

Chapter 8

The Emergence of Modern Chemistry With No Debt to Atomism

Abstract According to William Newman, Boyle set chemistry on its modern course by setting it in the framework of the mechanical philosophy. I challenge this. Talk of corpuscles of impenetrable matter characterised by their shape, size and degree of motion only was too impoverished and too far removed from what could be experimentally tested to be of any help to chemistry. Boyle accommodated to his mechanical philosophy a chemistry acquired by other means, and with less success than either he or Newman imply. The new chemistry focused on substances that could be built up from and broken down into their components, drawing on techniques developed in metallurgy and pharmacy. I draw on the work of Ursula Klein, who has argued this thesis and shown how characteristics of the new chemistry can be seen in what came to be regraded as the first of a series of tables of affinity, published by E. T. Geoffroy in 1718. Chemistry was set on the path to Lavoisier in a way that owed no debt to the atomism of the likes of Boyle and Newton.

8.1 Introduction

My account, in the preceding two chapters, of atomism as developed by the mechanical philosophers and Newton, does not involve a detailed engagement with chemistry. It is time to rectify that deficiency. Newman (2006, p. 3) insists that it is precisely in the area of chemistry that the details of the mechanical philosophy were fleshed out and substantiated. He presents the two as ‘indissolubly linked’. In taking this stand Newman is echoing the views of Robert Boyle who described himself as striving to ‘make chymical experiments useful to illustrate the notions of the corpuscular philosophy’ and who promoted ‘the desirableness of a good intelligence between the corpuscularian philosophers and the chymists’.¹

Robert Boyle has been hailed as ‘the father of modern chemistry’.² He is reputed to have revived Ancient atomism and applied it to chemistry in a way that led to the banishment of Aristotelian elements and alchemical or Paracelsian principles. This view has had its critics in recent decades. In the early 1990s Sik Yung Kim (1991) and Antonio Clericuzio (1990) challenged the idea that Boyle’s chemistry was part of and grew out of his mechanical atomism and I, in (Chalmers, 1993), sought to drive a wedge between Boyle’s experimental science, on the one hand,

and his mechanical philosophy on the other, although I paid scant attention to chemistry. More recently Newman (1996, 2006) has argued that Boyle's chemistry grew, not out of ancient atomism, but out of a corpuscular tradition dating back to Aristotle, incorporated into medieval alchemy and fashioned into an Aristotelian theory of natural minima by Daniel Sennert in the generation prior to Boyle. Whilst downplaying a productive link between Boyle's corpuscular chemistry and ancient atomism, Newman still sees Boyle's corpuscular chemistry as a crucial component of a revolution that put chemistry on its modern course. He retains the thesis that Boyle's chemistry and mechanical philosophy were inextricably linked, his view being facilitated by the broad interpretation he gives to 'mechanical'.³ Although Boyle drew heavily on the corpuscular tradition, the step from Sennert to Boyle was a major one, according to Newman, insofar as it involved the removal of the remnants of Aristotelian form. In this latter respect Newman endorses an assessment that Boyle himself frequently gave expression to and supports a somewhat attenuated version of the picture of Boyle as the father of modern chemistry.

My characterisation and assessment of the mechanical philosophy in Chapter 6 has already spelt out in general terms the position I will defend in the context of chemistry in the present chapter. In my view, Boyle's mechanical philosophy did not have the resources to productively guide experimental chemistry. That philosophy could at best be accommodated to a chemistry acquired by other means. The flexibility of the mechanical philosophy, stemming from the freedom to adapt the shapes, sizes and motions of corpuscles to the phenomena, extolled by Boyle as a key virtue of it, rendered it empty as far as offering guidance to the experimenter is concerned. In Chapter 6 I described how Boyle noted that experimental knowledge of the phenomena requires the framing of notions capable of grasping 'intermediate' causes capable of experimental investigation rather than ultimate ones that are mechanical in the strict sense. Notions of the spring and pressure of air provided Boyle with just what he needed in pneumatics, as we have seen. From this point of view it is natural to raise the question of what notions were needed to inform chemistry in the way that spring and pressure informed pneumatics. I cast doubt on the extent to which Boyle made helpful contributions in that respect. I regard his chemistry as a premature and unhelpful attempt to reduce chemistry to mechanical atomism.

My articulation and defence of the above view has been much facilitated by the recent work of Ursula Klein, in which she also casts doubt on the debt seventeenth-century chemistry owed to atomism, arriving at this conclusion by a quite different route than myself. I take maximum advantage of her work in what follows.⁴ Klein provides an answer to the question of what notions needed to be framed for the guidance of a significant segment of experimental chemistry. What was needed were the notions of chemical substance, chemical compound and chemical combination, notions that are taken for granted in contemporary chemistry but which, according to Klein, were not clearly articulated in a way that was able to inform and make possible a new experimental science until early in the eighteenth century. Klein makes it clear that the formulation and practice of, and historical path to, the new experimental chemistry was quite distinct from philosophical matter theories generally and what I refer to as mechanical atomism in particular.

It suits my purpose to use Klein's construal of the new science of chemical combination as a contrast to Boyle's chemistry. To this end, I summarise Klein's position in the next section before modifying it to my own purpose in Section 8.3. In the next five sections I analyse Boyle's chemistry and its relationship to his mechanical philosophy. I attempt to remove the remnants of the view that Boyle fathered modern chemistry by wedding it to a mechanical version of the corpuscular philosophy. To do so I must engage with and counter Newman's detailed defence of the view I oppose. In the last two sections I return to Newton's chemistry to view it in the light of the considerations of this chapter.

8.2 Klein on Geoffroy and the Concepts of Chemical Substance, Compound and Combination

A feature of seventeenth-century chemistry was the increased use of mineral acids. They were used to an increasing extent in pharmacy, making possible the addition of useful substances derived from minerals to those traditionally extracted from animal and plant materials. As the century progressed the action of acids increasingly became a focus of academic as well as artisanal activity, especially in France in the context of the French Academy and the Jardin des Plantes in Paris. The latter institution had been founded as an institution for training pharmacists. As the century progressed there was a gradual shift in the research done there from a concern with practical applications to more theoretical ones. Etienne François Geoffroy, son of a pharmacist, occupied the chair of chemistry at the Jardin des Plantes from 1712. He was able to take advantage of advances made in chemistry largely through the use of mineral acids to publish, in 1718, his 'Table of different relations observed in chemistry' (Fig. 8.1). Klein construes this table as encapsulating novel notions of chemical substance, compound and combination that had emerged in the chemistry of the time and which served to put chemistry on its modern track. In this section I summarise key aspects of Klein's views on the content and origins of the new conception without any pretence of doing justice to the historical detail she invokes.

Geoffroy's table, the first of a series of what came to be known as affinity tables in the eighteenth century, depicted chemical substances arranged in 16 columns and 9 rows. At the head of each of the 16 columns in the table is the symbol for a chemical substance, or class of substances, which form compounds with the substances appearing below it. The higher a substance is in the column below a reference substance the greater the 'rapport' it has for that reference substance. If a substance high in the list is added to a compound of the reference substance with a substance lower in the list, it will replace that latter substance and itself form a compound with the reference substance. The left half of the table summarises the formation of salts by the action of acids. The right half summarises the combinations of metals with sulphur, mercury and other metals and also the solution of substances in water.

The substances appearing in his table are described by Geoffroy (1996, p. 314) as 'the principal materials with which one usually works in chemistry' and the

TABLE DES DIFFERENTS RAPPORTS
observés entre différentes substances.

Esprits acides
 Acide du sel marin.
 Acide nitreux.
 Acide Vitriolique.
 Sel alcali fixe.
 Sel alcali volatil.
 Terre absorbante.
 SM Substances metalligues.
 Mercure.
 Regule d'Antimoine.
 Or.
 Argent.
 Cuivre.
 Fer.
 Plomb.
 Etain.
 Zine.
 PC Pierre Calaminaire.
 Soufre mineral.
 Principe huileu ou soufre Prusope.
 Esprit de vinaigre.
 Eau.
 Sel.
 Esprit de vinet esprits ardents.

Fig. 8.1 Geoffroy's affinity table

'different substances that are used in chemistry'. Chemical substances are those manipulated in the chemical laboratory. Any such substance is characterised in terms of the substances it can be broken down into, the substances it combines with, the substances it displaces and the substances it is displaced by. Geoffroy's table is a summary of these inter-relations which he refers to as 'laws'. Here is an example of a law for which Geoffroy (1996, p. 313) claimed much evidence and no known exceptions:

Every time that two substances that have some disposition to join with each other are found united together, if a third arrives that has a greater relationship (*rapport*) with one of the two, then it will unite with them by letting the other go.

Note that this law talks of combining substances, not combining atoms or corpuscles.

In the commentary on his table Geoffroy makes no reference to, and has no need for anything like, atoms. He also refrains from using the term 'affinity' that became associated with some elaborations of Geoffroy's table by later authors. Presumably he wished to dissociate himself from the vitalistic associations of that term. Geoffroy's table and his commentary on it make perfect sense as a reflection on and abstraction from aspects of the chemical laboratory practice of his time without need to invoke atomism or any other deep explanation. It is therefore doubly mistaken to see Geoffroy's table as a version of Newton's views on chemical affinity. Newton's

views were couched in terms of atoms and forces of affinity between them, whereas Geoffroy mentions neither atoms nor forces of affinity.

Klein raises the question of the principle of selection employed with respect to the substances included in Geoffroy's table. It is indeed a selection. Geoffroy's description of the substances in the table as 'the principal materials with which one usually works in chemistry' is not accurate since many of those substances, the ones involving the extraction of useful substances from plant and animal materials, are not included. So what is the principle of selection? Klein dismisses various suggestions in the literature as inadequate. One such suggestion is that Geoffroy's table stems from, and is an articulation of, the notion of chemical affinity introduced by Newton in Query 31 of the *Opticks*.⁵ We have already noted that Geoffroy did not employ the term 'affinity'. Further, there is no strong evidence that Geoffroy was influenced by the discussion of Query 31 which first appeared in the Latin edition of the *Opticks* in 1706 and not in the 1704 edition which we know Geoffroy was familiar with. It should also be noted that Geoffroy's table involves a wider range of substances than those discussed by Newton. A second suggestion is that Geoffroy's table is simply a convenient summary of empirical knowledge of the time.⁶ A major problem with this is that it does not adequately grasp the criteria of selection underlying Geoffroy's table. It is simply not the case that all chemical reactions known at the time are included. As we have already noted, most of the pharmacy of the seventeenth century involved the destructive distillation of animal and vegetable materials to extract their 'essence' but there is no reference to many of the substances involved in these reactions in the table. Larry Holmes suggested that the table summarises the knowledge involved in salt formation developed in the course of the seventeenth century and embodied in the work of Geoffroy's predecessors, especially W. Homberg.⁷ But this explains only the left hand side of the table. In any case, interpreting Geoffroy's table merely as a summary of empirical results fails to capture key features of the theoretical conception lying behind Geoffroy's table.

