Chapter 1
Atomism: Science or Philosophy?

Abstract Modern science includes a detailed theory of atoms and their structure. That theory, which goes well beyond what is directly observable, is nevertheless vindicated by experiment, living up to the stringent standards distinctive of science since its emergence in the seventeenth century. Speculations about an atomic structure of matter were prominent in the speculations of the Ancient Greek philosophers. However, it is very misleading to see the theories of the likes of Democritus as an anticipation of modern atomism. It is also a mistake to see modern atomism as emerging as a result of the development of its ancient precursor over the centuries. The methods of experimental science are quite distinct from the methods involved in the development of philosophical matter theories, from those of Leucippus and Democritus up to those of the seventeenth-century mechanical philosophers and beyond. A scientific version of atomism did not emerge until well into the nineteenth century and we learn much about the nature of science by appreciating this.

1.1 Introduction

There are about a million, million, million, million atoms in a typical coin. This has been established by modern science. What is more, much is known about the inner structure of atoms, knowledge that helps to account for the spectra of the radiation emanating from excited substances, for chemical combination, for how metals conduct electricity and so on. Given the minute sizes of atoms, which lie way, way beyond what could possibly be observed directly, how on earth could it be established that there are atoms? Whatever the difficulties standing in the way of the acquisition of this knowledge, they have been overcome to the extent that, not only can atoms be counted, but also their inner structure can be precisely specified. They are made up of a nucleus of protons and neutrons surrounded by electrons that are governed by quantum mechanical principles, principles quite different to those governing the world of our experience. This book tells the story of how knowledge of minute atoms became possible.

The protons, neutrons and electrons that make up atoms and the quantum mechanical principles that account for their behaviour are twentieth-century discoveries. Given this, and given the difficulties facing the project of unearthing

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knowledge of atoms that I have tried to dramatise in the previous paragraph, it seems startling to have to acknowledge that atomic theories were elaborated and defended in Ancient Greece in the fifth century BC, two and a half thousand years ago. Democritus, building on the ideas of his teacher, Leucippus, developed a view of the universe as consisting of nothing other than numerous invisible and unchanging atoms moving and colliding in the void and sometimes combining to form the macroscopic bodies of our experience. How was it possible for Democritus to anticipate the recognition that matter is composed of atoms? I believe the answer to this conundrum lies in the fact that Democritean atomism was far from an atomic theory that could do significant explanatory work and which could be empirically defended. Democritus's atoms are unchangeable and without inner structure and are akin to miniature inert stones. They bear little resemblance to the intricately structured quantum mechanical atoms of modern physics and are incapable of explaining much for that reason.

The atoms known to modern science are structured, potent and subject to change and they interact with and via fields. By contrast, the atoms of Democritus are inert and changeless, and reality consists of nothing other than the sum total of such atoms in the void. Democritean atoms interact only by coming in contact and there is no room for anything like the fields of modern science. However, these marked differences in the content of modern and ancient atomism is not the most important feature that distinguishes them. One additional feature is the extent to which the atoms of the Ancient Greeks were intended to represent the ultimate and only constituents of the world. They were invoked to explain the possibility of change in general whilst being themselves changeless. The credentials of modern atomic theory do not include the capacity to give ultimate accounts of the only constituents of the world. Who knows what inner structure of electrons will be revealed using the next generation of particle accelerators? Also, modern matter theory involves fields as well as particles. A second feature that involves a qualitative distinction between Ancient Greek atomic theories of the ultimate structure of matter and contemporary atomic theory is the nature of the case made for them. The case for contemporary atomism appeals, for example, to J. J. Thompson's experiments involving the deflection of cathode rays by electric and magnetic fields, that enabled him to estimate the ratio of the charge to mass of the particles constituting the rays, and Jean Perrin's experiments on Brownian motion, that established that gases are composed of a specifiable number of molecules in random motion. By contrast, Democritus's case for his atoms as the ultimate and changeless constituents of the world appealed to some very general intuitions about the nature of reality and change. Leucippus and Democritus, together with other Ancient atomists such as Epicurus, and also mechanical philosophers such as Pierre Gassendi and Robert Boyle who revived a version of Ancient atomism in the seventeenth century, offered a philosophical account and defence of atomism that went far beyond what could be adequately defended empirically. This contrasts with the experimental case made by scientists in support of modern atomic theory.

A key theme of this study is the difference between accounts of the structure of matter sought by philosophers and those substantiated in experimental science. Such a distinction is hardly something that needs stressing in a contemporary context. Science and philosophy are practiced within different Faculties in most universities. The former involves practical work requiring laboratories and elaborate equipment. The latter requires access to libraries and the facility to interact with other philosophers. The scientist can mock the armchair philosophers who think they can further knowledge simply by thinking and arguing and can take delight in the story of Thales, the first philosopher, walking into a pit whilst contemplating the stars. On the other hand, the philosopher can be scornful of the senior undergraduate scientist in his or her class who does not even know where the University Library is! The difference between distinct practices of science and philosophy that is now institutionalised began to emerge at the time of the increased use of experiment in the seventeenth century as a key tool for probing fundamental questions about the nature of the world. The capabilities of experimental science were to expand beyond anything that could possibly have been anticipated in the seventeenth century to the extent that many of the questions about the fundamental structure of reality that had been considered to be the province of philosophy were answered by science. The philosophical atomist's miniature stones were replaced by the scientist's quantum mechanical atoms.

