if the mechanical philosophy is interpreted in the strict sense summarised in Section 6.2 of this chapter, then support for it, even in the weak sense identified in the previous section, was scant. Boyle acknowledged that neither he nor any other of his fellow mechanical philosophers were able to devise mechanisms capable of explaining common properties such as weight and elasticity. Boyle cannot claim advances in hydrostatics, optics and gunnery as support for the mechanical philosophy in the strict sense, as he seems to do in the passage from the 'Excellency of the mechanical hypothesis' quoted early in this section until he has constructed strict, albeit hypothetical, mechanical mechanisms able to account for the properties, such as weight, involved in those sciences. Not even colliding billiard balls, an archetypal example of a mechanism, can be accommodated by the mechanical philosophy in the strict sense once it is realised that the colliding balls must be elastic to some degree.[28] Some of my critics insist that I am constructing a problem of my own devising here by imposing on Boyle's mechanical philosophy a sense of mechanical that is inappropriately strict. I take up this issue in the next section but one.

6.7 The Lack of Fertility of the Mechanical Philosophy

Suppose my claims about the lack of experimental support for the mechanical philosophy in the strict sense is accepted. There remains the possibility that that philosophy proved its worth by usefully guiding experimental science. Was Boyle's mechanical atomism fertile insofar as it encouraged lines of experimental enquiry that bore fruit? In this section I argue that it was not, nor was its general character conducive to its being so.

As we have seen, Boyle's mechanical philosophy was to be supported by contriving mechanisms able to account for the phenomena. This implies that the phenomena be known prior to the construction of the mechanisms and hence independent of them. A theme in the opening pages of Boyle's characterisation and promotion of experimental knowledge in his 'Proemial essay' is that most attempts at constructing grand systems of natural philosophy are deficient because based on inadequate knowledge of the phenomena. This implies that the latter knowledge is independent of and prior to the mechanical explanation. Later in the essay Boyle makes the point that research must necessarily start at the bottom of the scale of causes and work up.

> And though it must not be denied, that it is an advantage as well as a satisfaction, to know in general, how the qualities of things are deducible from the primitive affections of the smallest parts of matter; yet whether we know that or no, if we know the qualities of this or that body they compose, and how it is disposed to work upon other bodies, or be brought on by them, we may, *without ascending to the top of the series of causes, perform things of great moment*, and such as, without the diligent examination of particular bodies, would, I fear, never have been found out *a priori*, even by the most profound contemplator.[29]

Boyle (2000, Vol. 2, p. 12) does, nevertheless, raise the possibility that a philosophical system, of which I take his own mechanical philosophy to be an example, might guide and inspire the search for new experimental knowledge in its support.

> For such kind of Writings [compleat Bodies or Systems of Physiology], if their Authors be (as for the most part they are) subtle and inquisitive men, there may be very good use, not so much by their gratifying the Intellect with the plausible account of some of Nature's Mysteries; as because on the other side their Writers, to make good their new Opinions, must either bring New Experiments and Observations, or else must consider those that are known from a new Manner, and thereby make us take notice of something in them unheeded before; and on the other side, the curiosity of Readers, whether they like or disapprove the Hypothesis propos'd, is wont to be thereby excited to make trial of several things, which seeming to be Consequences of his new Doctrine, may by their proving agreeable or repugnant to experiment either establish or overthrow it.

There is a key feature of Boyle's mechanical atomism that stands in the way of interpreting it as capable of guiding experiment in the fruitful way depicted in the above passage. It is a feature that Boyle himself stresses when responding to the charge that the stark world of impenetrable atoms possessing only shape, size and motion is incapable of accounting for the vast variety of observable phenomena. The response that Boyle (2000, Vol. 8, p. 113) has to the objection is that the mechanical philosophy is so flexible, insofar as it is free to postulate atoms with innumerable shapes, sizes, arrangements and motions, that it is possible to accommodate it to phenomena of 'as great a variety as need be wish'd for, and indeed a greater than can easily be so much as imagin'd'. I stress the flexibility that Boyle presents as a positive attribute of his matter theory by quoting at length from his 'History of Particular Qualities' where he is responding to the 'Grand difficulty' that a great variety of phenomena should be derived simply from particles of matter in motion. A large part of his response stresses the extent to which his mechanical atomism is 'fertile' and 'comprehensive' on account of the vast variety of shapes, sizes, arrangements and motions that the mechanical philosopher is free to invoke.