The key to understanding the conceptualisation underlying Geoffroy's table has been identified by Klein. It is what she refers to as the reversibility of the processes involved in the formation of the compounds that can be inferred from the table. The chemical substances that combine to yield compound chemical substances can be recovered from those compounds. Chemical substances can be built up from their components and broken down into those components. Perhaps it is recoverability that should be stressed rather than reversibility, since the recovery of substances from their compounds need not be the exact reverse of the processes involved in their production from their components, and the recovery may involve more than one step. (For instance, silver nitrate can be prepared by adding nitric acid to silver, whilst the recovery of silver involves precipitating silver carbonate by adding potassium carbonate and then heating the silver carbonate.) By an immensely thorough and painstaking piece of historical research Klein has shown that the compounds implied by Geoffroy's table include all and only the chemical substances which could be synthesised from their components and from which the components could be recovered, given the practical knowledge of the time, including the contributions from Geoffroy's own laboratory researches.

Klein rightly stresses the theoretical character of the conceptual scheme at work in Geoffroy's table and his commentary on it. The notion of chemical substance and chemical combination at work involves a theoretical abstraction and goes beyond the deliverances of the senses or of the technological practices from which Geoffroy abstracts. The substances in the table are what Klein calls 'pure substances' characterised in terms of their interactions with other chemical substances. They can be approximated to in the laboratory but many of them are not to be found in nature, and those that are occur in an impure state, mixed up with other substances. At this stage in the history of chemistry, chemical substances are pure to the extent that they exhibit regular and repeatable behaviour as far as their interactions with other substances are concerned.⁸ Pure chemical substances interact with others in distinctive ways. In particular they 'combine' to form compounds. The difference between a mixture and a compound is precisely the idea that in a compound the component substances are held together by virtue of a *rapport*. The theoretical notion of combination is distinctive both in respect of what it includes and does not include. It includes the idea that the components of a compound exist in the compound as its components, held together by virtue of a characteristic *rapport*, even though neither the components while in the compound nor their *rapport* are directly observable. As far as what is not included is concerned, the conception of chemical substance and compound does not include a commitment to atoms nor to anything akin to Paracelsian principles, Aristotelian forms or Newtonian forces of affinity serving to explain *rapport*.

The justification of Geoffroy's conceptualisation lay in the productivity of the experimental practice it informed. How successful it would be and how far it could be taken was not something that could be foreseen. I have already followed Klein and noted that a large part of pharmacy at the time could not be accommodated in the new scheme because it involved the destructive distillation of plant and animal materials, where the term 'destructive' signals the fact that the organic materials from which the essences were extracted could not be recovered. As Klein and Lefèvre (2007, p. 58) note 'the window of opportunity was small for the emergence of the modern concept of chemical compound'. The conception embodied in Geoffroy's table worked only for binary compounds that could be analysed into and synthesised from their components. Nevertheless, this approach to chemistry blossomed over the ensuing decades, and its development set the scene for Lavoisier's chemistry.

Geoffroy's table and commentary on it made no reference to atoms and no such reference was needed. To this observation must be added Klein's claim that the historical path that led to Geoffroy's conceptualisation in fact owed nothing to atomism. Klein has traced this path back to the commercial practices of metallurgy and pharmacy. The notion of recoverability central to the conception implicit in Geoffroy's table and commentary was already present in aspects of those practices, so that the innovation involved isolating it and making it the cornerstone of a novel theoretical conception able to inform a progressive experimental practice. I summarise key elements of Klein's account of the historical path to Geoffroy's table in the next five paragraphs.

Procedures common in the sixteenth century involved the extraction of metals from their ores and the separation of metals from mixtures or alloys by reversible procedures involving notions of recoverability of the kind exploited by Geoffroy. Gold and silver could be extracted from ores by heating the ore with lead, which resulted in alloys of gold or silver with lead. Heating these alloys with carbon reduced the lead to litharge (lead oxide) leaving behind the gold or silver. If necessary, the original lead could be extracted from the litharge. Gold and silver could be separated by a process that we have already come across in connection with Sennert's 'reduction to the pristine state'. Adding nitric acid to a mixture of gold and silver and heating involves the formation of a solution of silver nitrate from which the gold is precipitated and can be filtered off. Adding a metal such as copper, or simply pouring the silver nitrate solution into a copper container, results in the precipitation of silver. Another method involved heating the gold/silver mixture with sulphur, resulting in a solution of silver sulphide from which gold is precipitated. Silver can be recovered by adding a metal such as lead to the sulphide. Those kinds of reactions are incorporated into Geoffroy's table. Those, like myself, who need constantly reminding which metals replace which can refresh their memory with a glance at the table.

As I mentioned above, in sixteenth century pharmacy substances of medicinal value were typically extracted from animal and vegetable matter and only rarely from minerals. This gradually changed with the increased availability and use of mineral acids to form salts. A difference at the practical level was the reversibility of the processes involved in the use of acids to produce salts as opposed to the irreversibility of the distillation of organic materials. In formulating his chemistry of combination, Geoffroy was in part extracting and making explicit what was already implicit in the experimental procedures involved in metallurgy and the transformations associated with the use of mineral acids.

The developments described above took place largely in Continental Europe and the main contributors to the science of salt formation were French. The new developments in pharmacy were published in a series of textbooks appearing in France through the seventeenth century.⁹ These books took a typical form. They started with an introductory chapter that included the sketch of a theoretical framework. This derived largely from Paracelsus. Base matter, perhaps the Aristotelian elements or something akin to Aristotle's prime matter, were informed by 'principles', sulphur, mercury and salt to which were sometimes added phlegm and earth. These latter principles were often portrayed, not as material additions to the elements or combinations of them but as non-material 'spirits' that enlivened the elements. Theoretical introductions along such lines typically played a minimal part in the rest of a textbook, which took the form largely of a collection of recipes. As we saw in Chapter 5, Daniel Sennert had conceptualised the action of acids in a way that was both corpuscular and Aristotelian, whilst a few decades later Robert Boyle offered strictly mechanical interpretations. In the latter decades of the seventeenth century leading researchers at the Jardin des Plantes, such as Nicolas Lemery and Wilhelm Homberg employed both mechanical and Paracelsian conceptions.

Apart from these philosophical interpretations, whether Paracelsian, Aristotelian or mechanical, there was more rough and ready talk of the action of acids using metaphors sufficient to engage with artisanal practice. Metallurgists of the mid sixteenth century, such as Georgius Agricola and Lazarus Ercker, understood metals together with impurities as existing in ores as parts needing to be separated. Separation by means of acids was viewed as a process on a par with separation by sieving, washing or heating. Nitric acid, when added to a mixture of gold and silver absorbed the silver, but the silver persisted in the resulting 'dissolution' as a part of it that could be extracted, by adding copper for instance. With the increased use of acids by pharmacists in the seventeenth century this kind of conceptualisation needed to be extended and refined. Attention needed to be given, not simply to the 'dissolutions' resulting from the addition of acids to substances such as metals but to the nature of the new substances so formed. These substances, that came to be classified as salts, needed to be extracted from the dissolution by one of a range of methods depending on the nature of the salt in question. Volatile salts, such as chlorides, were extracted by distilling the dissolutions resulting from the action of hydrochloric acid whilst non-volatile nitrates and sulphates were crystallised from dissolutions made concentrated by boiling. Others salts, such as mercury chloride, could be extracted by sublimation. Precipitation was another key procedure. By the mid-seventeenth century, pharmacists such as Johann Glauber and Christopher Glaser were talking of acids attacking metals and absorbing them. A metal, though persisting in the dissolution in an acid, does not fall to the bottom because the acid fastens on to it. The metal is precipitated through the addition of a substance which weakens the capacity of the acid to fasten on to the metal or which fastens onto the metal more readily.¹⁰

Geoffroy's predecessors at the Jardin des Plantes went beyond the conceptualisations involved in the talk of artisans, and helped pave the way for Geoffroy's innovations, in a number of ways. They became more concerned with conducting what Francis Bacon had described as experiments of light, designed to understand fundamental chemical processes, rather than experiments of fruit focussed on preparing useful substances. The concept of a salt that they forged was an abstraction that brought together a range of substances that differed widely as far as their observable properties were concerned. Another move implicit in chemical practice in the latter part of the seventeenth century was the erosion of the distinction between natural substances and artefacts. Productions of sixteenth-century artisans, such as alloys, were understood as artefacts; distinct from the natural substances that the metals composing them were presumed to be. The analysis of natural substances into their components and their synthesis from their components undermined this distinction. Nitre prepared in the laboratory came to be recognised as having the same properties as, and as being no less nitre than, the naturally occurring variety.¹¹

By 1718, then, the scene was set for Geoffroy to make what Klein construes as his crucial move. His table encapsulated a concept of chemical compound and combination the elements of which were latent in the chemical practice of his time. The historical path to it, as recounted by Klein, owed no particular debt to atomism. Geoffroy, in extracting and focussing on what was implicit in contemporary talk and

practice, seems to self-consciously avoid any philosophical notions associated with Aristotelian, Paracelsian or mechanical matter-theories, although he does not explicitly say that that is what he is doing. The merit of his contribution is that he devised a novel conceptualisation that was grounded in and sufficient to productively guide chemical practice whilst setting aside deeper philosophical questions about the ultimate nature of matter that lay beyond the resources at the disposal of chemists. Geoffroy did not explicitly construe his table in this way but, as Klein points out, a commentator on his table published in the 'Histoires' of the Academy did.

That a body which unites itself to another, e.g., a dissolvent penetrating a metal, which leaves that body to unite with another which is presented to it, is a subject-matter whose possibility could not have been guessed by the most subtle philosophers, and which they cannot easily explain, even today. . . . At first, one imagines that the second metal was better suited to the dissolvent than the first that has been abandoned. But what principle of action can one conceive with regard to this stronger suitability? This is the place where sympathies and attractions would begin to play a role – if they existed. However, leaving as unknown what is unknown, & holding to proven facts, all experience of chemistry proves that a body has more disposition to unite with one body than with another & that this disposition has different degrees The more chemistry will improve, the more M. Geoffroy's table will improve, as well. Be it through the inclusion of a greater quantity of substances, or through the arrangements and exactitude of the relationships.¹²

One further point about the theoretical conception implicit in Geoffroy's table and commentary is worth stressing. As we have seen, following Aristotle's distinction between 'mixts', such as bronze, and mixtures, such as one of wheat and barley, the distinction was often discussed in terms of homogeneity. Bronze is a mixt because any portion of it, however small, is still bronze, whilst a small sample of a mixture of wheat and barley may well be entirely wheat or entirely barley. This view clashed with Ancient atomism. From the point of view of atomism the distinction between a mixt and a mixture lies in the fact that, in the former, the least parts of a mixt are all alike, whereas in the case of a mixture of two substances, at the level of least parts there will remain a mixture of the least parts of each substance. The distinction between compounds and mixtures in Geoffroy's account differs from both of these. It involves the notion of *rapport*. The components of a compound are indeed *combined* by virtue of the *rapport* that exists between them. Components of a mixture are not combined, there being no such *rapport*.

