1

Purpose of this book

The *Principia* was to remain a classic fossilized, on the wrong side of the frontier between past and future in the application of mathematics to physics.†

1.1 The problem

1.1.1 **Principia** *as a plural*

Newton's *magnum opus* bears a title which is both imposing and perplexing. Undoubtedly, the great achievement referred to in the title consists in the application of 'mathematical principles' to the physical world, or better to 'natural philosophy'. However, when we ask ourselves which, and how many, are Newton's mathematical principles, the answer does not come so easily. We know much more about the natural philosophy. Many scholars have taught us about the laws of motion, absolute time and space, the law of universal gravitation, the cosmology of void and matter. The names of I. Bernard Cohen, Richard Westfall, Rupert Hall and John Herivel come immediately to the mind of any historian of science. But, from the point of view considered in my research, Tom Whiteside comes first.

In his many papers, but especially in the critical apparatus of the sixth volume of 'his' *Mathematical Papers*, Whiteside has given a profound and detailed analysis of Newton's mathematical natural philosophy. As a study of Newton's mathematical achievements in the *Principia* Whiteside's studies will endure and this book has not been written to replace them. Actually, I began the research which led to this book by reading and following Whiteside's studies. Reading the preparatory manuscripts for the *Principia*, aided by Whiteside's commentaries, I realized how complex, varied and stratified are Newton's mathematical methods for natural philosophy. Newton did not possess a universal mathematical tool which he applied to natural philosophy. He was rather in possession of a whole variety of mathematical instruments. These methods, sometimes in conflict between

† Hall (1958): 301.

themselves, originate from different periods of Newton's mathematical life and play different roles in the *Principia*. I thus learnt that '*Principia*' deserves to be treated as a plural. Newton's masterpiece, under its classic appearance given by division into Lemmas, Propositions, Corollaries and Scholia, hides a plurality of mathematical methods. This diversity, however, is somewhat hidden by Whiteside himself, who, for the benefit of the modern reader, translates Newton's demonstrations into modern mathematical language. Paradoxically, the effect of Whiteside's penetrating mathematical insight is that of suppressing diversity and plurality. From this point of view, he did not help me in finding an answer to my search for the mathematical methods of Newtonian natural philosophy.

The plurality and complexity of the methods employed in the *Principia* is reflected by the diversity of judgments that have been given during the centuries. A passage from the preface (which is now believed to come from the pen of Fontenelle) to l'Hospital's *Analyse* published in 1696 has remained justly famous since Newton himself quoted it in the 1710s:

> Furthermore, it is a justice due to the learned M. Newton, and that M. Leibniz himself accorded to him: That he has also found something similar to the differential calculus, as it appears in his excellent book entitled *Philosophiae Naturalis Principia Mathematica*, published in 1687, which is almost entirely about this calculus.†

This quotation shows us how Newton's mathematical natural philosophy was perceived by some late-seventeenth-century natural philosophers. It was perceived as based on modern techniques, geometrical limits or infinitesimals, and therefore ready to be translated into the language of the fluxional or differential/integral algorithms. A completely different evaluation was given in 1837 by William Whewell who wrote in *History of the inductive sciences*:

> The ponderous instrument of synthesis, so effective in [Newton's] hands, has never since been grasped by one who could use it for such purposes; and we gaze at it with admiring curiosity, as on some gigantic implement of war, which stands idle among the memorials of ancient days, and makes us wonder what manner of man he was who could wield as a weapon what we can hardly lift as a burden.‡

A few decades later Maximilien Marie seems to reply to Fontenelle, writing:

> In the *Principia* one finds excellent infinitesimal geometry, but I could not find any infinitesimal analysis: I add that to those who wish to see the calculus of fluxions in the *Principia*, one could then also show the differential calculus in Huygens's *Horologium*.§

† 'C'est encore une justice dûë au sçavant M. Newton, & que M. Leibniz lui a renduë lui-même: Qu'il avoit aussi trouvé quelque chose de semblable au calcul différentiel, comme il paroît par l'excellent Livre intitulé *Philosophiae Naturalis Principia Mathematica*, qu'il nous donna en 1687, lequel est presque tout de ce calcul.' L'Hospital (1696): xiv. For Newton's quotation of these lines see Cohen (1971): 294.

