BJHS **42**(2): 211–224, June 2009. doi:10.1017/S000708740800188X © 2008 British Society for the History of Science First published online 23 December 2008

Some reflections on Newton's *Principia*

E. B. DAVIES*

Abstract. This article examines the text of *Principia Mathematica* to discover the extent to which Newton's claims about his own contribution to it were justified. It is argued that for polemical reasons the General Scholium, written twenty-six years after the first edition, substantially misrepresented the methodology of the main body of the text. The article discusses papers of Wallis, Wren and Huygens that use the third law of motion as set out by Newton in Book 1. It also argues that Newton's use of induction is quite different from and subtler than the 'logical' and 'probabilistic' notions of induction discussed and then rejected by a number of twentieth-century philosophers.

The goal of this paper is to discuss comments that Newton made in *Principia Mathematica* about his own contribution and about the contributions of others. There are not many of these, but they reveal remarkable inconsistencies. These differences are explained by reference to changes in Newton's attitudes in the twenty-six years between the first and final editions of *Principia*. The relationship between Newton's comments about his own contribution to *Principia* and what is revealed by careful analysis is complicated. It is necessary to distinguish between the main text and the General Scholium, composed long after the first edition had appeared. For the most part, Newton represented his personal contribution to the material in *Principia* fairly in the main text. On the other hand, his comments about the contributions of his predecessors often used words such as 'others', and on some occasions were misleading, often by omission. The paper describes in some detail of articles by Wallis and Huygens, whom Newton named along with Wren in the Axioms preceding Book 1. It is concluded that the General Scholium was a polemical work that substantially misrepresented the contents of the main text. The terms 'Newton's theory', 'method', or 'style' are avoided here. Books 1 and 3 discuss two different topics, which will be called his Dynamics and his Gravitation. These are treated in very different manners. It does not make sense to talk about 'Newton's method' as if he applied the same method to both. This discussion is based, throughout, on the recent translation by Cohen and Whitman and also cites the commentary to that volume.[1]

* Department of Mathematics, King's College, Strand, London, WC2R 2LS, UK. Email: E.Brian.Davies@kcl.ac.uk.

I should like to thank A. C. Bellamy for many vigorous exchanges during the gestation of this article. I should also like to thank D. A. Gillies and J. R. Milton for valuable comments.

1 I. B. Cohen and A. Whitman, *Isaac Newton, The Principia, a New Translation*, Berkeley, 1999 (hereafter Cohen and Whitman *Principia*).

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

Newton's Dynamics

The term 'Dynamics' is used here to refer to the detailed mathematical development in Book 1, based on the three laws of motion in the previous passage, called 'Axioms, or the Laws of Motion'.[2] Newton calls this 'rational mechanics' and states unambiguously that he is not claiming credit for these laws, which he adopts from earlier authors.[3] On the basis of the effort that Newton put into formulating his definitions and laws of motion, as evidenced by his many drafts of 'De Motu',[4] one would conclude that this was extraordinary generosity on the part of Newton, who was otherwise one of the most ungenerous of individuals. However, it appears that others accepted his assessment. For example, when Halley, one of Newton's strongest admirers, reviewed *Principia*, he passed over the three laws of motion as 'necessary Praecognita' without comment.[5] This suggests he did not regard Newton's contribution to these as major. One of the more controversial items in *Principia* is Definition 5 before Book 1. This states,

> Centripetal force is the force by which bodies are drawn from all sides, are impelled, or in any way tend, toward some point as a center. One force of this kind is gravity, by which bodies tend toward the center of the earth …[6]

Newton's definition makes the assumption (or hypothesis) that it is reasonable to describe Gravitation in such a manner, even if the ultimate explanation for the force needs a separate investigation. This is discussed in more detail below. Newton describes how to compose and resolve oblique forces in Corollary 2 of the 'Axioms' section.[7] He then states that Corollary 2

> can be used very extensively, and the variety of its applications clearly shows its truth, since the whole of mechanics – demonstrated in different ways by those who have written on this subject – depends upon what has just now been said.[8]

He continues by enumerating applications to a variety of machines. The wording of this passage makes it clear that he regards Corollary 2 as a hypothesis justified by its successful application within the entire body of rational mechanics. This will be contrasted below with his much later comments about scientific method in the General Scholium. There are extensive commentaries on the first two laws. A version of Law 1 appears in Descartes's *Principia Philosophiae* of 1644, but Newton went to some lengths not to mention Descartes's name in his *Principia Mathematica*. Cohen states that Newton learned it from Descartes, but this does not imply that Descartes was the first to understand the law.[9] Similar statements can be traced into antiquity and, with time,