> And so though Figure be one of the most simple modes of Matter; yet it is capable, *partly* in regard of the surface of the figur'd Corpuscle (which may consist of Triangles, Squares, Pentagons &c.) and *partly* in regard of the shape of the body itselfe, which may be either flat like a cheese, or Lozenge; or Spherical like a Bullet; or Elliptical, almost like an Egge; or Cubical like a Dye; or Cylindical like a rolling-stone; or Pointed like a Pyramid, or Sugar-Loaf; Figure I say, though but a simple mode, is upon those and other scores, capable of so great a multitude of differences, that is concerning Them, and their Affections, that *Euclid, Apollonius, Archimedes, Theodosius, Clavius*; and later writers then he, have demonstrated so many Propositions. And yet all the hitherto nam'd Figures are almost nothing to those irregular Shapes, such as are to be met with among Rubbish, and among hooked and branched Particles &c. that are to be met with among Corpuscles and Bodies; most of which have no particular Appellations; their Multitude and their Variety having kept men from enumerating them, and much more from particularly naming them.
>
> To which let me add, that these Varieties of Figure, and Shape, do also serve to modifie the Motion, and other Affections of the Corpuscles endowed with them, and of the compounded Body whereof it makes a part. (Boyle, 2000, Vol. 6, p. 276)

Boyle proceeds to document the wide diversity of possible motions at even greater length.

There are two points I wish to make about this emphasis by Boyle on the flexibility of his mechanical philosophy. The first is that this flexibility detracts from the merit and significance of the ability of the mechanical philosophers to adapt their system to the phenomena. Boyle (2000, Vol. 8, p. 114) explicitly claims that the mechanical principles 'being so general and pregnant that among things corporeal there is nothing *real* (and I meddle not with *Chymerical* beings, such as some of *Paracelsus*'s) that may not be deriv'd from, or be brought to a subordination to, such comprehensive Principles'. If Boyle is able to specify in advance of the discovery of phenomena that the mechanical philosophy will be able to accommodate them, then instances of such adaptations can hardly be seen as significant confirmations of that philosophy. A theory that is adaptable to everything explains nothing. (Here Popperians prick up their ears.) Incidentally, the flexibility of the mechanical philosophy notwithstanding, its supporters were not able to devise mechanisms to account for gravity and elasticity at their own admission.

Closely allied with the foregoing point is the observation that just because of the freedom a mechanical philosopher like Boyle had to attribute shapes, sizes, arrangements and motions to atoms to suit the purpose in hand, the matter theory in its generality was not capable of predicting any phenomena and so not capable of guiding experimental investigation. This reinforces the point made above that knowledge of the phenomena needed to be in place before mechanical explanations of them could be contrived. Knowledge of Boyle's intermediate and subsidiary causes was needed prior to the possibility of reducing them to mechanical causes by contriving mechanisms. Claims to the effect that Boyle's mechanical philosophy in the strict sense was fertile and fruitful were unfounded as far as the production of experimental knowledge is concerned.

Newman (2006) might well object to the above assessment on the grounds that it neglects the main area in which there was a fruitful inter-relation between the mechanical philosophy and experiment, namely, chemistry. On this point he would have had the support of Boyle. I re-iterate my intention of treating the case of chemistry in detail, in Chapter 8.

6.8 The Various Senses of ‘Mechanical’

In the foregoing I have attributed to Boyle an adherence to a matter theory that I have referred to as ‘the mechanical philosophy in the strict sense’ and have insisted on a distinction between it and Boyle’s experimental science. There are senses of ‘mechanical’ other than the strict sense. Indeed, there are common usages of ‘mechanical’ that have stronger claims to the appellation than the strict sense, which is ‘mechanical’ in a technical sense that is strained and artificial compared with the common sense. As we have seen, according to the strict sense of ‘mechanical’ a clock is not a mechanism insofar as the weight of the pendulum bob that drives it has not been reduced to the shapes, sizes, arrangements and motions of particles of impenetrable matter. Is it not an unusual usage of ‘mechanical’ that denies the right of a clock to be called such? Surely a clock is an archetypal mechanism as far as common usage is concerned. Given this, the possibility has to be entertained that my key claims about the distinction between the mechanical philosophy and experimental science is an artificial consequence of the very strict and technical way I interpret the mechanical philosophy to be ‘mechanical’. If the mechanical philosophy is interpreted in some common, as opposed to the strict, sense, then perhaps that philosophy and the new experimental science championed by the likes of Boyle form a coherent whole just as Boyle implied that it did. My critics mentioned in Footnote 7 argue along these lines.

