Chapter 7
Newton's Atomism and its Fate

Abstract Newton's *Principia* contained a science of mechanics that was able to withstand experimental tests in a demanding way. Newton also articulated and defended an atomic theory of the ultimate structure of matter. His atoms bore the marks of his science insofar as inertia was attributed to them. In other respects they were particles of universal matter with a given shape and size very much like those of Boyle. Newton speculated that there were short-range forces at the atomic level analogous to the force of gravity acting between gross bodies at sensible distances identified in his mechanics. As was the case with the mechanical philosophers who came before him, Newton's atomistic matter theory was accommodated to rather than confirmed by observation and experiment. His atomism did not and could not fruitfully guide his experimenting. Eighteenth-century attempts to develop Newtonian atomism similarly did not bear fruit.

7.1 Introduction

In crucial respects the atomic theory of matter that can be gleaned from Isaac Newton's works is an extension and refinement of Boyle's atomism. As such it suffered from similar shortcomings and tensions. Both in Newton's scientific practice and his own exposition of the methodology involved in it we find a clarification and elaboration of Boyle's conception of experimental science based on matters of fact. But we also find Newton defending a natural philosophy and a matter theory that goes beyond what can plausibly be construed as significantly confirmed by matters of fact. Insofar as Newton defended those broader claims he did so by taking for granted assumptions that were akin to those involved in the mechanical philosophy. The roots of Newton's natural philosophy fed into his theology just as was the case with Boyle.

The comparison of Newton with Boyle needs to be qualified in a major way. A crucial difference was the use Newton made of his new science of mechanics, and especially the notion of force that it involved. As mentioned in Chapter 6, a limitation of Boyle's mechanical philosophy was its failure to identify the general laws of motion presumed to govern the motion of atoms. Not only did Newton correct this deficiency but he showed how his general laws of motion could be

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confirmed empirically. However, the notion of force was decidedly non-mechanical if 'mechanical' is taken in its strict sense and it created tensions within Newton's natural philosophy that his opponents took advantage of.

Newton's atomism, like Boyle's, was accommodated to, rather than confirmed by, experimental phenomena. This was especially the case in chemistry. Newton was able to exploit his notion of force to accommodate phenomena in a more convincing way than Boyle had, but his efforts in this regard in the context of atomism were mere accommodations nevertheless. Newton's new science of mechanics applied to macroscopic phenomena was to progress dramatically, with the identification of measurable forces associated with surface tension, electrical and magnetic attractions and so on, but at the atomic level his speculations, while highly influential, were unproductive. Revelations concerning Newton's extensive experimentation in alchemy, a natural outcome of his atomism, have done nothing to enhance his reputation as a pioneer of experimental science, but nor has it done anything to detract from the magnitude of his achievements in physics. It has not undermined the standing of his gravitational theory as an exemplary paradigm of a science that is extremely general, mathematically formulated yet experimentally confirmed.

7.2 Newton's Science

In referring to Newton's 'science' I refer to what Newton himself called 'experimental philosophy' (Newton, 1979, p. 394 and 1962, p. 547). The science of mechanics as set forth in Newton's *Principia* stands as one of the great scientific achievements of all time, although an adequate grasp of the achievement and its claim to fame requires an appreciation of the detailed way in which Newton brought his highly general, mathematically formulated theory to bear on the world. We need to understand the way and the extent to which Newton's mechanics, and especially his application of it to astronomy, was confirmed by and not merely accommodated to the phenomena.

The mechanics of the *Principia* was based on the three laws of motion. They involved a precise and novel conception of force as a cause of changes in uniform motion, rather than of motion itself. One such force, that of universal gravitation, is identified and given a mathematical formulation in the form of the inverse square law of attraction. Employing a primitive version of the calculus devised by Newton for the purpose, he derived within his theory explanations and predictions of a range of phenomena. At the terrestrial level these included free fall, projectile motion, the motion of pendulums and the laws of collision, while at the astronomical level they included the orbits of the planets and comets and a theory of the tides.

Newton insisted that his astronomy and mechanics were 'deduced from the phenomena' and involved no untestable hypotheses, as opposed to Descartes' system of vortices that Newton clearly saw as hypotheses devised to accommodate rather than genuinely explain or predict the phenomena. The distinctive feature of the *Principia* lies in the way that Newton was able to make good such claims in spite of the difficulties that stemmed from the generality of his claims and the complexity of the real world systems to which he applied his theory. This is most evident in Newton's astronomy, the 'system of the world' referred to in the full-length title of his masterpiece. The key to Newton's success was a method of successive approximation. He applied his theory first to idealised, simplified situations such as the motion of a point planet around a point sun. Guided by the results so achieved he then added corrections allowing, for instance, for the finite size of the bodies in the solar system and their gravitational interaction with each other as well as with the sun. The theory was borne out by the steady improvement of the match between theory and observation as this self-correcting procedure progressed.[1]