2 Cohen and Whitman *Principia*, op. cit. (1), 416–17.
3 Cohen and Whitman *Principia*, op. cit. (1), 382, 424.
4 R. Westfall, *Never at Rest: A Biography of Isaac Newton*, Cambridge, 1980, 409–20.
5 E. Halley, 'Review of Newton's Principia', *Philosophical Transactions of the Royal Society* (1685–6), **15**, 291–7.
6 Cohen and Whitman *Principia*, op. cit. (1), 405.
7 Cohen and Whitman *Principia*, op. cit. (1), 418.
8 Cohen and Whitman *Principia*, op. cit. (1), 419, 420.
9 Cohen and Whitman *Principia*, op. cit. (1), 109.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

became increasingly related to experimental test. Much less has been written about Law 3. In the Scholium that follows the Axioms or laws of motion and their corollaries, Newton identifies Wallis, Wren and Huygens as having used Law 3 to find the rules of the collisions and reflections of hard bodies and states that they communicated their results to the Royal Society, then published them.[10] This statement appears not to have been discussed in any detail by Cohen or D. T. Whiteside. Cohen's discussion of Law 3 in his introduction to the *Principia* translation is very brief and starts with a quotation of Mach's *Science of Mechanics*.[11] Cohen appears to accept Mach's claim that this law was discovered by Newton himself, and makes no comment about the attribution by Newton. Elsewhere Cohen repeats the comment about Mach, and does now refer to Newton's comments concerning Wallis and Huygens, but does not mention Wren, or that Newton gave a precise reference to the journal in which he claimed that all three used Law 3. Cohen does not comment on whether Newton's claim is justified.[12] In Volume 5 of his monumental commentary on Newton, Whiteside stated that Newton derived the laws for the collisions of both elastic and inelastic bodies in private waste books early in 1665.[13] Whiteside also states that Newton derived the law of conservation of momentum under the hypothesis of perfect elasticity.[14] He mentions Newton's reference to Wallis, Wren and Huygens but does not discuss it.[15] It is significant that Newton's attribution was made in the first edition of *Principia*, and was not a much later attempt to counter Huygens's criticism of *Principia*'s action at a distance. Newton is surely referring to articles that the three authors submitted to the *Philosophical Transactions of the Royal Society* in November 1668, December 1668 and January 1669 respectively. None of these papers contains experimental data: they are summaries of the results of even earlier investigations. Wallis uses some version of the laws, without writing them down, to perform a series of calculations algebraically.[16] In Item 4, he writes the formula $VT = PL$ for the momentum,[17] V, of a single body, where T is the time, P the weight[18] and L the distance. In Item 5 he writes down the formula $C = L/T$ for the speed (*celeritas*), which he then uses in Item 6 to derive $V = PC$.[19] In Item 10 he considers a body of weight P and speed C driving into (*impingat*) a second body of weight mP and speed nC. Assuming that the two bodies combine he uses the

10 Cohen and Whitman *Principia*, op. cit. (1), 424.

11 Cohen and Whitman *Principia*, op. cit. (1), 117, 118.

12 I. B. Cohen and G. E. Smith, *The Cambridge Companion to Newton*, Cambridge, 2002, 68.

13 *The Mathematical Papers of Isaac Newton* (ed. D. T. Whiteside), 8 vols., Cambridge, 1967–81, v, 148, n. 152.

14 *Mathematical Papers of Isaac Newton*, op. cit. (13), 149, n. 153.

15 *Mathematical Papers of Isaac Newton*, op. cit. (13), 148, 149, n. 152.

16 J. Wallis and C. Wren, 'A summary account of the general laws of motion', *Philosophical Transactions of the Royal Society* (1668), **3**, 864–8.

17 He actually uses the word *Vis*, which we take to mean *Vis Insita*. This was a well-known term at the time and was used by Newton in *Principia*, Definition 3. Cohen and Whitman *Principia*, op. cit. (1), 404.

18 In the context it is tempting to translate *Pondus* as 'mass' rather than 'weight'. A distinction between the two concepts was made in Cohen and Whitman *Principia*, op. cit. (1), 404, but it had not been well understood before that.