There certainly are common senses of ‘mechanical’ that differ from the strict sense. Clocks, watches and levers, and machines or engines generally, are mechanisms in the common sense. A feature of such mechanisms is that their behaviour is accounted for by reference to the inter-relations of their parts. Insofar as the workings of a clock involves the transmission of motion from that of the pendulum bob to the hands via inter-locking gear wheels, no appeal to something over and above the inter-related parts, such as substantial forms, is called for. As Newman (2006, p.186) points out, there is a long tradition of such mechanical explanations going back to the Hellenistic engineers of Antiquity. Boyle (2000, Vol. 2, p. 87) did use ‘mechanical’ in this common sense, and even cited the analogy between the explanations involved in his matter theory and those to explain the working of mechanical engines as the justification for the term ‘mechanical philosophy’. He also employed a common sense of mechanical when he described the spring and weight of the air as ‘mechanical affections’.

Another sense of mechanical, again with a long history, translates roughly as 'artisanal'. The mechanical arts were taken to include, not only the manufacture of mechanical devices, but also alchemy. According to this usage, the new experimental science championed by Boyle could be said to be mechanical insofar as it involved and encouraged artificial intervention in nature as a means of understanding it.

So why do I not heed my critics, drop my insistence on interpreting Boyle's mechanical philosophy in the strict sense and substitute for it a common sense of mechanical which would enable the new experimental science to be embraced as part and parcel of the mechanical philosophy just as its proponents often seemed to imply and many historians since then have assumed?

My main reason is this. When Boyle articulates and defends the mechanical philosophy, then, as we have seen, it is the strict sense of mechanical that is involved. What is more, most of the arguments for that philosophy, that the primary affections are simple, clear and intelligible and not in need of further explanation, do not work if mechanical is interpreted in the common sense. Size and shape as applied to pieces of universal matter may be perfectly clear and in need of no further explanation, but the mechanical philosophers, including Boyle, did not think this to be the case for weight and elasticity.

Boyle's mechanical philosophy was designed to eliminate, not just some extreme scholastic version of substantial form, but Aristotelian and scholastic form in general. His major essay outlining the mechanical philosophy aimed to give the 'origin of forms and qualities' generally, not substantial forms only. Anstey (2000, pp. 153–154)) rightly insists, with the approval of Newman (2006, p. 178, fn.), that Boyle's mechanical philosophy was, first and foremost, a theory of qualities. Boyle made it abundantly clear that the aim of his mechanical philosophy was to reduce qualities to the 'primitive affections' possessed by portions of matter that they necessarily possess by virtue of being such. In an essay devoted to the 'history of particular qualities', Boyle (2000, Vol. 6, p. 267) distinguished the primitive affections from all other qualities, requiring that the former be reduced to the latter.

> And there are some other Attributes, namely *Size, Shape, Motion* and *Rest*, that are won't to be reckon'd among Qualities, which may more conveniently be esteemed the *Primary Modes* of the parts of Matter; since from these simple Attributes, or Primordial Affections, all the Qualities are deriv'd.

As I have already noted in my summary of Boyle's case for his mechanical philosophy in Section 6.3 of this chapter, Boyle could not accept the intelligibility of the idea that qualities, such as the whiteness of a wall or any other quality other than the 'primary affections', which were something other than 'Matter or modes of Matter or immaterial substances', could be added to matter to confer on it the property in question. Boyle (2000, Vol. 5, p. 308) was intent on avoiding the assumption that 'there are in Natural Bodies store of *real Qualities* and other *real Accidents*, which not onley are no Moods of Matter, but are real Entities distinct from it'. Intermediate causes and subordinate principles involving properties such as the spring and weight of the air that played a crucial part in Boyle's experimental science are mechanical in a common sense. But to admit them into the mechanical philosophy in the strict sense is to undermine Boyle's main argument concerning the intelligibility of the latter.