Newton's derivation of the law of gravitation 'from the phenomena' can only be adequately understood in terms of the subtleties of Newton's method of approximation, which involved not only refinements of the theory but also of the phenomena. As George Smith (2001b, p. 328) remarks the 'commonplace statement "Newton's theory of gravity explained Kepler's laws" scarcely begins to describe the complex relationship between Newton's theory and Kepler's orbital rules'. Kepler's rules (as they were called before Newton raised their status) were known to Newton to be only approximately borne out by the data and there were competing rules that fitted the data as well as Kepler's rules. What is more, given the attraction of the planets for each other, Newton's theory predicts the falsity of Kepler's laws taken literally so there is no question of the possibility of deriving Newton's theory from laws that are inconsistent with it, a logical point forced on contemporary philosophers of science by Pierre Duhem (1962, pp. 190–195) early in the twentieth century and stressed by Karl Popper. What Newton was able to show was that, in the context of his theory of motion and with appropriate simplifying assumptions, the approximate truth of Kepler's second and third rules (that in their motions the planets sweep out equal areas in equal times with periods the square of which are proportional to the cube of the mean radius of their orbits) implies the approximate truth of the inverse square law.[2] Newton then proceeded to add corrections that increased the match of his theory with observation of planetary positions, but not by accommodating those corrections to the data. Newton took the approximate orbits and used them to calculate corrections to them utilising the inverse square law he assumed to govern attractions between neighbouring planets. The fact that applying them led to an improved match between theory and data was by no means inevitable and the fact that it did ensue constituted evidence for Newton's theory.

Newton's case for his theory by no means stemmed from Kepler's laws and deviations from them alone. In the *Principia* Newton applied his theory to a range of phenomena, including the non-sphericity of the Earth, the orbit of the moon, which was especially complex because of the joint attraction of the sun and Earth, the tides, the precession of the equinoxes and the tracks of comets. In each case approximations were involved and assumptions added to the fundamental laws. But the additional assumptions had independent empirical support and the extent of the ensuing match between the data and the predictions of the modified or augmented theory were by no means guaranteed. Newton's success was not total. The moon's orbit proved to be particularly recalcitrant and in fact required mathematical techniques not available to him. The detailed experimental support for his theory marshalled by Newton in the *Principia* was substantial and already sufficient to show that his theory could not conceivably have been totally on the wrong track. The next century was to see the scope of the successful application of Newton's theory extended, thereby strengthening the case for it even further. Eventually, of course, it was to prove to have its limits, and to stand in need of correction or replacement by the Theory of Relativity. But the fact that contemporary relativity theorists demand that their theories yield Newton's theory as a limiting case for speeds small compared with the velocity of light and as gravity tends to zero implies an acknowledgement of the strength of, rather than deficiencies in, the case made for Newtonian theory constructed by Newton and his followers.

In my introductory chapter to this book I drew a distinction between the confirmation of a theory by data and mere accommodation of a theory to data. I suggested that a theory is confirmed to the extent that it predicts or explains a range of phenomena that follow naturally from it in conjunction with independently testable hypotheses. Newton's mechanics, as I have described it above, can be seen as a detailed instantiation of that claim. Not only can Newton's practice be read as proceeding in accordance to my strictures but he came close to acknowledging as much explicitly. In his earlier writings, and perhaps in his determination to distinguish his *Principia* and its methods from Descartes' *Principles of Philosophy*, Newton stressed his mechanics as being free from hypotheses and derived from the phenomena in so strong a sense that his remarks are difficult to reconcile with his practice if taken literally. But in his later writings he modified and qualified his remarks in a way that brings them into line with the position I have tried to capture with my distinction between confirmation and accommodation. Thus, in the second English addition of the *Opticks* published in 1717, in Query 31, Newton (1979, p. 404) characterised the status of his science in the following way:

> This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exceptions occur from Phaenomena, the Conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from Experiments, it may then begin to be pronounced with such exceptions as occur.

I presume that Newton's reference to making inductions more general is intended to capture the sense in which the greater diversity of phenomena genuinely supporting a theory the better the theory is. It is also clear that Newton is under no delusion that a theory follows with deductive certainty from the evidence. Rule 4 that Newton added to the third edition of the *Principia* in 1726 re-iterates this latter point.

Newton's views on the experimental testability of science are manifest in his stand on gravity. Although Newton speculated about the cause of gravity in various ways, he was careful to separate such speculations from his science. He posited gravitational attraction, gave it a precise formulation and provided massive empirical support for it as our discussion of the *Principia* makes clear. The law of gravity had strong claims to being supported by experiment in a way that met stringent demands, and, as such, was quite distinct from speculations about the cause of gravity, for which there was no experimental support. Newton (1971, p. 401) appealed to gravity without being able to explain it in much the same way that Boyle appealed to the spring of the air without being able to justify an explanation of it by appeal to experiment. However, Boyle's insistence on sticking to matters of fact, if taken seriously, would have restricted him to low-level empirical claims embodied in his experimental investigations mainly in pneumatics and chemistry. Within a few decades of Boyle's efforts Newton had demonstrated that the experimental method was capable of confirming highly general, mathematically formulated theories. Given the breadth of the phenomena dealt with in quantitative detail by Newton, ranging from the motion of pendulum bobs to that of planets and comets, he had strong grounds for claiming his theory to be true of the mechanics of macroscopic bodies generally.