8.3 Reflections on Klein's Account of Chemical Combination

In this section I reflect on some of the implications of Klein's account of chemical substance, compound and combination as it had emerged by the time Geoffroy published his table in 1718 and mould them to my own purpose.

Klein and Lefèvre (2007, pp. 112 and 301) describe the substances depicted in or implied by Geoffroy's Table as 'pure substances' and they stress their 'artificial' nature. Whilst agreeing with the general tenor of this position I have the odd quibble and wish to put a somewhat different emphasis on it. I wish to stress the extent to

which the notion of chemical substances as natural kinds is implicit in Geoffroy's conception. From this point of view there is a sense in which it is misleading to describe chemical substances as 'artificial'.

In the main, chemical substances exist in a pure state only in the laboratory or workshop. Those that are to be found in nature are more or less impure insofar as they are mixed with other substances from which they need to be separated if they are to be 'purified'. There is a sense in which the notion of purity involved here was far from novel in 1718. Archimedes was concerned to develop a method for determining the purity of the gold composing a crown, whilst over half a century before 1718 Boyle discoursed at great length on the purity of chemicals, identifying impurity as one of the main sources of error in chemical experiments. Klein's focus on the 'purity' of the substances in Geoffroy's table and their artificial nature does not serve well to pick out distinctive features of the novel concepts at work. Further, describing pure chemical substances as artificial is inappropriate because it does not accommodate the sense in which many chemical substances are present and at work in nature whether they are extracted in the laboratory or not. There is an important sense in which chemical substances are natural, not artificial.

What is typically involved in the 'purification' of substances serves to support my point. Impure silver can be purified by first adding nitric acid, then adding potassium carbonate, and filtering off the silver carbonate formed as a result and finally recovering silver by heating the silver carbonate. Such a process understood in this way is typical of Geoffroy's conception of chemical substance and compound articulated by Klein. It implies that the silver exists in the original impure mixture in no less real a sense than it does at the end. Purification, and any other transformation that takes place in the chemical laboratory or artisan's workshop, involves chemical substances interacting in the ways that they do because of what they are, independently of us or our experimental practices. Even chemical substances that would not exist were they not produced in the laboratory have objective properties in this sense.

What is distinctive about the substances depicted in or implied by Geoffroy's table is, not that they are 'pure' but that they are substances characterised by their *chemical* properties. Chemical substances are what they are by virtue of the way they combine with other substances to form compounds, can be analysed into component substances, and can displace other substances or be replaced by them. It is precisely these factors that determine the location of substances in the table. Properties other than the ones I have designated as chemical, such as the boiling point of a substance or its smell and colour, whilst undoubtedly used as means of identifying the presence of substances, do not figure, and do not need to figure, in Geoffroy's table and commentary on it. Chemicals are natural kinds. A chemical is of a kind by virtue of the way it combines with other chemicals. Chemicals put themselves into kinds, as it were, by virtue of their mode of interacting with each other. Here I only give increased emphasis to a point that is at least implicit in the writings of Klein, both singly and with her co-author Lefèvre.¹³ The latter point out that whereas 'in eighteenth-century plant and animal chemistry chemists drew the boundaries of single substances by referring to observable properties, both chemical and physical, the individuation of pure chemical substances was determined by

experimentation and the tracing of substances in experiments'. Klein and Lefèvre (2007, p. 301) stress what I have referred to as the objective character of the distinguishing features of chemical substances by referring to their 'materiality', their 'potential for transformation in series of chemical experiments'. Substances that could be incorporated into Geoffroy's table are chemical substances that are what they are by virtue of the way they interact with other chemical substances. Chemical substances belong to natural kinds.

Geoffroy's table is best seen as a symptom of the new chemistry of combination rather than as constituting it. In the 70 years that separated its publication from the *Tableau de la nomenclature chimique* published by Lavoisier and his collaborators, the number of published affinity tables barely reached double figures.¹⁴ It is also the case that Geoffroy's table was not able to accommodate all that was involved in the chemistry of combination. As critics of it soon pointed out, the position of substances in the table is a variable rather than a given because of the way in which chemical affinities can depend on temperature. These reservations aside, it remains the case that the fact that Geoffroy could abstract from the chemical practice of his day a table displaying a range of chemical compounds that could be analysed into and manufactured from their components is testimony to the fact that there was such a practice to abstract from. Some of the practitioners providing the experimental data drawn on by Geoffroy worked in the context of some matter theory, whether Paracelsian, Aristotelian or mechanical, although many others were artisans subscribing to no articulated matter theory at all. The chemistry of combination at the basis of Geoffroy's table provided a basis for future research that was independent of fundamental matter theory. By the time Torbern Bergman published his table of affinities, in 1783, the number of substances included had been greatly increased and the reactions in which they take part documented in considerable detail.

It will not have escaped the readers notice that Klein's account of the emergence of modern chemistry fits well into my conception, discussed in Chapter 6, of the scientific revolution as involving the emergence of experimental science as distinct from philosophical matter theory rather than as a change from one matter theory to another. The new theory of chemical combination was not a theory of matter in general. It was a theory of chemical combination that did not even cover the totality of substances manipulated by chemists in their laboratories and workshops. In Chapter 6 I construed Boyle as, in effect, recognising the distinction between experimental science and philosophical matter theory. Boyle considered it necessary to invoke intermediate or subsidiary principles and causes in the conduct of experimental science as opposed to ultimate mechanical causes. His pneumatics invoked the spring and weight of the air, and his consequential results stood independently of any assumptions about corpuscular or mechanical atoms. But Boyle included the 'chymical' in his list of subordinate principles and he included the works of 'chymists' amongst claims to knowledge too hastily condemned 'because they cannot be clearly and easily deduc'd from ye doctrines of Atoms, or ye Catholick Laws of motion'.¹⁵ The implication is that chemical knowledge subject to experimental investigation and confirmation needs some 'intermediate principles' on a par with the spring of the air. The 'notions' of chemical substance and compound and the

rapport responsible for the combination of components in compounds that Geoffroy built into his table were just what was needed to facilitate a line of development that was to culminate in Lavoisier's new system.

In the remainder of this chapter I return, first to Boyle's chemistry and then to Newton's. I hope to show that the atomistic matter theories espoused by Boyle and Newton did not productively inform their chemistry nor were they significantly supported by it. Geoffroy's conceptualisation of chemistry serves as a contrast that helps bring out the significance of my position.

8.4 Boyle's Chemistry: Some Preliminaries

Suppose we accept some version of Klein's view that the notion of chemical substance, compound and combination implicit in Geoffroy's table was just what was needed to productively inform a significant part of experimental chemistry and set it on a track that was to lead to Lavoisier. How does mechanical atomism fit into this picture? More specifically, to what extent did the chemistry developed by Robert Boyle in the context of mechanical atomism contribute to a concept of chemical substance able to fruitfully inform the new experimental chemistry? Whilst acknowledging that Boyle's mechanical atomism did serve a useful negative function, insofar as it provided a case for removing Aristolian elements and substantial forms and Paracelsian principles from chemistry, I maintain that it did not serve a positive function. As a fundamental matter theory, Boyle's mechanical atomism was too far removed from what could fruitfully be experimentally tested to offer useful guidance to the experimenter.

In Chapter 6 I distinguished between Boyle's mechanical atomism and his experimental science, most notably exemplified in his pneumatics. Notions necessary for formulating the claims of an experimental science need to be framed and the claims tested by a range of experiments sufficient to render them strong contenders as 'matters of fact'. Boyle's pneumatics, involving notions of the spring, pressure and weight of air and supported by a range of experiments, many of them involving the air pump, conformed to this pattern. The spring, pressure and weight of air were 'intermediate' notions rather than strict mechanical ones as Boyle openly acknowledged. The support Boyle sought for strict mechanical explanations of phenomena was of a weaker kind than that demanded of experimental knowledge. That support involved the construction of hypothetical mechanisms, mechanical in the strict sense, which would be sufficient to account for known phenomena. That support is especially significant if the phenomena explained poses problems for rival matter theories. Boyle acknowledged that he could not go further and argue that the hypothetical mechanisms were the actual ones operative in nature. He pointed out that testing a fundamental matter theory against the phenomena in the way proposed presupposes knowledge of those phenomena. He complained that many philosophical systems were constructed without paying due heed to the phenomena, and advocated a separation between experimental matters of fact and philosophical

systems designed to explain them. He sometimes signalled this distinction in the context of his own exposition of mechanical atomism and experimental matters of fact that he sought to explain by appeal to it. These are the considerations, defended in detail in Chapter 6, that I bring to bear on Boyle's chemistry.

Whatever the merit of my distinction between Boyle's experimental science and his mechanical philosophy, there is no doubt that Newman (2006, p. 3) is absolutely correct in observing that, in Boyle's work, chemistry and his mechanical philosophy were 'indissolubly linked'. There is perhaps not a single chemical text of Boyle in which the chemistry is not related to mechanical explanations of it, and many instances of Boyle invoking chemical phenomena in support of the philosophy. I acknowledge this but maintain, first, that the degree of support for the mechanical philosophy based on Boyle's chemistry was not as strong as is typically supposed and secondly, and more importantly, that the mechanical philosophy did not feed productively into Boyle's chemistry. Insofar as Boyle was able to contrive mechanisms capable of accounting for chemical phenomena, knowledge of the phenomena involved was acquired by other means. As far as furthering the search for knowledge of chemical phenomena is concerned, Boyle did not have much to offer as far as the framing of appropriate notions is concerned, and the limitations of his efforts in this respect were very much tied up with what can reasonably be construed as his premature attempt to reduce chemistry to mechanical atomism.

8.5 Boyle's Mechanical Rather than Chemical Construal of Substances

Boyle did, of course, have some notion of chemical substances and their properties, but that notion did not involve a precision that went beyond the common sense of his time. In 'History of particular qualities' Boyle (2000, Vol. 6, p. 267) singles out chemical qualities as those that have

bin principally introduc'd and taken notice of by means of Chymical Operations and Experiments; such as are Fumigation, Amalgamation, Cupellation, Volatilization, Precipitation, &c. by which operations among other means, Corporeall things come to appear Volatile or Fixt, Soluble or Insoluble in some Menstruum's, Amalgamable or Unamalgamable, capable or incapable to precipitate such Bodies, or be precipitated by them, and (in a word) acquire or loose several powers to act on other Bodies or dispositions to be wrought on by them.

This is too imprecise and includes too much to be particularly useful.

There were general features of Boyle's mechanical philosophy, connected with his concern to dispense with notions akin to the substantial forms of the scholastics, which did not lend themselves to a categorisation of chemical substances useful for informing experimental chemistry. One of them was Boyle's view that classifications of substances into kinds are a human imposition rather than one arising naturally from the nature of the substances classified. This view, when taken seriously, barred Boyle from developing a precise notion of chemical substance that went beyond common sense.