Versions of Ancient atomism were revived in the seventeenth century by so-called mechanical philosophers such as Pierre Gassendi and Robert Boyle. Many of those philosophers were also at the forefront of the emerging emphasis on experiment as a key tool in the production of knowledge of the material world. I maintain that a gulf separated these two enterprises to an extent that was not adequately appreciated or acknowledged by the mechanical philosophers and continues to be inadequately appreciated today. The atoms of the mechanical philosophers resembled miniature inert stones just as those of the Ancient atomists did. The atoms in Boyle's philosophy, for instance, had an unchanging shape and size, had some degree of motion or rest, and were all made of universal matter characterised in terms of its impenetrability. The only source of activity and change latent in the natural world was the motion of the atoms. It is perhaps not surprising, from a modern point of view, that there was scant experimental evidence for these atoms and that explanations of phenomena that appealed to them were ineffective or inadequately defended. The state of affairs contrasts markedly with the status, for example, of the knowledge of air pressure defended by Boyle's experiments on air, especially those employing his air pump. Boyle's experiments clinched the claims that air has a pressure and that it is the cause of the height of the mercury in a barometer. The status of that experimental knowledge and the way in which that status was established by Boyle contrasts markedly with the corresponding status and mode of defence of his claims about atoms. This distinction, that I will elaborate and defend in detail later in this book, provides me with a key motif for my epistemological history of atomism. I raise the question of when knowledge of atoms was clinched in the same kind of way and to the same extent as knowledge of air pressure was and I answer, 'late in the nineteenth and early in the twentieth centuries'.

1.2 Science and Philosophy Transcend the Evidence for Them

At face value it would appear that science differs from philosophy insofar as the latter kind of knowledge is borne out by observation and experiment in a way that philosophical knowledge is not. The controlled functioning of modern technologies involving lasers, microchips and hydrogen bombs provides ample evidence that scientific knowledge has a validity that has no analogue in philosophy. The reality of lasers leaves no room for scientists to sensibly doubt the quantum mechanical nature of the stimulated emissions that are responsible for their functioning whereas philosophers endlessly debate the question of the nature of the mind and its relation to the brain, the relationship between facts and values, and whether, in observing a table, we are presented with a sighting of a table, a mental image of a table or a belief in the presence of a table or whatever. Whilst philosophers are wise to ensure that their claims are compatible with science, they do not expect to settle their disputes by appeal to observation and experiment in a way that scientists typically do. All this makes common sense.

A problem that needs to be faced stems from the fact that scientific knowledge is general knowledge no less than philosophical knowledge is. The Ancient Greeks knew how to make mercury by grinding cinnabar in a copper dish. They knew that heavy objects fall to the ground and also how to correlate the seasons with the positions of the sun in the ecliptic. But such knowledge is not sufficiently general to meet the demands of the philosopher or the scientist. Aristotle sought to comprehend why stones fall to the ground in terms of his theory of how the four elements constitute an earth-centred terrestrial domain and Newton did so by appeal to his universal law of gravitation. Both these claims transcend the evidence for them. If science differs from philosophy by being empirically confirmed then we need an account of how its generalities can be justified by appeal to empirical evidence in a way that those in philosophy cannot.

Scientific and philosophical claims about the world go far beyond the evidence on which they are based in two ways. They go deeper, as it were, to claim the existence of unobservable things, and they generalise beyond the circumstances in which evidence is identified. The evidence-transcending nature of philosophy such as involved in Aristotle's attempt to explain all terrestrial phenomena in terms of the interaction of four elements or that of the mechanical philosophers to reduce all phenomena to the motions of universal, inert matter is blatant. But it is characteristic of scientific claims too. In the late eighteenth and early nineteenth centuries chemists identified a range of gases such as oxygen, nitrogen and hydrogen that are not directly observable. Further, basing their claims on a few well-designed experiments, they included gases in their general accounts of the formation of compounds from elements, including an account of combustion that involved combination with oxygen. If we are to insist that evidence-transcendence is warranted in science in a way that it is not in philosophy then we need an account of how scientific claims are confirmed that will enable the distinction to be maintained.

1.3 How the Claims of Science are Confirmed

The demands that a theory has correct empirical consequences or that it be merely compatible with empirical evidence are much too weak to capture what is distinctive about science. One problem is that false theories can have true consequences. The hypothesis that the sun orbits a stationary earth was borne out by a range of evidence, and Aristotle's theory of the four elements entailed that stones will fall to the ground and flames rise in the air, but those theories are false nevertheless. A related problem concerns the possibility of there being alternative theories compatible with the same data. The stationary-earth theory correctly predicts that a stone dropped from a tower will land at its foot. But once Galileo had shown that this would also be the case for a steadily spinning earth, the experiment could not count as evidence for either a stationary or steadily spinning earth. A third point is that theories, if they have sufficient leeway, can be made compatible with the evidence by means of suitable adjustments. If we are free to pick the circular orbits corresponding to the cycles and epicycles in Ptolemy's astronomy so that they fit observations of planetary positions then that fit, in itself, is not genuine evidence for the theory.