Optics stands alongside mechanics as the second area in which Newton made significant scientific contributions. He is famous for his experiments on the splitting of white light into colours through refraction and he also discovered colours generated by reflection from and transmission through thin films, the related phenomenon of Newton's rings still bearing his name.[3] However, in this area, Newton was not able to proceed far beyond fairly low-level experimental claims. He clearly favoured a particle theory of light but was aware that he could not support it in the way he had come to demand of his science. Even his formulation of his experimental knowledge in his optics can be challenged for going beyond what his experiments supported, unless his references to 'rays of light' and 'fits of easy reflection and transmission' are interpreted in some vaguer way than is typically implied by the terms 'ray' and 'fit". It is also the case that parts of Newton's *Opticks* presuppose an atomic structure of solids. It is the nature of Newton's atomism and the status of the case he made for it that is the topic of the following two Sections.

7.3 Newton's Atomism[4]

The main published sources for Newton's atomic theory are the Rules of Reasoning and the General Scholium first published in the second edition of the *Principia* in 1713, the *Opticks*, especially Querie 31, first appearing in Latin in 1706 and in English in 1717 and also a short piece 'On the nature of acids' composed in 1692 but not published until 1710. In brief, Newton's atomism was Boyle's mechanical atomism augmented by the addition of inter-particulate attractive and repulsive forces governed by his laws of motion.

Central to Newton's atomism was the homogeneity of matter, the idea, common to the Ancient atomists and the mechanical atomists of the seventeenth century, that there is just one kind of matter. The homogeneity of matter is an assumption not confined to Newton's meta-discourse of the Rules of Reasoning or the more speculative Queries added to the *Opticks*. It appears as an assumption in the body of the text of the *Principia* and is taken for granted in the *Opticks*. In the *Principia*, from the second edition onwards, Newton noted that 'if all the solid particles of all bodies are of the same density, nor can be ramified without pores, a void space or vacuum must be granted'.[5] That is, if the matter composing bodies is of one, homogeneous, kind, then the differing densities of bulk materials must be due to the ratio of full to empty space within them. This explanation is taken for granted in the *Opticks*, where Newton (1979, p. 267) takes the argument further to conclude that the matter of our experience may in fact consist largely of space.

> And hence we may understand that Bodies are much more rare and porous than is commonly believed. Water is nineteen times lighter, and by consequence nineteen times rarer than Gold; and Gold is so rare as very readily and without the least opposition to transmit the magnetic Effluvia, and easily to admit Quicksilver into its Pores, and to let Water pass through it. ... From all of which we may conclude, that Gold has more pores than solid parts, and by consequence that Water has above forty times more Pores than Parts. And he that shall find out an Hypothesis, by which Water may be so rare, and yet not be capable of compression by force, may doubtless by the same Hypothesis make Gold, and Water, and all other Bodies, as much rarer as he pleases; so that light may find a ready passage through transparent Substances.

It is clear that the argument takes the homogeneity of matter for granted. It also, incidentally, takes for granted the material nature of light, magnetic effluvia, gold and quicksilver. The notion that material bodies consist largely of space, implicit in Newton and enthusiastically endorsed by Newtonian atomists of the eighteenth century, has been dubbed the nutshell theory by Arnold Thackray (1968), following Joseph Priestley's remark that on this theory the whole of the matter in the universe might well be collapsed into a nutshell.

Newton's atoms, invoked in the *Principia* in Newton's third Rule of Reasoning, are similar to Boyle's natural minima, although characterised by a slightly different list of properties. For Newton the 'least particles of all bodies' are all 'extended, and hard and impenetrable, and movable, and endowed with their proper inertia'.[6] Inertia is a fruit of Newton's mechanics and is a necessary addition if atoms are to be governed by the laws of motion. Newton (1979, p. 389) gave an argument for the addition of hardness in the *Opticks*. Bodies of our experience are hard or soft to a greater or lesser extent. Softness can be explained by appeal to the alterable space between hard atoms. However, if the atoms themselves are soft it is difficult to see how macroscopic hardness can ensue. Atoms are hard because they lack empty pores. Atoms, the particles at the base of the hierarchy, are incorruptible, no ordinary power being able to overcome their hardness. Were they to be subject to wear, Newton (1979, p. 400) observed, then the properties of the macroscopic bodies made of them would be subject to a corresponding alteration, contrary to what is observed.

> Particles being solids are incomparably harder than any porous Bodies compounded of them; even so very hard, as never to wear out or break in pieces; no ordinary Power being able to divide what God himself made one in the first creation. While the Particles continue entire, they may compose Bodies of one and the same Nature and Texture in all Ages: But should they wear away, or break in pieces, the Nature of Things depending on them, would

> be changed. Water and Earth, composed of old worn Particles and Fragments of Particles, would not be of the same Nature and Texture now, with Water and Earth composed of entire Particles in the Beginning. And therefore, that nature may be lasting, the Changes of corporeal Things are to be placed only in the various Separations and new Associations and Motions of these permanent particles.

The reference in the above passage to God as creator of the atoms is amplified in a way that makes is quite clear that Newton (1979, pp. 402–404) shared Boyle's view that the arrangement of atoms to form the planetary system, the order of the animal kingdom and so on required God as the designer.