The importance of the distinction between the common and strict senses of 'mechanical' and the importance of eliminating qualities other than the 'primitive affections' of matter from the mechanical philosophy in the strict sense can be brought out by attending to a general feature of the structural explanations that are involved in mechanical explanations in the common sense. As we have noted, such explanations involve explaining wholes in terms of the properties and inter-relations of their parts. Whether they are assumed implicitly or invoked explicitly, such explanations need to ascribe properties to the parts. Explanations of the workings of a clock that appeal to its mechanical structure presuppose the rigidity of gear-wheals and the weight of the pendulum bob. How are such properties to be regarded? From the point of view of the competing matter theories of the seventeenth century those properties can be attributed to the presence of forms or real qualities or they can be reduced to the primary affections of universal matter. As a mechanical philosopher Boyle was committed to the latter position. A third possibility is to recognise the need for appeal to non-ultimate 'subordinate causes' in science and to simply ignore the issue of an ultimate explanation of them. This is the course implicit in Boyle's experimental science.

Boyle's articulation of arguments for the mechanical philosophy does not make sense if mechanical is taken in the common rather than the strict sense. But there is more to it than that. The use to which Boyle put the mechanical philosophy outside of experimental science, especially in a theological context, required the strict rather than common sense of mechanical. For instance, the necessity of God's active intervention in nature can be persuasively argued for if nature is composed of inert pieces of matter possessing only shape, size and motion and becomes weakened by addition of properties such as weight and elasticity which, if attributed to matter primitively, has the consequence that matter can move itself, without the need for God's intervention. In putting his case for the active role of a deity in the workings of nature Boyle in effect makes explicit that his view requires mechanism in the strict rather than any weaker sense. Here are Boyle's own words:

> I shall next take notice, That Philosophers, who scorn to ascribe anything to God do often deceive themselves in thinking they have sufficiently satisfied our Enquiries, when they have given us the nearest and most immediate causes of some things; whereas oftentimes the assignment of those Causes is but the manifesting that such and such Effects may be deduc'd from the more Catholick affections of things, though these be not unfrequently as abstruse as the *Phenomena* explicated by them, as having onely their Effects more obvious, not their Nature better understood: As when, for instance, an account is demanded of that strange supposed Sympathy betwixt Quick-silver and Gold; in that we finde that whereas all other Bodies swim upon Quicksilver, it will readily swallow up Gold and hide it in its Bosom. This pretended Sympathy the Naturalist may explicate by saying, That Gold being the only Body heavier than Quick-silver of the same bulk, the known Laws of *Hydrostatikcs* make it necessary that Gold should sink in it and all lighter Bodies swim on it: But though the cause of this Effect be thus plausibly assign'd, by deducing it from so known and obvious affection of Bodies as Gravity, which every man is apt to think he sufficiently understands; yet will not this put a satisfactorie period to a severe Inquirer's Curiositie, who will perchance be apt to alledge, That though the Effects of Gravity indeed be very obvious, yet the Cause and

> Nature of it are as obscure as those of almost any *Phenomenon* it can be brought to explicate, and that therefore he that desires no further account desists too soon from his Enquiries, and acquiesces long before he comes to his Journey's end. And indeed, the investigation of the true nature and adequate cause of gravity is a task of that difficulty that, in spite of aught I have hitherto seen or read, I must yet retain great doubts whether they have been clearly and solidly made out by any Man. And sure, *Pyrophilus*, these are divers Effects in Nature, of which, though the immediate Cause may be plausibly assigned, yet if we further enquire into the Causes of those Causes, and desist not from ascending in the Scale of Causes till we are arriv'd at the top of it, we shall perhaps finde the more Catholick and Primary causes of Things to be either certain primitive, general and fix'd Laws of Nature (or rules of Action and Passion among the parcels of the Universal Matter); or else the Shape, Size, Motion, and other primary Affections of the smallest parts of Matter, and of their first Coalitions or Clusters, especially those endowed with seminal Faculties or properties; or (to dispatch) the admirable conspiring of the several parts of the Universe to the production of particular Effects; of all which it will be difficult to give a satisfactory Account, without acknowledging an intelligent Author or Disposer of things.[30]

Boyle here, in effect, makes explicit the point that experimental science employing mechanisms in the common sense falls short of explanations in the strict mechanical sense, and then proceeds to make use of mechanism in the strict sense to make his theological point. I defy my critics to make sense of this passage by sticking to an interpretation of the mechanical philosophy that interprets mechanical only in the common sense.