‡ Whewell (1837): 167. Quoted in Whiteside (1970): 132n.

§ 'On trouve dans les *Principes de Philosophie naturelle* d'excellente Géométrie infinitésimale, mais je n'y ai pas découvert d'Analyse infinitésimale: j'ajoute qu'à celui qui voudrait voir le calcul des fluxions dans le *Livre des Principes*, on pourrait aussi bien montrer le calcul différentiel dans l'*Horologium*.' Marie (1883–88), **6**: 13.

While Fontenelle stresses the modernity of Newton's mathematical methods in the *Principia*, underlining their equivalence with the new calculus, Whewell and Marie are impressed by the distance which separates Newtonian mathematical natural philosophy from modern analytical mechanics. Marie would completely disagree with Fontenelle and define Newton's method as one which is closer to the 'infinitesimal geometry' of Huygens, rather than to Leibniz's differential and integral calculus.

A text cannot be read out of context. The homogenization effect of Whiteside's notes and the diversity of judgments of Fontenelle, Whewell and Marie are caused by exposing the *Principia* to different audiences, who approach it with different background knowledge and for different purposes. It is thus that demarcation lines between method and method, tensions and equivalences, have now faded away: we cannot see them any longer. A possible way to re-establish the hidden structure of a text is to follow the reactions of its contemporary readers.

1.1.2 The debate on the mathematical methods of the **Principia**, *1687–1736*

At the beginning of the eighteenth century the *Principia* was read, and approved or criticized, by a number of *savants* all over Europe. The debate concerned a whole spectrum of issues: from theology to philosophy, from astronomy and cosmology to the principles of dynamics. The mathematical methods employed by Newton drew considerable attention, particularly in the first two decades of the eighteenth century. A motivation for this interest is easily found in the priority dispute on the invention of the calculus. The Newtonians and Leibnizians became divided during this notorious squabble. After a series of attacks and counterattacks, which began in the late 1690s, in 1713 a Committee of the Royal Society, secretly guided by Newton, formally accused Leibniz of plagiarism. The German diplomat was accused of having plagiarized Newton's fluxional algorithm, publishing in 1684 and 1686 his differential and integral calculus. The debate on the mathematics of the *Principia* is part of the priority debate. Actually Newton tried to use the *Principia* as a proof of his knowledge of calculus prior to 1684, the year in which he sent a first version to the Royal Society.

Newton himself was, however, well aware of the distance that separated the calculus of fluxions (the 'analytical method of fluxions', as he named it) from the geometrical methods employed in the *Principia*. In fact, he was not able to make much use of the *Principia* as proof of his knowledge of the algorithm of fluxions. He could only refer to a few propositions. The bulk of the work, he had to admit, was 'demonstrated synthetically'. He sought to maintain that he had discovered 'most of the propositions' with the help of fluxional calculus, the 'new analysis', but that it was now difficult to see the analysis utilized in the process of discovery.

He further suggested an analogy between his mathematical procedures and those of the 'Ancients'. In 1714, speaking of himself in the third person, Newton wrote:

> By the help of this new Analysis Mr Newton found out most of the Propositions in his *Principia Philosophiae*. But because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically that the systeme of the heavens might be founded upon good Geometry. And this makes it now difficult for unskillful men to see the Analysis by wch those Propositions were found out.†

However, external and internal evidence seems to be against Newton's statement. The preparatory manuscripts of the *Principia* reveal little use of calculus and seem to indicate that Newton wrote it in the form in which it was published.‡ An internal analysis of the structure of the demonstrations in the *Principia* furthermore reveals that Newton's geometrical natural philosophy is, in many significant cases at least, to a certain extent independent of calculus techniques. This independence should be neither overstated nor overlooked. Defining Newton's mathematical methodology employed in the *Principia* is a complex task, not only because of the plurality of Newton's geometrical techniques, but also because of their contiguity with the analytical method of fluxions. In some cases, the geometrical demonstrations of the *Principia* lend themselves to an almost immediate translation into calculus concepts; in other cases, this translation is complicated, unnatural, or even problematic. Needless to say, notwithstanding Newton's rhetorical declaration of continuity between his methods and the methods of the 'Ancients', his geometrical natural philosophy is a wholly seventeenth-century affair.