19 For twentieth-century scientists the quantity $V = PC$, with P interpreted as mass and C as velocity, is by definition the momentum, but it carries no connotation of being a force, as we understand the latter word.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

conservation of the total momentum $(1+mn)V$ without comment to deduce that their common speed afterwards is $C(1+mn)/(1+m)$. Item 11 considers the same problem but assumes that the bodies are initially travelling in opposite directions. The contribution of Huygens is preceded by two pages of historical comments on the three articles.[20] Huygens's laws of motion contain seven numbered statements. His Law 5 states,

> The quantity of motion of two bodies may be either increased or diminished by their collision; but the same quantity always remains in any direction, after subtracting the quantity of contrary motion.

This is what we would now call the law of conservation of total, or combined, momentum for a pair of colliding bodies; the quantity of motion in a direction minus the quantity of contrary motion is what we call the component of the total momentum vector in that direction. Newton deduces it directly from Law 3 in Corollary 3 of the Axioms, assuming also Laws 1 and 2. Corollary 3 states,

> The quantity of motion, which is determined by adding the motions made in one direction and subtracting the motions made in the opposite direction, is not changed by the action of bodies on one another.[21]

Newton's Corollary 3 makes no reference to the need to restrict attention to hard bodies or elastic collisions. Indeed, Newton states explicitly that, in his theory, perfect elasticity is not necessary for the validity of Law 3, and contrasts this with the theories of Wren and Huygens.[22] He does not mention that Wallis had used the conservation of momentum for inelastic impacts without suggesting that there was any novelty in this. Newton's Corollary 3 and Huygens's Law 5 are clearly conservation laws. Although Newton deduces Corollary 3 from Law 3, this does not imply that Law 3 is more fundamental. The proof is only a few lines long and the argument can be run in the reverse direction to show that the two statements are equivalent. I know of no comment of Newton to this effect, but he would surely have agreed at once had the point been put to him. Newton is particularly concerned to explain the application of the laws of motion to the collision of inelastic bodies, and to determine what we call the coefficient of elasticity for various materials.[23] The similarity between Newton's Law 3 and Huygens's Law 5 is apparent, but Newton's replacement of the word 'collision' by 'action' extends the range of applicability of the law to include gravity. It appears that Newton only came to the belief that Law 3 applies to gravity while writing 'De Motu' in 1685.[24] The plausibility of this extension is intimately bound up with his description of gravity as a centripetal force in Definition 5: if gravity resulted from the interactions of bodies with the aether, there would be no reason to suppose that Law 3 applied to it. Although Newton tries to justify the extension of Law 3 to gravity by means of a

20 C. Huygens, 'A summary account of the laws of motion', *Philosophical Transactions of the Royal Society* (1669), **4**, 925–8.

21 Cohen and Whitman *Principia*, op. cit. (1), 420.

22 Cohen and Whitman *Principia*, op. cit. (1), 427.

23 Cohen and Whitman *Principia*, op. cit. (1), 425–8.

24 I. B. Cohen, 'Newton's third law and universal gravity', *Journal of the History Ideas* (1987), **48**, 571–93, 583.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

thought experiment, it is really a hypothesis the justification of which depends on the predictive success of his entire theory.[25]

In the Scholium to the laws of motion he describes a simple experiment of his that indicates that Law 3 applies to magnetic interactions, but he does not make explicit the analogy between magnetism and gravity on that page until the second edition.[26] It is surprising that he did not pursue his experiments with magnetism further in order to bolster his case that remotely acting forces obeyed the same three laws as contact forces. It is plausible that he did not anticipate the degree of hostility that this aspect of *Principia* would cause. Earlier in the Scholium Newton refers to a book by the 'eminent Mariotte' on the subject of pendulums.[27] This must be *Traité de la percussion ou chocq des corps*, published in 1673, which became a standard reference on elastic and inelastic collisions of bodies. Somewhat surprisingly, Newton did not acknowledge the considerable influence exerted on his own work by Wallis's two books *Arithmetica Infinitorum* (1656) and *Mechanica: sive, De Motu, Tractatus Geometricus* (1671).[28] In the Scholium to Proposition 6, Newton refers to Huygens's excellent treatise *Horologium Oscillatorium*, also published in 1673.[29] This discusses many problems in Dynamics, including the oscillations of compound pendulums and the Dynamics of solid bodies, as opposed to particles. These references provide further evidence for a conscientious reader of *Principia* that Newton was well aware that many of the ideas in his laws of motion had already been used in some form or other by others. In places Newton gives the impression that his main debt is to Galileo, who, along with the latter's biographer, Viviani, may be partly responsible for the later myth that Dynamics was a seventeenth-century invention that owed nothing to earlier 'scholastics and peripatetics'.[30] Newton hardly mentions the contributions of pre-seventeenth-century philosophers. Newton's main contributions to Dynamics lie in his 'Definitions' section, which resolves the great confusion of incompatible concepts being used at that time, and in the fact that his mathematical development of Dynamics in *Principia* is much deeper than that in earlier studies. Newton himself describes his treatment of Dynamics as being mathematical rather than philosophical and states that Book 1 is not concerned to apply the proven mathematical theorems to the solar system.[31]