In the ‘Origin of forms and qualities’ Boyle takes a stand against the Aristotelian account of essential properties, and of the distinction between generation, corruption and alteration. In taking such a stand he in effect rules out the idea that there are natural divisions between chemical substances. His view makes such distinctions into conventions imposed on the world by us. We can define a globe as a metal sphere, in which case sphericity is an essential property of a globe and any volume of metal that lacks that shape is not a globe. In a similar way, says Boyle (2000, Vol. 5, p. 322), we can define the essential properties of substances by listing the properties essential to that substance. With respect to the classification of substances generally, men ‘did for conveniency, and for the more Expeditious expression of their Conceptions agree to distinguish them into several Sorts’ and have ‘for their Convenience agreed to signifie all the Essentials requisite to constitute such a Body by one Name’.

Given that objects or substances are defined and divided into kinds by way of human conventions, the need for a substantial form as the seat of the essential properties is removed. Also, Boyle can re-interpret and render less significant the Aristotelian distinction between generation, corruption and mere alteration. If liquidity is included in our definition of water, then ice is not water and the freezing of water into ice involves the corruption of water and the generation of ice. If liquidity is not included amongst water’s essential properties, then freezing involves the mere alteration of water from liquid to solid form. Whichever way we go, Boyle implies, there is only one change taking place, and it involves the re-organization of the particles that make up water into a new texture making up ice.¹⁶

Boyle (2000, Vol. 5, p. 356) drives home the point about the conventional character of kinds a few pages later.

It was not at random that I spoke when . . . I intimated, That ‘twas very much by a kind of tacit agreement, that Men had distinguished the *Species* of Bodies, and that those Distinctions were more Arbitrary than we are wont to be aware of. For I confess, that I have not yet, either in *Aristotle*, or any other Writer, met with any genuine and sufficient Diagnostic and Boundary, for the Discriminating and limiting the *Species* of Things; or, so to speak more plainly, I have not found any Naturalist has laid down a determinate Number and sort of Qualities or other Attributes, which is *sufficient* and *necessary* to constitute all portions of Matter, endow’d with them, distinct Kinds of Natural Bodies. And therefore I observe that most commonly Men look upon these as Distinct *Species* of Bodies, that have had the luck to have had distinct Names found out for them; though perhaps diverse of them differ much lesse from one another, than other Bodies, which (because they have been huddled up under one Name) have been look’d upon, as one sort of bodies.

In the text following this passage Boyle proceeds to stress that there is no clear-cut distinction between naturally occurring substances and artefacts prepared in the laboratory. He points out, for instance, that there is no good reason to make a categorical distinction between substances formed in nature by the heat of the sun or by a chance fire on a hillside from those formed by artificial heating or burning in the laboratory. His main intent here is to undermine the scholastic notion of substantial form. On a common scholastic interpretation of that distinction, naturally occurring substances differ from artefacts precisely by virtue of the substantial forms possessed by the former. Rather than focussing on the artefacts as opposed

to natural substances, as the site for developing a notion of chemical substances understood in terms of their interactions, Boyle is denying any useful distinction.¹⁷ Boyle adds to his point by appealing to his own mechanical viewpoint. Natural substances do not differ from artefacts because in both cases the properties of the substances arise alike from the arrangements and motions of the corpuscles of which they are composed. No substantial forms are necessary. Boyle makes this point in the case of the likeness of naturally occurring and 'factitious' vitriol, but the fact that his point is a general one, about the inadequacy of the Aristotelian notion of form and the superiority of the mechanical philosophy rather than some specific point about chemistry, is illustrated by the fact that he proceeds to give, as a second example, the similarity between a normal pear and one grown from a tree grafted onto a thorn.

According to Boyle (2000, Vol. 5, pp. 359–360), what he refers to as 'chemical concretes', whether natural or artificial, are to be characterised in terms of a 'concourse of accidents'.

Since, then, these Productions of the Fire, being of Nature's own making, cannot be deny'd to be Natural Bodies, I see not why the like Productions of the Fire should be thought unworthy of that Name onely because the Fire, that made the former, was made by chance in a Hill, and that which produc'd the latter was kindled by a Man in a Furnace. And if flower of Sulphur, Lime, Glass, and colliquated mixtures of Metals and Minerals, are to be reckon'd among Natural Bodies, it seems to me but reasonable that, upon the same grounds, we should admit flower of Antimony, Lime, and Glass, and Pewter, and Brass, and many other Chymical Concretes (if I may so call them), to be taken into the same number; and then 'twill be evident that, to distinguish the *species* of Natural Bodies, a Concourse of Accidents will, without considering any Substantial Form, be sufficient.

Boyle (2000, Vol. 5, pp. 322–323) does specify the essential properties of substances more easily recognised by us as *chemical* substances than glass. Gold, for instance, is designated as a body 'that is extremely ponderous, very malleable and ductile, fusible and yet fixed in the fire, and of a yellowish colour'. In the ensuing discussion the ability to 'resist aqua fortis' is added. Boyle's intent is to undermine the need to characterise substances in terms of substantial forms. Natural and artificial substances are what they are by virtue of the characteristic set of properties they possess, and those properties are presumed to arise from various arrangements and motions of component corpuscles. This treatment of properties in general does not point in the direction of the notion of a chemical substance understood in terms of what it does and does not combine with and to what degree.

Boyle's view that distinctions between kinds of substances are not natural but imposed on nature by us for our convenience fitted in with another view of his that did not help with the elaboration of a notion of chemical substances as 'intermediate' causes in chemistry. This was the idea that any substance could in principle be changed into any other by bringing about the appropriate change in the underlying mechanical structure.

So that though I would not say, that Any thing can immediately be made of Every thing – as a Gold Ring of a *wedge* of Gold, or Oyl, or Fire of Water; yet, since Bodies, having but one common Matter, can be differenc'd but by Accidents, which seem all of them to be

the Effects and Consequents of Local Motion, I see not, why it should be absurd to think, that (at least among Inanimate Bodies), by the Intervention of some very small *Addition* or *Subtraction* of Matter (which yet in most cases will scarce be needed), and of an orderly *Series of Alterations*, disposing by degrees the Matter to be transmuted, almost of any thing may at length be made of Any thing . . . (Boyle, 2000, Vol. 5, p. 332)

Boyle's views on the possibility of changing anything into anything else, and on the distinctions between substances being a matter of convention, both of them fitting naturally into his mechanical atomism, turned attention away from the task of understanding chemicals as kinds that combined in distinctive ways with chemicals of other kinds. They suited Boyle's purpose of undermining the need to appeal to substantial forms but they did not provide useful guidance to chemistry.

Another factor that stood in the way of Boyle framing notions that were able to inform an experimental program that was distinctly chemical was the extreme generality of his mechanical matter theory. It was designed to give an account of how qualities in general arise from the 'primitive affections' of pieces of universal matter. Boyle's use of chemistry to support his mechanical matter theory involved him in offering possible mechanisms for explaining the whole range of changes in qualities accompanying chemical change. His focus was as much on the changes of colour accompanying chemical change, for example, as on the changes of substances bearing the colours.

The treatment of chemistry by Boyle in the service of his general mechanical matter theory had the consequence that a focus on chemical combination of chemical substances was far from central, if not conspicuously absent. This point is well illustrated by Boyle's essay 'On the mechanical causes of chemical precipitation'. Boyle (2000, Vol. 8, p. 484) rejects appeal to antipathies and sympathies as the cause of precipitation and proposes, in its place, 'a greater congruity as to bigness, shape, motion and pores of the minute parts between the *Mestruum* and the *Precipitant*, than between the same Solvent and the body it kept before dissolved'. The reasons he rejected sympathies and antipathies can be gleaned from the article in question and elsewhere. First, Boyle (2000, Vol. 8, pp. 415 and 484) construed them as unacceptably mysterious and anthropomorphic. Secondly, he argued that the facility of substances to combine with or precipitate others need to be understood as relations between substances rather than as properties of single substances, which Boyle assumed to be the practice of those resorting to sympathies and antipathies. So, for instance, substances with a great antipathy towards each other can nevertheless react in a similar way with some third substance.¹⁸ The notion of *rapport* to be found in Geoffroy is sufficient to indicate that sympathies and antipathies between chemical substances can be put to work in chemistry in a way that construes them as relational properties that are not anthropomorphic. Boyle did not take that line, even though it would have been in accord with what he had to say elsewhere about the importance of appeal to 'intermediate causes and explanations'. He explicitly declined to offer a 'History of Precipitations' that would co-ordinate knowledge of them at the experimental level. Rather, Boyle (2000, Vol. 8, p. 481) proceeded directly to contrive 'the *Mechanical Causes* of Precipitation'. In like manner, Boyle devises mechanical

explanations of the action of acids and alkalis, volatility and fixedness and so on. In all these instances there is an emphasis on the mechanical breaking down or coherence of corpuscles by reference to mechanisms that are frequently highly contrived and which are not of a kind that can usefully guide the experimenter. An emphasis on chemical combination is, in the main, conspicuously absent.¹⁹

8.6 Boyle on the Properties of Chemical Corpuscles

The fact that Boyle proposed a mechanical or corpuscular chemistry suggests that his position can easily be accommodated to, and can even be read as an anticipation of, the view of chemical combination identified by Klein. Chemical substances are what they are by virtue of the nature of the corpuscles that compose them, and chemical combination comes about as the result of the association or dissociation of corpuscles. To the extent that such a position represents Boyle's position, Klein's view that the idea of chemical combination emerged as a significant novelty in Geoffroy's paper of 1718 would appear to be undermined. In this section I explore the precise character of, and role played by, Boyle's corpuscles and their relation to the chemical substances they are presumed to compose. I conclude that the details of Boyle's position do not undermine Klein's position. Boyle's corpuscular theory, as he construed it, did not provide him with a notion of a chemical substance adequate for chemistry.

If chemistry is to be explained by reference to corpuscles then there is a basic question that needs to be answered. What properties do the corpuscles possess that enable them to fulfil their role as explainers of chemical phenomena? Did the corpuscles figuring in Boyle's chemistry possess only strict mechanical properties, or did he need to attribute some further 'chemical' properties to them?

We have seen that Boyle's corpuscular chemistry grew out of a medieval corpuscular tradition via the work of Daniel Sennert. In that tradition, the minima of a substance possessed properties characteristic of the substance they were minima of. As Newman (1996) and Antonio Clericuzio (2000) have observed, the first version of atomism to appear in Boyle's writings shared this feature. The surviving pages of a manuscript on atomism written by the young Boyle (2000, Vol. 13, p. 228) contain the view that atoms are particles that nature cannot divide and which possess the properties of the homogeneous substances they are the least parts of. The main argument given by Boyle for the existence of atoms is a reproduction of Sennert's reduction to the pristine state, the recovery of silver after its dissolution in nitric acid.²⁰ By the late 1650s, when Boyle was composing the *Sceptical chemist*, he had adopted the mechanical philosophy. In that work, Boyle (2000, Vol. 2, p. 230) introduced his hierarchy of particles with mechanical atoms at the base and corpuscles of various degrees of complexity composed of them. (He also added further examples of reductions to the pristine state to strengthen the case for the existence of corpuscles.) Boyle (2000, Vol. 2, p. 229) made it clear that his 'natural minima', corresponding to what I call mechanical atoms, are composed of universal matter

‘actually divided into Particles of several sizes and shapes variously mov’d’. There remains the question of the properties that Boyle attributed to the corpuscles made from these atoms. Did they possess some ‘chemical’ properties over and above strictly mechanical ones?