Newton's atoms form the lowest level in a hierarchy of particles of increasing complexity as was the case with Boyle's. The idea is made explicit by Newton (1958, pp. 257–258) in 'On the nature of acids' and repeated in Querie 31 of the *Opticks*. Atoms can combine to form 'particles of the first combination' and the latter can combine to form 'particles of the second combination' and so on. In Querie 31 Newton (1979, p. 394) makes it clear that such a series goes through 'divers Successions until the Progression end in the biggest Particles on which the Operations of Chymistry, and the Colours of natural bodies depend and which, by cohering, compose bodies of a sensible magnitude'.

Newton appealed to attractive forces to account for the coherence of particles in complexes, the smaller the particle the larger the force. The heat generated in various chemical reactions is indicative of the strong attractions causing the acceleration of the combining particles. (Newton followed Francis Bacon and Boyle in associating heat with the rapid motion of particles.) The phenomenon of elasticity requires repulsive forces as well, since elastic bodies resist compression as well as extension. The turning of light back on its tracks in the phenomenon of reflection and the great speed at which light travels after reflection was taken by Newton (1979, p. 395) to be evidence of strong repulsive forces. The dispersion of the particles of a solute in a less dense solvent, in defiance of gravity, is attributed by Newton (1979, pp. 387–388) either to weak repulsive forces between particles of solute, or to the attractions between solvent particles being less than the attraction of solute to solvent particle.

Querie 31 of the *Opticks* comes as near to a chemical text as anything Newton published. Many chemical reactions are interpreted in terms of combinations of particles subject to mutual forces of attraction. Here the particles are not atoms but the much more complex clusters of atoms, several steps up the scale of degrees of combination, that constitute the least parts of chemical substances. Precipitation is explained in terms of preferential attraction. So, for instance, the precipitation of (the salt of) a metal from a solution of it in an acid brought about by the addition of salt of tartar (potassium carbonate) is attributed to the greater attraction of the acid particles for the salt of tartar than for the metal. Indeed, successive precipitations of metals from salt solutions by adding further metals suggests that the metals can be arranged in order of their degree of attraction for the particles of acid. For instance, according to Newton (1979, pp. 380–381), the addition of iron to a combination of copper and aqua fortis (copper nitrate) leads to the deposit of copper, the addition of copper to a combination of nitric acid and silver leads to the deposit of silver and so on.

Various physical, as well as chemical, properties are attributed by Newton (1979, pp. 394–395) to atoms, or complexes of atoms, and their attractions.

> If the Body is compact, and bends or yields inwards to Pression without any sliding of its Parts, it is hard and elastick, returning to its Figure with a Force rising from the mutual Attraction of its Parts. If the Parts slide upon one another, the Body is malleable or soft. If they slip easily, and are of a fit Size to be agitated by Heat, and the Heat is big enough to keep them in Agitation, the Body is fluid; and if it be apt to stick to things, it is humid; and the Drops of every fluid affect a round Figure by the mutual Attraction of their Parts, as the Globe of the Earth and Sea affects a round Figure by the mutual Attraction of its Parts by Gravity.

Finally, Newton (1979, pp. 385–386) adopted and adapted the view of the mechanical philosophers that qualities in bodies detected by the senses are caused by interaction between those bodies and the senses. He clearly spells out this view, exploiting his notion of attractive forces, in the case of taste.

> Do not the sharp and pungent Tastes of Acids arise from the strong Attraction whereby the acid Particles rush upon and agitate the Particles of the Tongue? And when Metals are dissolved in acid *Menstruums*, and the Acids in conjunction with the Metal act after a different manner, so that the Compound has a different Taste much milder than before, and sometimes a sweet one; is it not because the Acids adhere to the metallick Particles, and thereby lose much of their Activity? And if the Acid be in too small a Proportion to make the Compound dissolvable in Water, will it not by adhering strongly to the Metal become unactive and lose its Taste, and the Compound be a tasteless Earth? For such things as are not dissolvable by the Moisture of the Tongue, act not upon the Taste.

So much for the content of Newton's atomism. Let us now assess the extent to which Newton was able to make a case for it.

7.4 The Case for Newton's Atomism

Newton laid down stringent conditions that needed to be satisfied if a claim is to be regarded as sufficiently confirmed by observation and experiment to qualify as a part of science. His *Principia* provided a compelling example of how extremely general knowledge claims could be confirmed to an extent that lived up to those conditions and gives ample grounds for making good sense of and defending his claim that hypotheses should not be admitted into science. His optics supplies further evidence of Newton's strictures being put into practice. However, when it comes to Newton's atomism, it not surprisingly fell far short of meeting Newton's demands that ungrounded hypotheses be avoided. Newton's atomistic matter theory is best seen, like Boyle's mechanical philosophy, as a speculative fundamental matter theory supported by accommodating it to, rather than confirming it by, the phenomena.

Newton's recognition of the distinction between claims confirmed by experiment and those transcending such support is evident from his stand on gravity. Here is his exemplary summary of the situation in the General Scholium to the *Principia* (Newton, 1962, pp. 546–547).