An account of theory confirmation that meets the worries raised in the previous paragraph needs to capture some suitably demanding relationship between theory and evidence. Some inter-related ideas that go some of the way are as follows.[1] Evidence counts in favour of a theory only if that evidence is acquired in a way that constitutes a genuine test of that theory. A genuine test of a theory will be such insofar as the theory is unlikely to pass it if it is false. A theory will not be tested against data if the theory is contrived to fit it rather than following naturally from it, and, even if the data does follow naturally from it, it will not be tested against the data if there is an alternative theory that fits the data equally naturally and equally well. These thoughts seem to capture intuitions about the tower argument and Ptolemaic astronomy that I mentioned in the previous paragraph. But they are not adequate as they stand.

More needs to be said about the demand that evidence follow in a natural, rather than contrived, way from the theory it is meant to test. Theories alone rarely imply any evidence that might serve as a test of them. They need to be supplemented by a range of supplementary laws and data before they can do so. Consider, for example, what it takes to test Newtonian astronomy against some observed positions of the planet Mars. The fundamental assumptions of the theory are Newton's three laws of motion plus the universal law of gravitation. Before an orbit for Mars can be derived from the theory a range of observations of past positions of Mars relative to the sun and, once development of the theory is sufficiently advanced, relative to the other planets too, need to be fed in. Observations need to be adjusted to allow for refraction in the earth's atmosphere and to take account of the fact that the position from which the readings are taken varies from moment to moment because of the motion of the earth. Newton's astronomy can be tested against some predicted position of Mars only by adding a host of assumptions and observations such as these. What is the difference between adding these assumptions to Newton's theory to make a test possible, and adding epicycles to Ptolemy's theory in my previous example? The difference seems to be that the assumptions added in the Newtonian case have support independent of the information gathered in the test situation. In my example involving Ptolemy's theory, which is to some extent a caricature of the historical situation,[2] epicycles are added to the theory solely to bring about a match between theory and data. There is no support for the addition independent of the data supposedly predicted or explained.

Single tests are rarely, if ever, conclusive. Theories, in conjunction with appropriate hypotheses and observations, can yield correct predictions even though they are false and some alternative true and they can also make incorrect predictions even though they are true. The assumption that light travels as waves in an aether made many correct predictions in the nineteenth century in spite of the fact that there is no aether, and Newton's astronomy, combined with the necessary additional information, clashed with observations of the orbit of Uranus, not because of failings of the central theory but because the influence of the yet to be discovered planet Neptune had not been taken into account. False theories can have true consequences and failed predictions can be due to shortcomings in auxiliary assumptions or observations added to the theory rather than in the theory itself.

The uncertainties involved in theory testing can be ameliorated only by further testing. One strategy is to test a theory in a variety of circumstances involving differing sets of auxiliary assumptions. The basic laws involved in Newtonian astronomy can be tested by observing that the period of a pendulum varies with height above the earth's surface in just the way predicted by that theory. Here the auxiliary assumptions, such as an estimate of the radius of the earth, are quite different from those needed in the astronomy example. The fact that Newton's theory gets it right about the existence of Neptune, the return of Halley's comet, the lack of sphericity of the earth and the variation of the earth's gravity with height is a sure sign that it has passed severe tests, so much so that when Cavendish added further support to the theory by measuring the attraction between laboratory-sized objects the positive result was pretty much a forgone conclusion. The logical gap between theory and evidence notwithstanding, it is rarely the case that a theory that has survived a few crucial tests that differ in kind turns out to be totally on the wrong track. If it were on the wrong track then the existence of a wide variety of evidence in its favour would involve a remarkable and unexplained coincidence.

So we have a rough characterisation of a significant test of a theory. Such a test involves confirming predictions deduced from a theory in conjunction with independently testable and successfully tested auxiliary assumptions. The so-called tacking paradox is an indication that more needs to be said. Given the characterisation of a severe test that I have proposed, the tests of Newton's theory that I have mentioned above, and others like them, are also tests of the theory consisting of Newton's three laws of motion and the law of gravitation plus the claim that there is a devil with four legs spreading evil about the world. The suggestion is of course silly. But why, exactly, is it silly? The answer is surely that the addition of the devil hypothesis adds nothing to the successful content of the theory. If we ask of the augmented theory, 'could it pass these tests if it were false?' then the answer is, of course it could. It could pass the tests if the devil had only two legs or if there were no devil at all.

It is no coincidence that the augmented theory yields correct predictions because they follow from the unaugmented part. We need to separate the portion of a theory responsible for its testable predictions from the redundant part. In my contrived example it is trivially obvious how to do this. But this is not always the case and there is a serious issue involved.