Textual evidence apart, there are conceptual difficulties associated with the claim that corpuscles have chemical properties. Such a position is implied by Boyle’s youthful assertion that atoms possess the properties of the wholes they are atoms of. But such a view cannot be coherently sustained. Insofar as atoms are invoked to explain various chemical properties they cannot also possess them. Take, for example, the dissolution of silver in nitric acid and its subsequent recovery, the reaction so central to the arguments of the atomists. The property, possessed by bulk silver, of being dissolved in nitric acid cannot also be a property of the corpuscles of silver for atomists like Sennert and Boyle. The whole point of their argument for the existence of corpuscles or natural minima of silver is that these particles persist as such in the solution thus accounting for the fact that they can be recovered. Again, silver melts at high temperatures but an atomist could not afford to conclude from this that corpuscles of silver do the same. Whilst it is true that corpuscles need to possess properties sufficient for them to play the chemical roles required of them, it cannot coherently be claimed that they possessed chemical properties in an unqualified way.

It is undoubtedly the case that the corpuscles figuring in Boyle’s chemistry are not mechanical atoms but structures built up from those atoms. In spite of their compound character, strict mechanical properties can be attributed to corpuscles in a straightforward and non-mysterious way. A corpuscle will possess a shape, size and motion that is the resultant of the shapes, sizes and motions of the mechanical atoms that compose it. The shapes, sizes and motions of corpuscles are derivative, but they are strict mechanical qualities nevertheless. I maintain that the textual evidence strongly points to the fact that, from the *Skeptical chemist* onwards, Boyle attributed only strict mechanical properties to chemical corpuscles.

According to Boyle, semi-permanent corpuscle composed of mechanical atoms will have a shape, size and degree of motion that is determined by and is the resultant of the shapes, sizes and motions of the mechanical atoms that compose it. Boyle referred to the structured arrangement of mechanical atoms comprising a particle as its texture. The shapes, sizes and motions of corpuscles that result from their texture are not primitive mechanical properties but they are strict mechanical properties nevertheless. Because corpuscles are complex structures of mechanical atoms, their shapes, sizes and motions typically change when those structures are modified by adding or removing mechanical atoms or by rearranging or changing the motions of the mechanical atoms composing a corpuscle. This is made quite explicit by Boyle (2000, Vol. 5, p. 326) in the following passage from the ‘Origin of forms and qualities’:

That as well each of the *Minima Naturalia* as each of the Primary Clusters above mention’d having its own Determinate Bulk & Shape, when these come to adhere to one another, it must *alwaies* happen that the Size, and *often*, that the Figure, of the Corpuscles compos’d by their Juxta-position and Cohesion, will be chang’d; and *not seldome*, too, the Motion either

of the one or the other, or both, will receive a new Tendency, or be alter'd as to its Velocity or otherwise. And the like will happen, when the Corpuscles, that compose a Cluster of Particles, are disjoin'd, or anything of the little Mass is broken off. And whether anything of Matter is added to a Corpuscle or taken away from it, in either case, (as we just now intimated,) the Size of it must necessarily be alter'd, and for the most part the Figure will be so too, whereby it will both acquire a congruity to the Pores of some Bodies (and perhaps some of our Sensories), and become Incongruous to those of others, and consequently be qualify'd, as I shall more fully show you hereafter, to operate on divers occasions, much otherwise than it was fitted to do before.

Qualities of substances are due to the sizes, shapes, motions and arrangements of the corpuscles that compose them and will change if those shapes, sizes, motions and arrangements are changed in the kinds of ways listed in the above quotation.²¹ Boyle embraced chemical qualities and chemical change in this scheme. He aspired to reduce chemistry to mechanism in the strict sense.

Both Newman and Clericuzio find it necessary to depart from the strict mechanical interpretation of Boyle and construe him as attributing chemical properties to corpuscles. The text they use to this end involves some experiments by Boyle on colour changes accompanying chemical reactions. White mercury sublimate (mercuric chloride) is dissolved in water to form a colourless solution. This turns orange when salt of tartar (potassium carbonate) is added. Addition of oil of vitriol (sulphuric acid) results in a colourless solution once again.²² Boyle employs this experiment to aid him defend his 'mechanical' account of colours generally. On this account, colours do not inhere in coloured objects, as forms or principles. Rather, they are modifications in light, itself an (unspecified) mechanism, brought about by its interaction with the object appearing coloured. Because colour results from an interaction of light with a structured object, a change in that structure can result in a change in colour, the removal of colour, or the introduction of a colour where there was none before. On the one hand, Boyle regards these considerations to be the 'fittest to recommend the Doctrine propos'd in this Treatise', that is, the Treatise defending a mechanical account of colour. On the other hand, he acknowledges that his account of the colour changes involved in his experiments with mercury sublimate employs 'Chymical' reasoning and 'Chymical Notions'. He acknowledges that there is a difference between a 'chymical Explication of a *Phenomenon*' and a 'truly Philosophical or Mechanical' one and admits that his account of colour change falls short of the latter, such truly mechanical explanations being 'more than I dare as yet pretend to'.

Newman (2006, p. 185) takes Boyle to be appealing to 'chymical properties of corpuscles in order to explain the source of their mutual association and dissociation'. For Newman, this does not undermine the status of Boyle's corpuscular chemistry as 'mechanical'. It remains mechanical in the sense that clocks and watches are. The behaviour of chemical substances are explained by structures of corpuscles with relevant properties just as clocks are explained by structured arrangements of rigid gear-wheels, heavy pendulum bobs and so on. That is, he construes Boyle's chemistry as mechanical in a common, rather than a strict, sense. I have already aired my

dissatisfaction with such a move. It runs counter to Boyle's persistent characterisation and defence of a strict, not a common, version of the mechanical philosophy.

Clericuzio (1990, p. 145) takes Boyle's discussion of colour changes in his experiments with mercury sublimate as evidence for his claim that Boyle's corpuscular chemistry was not mechanical because the explanation of colour change offered by Boyle 'is based on the substitution of compound corpuscles having chemical properties'. In the elaboration of his position Clericuzio invokes Boyle's distinction between intermediate and ultimate, mechanical causes that I have described in Chapter 6. It is true that if corpuscles are to be situated in Boyle's scale of causes then they will not be at the top of the scale since they are compound structures composed of mechanical atoms. In this sense they will be intermediate causes. But the whole point of Boyle's distinction is to demarcate the ultimate mechanical causes that are remote from what is empirically accessible and causes, such as the spring of the air in his pneumatics, that are empirically accessible. The corpuscles Boyle invoked in his chemistry were no more accessible than the mechanical atoms of which they were composed.

My own view retains the idea that Boyle aspired to a corpuscular chemistry that was mechanical in the strict sense. His chemical corpuscles were to bear only the strict mechanical properties of shape, size and motion, notwithstanding the fact that they were compound particles. Boyle hoped, at best, to contrive, but not vindicate, corpuscular mechanism that would serve to explain chemical phenomena. Knowledge of the chemical phenomena was itself to be established by experiment. The fact that a colourless solution of mercury sublimate turns orange on the addition of salt of tartar and can be rendered colourless again by adding oil of vitriol constitutes chemical knowledge. At the level of experiment we have 'chymical reasoning' invoking 'chymical notions'. The 'chymical explication of a phenomenon' established at this level is distinct from a 'truly philosophical or mechanical' one.²³ This interpretation avoids the problematic ascription of 'chemical' properties to corpuscles, and spares us the task of identifying in Boyle just what those chemical properties were and how he sought to identify them.

8.7 Chemical Properties and Essential Properties

There is a view on chemical substances to which a corpuscular theory would seem to readily lend itself. On this view, chemical substances are divided into kinds insofar as the corpuscles that constitute them are divided into kinds. Newman (2006, p. 198) has recently attributed a position akin to this to Boyle. He interprets Boyle's mechanical chemistry involving semipermanent corpuscles as providing Boyle with a means to identify the essential properties of substances by way of their chemical as opposed to physical properties.

It was chymistry that allowed him to distinguish the essential differences of bodies in a relatively certain fashion, and without such stable essences Boyle could not argue that the qualitative mutability of the phenomenal world was mostly a matter of alterations in texture imposed on fundamentally unchanged corpuscles by mechanical means.

The essential properties of substances stem from the character of the semi-permanent corpuscles that compose them. Chemical substances fall naturally into kinds because corpuscles fall naturally into kinds, distinguished from each other by the essential character of those corpuscles. Non-essential properties arise from arrangements or motions of the corpuscles or perhaps their interaction with other particles such as those presumed to constitute light. On this view, the essential properties of gold, such as its resistance to nitric acid, stem from the nature of the corpuscles that comprise it. Inessential properties such as temperature are to be attributed to the rapid motions of those corpuscles that retain their identity, so that gold is gold whether it is hot or cold.

The main text that Newman appeals to in order to support his attribution of a position such as this to Boyle is the latter's 'History of particular qualities', published in 1670. In that essay Boyle (2000, Vol. 6, p. 280) does make a distinction between essential and what he terms 'extra-essential' qualities.

For here it is to be considered, that besides that peculiar and Essential Modification which constitutes a Body, and distinguishes it from others that are not of the same Species, there may be certain other Attributes that we call *Extra-essential*; which may be common to that Body with many others, and upon which may depend those more external Affections of the Matter which may suffice to give it this or that Relation to other bodies, divers of which relations we style Qualities.

Boyle's position is illustrated by examples. Degree of hotness is an extra-essential property of iron, an iron rod being as much iron after it is made red hot by beating as it was before. Pieces of iron, silver and wood retain their essential properties whether they are rough or rendered smooth enough to regularly reflect light.

In 'History of particular qualities' Boyle writes of the essential properties of 'bodies', where he clearly intends by that term samples of substances, such as iron, gold and so forth. But Boyle (2000, Vol. 6, p. 281) also refers to the essential properties of corpuscles. Given Boyle's mechanical matter theory it can be said, in general terms, that corresponding to the essential properties of a body, such as a lump of iron, there must be some permanent underlying corpuscular structure responsible for those essential properties.²⁴ Any change in the structures responsible for the essential properties of iron will result in the iron changing into some other substance or substances. By contrast, the structures responsible for the extra-essential properties of the iron, such as its temperature, can change without the iron losing its identity as such. This much is clearly implied by Boyle's text. But Newman (2006, p. 197) assumes more. He construes Boyle as identifying the essential properties of a substance with its chemical properties. 'The heat that one feels upon rubbing iron, brass, wood, or stone can be induced and allowed to depart without altering the chemical properties of the material being rubbed, which are taken as a measure of its essential character.'

I do not find grounds for identifying essential qualities with chymical qualities in Boyle's text. There is the further difficulty of identifying which qualities are chymical ones in Boyle's schema. When Boyle gives examples of the essential properties of substances he lists qualities other than what might reasonably be regarded as

chemical ones. For instance, yellowness and ductility are included in the list of the essential properties of gold given by Boyle (2000, Vol. 5, pp. 322–323). If there are semi-permanent particles in nature distinguished by their essential structure and responsible for the chemical properties of the substances they are least parts of, then there is a sense in which corpuscles, and hence chemical substances fall into natural kinds. This clashes with Boyle's view, that I have described at length, that the division of substances into kinds by specification of the essential properties of those kinds is a result of human convention.