> Hitherto we have explained the phenomena of the heavens and our sea by the power of gravity, but we have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and the planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always as the inverse square of the distance. Gravitation towards the sun is made up out of the gravitation towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately as the inverse square of the distances as far as the orbit of Saturn, as evidently appears from the quiescence of the aphelion of the planets; nay, and even to the remotest aphelion of the comets, if those aphelions are also quiescent. But hitherto, I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental science. In this philosophy, particular propositions are inferred from the phenomena, and afterwards rendered general by induction. . . . And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.

Newton's stand on gravity is akin to Boyle's stand on the 'spring' of the air. He cannot explain it but he can accurately characterise it and appeal to it to explain a wealth of phenomena quantitatively and accurately. Newton here is in effect echoing Boyle's point that intermediate explanations are not fundamental, ultimate ones but are highly significant and useful ones nevertheless.

Newton had less dramatic success in optics. Here he clearly favoured a particle theory of light but realised that he could not adequately substantiate it by experiment. True to his own standards, he declined to include the particle theory into his optics, framing his claims in terms of light rays and fits of easy reflection and transmission.[7]

Newton's atomism did not come close to meeting the standards he brought to bear in his mechanics and optics. If one asks what evidence there was, independent of the phenomena to be explained, for the assumption that elasticity arises from attractive and repulsive forces acting between least parts or that chemical combination involves the combination of least parts, then the answer is that there was none. Nothing is added to our knowledge of chemistry by assuming that the measurable affinity between chemical substances is due to affinities between their least parts so long as there is no evidence for those least parts independent of the facility with which those chemicals combine in the laboratory. Needless to say, evidence for atoms and their interaction, a few levels of complexity below the least parts, was even more remote. It should also be remembered that a specification of the force laws involved, a crucial feature of Newton's success in astronomy, is entirely lacking as far as interacting atoms or least parts are concerned. It is not difficult to offer alternatives to Newton's atomism that cannot be ruled out given the evidence available to him. Each portion of a substance, however small, could have the properties attributed to the whole, such as elasticity, density and various chemical affinities, and could have them primitively. Alternatively, the interaction of parts of substances might be mediated by an aether, an assumption that Newton himself flirted with both in his mechanics and optics. He gave some thought to explaining gravitation at a distance in terms of the properties of an all-pervasive ether and he invoked the ether in an attempt to solve a problem in his optics that was in fact a consequence of his commitment to atomism. The regular reflection of light from a plane surface is difficult to reconcile with the fact that, for an atomist like Newton, that plane surface is very irregular on the atomic scale or the scale of least parts. Newton played with the idea that reflection is due to the repulsive force of a film of ether on the reflecting surface. If that isn't a flagrant example of accommodating one's theory to the phenomena then I don't know what is!

The arguments that Newton did mount in favour of his atomism are similar in status to those offered by Boyle to support his mechanical philosophy. Some rest on notions of intelligibility and others are empirical in a weak sense, presuming an analogy between the macroscopic and atomic realms or involving some accommodation of atomism to the phenomena.

Howard Stein (2002) has recently drawn attention to the metaphysics involved in a tract by Newton that he did not publish, now known by the words with which it opens, 'De gravitatione'.[8] In it Newton spells out his general views on the nature of space and of bodies. Just as Boyle made plausible his attribution of some essential properties of a corpuscle by inviting us to imagine what properties a lone particle must have to qualify as matter, so Newton contemplates what God must have to do to create a new material body in the Universe. He concludes that a body must be impenetrable, to distinguish it from space, that it must have a definitive and unchanging shape and size and that it must obey the laws of inertia and collision. The latter fact implies that a body must possess a definite inertia. To the extent that the force of Newton's case draws on common sense notions of what distinguishes body from space it is a dangerous one. That this is the case is a moral that might well be drawn from Newton's own stand on gravity. The action of gravity at a distance, which would appear to involve masses acting where they are not, might well be considered to be unintelligible, just as some of Newton's opponents insisted. Newton's reply, in effect, is that, unintelligible or not, gravity exists and acts just as it is shown to exist and act in the *Principia*. If the successes reported in that book are anything to go by, then science proceeds by explaining the familiar by reference to the unfamiliar and perhaps unintelligible.

Stein (2002) is at least partly right to insist that Newton's case for the nature of atoms is not entirely *a priori*. He does appeal to experience in 'De gravitatione' to defend his notion of body (or atom) just as he does in the *Principia* and the *Opticks*. The mode of argument is most strikingly put in the *Principia*. Rule III of that work is used by Newton (1962, p. 398) to move from properties of observable objects to properties of atoms. Rule III states that 'The qualities of bodies, which admit neither intensification nor remission of degree, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever'. As it stands there are problems with both the interpretation and the truth of this rule. A reasonably clear idea of how Newton interpreted it is evident from the way he proceeds to employ it to argue for the properties of atoms.