It is generally maintained that Newton's statements, such as the one quoted above, were not honest. In his recent biography of Newton, Rupert Hall has defined Newton's manoeuvres aimed at stating a use of calculus in the art of discovery of the *Principia* as the 'fable of fluxions'.§ Newton would have been desperately trying to use this fable as proof of his knowledge of calculus before the publication of Leibniz's *Nova methodus* in 1684. It is certainly true that the above quotation has a role in Newton's rhetorical manoeuvres against Leibniz. It is an ambiguous statement and should not be taken as a reliable historical record. However, as I will show in this book, there is some truth in it. At the end of the nineteenth century a manuscript dating from the years of composition of the *Principia* was found: in these pages in Newton's hand the result on the solid of least resistance, expressed in geometric terms in the second book of the *Principia*, is achieved via a fluxional equation.¶ But this manuscript has been considered the exception which confirms

† *Mathematical Papers*, **8**: 598–9. This quotation comes from Newton's review of the *Commercium epistolicum* that appeared anonymously in the *Philosophical Transactions* in early 1715.
‡ Whiteside (1970).
§ Hall (1992): 212–13.
¶ *Mathematical Papers*, **6**: 456–80.

the rule. I will add evidence that Newton had his own good reasons when he stated that the 'new analysis' was one of the ingredients which allowed the writing of the *magnum opus*.

Today, we take it for granted that calculus is a more suitable tool than geometry, particularly in the case of applications to dynamics. But at the beginning of the eighteenth century, the choice of mathematical methods to be applied to the science of motion and force was problematic. Firstly, a plurality of geometrical methods had to be compared not with a calculus, but with a plurality of calculi. Calculus came in at least two forms: it could be based either on infinitesimal concepts or on limits. Furthermore, there were several competing notations broadly speaking falling in two groups, the fluxional and the differential/integral notation. Secondly, the way in which dynamical concepts should be represented was not (and is not) obvious. Thirdly, the calculus (at least up to Euler) was never thought of as completely independent from geometrical representation. Calculus was far from being understood as an abstract uninterpreted formalism: there were geometric 'equivalents' of the algorithm (Archimedes' exhaustion methods, the geometry of fluent and fluxional quantities, the geometry of the infinitely smalls) which were deployed in different ways and for different puroposes. Early-eighteenth-century mathematicians debated such issues.

The question of the equivalence of the *Principia*'s mathematical methods with the calculus was faced in a systematic way in the first two decades of the eighteenth century. Mathematicians belonging most notably to the Basel and to the Paris schools initiated a programme of translation of the *Principia* into the language of the Leibnizian calculus. Their efforts were followed and contrasted all over Europe. If the *Principia*'s mathematical procedures were simply calculus in disguised form, as Fontenelle seems to maintain, translation into the language of Leibniz's differential/integral or Newton's fluxional algorithms would be a *routine* exercise. This, however, was not the case. The Continental mathematicians who, at the beginning of the eighteenth century, set themselves the task of applying the calculus to Newton's *Principia* (most notably Pierre Varignon, Jacob Hermann and Johann Bernoulli) had to surmount difficult problems. This task was part of a wider programme of application of calculus to dynamics, a programme to which the Leibnizians gave priority and publicity.

In this book I will prove that the programme of translation of the *Principia* into calculus language was not an exclusively Continental affair. Newton and a restricted group among his disciples (David Gregory, De Moivre, Cotes, Keill, Fatio de Duillier) were able to apply the analytical method of fluxions to some problems concerning force, motion and acceleration. By reading the correspondence and notes exchanged within the close circle of Newton's more mathematically competent associates we discover that they applied fluxional equations to central forces and motion in a resisting medium. However, unlike the Leibnizians, the Newtonians gave very little publicity to these researches. When studying the Newtonian school one has to draw a distinction between what was public and what was meant to remain private. It is by following the private exchanges amongst the Newtonians that we can begin to discover the validation criteria that they accepted, criteria that determined a publication policy which differed dramatically from that of the Leibnizians.