There is a further difficulty posed by Boyle's text for Newman's position. Whilst it is clear that in Boyle's view the essential properties of bodies must correspond to some underlying mechanical structure, that structure need not consist in some semi-permanent particle. According to Boyle (2000, Vol. 6, pp. 281–282, in the very article drawn on by Newman as his source for the views he attributes to Boyle, explains that only part of the structure of corpuscles can be regarded as essential to the nature of the bodies they form, other parts being 'extraessential'.

For if Corpuscles without looseing that Texture which is Essential to them, may (as we have show'd they may) have their Shape, or their Surfaces, or their Scituation changed; may also admit of Alterations, (especially as these Corpuscles make up an Aggragate or *Congeries*.) as to Motion or Rest; as to these or those degrees or other circumstances of Motion; as to Laxity and Density of parts, and divers other Affections; why should we not think it possible, that a single (though not indivisible) Corpuscle, & much more an Aggregate of Corpuscles, may by some of these, or the like changes, which, as I was saying destroy not the Essential texture, be fitt'd to produce divers other Qualities, besides those that necessarily flow from it.

Newman (2006, p. 197) raises the question of how Boyle distinguished essential from non-essential properties. 'How did Boyle know which properties of a body were essential?' Given Boyle's views on the conventional character of essential properties it would seem that fixing a list of essential properties is something to be decided rather than discovered. But this issue aside, I find the answer to the question that Newman (2006, p. 198) attributes to Boyle problematic.

[I]t was above all the classification into chemical species that allowed Boyle to determine the essential differences of the aggregate corpuscles. *Colours* contains large sections devoted to indicator tests for deciding whether a particular substance belongs to the class of 'acid salts'. 'alkalitate salts', or 'urinous salts'. In other contexts, he employs such time-honoured tests as cupellation, dissolution in different mineral acids, and colour of flame to detect a metal or other substance when its presence is not obvious to the senses.

Many of these tests are based on the assumption that the aggregate corpuscles being tested for are not destroyed by the test itself – instead they remain undivided during the procedure and hence retain their identity. At the same time, the reagent employed to reveal the hidden substance is assumed to react selectively on the latter's aggregate corpuscles (by causing them mutually to disperse or coalesce, for example) and hence to circumvent the sort of generalised mechanical effect that Boyle describes as 'extraessential'.

A problem I have here is that I am not convinced that tests of a substance that leave it unchanged are more revealing and significant than those that do not. I can distinguish between a box of genuine matches and a box of fakes by striking a few of each. The effectiveness of the test is not impaired by the fact that the genuine

matches are destroyed in the process. For Boyle, some colour changes arise from essential changes (changing mercury into its red oxide by heating) and others do not (raising steel to a high temperature). Further, the tests for identifying and classifying salts mentioned by Newman do involve chemical transformation of them. It is true that the metallic bases of the salts remain unchanged by the indicator tests but it is salts that are divided into families by the tests, not metals.

If Newman's interpretation of Boyle's texts were correct, then we would have a neat account of what a chemical substance amounts to in Boyle's chemistry. Substances are characterised by their essential, chemical, properties as opposed to their inessential, physical, ones. What is more, chemical substances retain their identity so long as the semi-permanent particles that compose them remain intact. I reject this view because I cannot find in Boyle the identification of essential with chemical properties nor do I find a distinction in Boyle between chemical and other kinds of properties. Boyle's mechanical contrivances aimed at explaining qualities in general and chemical properties in particular are too diverse and ad hoc to lend themselves to an interpretation that would identify semi-permanent particles as the seat of chemical properties, whatever those latter might be. We have seen that Boyle divided 'chemical concretes' into kinds by means of conventional definitions specifying a 'concourse of accidents' that involved properties, such as colour and degree of malleability, not obviously or unproblematically classified as chemical.

8.8 The Mechanical Philosophy Versus the Experimental Philosophy

Like his fellow advocates of the mechanical philosophy, Boyle used the term 'mechanical' not only in the context of a mechanical matter theory but also in a more common and less strict sense that fitted well with the emphasis the mechanical philosophers placed on the centrality and importance of experiment. When Boyle's contemporary Henry Powers published a book summarising an approach very similar to that of Boyle he chose to call it 'Experimental philosophy' although 'The mechanical philosophy' would have served as well, given the contents. I have argued that seventeenth-century chemistry owed little to mechanical matter theory in the strict sense. But what of the more general senses of mechanical that had natural links with experiment? To what extent were advances in seventeenth-century chemistry the fruits of the mechanical philosophy interpreted in some of the common senses described in 6.8 rather than in the strict sense?

There is no doubting that seventeenth-century chemistry was advanced and substantiated by way of experiment. Chemists, whether they were Aristotelians, Paracelsians, mechanical philosophers, or apothecaries and metallurgists subscribing to no explicit philosophy at all, conducted their search for chemical knowledge by way of experiments conducted in the contrived situations of the workshop or laboratory. Their practice clashed with a hardline Aristotelian view to the effect

that it is misguided to attempt to understand the natural world by upsetting the course of nature by way of artificial interventions. Seventeenth-century chemistry was mechanical in the sense of artisanal.

We have noted that another common sense of mechanical was that whereby the behaviour of a clock is understood in terms of the relations between its component parts. Some of chemistry was mechanical in this sense insofar as the nature of complex substances was understood in terms of their components. Understanding chemicals by reference to their chemical structure is like understanding clocks and watches by reference to their mechanical structure, both involve breaking down wholes into their 'parts' and building them up from their 'parts'.

Having pinpointed some common senses in which chemistry might be said to be mechanical, it is important to be clear about what this means for the nature of seventeenth-century chemistry. Chemical substances can be learnt about and even brought into existence in the artificial conditions of a laboratory experiment but this does not mean that chemicals are mechanical artefacts. Boyle makes part of the point I am getting at. He makes it about the products of tradesman but it applies equally to the productions of the experimental chemist. Many artificial productions, Boyle (2000, Vol. 6, pp. 467–468) writes,

do differ from those that are confessedly natural, not in essence, but in efficiencies; there are very many things made by tradesmen, wherein nature appears manifestly to do the main parts of the work: as in malting, brewing, baking, making of raisins, currants, and other dried fruits, as also hydromel, vinegar, lime etc. and the tradesman does but bring visible bodies together after a gross manner, and then leaves them to act one upon another, according to their respective natures.

In the *Sceptical chemist* Boyle makes this point in the context of chemical productions of the laboratory. The instruments used in the laboratory such as acids or heat are 'Agents of Nature's own providing and whose chief Powers of Operation they receive from their own Nature or Texture, not the Artificer' so that their effects are produced 'whether the Artificer intended it or no'. Boyle (2000, Vol. 2, p. 300) proceeds to draw an analogy between chemistry and gardening.

And, indeed, the Fire is as well a Natural Agent as Seed; and the Chymist that employs it, does but apply Natural Agents and Patients, who being thus brought together, and acting according to their respective Natures, perform the worke themselves; as Apples, Plums, or other fruit, are natural Productions, though the Gardiner bring and fasten together the Sciens of the Stock, and both Water, and do perhaps divers other ways Contribute to its bearing fruit.

Chemists can bring about chemical changes by mixing, dissolving, grinding, heating and so on. They can create the situation in which chemicals combine, but which chemicals combine with which and to what degree is not something the experimenter can determine. Chemical substances, whether produced in nature or in the laboratory, 'act upon one another according to their respective natures'.

The above points do not rest upon knowledge of some ultimate matter theory that explains the origins of the 'natures' of chemical substances. Boyle, of course, considered those natures to arise from the shapes, sizes and motions of corpuscles according to the mechanical philosophy in the strict sense. A problem here lies in

dis-analogies between chemical substances and mechanical artefacts such as clocks and watches (disregarding the point that the latter are not mechanisms in the strict sense). Parts of a watch or clock do not combine spontaneously to form the whole in the way that chemicals combine to form compounds. Further, watches do not combine with other watches in ways characteristic of watches, whilst chemical compounds do combine with each other in ways characteristic of their kind. Admitting that chemistry is mechanical in the common senses I have identified gives a defender of the mechanical philosophy in the strict sense much less than is required.

These considerations make it possible to appreciate that Newman (2006, pp. 198–215) has given too much credence to some key arguments of Boyle to the effect that chemical qualities, along with all other qualities, are basically mechanical.²⁵ As Newman documents, Boyle gave many examples of how properties of substances can be changed ‘mechanically’. He identifies experiments that Boyle described as the ‘fittest’ to support his position. It involves (mechanically) adding one colourless liquid to another colourless liquid with the result that a third liquid is produced that is permanently deeply coloured. The mechanical addition of a third colourless body results in this deep colour disappearing leaving a colourless liquid once again.²⁶ Experiments such as this do help to establish that colour is an emergent property of a body depending on underlying features of that body which can change when those underlying features are changed. It also undermines the idea that colours stem from immutable substantial forms or Principles. But does it establish that colours are ‘mechanical’? An analogy will help illustrate the point that the argument does not take us as far as Boyle or Newman imply. An acorn can be induced to grow into an oak tree by ‘mechanically’ placing it in the ground and ‘mechanically’ adding water and nutrients. This is hardly sufficient to establish that the process is ‘mechanical’ any more than the fact that I can be rendered unconscious by a mechanical blow to the head establishes a materialist theory of the mind. As Boyle put it when discussing changes brought about by the interventions of tradesmen, a chemist who adds one colourless liquid to another ‘leaves them to act on one another according to their respective natures’. In the experiment highlighted by Boyle the natures of the combining colourless liquids are such that they combine to yield a deeply coloured resultant liquid. The nature of that coloured liquid is such that it can be transformed into a colourless liquid by the addition of a small particle of an appropriate solid. The additional assumption, that the natures of the substances involved and the processes involved in their transformation are ‘mechanical’ in a sense sufficiently strong to support Boyle’s mechanical philosophy is gratuitous. Of course, if a reliance on contrived experiments is sufficient to qualify a practice as ‘mechanical’, then Boyle’s arguments do support the mechanical philosophy. But this move makes a host of Aristotelians and Paracelsians into mechanical philosophers and renders the appellation relatively innocuous.