Part of Newton's argument has some force. In his mechanics he has demonstrated that his three laws of motion are scale-invariant, applying alike to large masses like the sun and small ones like pendulum bobs. It is a reasonable assumption, until there is evidence to the contrary, that all bodies, including atoms (if there are such), possess inertia and obey the laws of motion. Further, it might reasonably be concluded that atoms must have some shape and size just as observable bodies do. Such arguments are not logically conclusive but adhering to them make for a sensible research strategy. This is the way Smith (2002b, p. 58) suggests we understand Newton's strictures about accepting well-confirmed theories to be unrestrictedly true until exceptions are discovered.[9] Whatever sense and force these arguments have, they are not shared by the additional moves that Newton (1962, p. 399) makes to ascribe properties to atoms. His argument that they must have (absolute) impenetrability and (absolute) hardness is presented as an empirical one. It goes like this:

> That abundance of bodies are hard, we learn from experience; and because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles, not only of the bodies we feel but all others. That all bodies are impenetrable, we gather not from reason, but from sensation. The bodies that we handle we find impenetrable, and thence conclude impenetrability to be an universal property of all bodies whatsoever.

There are all sorts of problems with this argument for anyone not already committed to atomism. What experience shows us is that bodies are impenetrable and hard in various degrees depending on what they are made of. Liquids are penetrable in a way that solids are not, glass is penetrable by light but not by tennis balls whilst metals are penetrable by neither. Likewise, the bodies of our experience differ in their degrees of hardness. Why should not atoms show an analogous variability in their degrees of penetrability and hardness? Newton's stand that hardness of macroscopic objects cannot arise from soft atoms is undermined once inter-particulate forces are admitted, and, in any case, the argument presupposes that there are atoms. Even if one concedes the case that atoms must be absolutely impenetrable and hard, there remains the question of what other properties they might have. Newton's assumption that atoms must possess all and only those properties possessed by all observable bodies presupposes that there is just one kind of matter.

It is quite clear that Newton's atomism requires attractive and repulsive forces to act between atoms and the relatively stable complexes they combine to form. Newton's dream, as expressed in the Preface to the first edition of the *Principia* (p. xviii), was that, if these forces were known, then a theory of their action could be developed on a par with his theory of gravity. But of course, as he acknowledged, they are unknown. What is more, given that they must differ from forces between observable bodies, as is implicit in Newton's assumption that the forces between particles are larger the smaller the particle, there is a strong sense in which the micro-world is unlike the macro-world, contrary to the mode of argument that led Newton to attribute impenetrability and hardness to atoms.

The case for Newton's atomistic matter theory in the main rested on the extent to which it could accommodate the phenomena in a way that was superior to its rivals. It must be acknowledged that Newton's theory was an improvement on Boyle's in this respect. By introducing attractive and repulsive forces Newton could accommodate phenomena such as elasticity, gravity and chemical combination and precipitation that Boyle was not able to accommodate or which he was able to accommodate only in a highly contrived way. Given the success of his gravitational theory, Newton's assumptions about other forces acting at the sub-microscopic level were reasonable and appealing however short of experimental confirmation they fell.

But Newton's inclusion of attractive and repulsive forces in his atomism rendered it problematic in a significant respect. As we saw in our discussion of Boyle's mechanical philosophy, a requirement that was placed on matter theory was that it be ultimate, that is, free of entities or claims in need of further explanation. While Boyle aspired to a matter theory that met that demand Newton's atomism fell short of it just because of its inclusion of forces, which Newton acknowledged to be unexplained yet subject to some explanation. In the mechanics of the *Principia* he could defend his appeal to gravity on the grounds that he could clearly specify the law governing it and that he could explain many phenomena by appeal to it. As far as the forces involved in his atomism were concerned, Newton could do neither of those things. It was the fact that Newton's matter theory involved forces that were not ultimate and so in need of explanation that exposed it to criticism from Cartesians and Leibnizians.

The fact that Newton was unable to specify or gain experimental access to the forces he presumed to be operative at the sub-microscopic level, so that he could at best accommodate his atomism to the phenomena, had the consequence that his matter theory was unable to offer useful guidance to experimental science. In this respect the flexibility inherent in Newton's atomism stemming from the freedom to choose force-laws to meet the demands of the phenomena, whatever they might be, was a shortcoming rather than a strength. This assessment is borne out by subsequent developments. Newton's commitment to and elaboration of atomism is best understood in the context of his philosophical disputes, especially with Leibniz, rather than as a component of Newton's endeavour to extend experimental knowledge. His debate with Leibniz, for example, on metaphysical and religious issues, involved the possibility of the existence of space devoid of matter and on the intelligibility of action at a distance. Newton's atomism played a central role in both these issues. (Recall, for example, Newton's use of the assumption that atoms are all composed of the same stuff to argue that bodies of our experience consist largely of space.) I do not pretend to engage with the details of those debates in this book. My focus is on how experimental access to atoms became possible. Newton's speculations certainly did not supply the key.