Deborah Mayo, whose work convinced me of the importance of the idea that theories can be partitioned into those parts that have been and those that have not been tested, has usefully illustrated the point by reference to testing of the General Theory of Relativity.[3] Subsequent to Einstein's formulation of his General Theory of Relativity, investigation of that theory's structure made possible the separation of the assumption that space-time is curved from assumptions about the cause and degree of the curvature. Some testable consequences of General Relativity follow from the former assumption alone. They stand whether Einstein's own more specific theory about the curvature is right or not. Consequently, successful tests of those predictions constitute tests of curved space-time but not of Einstein's theory as a whole. Further tests are necessary to test Einstein's General Theory of Relativity against alternatives. Current theoretical and experimental work on General Relativity is construed with this kind of problem in mind, for example by Clifford Will (1993 and 1996). Theorists explicitly seek to partition the theory into the parts that have been tested and those that have not. A similar situation arose when Einstein formulated electromagnetic theory in a way that dispensed with the aether and challenged physicists to produce experimental evidence for the prediction of which the addition of the aether made a difference. Their failure to do so constituted a case for dropping the aether. Partitioning of a theory into separate parts is not obvious, is not easy, and cannot always be done. But in those instances where it can be done it is possible to identify which parts of a theory are tested and which not by specified tests. A simpler example is the removal of absolute space from Newtonian mechanics. Once it was realised that all the tests of Newtonian mechanics including absolute space could be passed by the theory minus that assumption, then absolute space was dropped as part of that science.

So far I have argued that a scientific theory is confirmed if (i) a range of kinds of prediction that follow from it in conjunction with independently testable and successfully tested auxiliary assumptions are vindicated by experiment and (ii) the successful tests cannot be accounted for by some specified sub-set of the theory. It is no part of this position that the confirmation of a theory in this sense shows it to be true in an unqualified sense. The fact that a theory has survived tests so far is no guarantee that it will not fail new kinds of test in the future. By the turn of the twentieth century even Newton's theory proved to have its shortcomings. It failed to account, for example, for the motions of fast moving electrons in discharge tubes, where the variations in mass with velocity, un-anticipated in Newton's theory, become consequential. It would be absurd to deny Newton's theory the status of 'scientific knowledge' for this reason. Scientific knowledge typically gets corrected or absorbed as a limiting case of a more adequate theory. However, I claim that items of scientific knowledge that have been significantly confirmed in something like the way I have indicated continues to have a range of applicability that is absorbed into and explained by the more adequate knowledge that transcends and replaces it. So the fact that scientific knowledge is fallible, improvable and replaceable does not undermine the distinction I am invoking between science and philosophy.

On my account, philosophical, as opposed to scientific, claims about the structure of mater are not confirmed by, but at best only accommodated to, the phenomena. Aristotle's account of terrestrial matter as composed of the four elements, air, earth, fire and water, was of this kind. That account has not survived as a limiting case of contemporary science. But then, it was never significantly confirmed in the way I have argued is typical of science.

When contemporary philosophers identify some claim as 'an empirical matter' (perhaps some claim about the functioning of sight in the context of the philosophy of perception) they mean that it is a matter for science to decide and so outside of the domain of their philosophy. In my attempt to outline a sense of experimental confirmation involved in science I seek to make explicit a distinction that is taken for granted in the contemporary academic scene. But this was not the case historically. The emergence of scientific knowledge that was both general and experimentally confirmed as distinct from what was referred to as natural philosophy is very much tied up with the story of the emergence of atomism as a scientific theory. The account of confirmation that I have sketched and that has become distinctive of science will be used in the following chapters to inform my investigation and evaluation of atomic theories of the past.

1.4 Inference to the Best Explanation

Theories can be assessed in terms of their explanatory power. On this view, theories are adequate to the extent that they explain a wide range of phenomena, the wider the range the better the theory. In our quest for knowledge we should opt for the theory with the greatest explanatory power.

This account is in need of some sharpening up if it is to be up to the task of distinguishing between science and philosophy. It may well be the case that in the middle of the fifth century BC ancient atomism was the best explanation of change. A contemporary philosopher may well argue that his or her philosophy of perception gives the best explanation of the relevant facts. But in neither of these cases is it appropriate to regard the explanatory power that we are conceding for the sake of argument as sufficient to confer on the theories involved the status we have learnt to demand of science. The explanatory power exhibited, for example, by modern quantum mechanics and its ability to explain chemical bonding, line spectra, lasers, the spectrum of black body radiation and the tunnel effect exhibited by alpha-particle radiation is of a qualitatively different kind. Whilst philosophers may well have to rest content with inference to the best explanation, scientists aspire to do better and infer the right explanation. What is needed to make sense of these intuitions is some demanding standards for what is to count as an adequate explanation which are met by science but not by philosophy.

The account of confirmation sketched in the previous section helps to formulate two demands that can appropriately be made of an explanation in science. Firstly, a phenomenon or event is explained by a theory only if it follows from that theory, in conjunction with auxiliary assumptions, in a natural way. Any auxiliary assumptions used in the derivation need to have independent support. The powders that result from burning metals are heavier than the samples of metal from which they originate. Chemists who understood combustion as the driving off of phlogiston explained the increase in weight by assuming the phlogiston to be replaced by air denser than it. This is not an adequate scientific explanation so long as there is no evidence, independent of the combustion experiments, for the low density of phlogiston and its replacement by denser air. Secondly, and along similar lines, it can be insisted that a theory only adequately explains a phenomenon that naturally follows from it if there is independent support for the theory. The particle theory of light explains why light travels in straight lines. But is it the right explanation? An affirmative answer can be given to the extent that there is independent support for the particle theory. It would be an enormous co-incidence if a theory that can naturally explain a wide range of phenomena is giving explanations that are on the wrong track. Once we have an adequate account of confirmation in science then it can be exploited, in the way I have tried to do here, to make a distinction between explanations that are merely the best available and explanations that have strong claims to be the right ones.