This is confirmed by the fact that Book 1 of *Principia*, as distinguished from his previous Axioms, contains no observational data. Another measure of the difference in style between the two books is the number of diagrams in each. The first diagram in Book 3 is contained in the discussion of Proposition 19. By way of contrast, Proposition 19 of Book 1 contains its thirtieth diagram. Book 1 is mainly developed using a geometrical formalism, but there are several places in which Newton reveals his mastery of

25 Cohen and Whitman *Principia*, op. cit. (1), 427, 428.

26 Cohen and Whitman *Principia*, op. cit. (1), 428.

27 Cohen and Whitman *Principia*, op. cit. (1), 425.

28 J. F. Scott: 'The Reverend John Wallis (1616–1703)', *Notes and Records of the Royal Society* (1960), **15**, 57–67.

29 Cohen and Whitman *Principia*, op. cit. (1), 452.

30 Cohen states that Newton was not in fact familiar with Galileo's work. Cohen and Whitman *Principia*, op. cit. (1), 113, 146.

31 Cohen and Whitman *Principia*, op. cit. (1), 793.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

algebra, powers-series expansions and integration. He does this when the effort to present the material geometrically defeats even him, and recognizes that algebra and calculus are more powerful tools than geometry on its own. Important instances are Newton's discussion of motion in a general centripetal force in Book 1, Proposition 41, where he refers to quadrature twice,[32] and his discussion of the motion of the apsides in Book 1, Proposition 45, where he twice uses 'our method of converging series'.[33] Book 2 is less geometrical than Book 1, with many passages relying on the use of algebra, logarithms and even fluxions.[34]

Rules and phenomena

Book 3 is devoted to elucidating the laws of motion of a body under the influence of gravity. It is evident from the prefaces to the three editions that Newton regards his theory of Gravitation as the main subject of *Principia*. Newton starts by formulating four rules for the development of experimental philosophy. His Rules 3 and 4 relate to the use of induction. Rule 3, added in the second edition, states,

> Those qualities of bodies that cannot be intended and remitted [i.e. qualities that cannot be increased and diminished] and that belong to all bodies on which experiments can be made should be taken as qualities of all bodies universally.[35]

Rule 4, only added in the third edition, states,

> In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions.[36]

An even fuller statement is found in the following extract from Query 31 of *Opticks*:

> And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such exceptions as occur.[37]

In Rule 4 the phrase 'more exact' makes it clear that the translation 'exact' chosen by Cohen and Whitman is not a logical term. Rules 3 and 4 are not laws of logic, nor are they infallible routes to the truth. They both use modal verbs and are therefore not *laws of nature*, but *rules of procedure* which still lie at the heart of all scientific activity.

32 Cohen and Whitman *Principia*, op. cit. (1), 529, 532.

33 Cohen and Whitman *Principia*, op. cit. (1), 539–43.

34 See, for example, Cohen and Whitman *Principia*, op. cit. (1), 647, 662, 749.

35 Cohen and Whitman *Principia*, op. cit. (1), 795.

36 Cohen and Whitman *Principia*, op. cit. (1), 796. The original Latin is '*In philosophia experimentalis, propositiones ex phaenomenis per inductionem collectae, non obstantibus contraries hypothesibus, pro veris aut accurate aut quamproxime haberidebent, donec alia occurrerint phaenomena, per quae aut accuratiores reddantur aut exceptionibus obnoxiae*'.