7.5 The Fate of Newtonian Atomism in the Eighteenth Century

In Chapter 5 we saw how Boyle championed experimental practice by means of which knowledge of intermediate causes can be acquired, his pneumatics standing as an exemplary example of what can be achieved in this regard. In a sense, Newton's theory of gravitation can be seen as a Boylean science offering explanations appealing to an intermediate cause, namely gravity, although that science differed from anything Boyle had to offer because of its great generality and precise mathematical formulation. Much of the progress in eighteenth-century science can be seen as a further development in the quest for knowledge of intermediate causes, involving such notions as surface tension, viscosity, elasticity, electrical and magnetic attraction, temperature and heat and chemical combination. In some areas forces governed by precise laws were identified, ranging from Hooke's law governing elastic deformation in the late seventeenth century to Coulomb's inverse square law governing electrical attractions at the end of the eighteenth century, so that the mathematical apparatus of Newtonian mechanics could be exploited. We should also not forget the spectacular completion of Newton's programme in astronomy, through the efforts of the likes of Euler, D'Alambert and Lagrange and culminating in Laplace's *Mécanique Céleste* early in the nineteenth century. Other areas are best seen as Boylean, rather than Newtonian, sciences.[10] The latter developments include the move towards thermodynamics and a theory of heat with the fashioning of a notion of temperature and, for instance, the discovery of the gas laws, and the elaboration of the notion of chemical combination culminating in Lavoisier's chemistry.

A detailed history of these eighteenth century advances in science is beyond the scope of this book. I do not discuss details here, with the exception of some related to chemistry that are the topic of the next chapter. The general features of these advances which I wish to stress, and which I intend to signal with my labelling of them as Boylean and Newtonian science are as follows. These new fields were both experimental and theoretical. They were experimental insofar as they involved claims that could be pursued and sometimes established experimentally. Such notions as elasticity, temperature and electric charge were all intermediate notions in Boyle's sense insofar as the hidden, ultimate explanation of these notions did not figure in the sciences in question. They were on a par with gravity in Newton's physics and pressure in Boyle's pneumatics. Nevertheless, the new sciences were theoretical in the sense that appropriate conceptions, such as electric charge and temperature, needed to be fashioned and law-like relations governing them established. As we have seen, the physics of the *Principia* could be defended by appeal to observation and experiment yet its contents could hardly be described as eschewing theory!

My construal of the productive aspects of eighteenth century physical science clash with, and is intended to clash with, atomism of the various kinds we have met so far in this book, which, I have argued, sought ultimate explanations, could not be adequately tested by experiment, and is best seen as speculative philosophy or metaphysics rather than science. Newton's atomism was in fact very influential in the eighteenth century. It was also unproductive as far as experimental science was concerned. I elaborate on this theme in the remainder of this section.

Some inadequacies and incoherencies in Newtonian atomism were identified and removed by eighteenth-century figures. Perhaps the best version of the theory was that formulated by the Croatian philosopher, Roger Boscovich, who attempted to reconcile Newton's atomism with the philosophy of Leibniz. One problem he identified in Newton's version had already been picked up by Leibniz.[11] Since Newton's atoms are perfectly hard then the change in velocity they experience on impact must be instantaneous. Boscovich did not allow such lack of continuity to occur in nature any more than Leibniz had, and, in any case, it implies infinite forces acting on colliding atoms. Another problem was already hinted at in my discussion of Newton's argument for hard atoms earlier in the chapter. Once attractive and repulsive forces are granted, and appealed to in order to explain elasticity, then the question of whether atoms themselves are hard or soft becomes incidental. Indeed, given the work that the short-range forces do, or are capable of doing, in Newton's atomism, the shapes and sizes of atoms become a dispensable part of the picture too.[12] Boscovich attempted to remove all these difficulties by construing Newton's atoms as points possessing inertia and as the origin of forces. Here is his own summary of his theory in the Synopsis of his *Theory of natural philosophy* (Boscovich, 1966, p. 10).

> [M]atter is unchangeable, and consists of points that are perfectly simple, indivisible, of no extent and separated from one another; that each of these points has a property of inertia, and in addition a mutual active force depending on the distance in such a way that, if the distance is given, both the magnitude and direction of this force is given; but if the distance is altered, so is the force altered; and if the distance is diminished indefinitely, the force is repulsive and in fact also increases indefinitely; whilst if the distance is increased, the force will be diminished, vanish, be changed to an attractive force that first of all increases, then decreases, vanishes, is again turned into a repulsive force and so on many times over; until at greater distances it finally becomes an attractive force that decreases approximately in the inverse ratio of the square of the distances.

The problem of instantaneous changes in velocity on collision between atoms is removed because the large repulsive force acting close to them prevents them ever touching. At large separation the force becomes the Newtonian gravitation. The various forces in between are to explain forces responsible for cohesion, fluidity, elasticity, electricity, magnetism and chemistry. Boscovich (1966, pp. 76–96) elaborated on the idea that atoms can combine in relatively stable arrangements to form complex particles by analysing the equilibrium conditions for groupings of two, three and four particles, although he did not claim to be able to proceed so far as to be able to derive the more complicated complexes actually existing in nature.

For those eighteenth-century figures who were attracted to Newton's atomism as a natural philosophy and who had learnt to be more relaxed about introducing the notion of force as a primitive than their seventeenth-century precursors had been, Boscovich's theory might well have been seen as having a lot going for it. However, it was not destined to be of much assistance to experimental science. Boscovich applied his atomic theory to a range of physical phenomena in Part III of his *Theory of natural philosophy*. Since he was free to select forces to suit is purpose he was able to meet with some success. He could accommodate his philosophy to the findings of science. But, because he was unable to even formulate, let alone verify, force laws between atoms or complexes of atoms of various kinds, he could not predict any phenomenon and could not give any guidance to experimental research. I will take just one example to illustrate how Boscovitch did relate his natural philosophy to the results of science. I choose an example from chemistry since developments in that subject are to be our concern in the next chapter.