A scientific theory explains a phenomenon if that phenomenon is a natural consequence of it, and it can be argued to be the right explanation to the extent that it can explain other, independent, phenomena in a similar way. Philosophical accounts of the way of the natural world fall short of this insofar as they are merely accommodated to the phenomena. Modern philosophies of perception, for instance, take, or should take, heed of the latest scientific findings about perception and should be constructed in a way that does not clash with that science. A philosophy of perception that cannot be accommodated to scientific findings is inadequate for that reason. But the rival accounts that can be accommodated to those findings go beyond what is sanctioned by science just because they are merely accommodations to science. In making this distinction I do not aim to discredit philosophy. Perception is in many respects a puzzling phenomenon. There is the question of exactly what it is that we are presented with in an act of perception and how that presentation relates to the object perceived. There is also the issue of whether perceptions in the 'mind' commit us to a mental as distinct from a physical world. These are questions that should not be dismissed simply on the grounds that science cannot answer them.

The claim that science aspires to the right explanations of phenomena needs to be qualified in the same kind of way that claims that scientific theories can be confirmed needed to be qualified. Theories turn out to have their limits and need to be modified or transcended. However, significantly confirmed theories need to live on as limiting cases of their successors. Newton's theory provides an explanation of the precession of the equinoxes that is approximately correct because that theory does follow as a limiting case of relativity theory. If something like this were not the case then the fact that Newton's theory was capable of yielding explanations of a wide range of phenomena meeting the stringent demands I have outlined above would be a mystery. Explanations in science have claims to be the right ones to the extent that the theories appealed to in those explanations have been confirmed in the demanding way that has come to be characteristic of scientific as opposed to other kinds of knowledge.

1.5 Science Involves Experimental Activity and Conceptual Innovation

Science, as it has evolved as a discipline distinct from philosophy, is not a passive, armchair activity designed to apprehend the world as revealed by the unaided senses. Testing the adequacy of scientific claims involves active experimental intervention. What is more, the construction of the conceptual apparatus needed to frame scientific claims requires intellectual innovation. The story of the path from philosophical to scientific atomism will involve identification of the emergence of the appropriate kind of experimentation and the appropriate modes of conceptualisation.

Evidence bearing on scientific laws and theories typically involves intervening in and interrogating nature in a deliberate way. Common-sense knowledge that objects fall to the ground is borne out by acquaintance with everyday happenings, but scientific versions of the law of fall, that freely falling bodies move with a uniform acceleration, need to be tested against experimental as opposed to mere observational evidence. Times and distances of fall need to be measured and interference from non-gravitational forces such as friction or air-resistance needs to be eliminated, controlled or allowed for. Galileo's experiments involving the rolling of balls down inclined planes were early attempts to provide what is necessary. Experimental evidence for scientific knowledge claims is not any old kind of observational evidence, but a special kind of evidence generated in demanding circumstances. To seek and vindicate scientific knowledge we need to 'twist the lions tale', as Francis Bacon put it at the dawn of the scientific revolution.

The law of fall also serves to illustrate my second point. Its formulation requires precise notions of uniform velocity and uniform acceleration. At the time he was conducting the inclined-plane experiments invoked in the previous paragraph, Galileo was still struggling to fashion adequate notions of these concepts and the mathematics able to cope with them. Boyle supported a version of the law that bears his name by varying the pressure on a volume of air trapped by mercury in a U-tube. But the notion of pressure involved was not simply given. It is quite a tricky one[4] that gradually evolved as Boyle and his contemporaries struggled conceptually as well as experimentally with phenomena involving air pressure.

A third point involves the recognition that learning from experiment typically involves prior knowledge of the experimental situation. Newton provided powerful arguments for the inverse square law of gravitation by appealing to detailed observations of the motions of the planets, but his arguments involved assuming the three laws of motion. Further, correction of observed planetary positions needed to allow for the earth's motion and for refraction in the atmosphere. Sources of error can only be eliminated or allowed for insofar as they are known about. Possession of knowledge is a precondition for its acquisition and improvement. There is a sense in which science pulls itself up by its bootstraps.[5] An understanding of how preconditions necessary to make possible an atomic theory that is experimentally testable came to be satisfied is a key focus of this book.

1.6 Reading the Past in the Light of the Present

The rough characterisation of science I have sketched in the preceding sections, and my distinction between scientific and philosophical knowledge of the natural world, is a contemporary perspective. I need to be careful not to impose this perspective on the past in a way that is illegitimate and misleading from a historical, and, indeed, from a philosophical point of view. Writing a history of science that simply picks out those claims and practices that come close to living up to a contemporary conception of science would not in itself be particularly instructive. It would not serve to explain how those claims and practices came to be set in place, nor would it establish how their status was viewed at the time.

There is a sense in which my study of the history of atomism is informed by a contemporary perspective. I aim to throw light on the nature of science, and aim to do so by studying how a scientific knowledge of atoms became possible. I claim that atomism prior to the nineteenth-century amounted to something less than scientific knowledge and I intend to show that it acquired the status of a scientific theory late in the nineteenth and early in the twentieth centuries. In order to diagnose the situation in this way I first need to establish just what claims and modes of argument past versions of atomism actually involved and identify the historical process that led to them. I intend my characterisations of past versions of atomism to meet the highest standards a historian would aspire to.