Boyle did attempt to take his case for the mechanical philosophy further, as we have seen. He attempted to contrive possible mechanisms, in the strict sense, that can serve as possible explanations of qualities and their transformation. Some of his attempts were more contrived and less plausible than others. His suggestion that the temperature rise of a piece of lead is due to an increase in the rapidity of

the motions of the corpuscles that comprise it enabled Boyle to accommodate the fact that lead is lead as much when it is hot as when it is cold. But lead changes colour when its temperature is sufficiently high, as Boyle noted. Here Boyle was less able to contrive a possible mechanism. He believed colour of a body to result from the interaction of the corpuscular structure of that body with light corpuscles but was unable to supply details let alone substantiate them. There was a further complication here. Whilst lead remains lead through the colour changes brought about by heating, the colour changes involved in his mixing of liquids involved chemical changes. Maybe I am employing a distinction here not available to Boyle. His characterisations of substances by reference to a 'concourse of accidents' did not lend itself to a precise identification of chemical substance as we have seen. But this simply reinforces one of my central points with respect to Boyle's chemistry. So intent was he on invoking chemistry in support of a strict sense of the mechanical philosophy that he speculated on possible mechanical causes of the full range of changes accompanying chemical change without having framed appropriate chemical notions able to guide and be substantiated by experiment. This is the point I have been emphasising by contrasting Boyle's chemistry with the chemistry of chemical combination encapsulated in Geoffroy's table of *rappports* highlighted by Klein.

Like many of his contemporaries, Boyle was an experimental philosopher. He attempted to understand nature by intervening in it. He recognised that concepts, that he referred to as notions, needed to be constructed that would make possible the formulation of hypotheses capable of guiding research and liable to be converted into 'matters of fact' once the degree of independent experimental support warranted it. This aspect of his work is best exemplified in his pneumatics. Boyle was also a mechanical philosopher in a strict sense. He aspired to make a case for the mechanical philosophy by contriving mechanisms capable of explaining the phenomena. Chemistry was the main area in which he sought to make his case. He declined to do so in pneumatics, freely admitting that he was unable to contrive mechanisms for the weight and spring of the air. The moral I draw from this is that experimental science called for appropriate concepts and hypotheses capable of guiding, and liable to confirmation by, experiment rather than the mechanical philosophy. Boyle made major contributions to pneumatics by way of concepts that were not mechanical in the sense of the strict mechanical philosophy. He did not make comparable contributions in chemistry because he was over-concerned to contrive mechanical explanations and less concerned with framing notions capable of productively informing chemical research.

8.9 Newtonian Affinities

I have argued in the foregoing sections that Boyle's mechanical philosophy did not help him make experimental progress in chemistry and that the mechanisms he contrived to explain known chemical phenomena were *post hoc* and highly artificial.

Newton's atomic chemistry, made possible by his transformation of the mechanical philosophy by introducing forces, is subject to a similar critique.

As we have seen, Newton, like Boyle, assumed a hierarchy of particles, with mechanical atoms as the foundation, combining in various ways to yield particles of higher degrees of composition. A difference was that Newton assumed attractive forces to be responsible for holding composite particles together. Such a stance did not point in the direction of a notion of chemical kinds of the sort implied in Geoffroy's table. Given chemical substances and their mode of interacting with other substances, Newton could attribute this behaviour to attractive forces acting between the composite particles he presumed to be the least parts of those substances. But this presumes knowledge of the substances and their properties. Newton's atomic chemistry did not have the resources to non-arbitrarily postulate kinds of corpuscles, nor any access to the force laws that might govern the attractions acting between them.

As we saw in the previous chapter, the most detailed treatment of chemistry that Newton published was *Querie 31* of the *Opticks*. Several features of his treatment of chemistry in that text supports my contention that his atomic chemistry involved adapting his atomism to chemical, and other knowledge, acquired by other means. His matter theory was not capable of guiding experimental chemistry any more than Boyle's was. In *Querie 31* Newton (1979, pp. 380–381) does mention series of precipitations of the kind central to Geoffroy's classification and he does refer to the important regularity that salts result from the combination of an acid and a 'dry earth'. But to interpret these passages as important advances in chemistry emerging from Newton's atomism is to ignore several features of the contents of *Querie 31*. Firstly, the experimental facts that Newton accommodates to, and takes as evidence for, his atomism are not novel fruits of Newton's theory but experimental knowledge common to all chemists of the time. Secondly, Newton's remarks about series of precipitations and salt formation are not singled out as important features of experimental chemistry. Rather they are interspersed with many other accommodations of Newton's atomism to experimental facts, many of them not 'chemical' at all. I proceed to illustrate and support these claims.

An interpretation of Newton's paragraphs on precipitation as the source of the affinity tables that helped to shape eighteenth-century chemistry is historically false and attributes to Newton's atomism a facility to guide chemical experimentation that it did not possess.²⁷ As Klein (1994, 1995, 1996) has insisted, by the early eighteenth century preferential precipitations had a history of two hundred years or more and had become a theoretical and experimental focus of chemists working in association with the Botanic Gardens in Paris in the last few decades of the seventeenth century. One of them, Christopher Glaser, described a series of precipitations in much the same way that Newton came to do, in his text, *The Compleat Chemist*, published in English in 1677,

The Silver dissolv'd in the Aqua-fortis, and poured into the Vessel of water, precipitates and separates itself from its Dissolvent, by putting a plate of copper into it . . . The Silver is found in the bottom. It must be wash'd, dry'd, and kept (if you please) in form of Calx, or else reduc'd into an Ingot in a Crucible, with a little Salt of Tartar. But if into this second

water, which is properly a Solution of Copper, you put a body more earthy and porous than Copper, as Iron is, the Copper precipitates, and the Corrosive Spirits of the Aqua-fortis fasten to the substance of Iron; which may likewise be precipitated by some Mineral more earthy and porous than Iron, as Lapis Calmonaris and Zink.²⁸

Far from being a product of atomism these series of precipitations posed a problem for it, as is clear from the text of 1975 written by Nicolas Lemery, Glaser's successor as Professor at the Botanic Gardens in Paris. If the absorption of silver, copper and iron by nitric acid is explained in terms of some congruity between the shapes of acid particles and pores in the metal particles then how can one explain why the silver particles, once absorbed by the acid, are displaced by added copper particles which are in turn replaced by iron particles? To be sure, Newton was able to counter the problem by appealing to attractions of varying degrees between particles. He was able to accommodate phenomena of precipitation to his atomism in a superior way to previous atomists, but it was a mere accommodation nevertheless, and an accommodation to phenomena that had been known for decades.

Any temptation to read Newton's remark that salts are the product of the combination of an acid with a dry earth as a reference to an experimental law of the kind identified by Geoffroy should be dissolved once Newton's remark is placed in its context. I reproduce the whole paragraph, which was quoted in part in 7.3, to show the extent to which Newton's remarks about salt formation occur alongside a range of other phenomena, including tastes of acids, which Newton (1979, pp. 385–386) accommodates to his atomism.

When Mercury sublimate is re-sublimed with fresh Mercury, and becomes *Mercurius Dulcis*, which is a white tasteless Earth scarce dissolvable in Water, and *Mercurius Dulcis* re-sublimed with Spirit of Salt returns into Mercury sublimate; and when Metals corroded with a little acid turn into rust, which is an earth tasteless and indissolvable in Water, and this Earth imbibed with more acid becomes a metallick Salt; and when some Stones, as Spar of Lead, dissolved in proper *Mentruums* become Salts; do not these things shew that salts are dry earth and watery Acid united by Attraction, and that the earth will not become a salt without so much acid as makes it dissolvable in Water? Do not the sharp and pungent Tastes of Acids arise from the strong Attraction whereby the acid Particles rush upon and agitate the Particlees of the Tongue? And when Metals are dissolved in acid *Mentruums*, and the Acids in conjunction with the Metals 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 are not dissolvable by the Moisture of the Tongue, act not upon the Taste.

Elsewhere in *Querie 31* Newton gives an atomic interpretation of a range of other phenomena, including the absorption of water by deliquescent substances, the solution of salts in water and the generation of heat accompanying the mixing of acids and alkalis. Because Newton's discussion involves an accommodation of chemical, along with other, phenomena to his atomism involving attractions, it does not involve a conceptualisation of chemistry that can feed productively into its experimental practice.

8.10 Chemistry from Newton to Lavoisier

I have followed Klein and highlighted the significance of the general notion of chemical combination implicit in Geoffroy's table, that was abstracted from metallurgy and pharmacy and from the studies of salt formation carried out by his predecessors at the Botanical Gardens in Paris, Lemery and Homberg. The centrality of the idea that chemical compounds can be synthesised from as well as analysed into their components was made explicit by Geoffroy, as Klein has noted.²⁹ In none of his published works on chemistry did Geoffroy invoke, nor did he need to invoke, atoms or corpuscles.³⁰ The path to the empirical regularities implicit in Geoffroy's table, which he referred to as 'laws', owed no debt to atomic or corpuscular theories and their formulation required no reference to them.

Having said this, it must be acknowledged that Geoffroy's achievement did not usher in a clear separation of an experimental chemistry and philosophical matter theories. Those in the business of articulating matter theories were very quick to take advantage of the new chemistry and accommodate their matter theory to it. Newton's atomism could readily be adapted to it by interpreting Geoffroy's *rapports* as representing attractions between atoms or corpuscles. So natural was this step that, as we have seen, Geoffroy's table was read by contemporaries and by some subsequent historians as a rendering of the affinities invoked in *Querie 31* of Newton's *Opticks*. However, whilst atomism could be readily accommodated to 'laws' of the kind referred to by Geoffroy, and this fact may have contributed to their ready acceptance, atomic theories were not capable of predicting them or guiding experimental chemists towards them. Atomic theories were totally open and un-specific on the question of where the corpuscles presumed to take part in chemical combinations were to be placed in the hierarchy of structured particles, mechanical theories of the kind championed by Boyle were completely open and un-specific on the question of the shapes, sizes and motions of atoms or corpuscles whilst Newtonian atomism was correspondingly open and un-specific on the question of the specification of inter-atomic or inter-corpuscular forces. Articulations of atomic matter theories and their accommodation to the phenomena owed a debt to advances in chemistry of the kind embodied in Geoffroy's table but the reverse is not true.

As a fundamental matter theory, Newton's atomism was an improvement on its competitors provided one could learn to live with the unexplained (and unspecified) forces that it involved. It was an improvement because of the extent to which it could be accommodated to the phenomena and because the mechanics of the *Principia* served as a model of how complex systems could be explained by appeal to the forces governing their components. Because of this, Newton's matter theory was, in a sense, highly influential in many quarters. Thackray (1970) has traced the development of Newtonian matter theory in the eighteenth-century. One striking feature of it is its lack of productiveness as far as experimental chemistry is concerned, a point already implicit in my discussion of Boscovich's elaboration of Newtonian atomism described in the previous chapter. Robert Siegfried has recently published a book, *From elements to atoms* (2002), in which he traces the history of chemical composition from the seventeenth century to Dalton's atomism early in the nineteenth.

The fact that he invokes pre-Daltonian atoms only in the context of the failure of the mechanical philosophers to develop an atomic chemistry, and that there are only three minor references to Newton's matter theory in his study, lends support to my view that eighteenth-century developments in chemistry owed little to atomic matter theory.