The debate on the mathematical methods of the *Principia* was concerned with a number of specific issues, such as projectile motion in resisting media, the inverse problem of central forces, and the representation of trajectories. This book is devoted to that debate. I hope that by following it, we will be able to appreciate distinctions and tensions between methods – distinctions and tensions that are difficult to discern with the modern eyes of the mathematician accustomed to present-day analytical mechanics.

1.2 Plan of the work

The book is divided into three parts. In Part 1 I introduce the reader to Newton's *Principia*. In Part 2 I consider the reactions to the *Principia* of three giant readers: Newton himself, Huygens and Leibniz. In Part 3 I devote attention to the two schools that divided Europe in the period considered: the British Newtonian and the Continental Leibnizian. It should be made clear that by 'school' I mean here those very small groups of competent mathematicians that gathered around the two great heroes, defended them and shared with them certain values and methods. The time span goes from the year of publication of the *Principia* to the year of publication of Euler's *Mechanica*. As I will argue in Chapter 9, the advent of Euler's calculus and mechanics marks a watershed. After Euler the *Principia*'s mathematical methods belong definitely to what is past and obsolete.

In Chapter 2 the reader will find a concise presentation of Newton's method of series and fluxions. My aim is to give an idea of the mathematical method, the 'new analysis', that Newton had already developed in its full extent before writing the *Principia*. The experts on Newton's mathematics can skip this chapter. They should, however, take into consideration §2.3 where I contrast what Newton called the 'analytical' and the 'synthetic' methods of fluxions. In fact, in the 1670s Newton distanced himself from his early analytical method, an algorithm based on infinitesimals, in order to develop a geometrical method based on limits. It is the latter which is employed in most of the *Principia*'s demonstrations. Newton's refusal of the analytical fluxional method (which in Leibnizian terms we would call the differential and integral calculus) is indeed one of the most spectacular processes in the history of mathematics, comparable to Einstein's

Cambridge University Press
978-0-521-64066-4 - Reading the Principia: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736
Niccolo Guicciardini
Excerpt
More information

refusal of quantum mechanics. Einstein, after having contributed to the birth of quantum theory, distanced himself from it because of epistemological concerns. Similarly, Newton after having participated in one of the most fruitful discoveries in the history of mathematics, the calculus, refused to rely on it.

Chapter 3, which completes the introductory Part 1, is devoted to the mathematical methods employed by Newton in the *Principia*. It is not an easy chapter and many might find it prolix. Again the experts, i.e. people who have mastered Newton's *magnum opus*, or who have studied commentaries by Chandrasekhar, Cohen, Densmore, De Gandt or Brackenridge, may read it only cursorily. I have very little to add in terms of depth of analysis to these recent commentaries or to Whiteside's notes to Volume 6 of Newton's *Mathematical Papers*. I am heavily indebted to all these technical introductions to the *Principia*.†

My book is not to be taken as an introduction to, or a critical analysis of, the *Principia*, but rather as a work devoted to the reception of Newton's *magnum opus*. Chapter 3 is, however, necessary in the economy of my book since I must present some of Newton's mathematical techniques, which are referred to in later chapters, to my readers. In Chapter 3 I have thus tried to convey to the reader an awareness of the plurality of the mathematical methods deployed by Newton. I do not aim at completeness. Rather, I have been selective, picking those demonstrations that seem to me more exemplary of Newton's methods. The analysis of Newton's methods will deepen in Parts 2 and 3, when I leave the word to Newton's contemporaries: it is *their* analysis, not mine, that constitutes the subject matter of my book. In fact, what I have done in Chapter 3 is simply to paraphrase and explain some of the basic parts of the first two books, while I devote just a few comments to the third book. Generally I use the third edition of the *Principia*, and I refer to the previous editions when relevant variants occur. In the last stage of preparation of my book I was able to use the recent English translation by I. Bernard Cohen and Anne Whitman. In my commentary I adhere almost entirely to Newton's notation (e.g. the symbol $\propto$ has been used for phrases such as 'is proportional to'). The demonstrations have not, however, been reproduced in entirety. I have tried to reproduce only the structure of Newton's proofs. There are, in fact, in Newton's imposing and difficult masterpiece, many lines which are devoted to rather dull and uninteresting passages. Of course, these lines are