In a way, it is the view that I contest, namely, that Ancient Greek atomism and the corpuscular theories of the seventeenth century were important anticipations of modern atomism and set in train a historical process that led to it, that is guilty of illegitimately projecting present knowledge onto the past. It is as if those early speculations about atoms must have been meritorious and productive because there are atoms! Lancelot Whyte (1961, p. 3), in an extended essay on atomism, exemplifies aspects of the view I oppose when he writes that the 'conception of atom has been the spearhead of the advance of science' so that 'the fertility of the Greek atomic philosophy proves the power of speculative reason'. A. G. van Melsen (1960, p. 83) asserts that the seventeenth century 'owes its outstanding importance to the fact that *scientific* atomic theory came into existence'. Even William Newman (2006), a leading contemporary historian of chemistry whose work I draw on extensively in later chapters, sees Boyle's atomic chemistry as putting that area of investigation on a path that led to Lavoisier. An implication of the case defended in this book is that these views are historically mistaken and are so because they fail to appreciate the qualitative difference between scientific and philosophical claims about the structure of matter and portray the former as emerging from the latter.

I will be contesting the view, expressed by van Melsen, that the seventeenth-century witnessed the emergence of a scientific version of atomism. However, I do locate in that century the serious beginnings of a split between scientific and philosophical modes of understanding. Not only do I find the distinction implicit in seventeenth-century practice but also I find some of the practitioners making the distinction explicit. I will be arguing that most of the points I have made in this introduction about the nature of science were made at some place or other by Robert Boyle when he distinguished between ‘matters of fact’ and philosophical claims. He even made a distinction that maps onto the one I make between accommodation and confirmation.

The appreciation and formulation of a distinction is one thing. What is made of it is another. Seventeenth-century intellects were intent on articulating world-views that would underpin and help comprehend the new social order and also to recast Christian theology in light of the undermining of the Aristotelian philosophy with which it had become entwined. Empirically confirmed knowledge that conformed to my characterisation of science such as the circulation of the blood or Boyle’s law was not up to such tasks. It was to be several centuries before it was apparent that science was capable of yielding a matter theory of the generality sought by seventeenth-century natural philosophers that was also confirmable by experiment. By that time, science and philosophy were separated institutionally and many of the questions concerning the structure of matter that had been seen as in the province of philosophy became answered by science.

1.7 Writing History of Science Backwards

One important piece of evidence for modern atomism came in the form of J. J. Thomson’s experiments with cathode rays completed in 1897. Those experiments established that cathode rays are beams of minute charged particles and yielded a measure of the ratio of charge to mass of those particles. It is pertinent to ask what conditions needed to be fulfilled for Thomson’s experiments to be possible. As we will see in Chapter 13, some of those preconditions concerned the availability of the appropriate technology, such as that involving the production of suitably low pressures in the vacuum tubes employed in the production of cathode rays. On the theoretical side, one crucial piece of knowledge that Thompson needed and presupposed was what is now known as the Lorentz force law, the law that specifies the force experienced by a charged body moving in electric and magnetic fields of specified strength. It is only by employing instances of that law, combined with Newton’s second law of motion that he also needed to presuppose, that Thomson was able to deduce information about the charge and mass of the cathode particles from observed deflections of the cathode rays. The Lorentz force law was in fact relatively novel in 1897. Arguments for it emerged in the work of Oliver Heaviside,

H. A. Lorentz and Thomson himself as they struggled with what proved to be a difficult problem of the interaction between charged bodies and the electromagnetic field.

Having identified preconditions for the possibility of Thomson's experiments the process can now be taken a stage further. One can ask, for instance, what the preconditions were for making sense of the arguments for the Lorentz force law produced by Lorentz, Heaviside and Thomson. One of those preconditions was the notion of the electromagnetic field itself, which had emerged in the work of James Clerk Maxwell. Maxwell's work itself built on Faraday's conception of lines of force. Our request for the preconditions for the possibility of establishing various claims in science lead us on paths back through the history of science.

Thomson's experiments were just some of many that contributed, in the late nineteenth and early twentieth centuries, to experimental knowledge of atoms that is now taken for granted. Those other experiments had their own preconditions. Atomism applied to chemistry took for granted chemical elements and formulae and theories of ionisation built on knowledge of electrolysis dating back to Humphry Davy and Michael Faraday whilst the notion of energy levels in atoms and molecules relied on measurements of spectra in the light emitted from gases in discharge tubes. For each component of twentieth-century knowledge of atoms, paths can be traced backwards in history that result from the repeated request for the theoretical and experimental preconditions for the various moves made. I claim that were a history of atomism to be written in this way, then the story that resulted would be vastly different from one which traces a path from the speculations about atoms found in Democritus forwards through the mechanical philosophers, and beyond. I doubt if the atomism of the Ancients would figure in the backwards-written history at all!

I am not going to follow my own advice in this book and write the history of atomism backwards by tracing preconditions in the way I have suggested above. I have already indicated that, were I to do so then it is doubtful whether much of the history of philosophical atoms from the Ancients on would figure in the story at all. Because of that, a backwards-written history would not enable me to fruitfully draw a contrast between philosophical and scientific atomism in the way that I aspire to do in order to illustrate some instructive differences between the two modes of knowledge.