37 Isaac Newton, *Opticks* (4th edn, 1730), New York, 1952, 404.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

Although he rejects induction, in the sense defined below, Popper describes the use of such methodological rules with approval in *The Logic of Scientific Discovery*.[38] Newton's rules do not appear in Book 1, because they have no place in a mathematical treatment. They are not related to what is now called mathematical induction, nor are they the same as the notion of induction that Popper presents in *Realism and the Aim of Science*: 'By induction I mean an argument which, given some empirical (singular or particular) premises, leads to a universal conclusion, a universal theory, either with logical certainty, or with "probability"'.[39] I have elsewhere discussed several other references to induction by Popper and by Duhem.[40] Keynes described inductive generalization in such logical/probabilistic terms in the heyday of the logicist movement.[41] Although Popper was right to deny the usefulness of induction as he had defined it, there is a fundamental difference between saying that a statement is logically true or true with probability, and saying that it is approximately true. For example, the probability that the length of a carefully machined metre rule is exactly a metre is vanishingly small, although its length may approximate to a metre with great accuracy. Almost every proposition in *Principia* Book 3 is approximate in this sense. Popper and Duhem failed to recognize that they were using the word in a different sense than Newton and that their criticism of induction as they had defined it could not be a criticism of Newton's use of the word. The section on Rules is followed by a section called 'Phenomena'.[42] All six of these make specific statements about the motion of bodies in the solar system. For three, he provides direct observational data, some of it new, and for the others he explains the evidence in varying degrees of detail. Newton did not distinguish between Rules and Phenomena in the first edition, but the difference is a real one: none of the Rules refers to bodies in the solar system, while all of the Phenomena do so. Newton may have made the change because of criticisms of his use of 'hypotheses', but the change is justified. It can be argued that Newton's Phenomena are in fact hypotheses in disguise. They are certainly not reports of direct observations, but general statements based on the interpretation of evidence. If one considers that the statement that the Earth is round is a hypothesis, then one will certainly consider that all of Newton's Phenomena are hypotheses. On the other hand, one may take the view that the evidence for a statement can become so overwhelming that it is unreasonable, although still logically possible, to withhold assent from it. Newton considered that his Phenomena had reached that degree of certainty, and history has confirmed this judgement.

Newton's theory of Gravitation

Following the sections on Rules and Phenomena, Book 3 applies the laws of motion to the study of Gravitation as it affects the planets, their satellites and the comets. Newton

38 K. R. Popper, *The Logic of Scientific Discovery*, London, 1974, 53.
39 K. Popper, *Realism and the Aim of Science*, London, 1983, 147.
40 E. B. Davies, 'The Newtonian myth', *Studies in History and Philosophy of Science* (2003), **34**, 763–80.
41 J. M. Keynes, *A Treatise on Probability*, London, 1921, Chapter 20.
42 Cohen and Whitman *Principia*, op. cit. (1), 797–801.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

proves that the gravitational force between two bodies is directed along the line joining them; it is proportional to the masses of the bodies and inversely proportional to the square of the distance between them; the gravitational mass is equal to the inertial mass; gravity acts in the same way between celestial bodies as it does on the surface of the Earth; comets move in orbits that are close to parabolae. In contrast to Book 1, his Book 3 contains substantial quantities of observational data. He attributes these to a variety of people, including John Flamsteed, the Astronomer Royal. He also describes experiments that he had carried out himself. The idea that gravity might satisfy laws of this general type was not completely novel. In 1670 Robert Hooke proposed similar ideas, but in much less detail, in a Cutler lecture published in 1674.[43] It was Hooke who wrote to Newton about these matters in 1679, starting the process which led to the publication of *Principia*. Hooke had also conjectured the inverse square law for Gravitation, but he was unable to prove it, in any sense of the word 'prove'.[44] The main laws governing gravity are stated and proved in Book 3, Propositions 1, 2 and 3, dealing with the satellites of Jupiter, the planets and the Moon respectively.[45] The fact that gravity acts on the planets towards the Sun is deduced from Book 1, Proposition 2, and the equal area law, Book 3, Phenomenon 5.[46] Newton gives two proofs of the inverse square law of Gravitation. The first relies on Book 3, Phenomenon 4, and Book 1, Proposition 4, which applies to bodies moving in circular orbits.[47] He continues,

> But [the inverse square law] is proved with the greatest exactness from the fact that the aphelia are at rest. For the slightest departure from the ratio of the square would (by book 1, prop. 45, corol. 1) necessarily result in a noticeable motion of the apsides in a single revolution and an immense such motion in many revolutions.[48]