Eighteenth-century developments in the chemistry of combination can reasonably be seen as extensions and elaborations of the notions of chemical compound and combination implied in Geoffroy's table of 1718. Subsequent affinity tables, as they became known, expanded the range of substances that were included. By the second half of the century the gases became recognised as chemical substances, beginning with 'fixed air' (carbon dioxide) discovered by Joseph Black in the 1750s.³¹ This move enabled consistent sense to be made of the weight relations involved in chemical combination and helped to distinguish between reactions that involved building up and those that involved breaking down of component substances. The culmination of these developments was Lavoisier's chemical 'revolution'. It involved the recognition that certain chemical substances, namely the elements (that Lavoisier referred to as 'simple substances'), are components of chemical compounds but do not themselves have chemical components. A basic principle of the new chemistry was that the weight of elements is conserved in chemical reactions.³² The new chemistry made it possible to recognise that the combustion of metals involves the combination of metals with oxygen rather than the expulsion of a substance (phlogiston) from them. It is significant for my story that Lavoisier explicitly separated his chemistry from speculations about atoms. He understood elements as those substances which cannot be broken down further by chemical means. As for the atoms that might be supposed to compose the elements Lavoisier (1965, p. xxiv) judged that 'it is extremely possible but we know nothing at all about them'.

Besides the chemistry of combination that had resulted in Lavoisier's chemistry of elements and compounds by the end of the century there was the chemistry of the materials comprising plants and animals. As Klein and Lefèvre (2007, Part III) describe in detail, this branch of chemistry could not be accommodated by the chemistry of combination highlighted by Geoffroy. What came to be described as 'organic' substances could be analysed into components but they could not subsequently be synthesised from those components as 'inorganic' substances could. The way in which organic chemistry was eventually subsumed into the chemistry of combination is a highly significant moment in our investigation of the history of atomism as we will see in the next chapter.

Notes

1. From the opening of 'Some specimens of an attempt to make Chymical Experiments Useful to illustrate the notions of the Corpuscular Philosophy' (Boyle, 2000, Vol. 2, pp. 85–92).
2. See, for example, Partington (1961, Vol. 2, p. 496).
3. See the 'Concise conclusion' of Newman (2006) for his characterisation of Boyle's chemistry as revolutionary.
4. Key references are Klein (1994, 1995 and 1996) and, more recently, Klein and Lefèvre (2007).

5. See, for instance, Thackray (1970, pp. 85–88).
6. See, for instance, A. M. Duncan (1964, pp. 177–194).
7. See F. L. Holmes (1989) for a detailed account of the emergence of the chemistry of salt formation.
8. As is noted by Klein and Lefèvre (2007, p. 111) this notion of purity of a substance falls short of the notion that involves composition from elements in the right proportion, which comes later.
9. Notable examples are Jean Beguin (1624), Nicaise Le Febvre (1664), Christopher Glaser (1677), and Nicolas Lemery (1677).
10. For details and documentation of these modes of construing the action of acids by metallurgists and pharmacists see Klein and Lefèvre (2007, pp. 136–147).
11. In their account of the Paracelsian philosophy employed by the French chemists, Klein and Lefèvre (2007, pp. 39–44) stress the non-material nature of the principles informing chemical substances, rendering them akin to Aristotelian forms in this respect. They insist that those principles did not exist in compounds as material parts of them. As a consequence, the ‘essences’ generated by the distillation of plant and animal materials could be seen as resulting from transmutions rather than extractions of already existing components. Stressing this aspect of the Paracelsian view helps Klein and Lefèvre portray the view of combination involved in Geoffroy’s table as a novelty. Newman (2008, pp. 176–182) objects, finding plenty of examples in Paracelsian texts of talk of analysis and synthesis that seems to imply the continued existence of constituents, and even of ‘principles’, in the substances they are components of. Newman is correct to point out that the Paracelsian texts are more complex than the generalisations of Klein and Lefèvre allow for. Further, it is not surprising that some talk of analysis and synthesis of chemical substances can be found in Paracelsian texts since reversible reactions (in the sense of Klein and Lefèvre) were as much a reality for the authors of those texts as for any other chemist of the time. I am not surprised that chemical philosophers of a Paracelsian or Aristotelian persuasion were able to adapt their systems to fit reversible as well as irreversible reactions. Those philosophies were sufficiently imprecise and flexible to make such accommodations possible. (I cannot resist reminding the reader that Boyle’s mechanical atomism was also highly flexible and adaptable, a point I will be putting to use later in this chapter.)
12. As cited by Klein (1996, p. 276). I have omitted the italics added by Klein.
13. See, for instance, Klein and Lefèvre (2007, p. 111).
14. For a discussion of eighteenth-century affinity tables see Duncan (1964 and 2001) and Klein and Lefèvre (2007, chapter 9)
15. Boyle (1990, Vol. viii, f184, reel 5, frame 189).
16. Boyle’s views are spelt out in ‘Origin of forms’ (Boyle, 2000, Vol. 5, pp. 322–330).
17. For other references to this distinction in Boyle’s ‘Origin of forms’ see Boyle (2000, Vol. 5, pp. 322, 324–325, 330 and 351–352).
18. ‘I observe also, that a Dissolution may be made of the same by *Menstruums*, to which the Chymists attribute (as just now I observed they did to some bodies) a mutual Antipathy, and which therefore are not like to have a Sympathy with the same third body, as I found by trial, that both *Aqua Fortis*, and Spirit of Urine, upon whose mixture there ensues a conflict with a great efforvescence, will each of them apart readily dissolve crude Zince, and so each of them will, with Filings of Copper.’, from ‘Experiments and Notes about the Mechanical Origine or Production of Corrosiveness and Corrosibility’ (Boyle, 2000, Vol. 8, p. 467).
19. The various essays in Boyle’s ‘Experiments, Notes &c. About the Mechanical Origine or Production of divers particular Qualities’ (2000, Vol. 8, pp. 315–523) well illustrate the feature of Boyle’s mechanical theory that I am referring to. As an example of the contrived nature of some of Boyle’s mechanical explanations consider the following passage in which Boyle (2000, Vol. 8, p. 470) offers possible explanations of how Mercury sublimate (mercuric chloride) can lose its corrosiveness when converted to *Mercurius dulcis* (mercurous chloride) by grinding it with mercury. ‘It may perhaps somewhat help us to conceive, how this change

may be made, if we imagine, *that* a company of mere Knife-blades be first fitted with Hafts, which will in some regard inhibit their wounding power by covering or casing them at that end which is design'd for the handle; (though their insertion into those Hafts, turning them into knives, makes them otherwise the fitter to cut and pierce and *that* each of them be *afterwards* sheathed, (which is, as it were, a hafting of the Blades too;) for then they become unfit to stab and cut, as before, though the blades be not destroyed: Or else, we may conceive these Blades without Hafts or Sheathes to be tied up in bundles, or as it were in little faggots with pieces of wood, somewhat longer than themselves, opportunely placed between them. For neither in this new Constitution would they be fit to cut and stab as before. And by conceiving the edges of more or fewer of the Blades to be turn'd inwards, and those that are not, to have more or less of their points and edges to be sheath'd, or otherwise cover'd by interpos'd bodies, one may be help'd to imagine, how the genuine effects of the Blades may be variously lesson'd or diversifi'd. But, whether these or any like changes of Disposition be fancy'd, it may by Mechanical Illustrations become intelligible, how the Corrosive Salts of common Sublimate may lose their efficacy, when they are united with a sufficient quantity of quicksilver in *Mercurius dulcis*.'

20. See Newman (1996) for a detailed account of Boyle's borrowings from Sennert..
21. Boyle's position provides an answer to a longstanding question, that of the way in which components of a compound exist in the compound. They exist in the compound in some strong sense since they can be recovered from it. But they cannot exist in too literal a sense because the compound does not have properties that are the sum or average of those of its constituents. The answer implicit in Boyle's theory is that the corpuscles of the ingredient substances are present as parts of the corpuscles of the compound, but the shapes, sizes and motions of the corpuscles of the compound, on which its properties depend, differ from the shapes, sizes and motions of the corpuscles of the ingredients. Boyle has an answer to the puzzle. Whether it was a satisfactory or the right answer is another question. (The contemporary answer to the question of the sense in which elements exist in their compounds is quantum mechanical.)
22. Boyle's account is in his 'Experiments touching colours' (Boyle, 2000, Vol. 4, pp. 150–153). Clericuzio's discussion is in Clericuzio (1990, pp. 578ff.) and Newman's account, and his critique of Clericuzio, is in Newman (2006, pp. 181ff.).
23. My interpretation is not completely borne out by Boyle's words. When Boyle spells out the 'chymical reason' for the happenings in his experiments with mercury sublimate most of his discussion is couched in terms of experimental manipulations perfectly in line with my interpretation, but corpuscular talk slips into the discussion. Boyle talks, for instance, of the 'Coalition of the Mercurial particles with the Saline ones'. So convinced is Boyle of the corpuscular nature of chemical substances that he can substitute talk of the combination of mercury sublimate with salt of tartar in terms of combining particles without realising the shift in levels.
24. Boyle (2000, Vol. 5, p. 324) talks of the mechanical structure responsible for the essential qualities of a body in 'Origin of forms' referring to that mechanical structure as the 'stamp' of the body.
25. Newman (2004, pp. 271–283) makes many of the points I have made about Boyle on the relation between the natural and the artificial. He explicitly makes the point that Boyle's arguments 'would have been effective only against the most rigid proponents of an absolute distinction between art and nature' clearly implying that a range of Aristotelean alchemists from Geber to Sennert would have had no problem about endorsing Boyle's views. This does not sit well with Newman's insistence in *Atoms and Alchemy*, that Boyle's chemistry supported the mechanical philosophy in a way that distinguished his position from that of his Aristotelian and chymical opponents.
26. Newman (2006, pp. 182–185) utilises Boyle's discussion of colour changes in Boyle (2000, Vol. 4, pp. 150ff.)
27. For an example of the identification of Newton as the source of affinity tables see Maurice Crossland, (1963, pp. 369–441), especially p. 382.
28. As cited by Klein (1995, p. 89).

29. In a 1704 article Geoffroy wrote: 'What completely assures us that we have succeeded in investigating the composition of bodies is, having reduced mixta into their simplest substances that chemistry can provide, we can recompose them by reuniting these same substances', as cited by Klein (1996, p. 272).
30. Geoffroy did invoke a corpuscular theory in a posthumously published pharmaceutical work as Klein (1995, p. 93) has noted.
31. The work of Black serves to illustrate the point that, on the one hand, many eighteenth-century chemists did not separate their chemistry from matter theory and that, on the other hand, their experimental progress owed little debt to matter theory. In his lectures at the University of Edinburgh Black articulated a Newtonian matter theory and he also construed affinity tables in terms of Newtonian attractions. See Thackray (1970, pp. 223ff.). But the experiments involved in his preparation and identification of fixed air (carbon dioxide) involved noting the weight loss accompanying the conversion of limestone into quicklime by strong heating and the recovery of limestone by heating an aqueous solution of quicklime with potassium carbonate. The experimental argument was quite independent of his atomism. The same can be said for Black's experimental investigations of heat that led to his identification of latent heat.
32. Attention to weight relations in chemistry had a long history that goes back at least as far as Geber. Lavoisier himself drew on the seventeenth-century researches of Von Helmont. On the latter point see Newman and Principe (2002, pp. 296–309).