1.8 The Structure of the Book

Our investigation of atomism begins in Ancient Greece. Chapters 2 and 3 describe and assess the atomic theories of Democritus and Epicurus. Those theories attempted to characterise a reality behind the appearances in an ingenious way that confronted philosophical problems. Whatever their merits, these were philosophical rather than scientific theories. They were not confirmed by observational evidence in any significant sense. In the Ancient Greek context atomism was just one of a range of attempts to give a general characterisation of the ultimate nature of reality. Chapter 4 situates Greek atomism in its context with special attention given to Aristotle's philosophy. It was, of course, Aristotle's philosophy that, in the main, became generally adopted in Western Europe prior to the Scientific Revolution. What is less appreciated is the extent to which the more empirically-orientated works of Aristotle contain germs of an atomic theory that were influential in medieval philosophy and fed into a version of atomism that was very different from that of Democritus and Epicurus and had stronger claims than the latter to be empirically based.

The beginnings of a kind of atomism in Aristotle alluded to above were to be built on in medieval Europe in ways that have only been adequately appreciated in recent decades. William Newman is a major contributor to the new historical picture and I draw heavily on his work. Chapter 5 deals with two areas in which the relevant Aristotelian texts were deployed in novel ways, alchemy and the medieval theory of natural minima. The chemistry of the scientific revolution owes more to the development of alchemy than is typically appreciated. If Newman is right, then there was an important tradition in medieval alchemy that incorporated an atomic theory of matter. In the early seventeenth century, Daniel Sennert, a German philosopher and professor of medicine, constructed an atomic theory of chemistry which drew on this tradition and combined it with a second tradition having its roots in Aristotle, the theory of natural minima. Natural minima were atoms insofar as they were least parts of homogeneous substances, but they differed markedly from Democritean atoms, as we shall see. Sennert's atomism is described and assessed in the closing sections of Chapter 5.

Atomism in the second half of the seventeenth century, championed by philosophers such as Pierre Gassendi and Robert Boyle, is typically seen as part of the Scientific Revolution and as involving a revival of Ancient Greek atomism. Insofar as its proponents construed their atomism as embedded in the new anti-Aristotelian natural philosophy called the 'mechanical philosophy' they themselves construed things in this way. My account, in Chapter 6, of the mechanical philosophy and the atomism embedded in it challenges this picture. Focussing on the work of Boyle, I distinguish between the mechanical philosophy and the new experimental science and argue that the latter owed little to the former. I argue that the mechanical philosophy was supported and fruitful to a much lesser extent than is typically supposed. Boyle's experimental science was progressive, sure enough, but it was able to be so by drawing on the work of artisans, alchemists and a range of philosophers such as Van Helmont and Sennert, usually presumed to be, and presented by Boyle as, the opposition. If my distinction between science and philosophy is taken seriously then atomism as characterised by the mechanical philosophers was no more part of what has become known as science than that of Democritus, and it needs to be distinguished from the experimental advances of the seventeenth-century that did constitute major beginnings of a practice that resembles and marks the beginnings of modern science.

I stick with the theme of atomism and the mechanical philosophy in Chapter 7 to describe Newton's elaboration and transformation of it. Once again, I urge that it is important to distinguish between natural philosophy, of which Newton's atomism was a version, and the new science, of which the mechanics of Newton's *Principia* was and remains an exemplary instance. I argue that Newton's atomism was not supported by evidence in a way that his mechanics and parts of his optics undoubtedly were. I also concur with the view of some recent historians that Newton's atomism, though influential in the eighteenth century, was unproductive,

Chapter 8 is devoted to the origins of modern chemistry. I revisit the work of Boyle, who has been referred to as the 'father of chemistry' to argue that his mechanical philosophy was in fact relatively unproductive in chemistry. In a sense, the limitations of Boyle's chemistry can be attributed to the extent to which he integrated it into his mechanical atomism. There is much to be said for the position recently defended by Ursula Klein, whose work I freely draw on. According to her, the notion of chemical combination that was to prove central for chemistry emerged out of craft practices of metallurgy and pharmacy insofar as those practices involved breaking compounds into their components and reconstituting them from their components. Klein portrays a table schematising such reactions, published in 1718 by Etienne Geoffroy, as capturing the essentials of those developments and setting the scene for further developments that were to lead to the chemistry of Lavoisier later in the century in a way that owed no debt to atomism.

John Dalton introduced atoms into chemistry early in the nineteenth century, with one kind of atom for each element. There was a sense in which this atomism made contact with experiment insofar as it predicted and explained why substances combine in constant proportions by weight. My main objective in Chapter 9 is to argue that the considerable advances in nineteenth century chemistry did not owe much to Dalton's atomism. They came about through a use of chemical formulae not dependent on atomism of the kind Dalton had proposed. My case exploits the historical work of Alan Rocke and Ursula Klein.