A further reason why it is inappropriate to interpret Boyle's mechanical philosophy only in the common sense, or to confuse that sense with the strict sense, is that allegiance to the common sense of mechanical is insufficient to distinguish Boyle from his opponents in the way that he clearly wished to be. None of the Aristotelians or chemists with whom Boyle took issue need have had a problem with structural explanation of levers that is mechanical in a common sense. Explanation of the properties of chemicals in terms of their components was central to an alchemical tradition embraced by Aristotelians such as Daniel Sennert, as we have seen. The funicular hypothesis proposed by the Aristotelian Franciscus Linus as an alternative to Boyle's explanation of pneumatical phenomena had as strong a claim as Boyle's appeal to the spring of the air to be mechanical in the common sense. As we have seen, the artisanal sense of mechanical involved in the mechanical intervention into nature as a means of understanding it had been pioneered by Aristotelian alchemists from the thirteenth century up until Sennert and beyond. If there was a sense of mechanical that distinguished Boyle from his opponents it involved the radical rejection of form involved in his mechanical philosophy in the strict sense.

There are two senses of mechanical, and both are evident in the work of Boyle. One is the fundamental matter theory that I refer to as the mechanical philosophy in the strict sense. The other involves a number of inter-related notions that I have grouped together under the term 'mechanical philosophy in the common sense'. There are ways in which the experimental science that blossomed in the seventeenth century can be construed as mechanical in the common sense. But if we are to construe the scientific revolution in terms of the emergence of this kind of knowledge then we must distinguish that change from the transformation in matter theory from an Aristotelian to a mechanical view. I claim that that change was distinct from the emergence of experimental science, was not supported by it and did not contribute significantly to it.

6.9 Boyle's Mechanical Philosophy and Experimental Support for Atoms

The attempt I have made to distinguish between Boyle's mechanical philosophy in the strict sense and his experimental science, insofar as it is successful, undermines any claim to the effect that Boyle's theory constituted important progress towards an atomic theory of matter that was supported by experiment. To the extent that Boyle's mechanical philosophy assumed permanent particles of universal matter with unchanging shape and size as the building blocks of the material world, it was an atomic theory. But I have argued that that philosophy was far removed from what could be tested and supported experimentally. Boyle did make significant contributions to experimental science to be sure, with his pneumatics constituting an outstanding example. But the claims Boyle established as experimental matters of fact did not invoke or imply atoms.[31]

Notes

1. The main source is Descartes (1983).
2. Peter Anstey (2000, pp. 153–154) suggests that the term 'mechanical philosophy' was first introduced by Henry More in the context of Descartes' philosophy. But there is no doubt that Boyle was largely responsible for bringing the term into wide currency.
3. In focussing on Boyle I do not intend to imply that he brought about the scientific revolution single-handed. My focus is due to the fact the Boyle was both a key articulator of the mechanical philosophy and a prolific experimenter. These two aspects of his work are inter-related in important and instructive ways, and Boyle himself explicitly discussed that relationship.
4. See Hunter (1995), Newman (1994, pp. 54–91) and Newman and Principe (2002).
5. Boyle (2000, Vol. 5, pp. 281–442 and Vol. 8, pp. 99–117). These two essays are reproduced in Stewart (1979, pp. 1–96 and 138–154) along with other key papers by Boyle concerned with an exposition and defence of the mechanical philosophy. For a detailed exposition of Boyle's mechanical philosophy see Anstey (2000)
6. Boyle's use of the term natural minima here is misleading, since Boyle's natural minima have more in common with the atoms of the Ancients than with the natural minima of the medieval Aristotelians. The latter were minima of the substance they were minima of, whereas Boyle's minima, like those of the Greeks, are composed of the one universal matter.
7. Anstey (2002a), Pyle (2002) and Newman (2006, pp. 175–189) criticised the specification and critique of Boyle's mechanical philosophy in Chalmers (1993) on the grounds that I give an inappropriately strict account of 'mechanical' ignoring the less strict usages at work in Boyle. I have defended myself against Anstey and Pyle in my (2002b). I revisit the issue and take Newman's critique on board in some detail later in this chapter.
8. In similar vein, Anstey (2002b, p. 627) writes that Boyle's seminal principles 'were required to explain those phenomena that appeared beyond the capabilities of the corpuscular hypothesis'.
9. On Boyle's debt to Sennert see Newman (1996).