† Chandrasekhar (1995) is an analysis of *almost* all the *Principia*. It is mathematically masterful, but rather a-historical. I was able to use, in the final stage of preparation of my book, the very erudite *Guide* which accompanies Cohen and Whitman's new translation of the *Principia* (cited as Newton (1999)). Densmore (1995) is a complete, thorough and historically accurate analysis of the basic Propositions of Books 1 and 3 related to gravitation theory. De Gandt (1995) considers in every detail the *De motu* and Propositions 39–41, Book 1. Brackenridge in (1988), (1990) and (1995) studies the first three sections of Book 1 and has a great deal to say on the variants between the three editions of the *Principia*. On Whiteside's notes see above. Herivel (1965) charts Newton's studies in dynamics preliminary to the *Principia*. A classic introduction to the *Principia* which is still useful is Brougham & Routh (1855). See also Jourdain (1920).

necessary to complete the proof, but in following them there is a risk of losing sight of the ingenuity and elegance of Newton's demonstrations. These seem to many, especially today when we are not trained in geometry, very difficult and intricate. But when you grant, say, some properties which depend on long trains of proportions between the sides of similar triangles and go to the core of the demonstration, there does emerge a demonstrative structure which is often striking in its simplicity.

Any vague discourse about the 'geometrical method' of the *Principia* should vanish after an analysis of Newton's demonstrations. It will appear that 'geometrical method' is a misuse for two reasons. Firstly, in several important instances Newton abandons geometry and deploys quadratures and infinite series. Second, it is a singular: we should speak about the geometrical *methods*! We have to distinguish several *geometries* in the *Principia*. Some geometrical demonstrations are in fact very close to, or implicitly refer to, the calculus, others are real alternatives to calculus techniques. Section 3.16 might be read with interest (or disapproval) also by experts in the Newtoniana.

In Part 2 I consider the three giant 'readers' of the *Principia*. Chapter 4 concerns Newton's evaluation of his own published masterpiece. After having published the *Principia*, Newton took into consideration radical restructurings. These projects are interesting, since they reveal what he thought of his *Principia*. He was divided between the idea of revealing the analytical methods necessary to complete some of the demonstrations (typically advanced quadrature techniques), and the idea of stressing the classical appearance of his work. These worries became even more acute during the priority dispute with Leibniz. Newton found himself in an anomalous position. On the one hand, he wanted to use the *Principia* as proof of his knowledge of calculus. On the other hand, he wished to underline the superiority of his methods when compared with the Leibnizian algorithm. Thus, in this chapter we will follow Newton's attempts to defend the mathematical methods of the *Principia* against the criticisms of the Leibnizians. Most notably he had to justify the almost total absence of calculus. In manuscripts and letters related to the *Principia* Newton gives us some clues for understanding the values that directed his mathematical research during his mature life. We will see evidence of his concern for rendering his mathematics compatible with Ancient geometry. He went so far as to state that his mathematical work was mainly a development of the lost analysis of the Ancients' geometers. It is well known that Newton believed in the *prisca sapientia* of the Ancients: gravitation, the Copernican system and atoms were known to the priests of Israel, Egypt and Mesopotamia. These priests were also in possession of a hidden analysis which they did not reveal outside a circle of initiates. Newton thought of himself as a rediscoverer of this lost mathematical wisdom (may we call it a *prisca geometria*?). Of course, his mathematical methods are a wholly seventeenth-century affair, but the conviction of a continuity with ancient exemplars played a role in Newton's mature life and was transmitted to some of his disciples. The classical appearance of the *Principia* thus had a meaning which goes beyond a search for elegance and rigour. But in Chapter 4, we will see that Newton also had other concerns. We will show his interest in the mathematical competence of his readers and we will find him complaining about the fact that this competence had changed in the decades following 1687. According to Newton, most of the *Principia* was written in a style understandable for 'philosophers steeped in geometry', who by the early eighteenth century had disappeared. The early-eighteenth-century readers of the *Principia*, he regretted, were rather versed in 'algebra' and 'modern analysis'. We will also show that Newton, in the 1690s, took into consideration the possibilities of applying the analytical method of fluxions to central forces. In some manuscripts and letters he gave evidence of his ability in handling central forces via fluxional (i.e. differential) equations. This should refute a widespread belief that Newton was not able to write differential equations of motion!† It is beyond doubt that he developed some propositions of the *Principia* in terms of fluxional equations. However, he was reluctant to publish his natural philosophy in the language of the analytical method of fluxions. He rather shared this language in private communications with his close accolytes.