In Chapter 10 I contrast my view about the emergence of definitive chemical formulae and the relative atomic weights of elements that followed from them with the traditional one. The common story is that, in 1858, Stanislao Cannizzaro put atomic chemistry in good shape by showing how Avogadro's hypothesis (equal volumes of gases at the same temperature and pressure contain equal numbers of molecules) could be used to calculate relative molecular weights and how these, combined with measurable equivalent weights, could be used to determine atomic weights and formulae. I challenge this story on the grounds that the problem of atomic determination was not the key one that it is presumed to have been, that, in any case, organic chemists solved the problem chemically with no need for Avogadro's hypothesis, and that, what is more, Cannizzaro's method did not give chemists the structured chemical formulae they needed. My intent in this chapter is not merely to challenge the received view on historical grounds but to help elucidate the kind of theory that nineteenth century chemistry was. I claim that it was not dependent on knowledge of atoms. The development of nineteenth-century chemistry was a precondition for, not the result of, the introduction of atoms into chemistry.

In Chapter 11 we turn our attention away from chemistry to physics, more specifically to the rise of thermodynamics and the kinetic theory. Thermodynamics was a phenomenological theory based on two fundamental laws, the conservation of energy and the increase in entropy. It made significant progress in the last few decades of the nineteenth century. In particular it was able to solve two problems in chemistry that had confounded atomists, namely, the measurement of chemical affinities and the anomalous vapour densities of some gases that could be explained thermodynamically by appeal to thermal dissociation. The kinetic theory of gases also met with successes in the 1860s but it did have problems. Its predictions were not completely borne out by measurements of the specific heats of gases and it had difficulty coping with irreversibility. (Heat flows only from a hot to a cold body. But the reverse process, conceived of as arising from the reversal of the molecular motions, is a perfectly valid Newtonian mechanical one.) According to the kinetic theory the second law of thermodynamics is only statistically true, but its proponents had no evidence, apart from irreversibility itself, that this was the case.

Any reasonable doubts about the existence of the molecules of the kinetic theory were dispelled by Jean Perrin's classic experiments on Brownian motion. In Chapter 12 I discuss how these experiments gave strong evidence for the existence of molecules and established the (qualified) truth of the kinetic theory in a way that lived up to the most stringent demands on what it takes to confirm a theory. There is no doubt that Perrin gained experimental access to molecules. A decade earlier, Pieter Zeeman and J. J. Thomson had gained experimental contact with electrons via experiments on spectra and cathode rays. These experiments, discussed in Chapter 13, gave experimental access not only to atoms but to components of atoms. What made these experiments possible were developments in physics and technology in the nineteenth century.

There is a sense in which the existence of atoms was firmly established early in the twentieth century, but they are very different from the kinds of entities envisaged in the philosophical tradition. Atoms are not fundamental insofar as they have an inner structure which was already being explored experimentally in the years immediately following those that mark the ending of this book. The task of exploiting the electronic structure of atoms to explain their stability, chemical bonding and spectra was still one for the future at the time our story ends. The tasks that lay ahead were scientific not philosophical tasks and the accomplishment of many of them were to pose more headaches for mechanistically-inclined philosophers. Many of the intuitions that had been transformed into fundamental principles by the Ancient Greeks and taken over by the mechanical philosopher, such as the idea that there is only one kind of matter and that it is impenetrable, turn out to be false. There is a range of fundamental particles, some of them charged and some of them not, not to mention fields. Wave-functions representing electrons superimpose and add up and alpha particles emitted in radioactive decay tunnel through the potential barrier holding them in an atomic nucleus. What is more, many of the properties possessed by atomic or sub-atomic particles, such as the half-integral spin of the electron, differ markedly from the shape and size attributed to atoms by Democritus, Epicurus and the mechanical philosophers. The old philosophical concepts were undermined, not by an improvement in philosophical argument but by way of a clash with the findings of science. These are the issues summarised in the concluding chapter.

1.9 A Note on Terminology

The word ‘atom’ stems from the Greek word for something that cannot be broken down. On that definition the modern atom is not worthy of the name. My book is a history of atomism which stresses the difference between the atoms of science and the atoms of philosophy. To be able to describe it in this way I need to use the terms ‘atomism’ and ‘atom’ in a suitably vague way so that, for instance, the atoms of Democritus, natural minima and the modern atom all qualify as atoms. Atoms, in the general sense in which I use it, are discrete parts of macroscopic objects or substances whose properties serve to account for the wholes they are parts of. Where I need to be more precise I use terms such as ‘Ancient Greek atoms’, ‘natural minima’, ‘Daltonian atoms’, ‘the modern atom’ or ‘electron’.

Notes

1. The construction of an adequate account of confirmation in science is a major issue on which there is a vast literature. Contemporary accounts which I largely endorse and draw on here are due to John Worrall and Deborah Mayo. See, for instance, Worrall (2002), Mayo, (1996) and Mayo (2002). I have outlined some quibbles with Mayo’s position in Chalmers (2002a) but the quibbles should not disguise the broad agreement.
2. The status of Ptolemy’s theory was superior to that suggested by my caricature. Some of its claims did meet the requirement that there be independent support. For instance, the epicycles added to account for retrograde motion had the additional consequence that the planets be brightest, because nearest to the earth, when retrogressing, a ‘natural’ prediction that was confirmed.
3. See Mayo (2002).
4. If downwards pressure is exerted on a perforated tennis ball filled with water, the water is ejected in all directions, not just in the direction of the applied force.
5. Clark Glymour (1980) introduced the term ‘bootstrapping’ to characterize the way scientific progress is made via the testing of hypotheses.