Here is the treatment Boscovich (1966, p. 161) gave of solution and precipitation from solution:

> When certain solids are mixed with certain fluids, we see that the mutual connection which there used to be between the particles of each is dissolved in such a way that the solids are no longer visible; and yet that they are still there, reduced to extremely small particles and dispersed, is shown by precipitation. For, if a certain other body is introduced, there falls to the bottom an extremely fine powder of the original solid, as it rained down. So metals, each in its own solvent, dissolve, and with the help of other substances are precipitated. 'Aqua regia' dissolves gold; and this, on the addition of common salt, is precipitated. It is quite easy to get a clear picture of the matter. Suppose that the particles of the solid have a greater attraction for the particles of the water than for one another; then they will certainly be torn away from their own mass, and each of them will gather around itself fluid particles, which will surround it, in the same manner as iron filings adhere to a magnet; and each would become something in the nature of little spheres similar to what the Earth would resemble, if a sufficiency of water were to be poured over it to submerge it deeply; . . . Hence the solid will be dissolved, and each of the little spheres, so to speak, would represent a little earth with its great abundance of sea surrounding it; and these little earths, on account of their exceedingly small volume will escape our notice; and they cannot fall, sustained as they are by the force that connects them with the sea that surrounds them.
>
> If now another substance is introduced into a fluid of this kind, the particles of which attract the particles of the fluid to themselves with a stronger force, and perhaps too at greater distances, than they are attracted by the particles of the first solid; then his second solid will be dissolved in every case, and its particles will be surrounded by the particles of the fluid, which formerly adhered to the particles of the first solid, being torn away from the latter and seized by the particles of the second solid. The particles of the first solid will then rain down on account of their own weight within the fluid which is specifically lighter, and there will be precipitation.

Boscovich is taking the experimental results of the chemists and adapting his theory to fit them. It could conceivably have been the case that Boscovich employed reference to one phenomenon, precipitation in this case, to formulate a force law that could then be employed to predict other phenomenon, known or unknown. But Boscovich does not do that. Indeed, it is quite clear that the empirical information he has is quite inadequate for leading to any force law. Whatever the status Boscovich's theory had as natural philosophy it did not, and could not, aid experimental science.

Several eighteenth-century figures attempted to construct a chemical theory that would emulate Newton's gravitational theory. They were as unproductive as that of Boscovich as far as aiding chemistry is concerned. A number of chemists, especially G. F. Venel and P. J. Macquer in France, began to distance themselves from such enterprises and recommend an approach more in touch with experiment. Others, such as William Cullen, and Joseph Black, payed lip-service to Newtonian natural philosophy in a way that did not engage with their practice. By the time we reach Lavoisier late in the eighteenth century, we find him explicitly rejecting atomism as an aid to chemistry and defining a chemical element as a substance that cannot be broken down further by chemical means. Newtonian atomism was unproductive as far as eighteenth-century chemistry is concerned, a point made in detail by Thackray (1970). I elaborate on the case of chemistry in the next chapter.

Notes

1. For thorough analyses of Newton's method of successive approximations see Smith (2001a, 2001b, 2002a, 2002b). An analysis of Newton's methodological remarks concerning the status of his experimental philosophy is in Shapiro (2004).
2. As Smith (2002b) has shown the approximate truth of the first law, that planets move in ellipses with the sun as focus, is not sufficient to imply the approximate truth of the inverse square law, which is presumably why Newton did not argue from elliptical orbits to the inverse square law in the *Principia*.
3. For relevant details of Newton's optics see Shapiro (1993).
4. A young Newton flirted with Epicurus's version of atomism, including atomic space and time, but he did not persist with it. These early speculations have been described by McGuire and Tamny (1983) and I will not reproduce their findings here.
5. As quoted in Thackray (1970, pp. 18–19).
6. *Principia*, Vol. 2, p. 399. The same list of properties appears in the *Opticks*, p. 400.
7. For details of Newton's attitude to the particle theory of light see Shapiro (2004) and the more detailed discussion in Shapiro (1993). For a discussion of Newton's 'deductions from the phenomena' in optics see Worrall (2000).
8. Newton's work appears in Hall and Hall (1962, pp. 89–121 (Latin) and pp. 121–156 (English)). Stein warns that the translation is not to be trusted.
9. Smith (2002b, p. 58).
10. My distinction between Newtonian and Boylean science roughly maps onto the distinction between mathematical and experimental science made by Thomas Kuhn (1977) although on my reading Newton's mechanics was experimental as well as mathematical.
11. See Loemker (1969, p. 446).
12. Thackray (1970, p. 15) has stressed this point in the context of the shapes of atoms. He (p. 151) cites expressions of this worry in precursors of Boscovich amongst British Newtonians such as Robert Green and Gowin Knight.