In Chapter 5 I discuss Huygens' reaction to the *Principia*. The great Hollander died in 1695: he just had the time to read and comment on Newton's achievement. He was immediately aware of Newton's stature as a mathematician. We have some evidence, however, that he was dissatisfied with Newton's use of proportion theory. In an interesting commentary to Proposition 6, Book 1, he criticized the use there of this classic ingredient of Ancient geometry. It is highly probable that Newton wrote the *Principia* having the *Horologium oscillatorium* in mind as a model. Despite his admiration for Huygens, Newton was departing from the high standards of rigour of the Dutch natural philosopher.

In Chapter 6 I complete the trio with Leibniz. Much has been written by Aiton, Bertoloni Meli and Bos on Leibniz's calculus and mathematization of dynamics and I rely heavily on their historical work.‡ In this chapter the reader will find some information on Leibniz's first attempts to apply the calculus to planetary orbits and resisted motion: i.e. to some of the main problems faced by Newton in the *Principia*. In §6.4 I discuss Leibniz's approach to some foundational aspects of his mathematical methods. It has been shown by Aiton and Bertoloni Meli that important differences exist between Leibniz and Newton regarding the use of infinitesimals and the representation of trajectories. In my opinion, in

† See, for example, Truesdell (1960): 9, and Costabel (1967): 125–6.
‡ Aiton (1989a), Bertoloni Meli (1993a), Bos (1974).

the comparison between Leibniz and Newton it is useful to employ a category first introduced by Feynman and rediscovered in historical context by Sigurdsson. According to Sigurdsson, Newton's and Leibniz's calculi can be defined as 'not equivalent in practice'.† I understand this as meaning that, despite the fact that the two calculi are equivalent syntactically and semantically, it would be wrong to think that they were equivalent, since they were *used* in different ways. The two algorithms were translatable one into the other. Furthermore, at a foundational level (as Knobloch's recent studies have proved), Leibniz and Newton agreed on many basic issues.‡ However, they differed in mathematical practice: they oriented two equivalent mathematical tools towards different directions. In order to appreciate this pragmatic difference, we have to take into consideration the values that orient research along different lines. In contrast to Newton, Leibniz stressed as positive values the novelty of his method, its mechanical algorithmic character, and the possibility of freeing mathematical reasoning from the burden of geometrical interpretation. Leibniz's pragmatic approach, so different from that of Newton studied in Chapter 4, leads to a critical attitude towards the mathematical methods of the *Principia*.

In Part 3 I move on to chart the mathematically minded readers of the *Principia* up to the publication of Euler's *Mechanica* in 1736. As a matter of fact there are already detailed studies on the reception of the *Principia*: let me just mention here the monumental *Introduction* by Cohen.§ The mathematical side of this story has, however, been somewhat neglected. We will follow the formation of two schools – of two small groups of mathematicians who were in close contact with their masters – the Newtonian British (Chapter 7) and the Leibnizian Continental (Chapter 8), which differed in mathematical practice. We will see that some of the values that directed Newton and Leibniz along different lines were accepted by their followers. The contrasting approaches of the two schools to the mathematical methods of the *Principia* reflect these differences in formation, skills and expectations. This confrontation became part of the priority dispute on the invention of the calculus. We can refer to Hall's book on the topic for definitive information on this famous squabble.¶ However, once again, the mathematical content of this polemic has been given little attention. The tendency has been to describe the quarrel between Newtonians and Leibnizians only as a sociological event. This is in part legitimate. However, despite the fact that the waters were muddied, the combatants debated issues whose content is worth considering. The cluster of problems addressed which concern the *Principia*'s mathematical methods is particularly interesting.

† Sigurdsson (1992).
‡ Knobloch (1989).
§ Cohen (1971).
¶ Hall (1980).