10. These three terms occur in the opening pages of 'A proemial essay' (Boyle, 2000, Vol. 2, pp. 9–35) written as an introduction to a variety of 'experimental essays' or 'physiological essays' that followed.
11. Boyle (1990, Vol. vii, f184, reel 5, frame 189). I have consulted the microfilm version of Boyle's papers at the Royal Society and have included the reel and frame numbers in my references.
12. The quotation is from Boyle's 'Proemial essay'. The scale of causes is also invoked in Boyle's 'Essay containing a requisite digression, concerning those that would exclude the deity from intermeddling with matter', in Boyle (2000, Vol. 3, p. 245). Something like Boyle's scale is found in Francis Bacon's *Novum organum*, Book 1, CIV.
13. Boyle (1990, Vol. viii, f184, reel 5, frame 189), underlined in the original.
14. Boyle (1990, Vol. ix, f40–41, reel 5, frame 250).
15. The quotations are from Boyle (2000, Vol. 2, pp. 21–22) and Boyle (1990, Vol. viii, f184, reel 5, frame 189) respectively.
16. Boyle (1990, Vol. ix, f40, reel 5, frame 250).
17. Boyle (1990, Vol. viii, f166, reel 5, frame 168).
18. We noted in the previous chapter that Sennert had made a similar point.
19. When I prepared the section on experiment for the revised edition of my introductory text, *What is this thing called science?* (Chalmers, 1999, pp. 27–40) I was insufficiently aware that most of the points I was making had already been made explicit by Boyle.
20. For the use of the expression 'grand hypotheses' see Boyle (2000, Vol. 3, p. 125).
21. Hobbes, *Dialogus physicus*, translated in Shapin and Schaffer (1985, pp. 254–255).
22. For Boyle's categorisation of issues, such as the possibility of a vacuum or the infinite divisibility of matter, see 'Some specimens of an attempt to make chymical experiments useful to illustrate the notion of the corpuscular philosophy' (Boyle, 2000, vol. 2, p. 87).
23. The dispute between Boyle and Hobbes, and also other adversaries of Boyle, namely More and Linus, is dealt with in detail in Shapin and Schaffer (1985). Much of their discussion is conducive to my interpretation of the status of Boyle's experimentation.
24. For the relevant correspondence and discussion by the translators see Hall and Hall (1965, pp. 458–470). The dispute is discussed by McKeon (1928, pp. 137–152) and, more recently, by Antonio Clericuzio (1990, pp. 561–589).
25. Here I distance myself from contemporary sociologist of science and social constructivists who see the need to bring wide-ranging social elements involving consent into the evaluation of scientific knowledge, so that knowledge has the status of a social convention, a line that forms part of Shapin and Shaffer's study of Boyle's experimentation.
26. Here and elsewhere, Boyle (2000, Vol. 8, p. 327) invoked an analogy, common amongst mechanical philosophers, with the status of mechanisms contrived to explain the observed motions of the hands of a clock, where mere compatibility with observation is insufficient to establish a conjectured mechanism as the correct one.
27. For an analysis of various uses of the clock analogy by the mechanical philosophers see Laudan (1966).
28. If Boyle's natural minima were to rebound on collision as billiard balls do then they would change direction instantaneously and so be moving in two directions at the same time at the instant of collision. The problem was noted by Leibniz. The problems that collisions posed for the mechanical philosophers have been discussed by Alan Gabbey (1985).
29. Birch (1744, Vol. 1, p. 199, my italics). The corresponding passage in Boyle (2000, Vol. 2, p. 21) is very nearly the same.
30. The quotation is from Boyle's 'Requisite digression concerning those who would exclude the deity from intermeddling with matter' in Boyle (2000, Vol. 3, p. 245).
31. Once again, I acknowledge that I have yet to tackle the details of Boyle's chemistry where Newman, for instance, finds the strongest grounds for the existence of an experimentally-supported corpuscular theory. I focus on that issue in Chapter 8.