Kepler's law of elliptic orbits is not mentioned in this context, contrary to frequent later claims that it was a key ingredient in Newton's theory. Newton argues in a Scholium that the 'force by which the Moon is kept in its orbit is the very one that we generally call gravity'.[49] This is basically a thought experiment, and is not based on direct evidence. The identity of inertial and gravitational mass is to be found in Book 3, Proposition 6, but Newton de-emphasizes his contribution to this. He reports his own very ingenious pendulum experiment that confirms this law to within one part in a thousand. He also reports calculations of the relative gravitational attraction of the Sun and Jupiter on the moons of Jupiter that confirm the equality of inertial and gravitational mass in that context to within one part in a thousand.[50] In the General Scholium he refers to Boyle's observation that 'a tenuous feather and solid gold fall

43 S. Inwood, *The Man Who Knew Too Much: The Strange and Inventive Life of Robert Hooke*, Oxford, 2002, 84, 85.
44 Inwood, op. cit. (43), 290.
45 Cohen and Whitman *Principia*, op. cit. (1), 802.
46 Cohen and Whitman *Principia*, op. cit. (1), 446 and 801.
47 Cohen and Whitman *Principia*, op. cit. (1), 800 and 449.
48 Cohen and Whitman *Principia*, op. cit. (1), 802.
49 Cohen and Whitman *Principia*, op. cit. (1), 805.
50 Cohen and Whitman *Principia*, op. cit. (1), 806–8.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

with equal velocity in such a vacuum'.[51] At that time the very existence of a vacuum was controversial, but it was a key element of Newton's theory.

The last third of Book 3 is devoted to the study of cometary orbits. This part contains more observational data than any other, because very little was known about these orbits before Newton's time. Galileo had even rejected the view of Tycho Brahe and other astronomers that they were material bodies, arguing in *Il Saggiatore* in 1623 that they were merely optical phenomena. Hooke and Wren only came to the tentative conclusion that they did not move in straight lines but in circles or some other curves in 1665.[52] It is a sign of the breakdown of their relationship that Newton makes no mention of Hooke's *Cometa*, published by the Royal Society in 1678. This book does not begin to approach the level of analysis of cometary orbits to be found in *Principia*, but Hooke states that he has collected a considerable amount of evidence that the comets do not move in straight lines. In Book 3, Proposition 40, Newton deduces that comets move in conical paths. After discussing the possibility that they move in elliptical orbits that take them far beyond the orbit of Saturn, in Corollary 2, he continues,

> But these orbits will be so close to parabolas that parabolas can be substituted for them without sensible errors.[53]

This was quite definitely a hypothesis – the paths could equally easily be hyperbolic. Newton then embarks on a difficult graphical procedure for determining the elements of a parabolic orbit of a body, and applies his results to data obtained by Flamsteed and others for the comet of 1680. In the later editions he quotes with approval more accurate calculations by Halley for a comet with a period of 575 years based upon using a highly eccentric elliptical orbit.[54] At the very end of Proposition 41 he states that 'comets are a kind of planet revolving about the Sun in very eccentric orbits'.[55] We see that Newton is perfectly happy to draw approximate conclusions from approximate data and hypotheses, as are modern scientists. He can make the hypothesis that the orbits are parabolic and then check this against observational data without believing that the hypothesis is logically exact. Newton uses the same method when discussing the motion of the planets. He generally refers to their orbits as elliptical, although he knows that this is not exactly true. Indeed his discussion of the orbit of the Moon and of the perturbation of the orbit of Saturn by Jupiter would be incomprehensible if he thought that he had proved that the orbits were exactly elliptical. Duhem and Popper say, correctly, that the absolute logical truth of Newton's inverse square law of Gravitation cannot be deduced from Kepler's laws because in fact it contradicts them.[56]

51 Cohen and Whitman *Principia*, op. cit. (1), 939.
52 Inwood, op. cit. (43), 81.
53 Cohen and Whitman *Principia*, op. cit. (1), 895.
54 Cohen and Whitman *Principia*, op. cit. (1), 911.
55 Cohen and Whitman *Principia*, op. cit. (1), 928.
56 P. Duhem, *The Aim and Structure of Physical Theory*, New York, 1981, 192 (French original 1914); K. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, London, 1989, 185.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

However, Newton had made it clear that the approximate validity of Kepler's laws was enough to draw approximate conclusions from them.[57] He derived a general law not by the use of some mysterious logical principle, but by reference to the methodological Rules 3 and 4. He did not use Kepler's first law to derive the inverse square law of Gravitation because the quiescence of the apsides (still only approximately true) provided sharper evidence for the law.

'Newtonian' relativity

In the second edition of *Principia*, Newton called many of his earlier hypotheses by different names. However, he retained his Hypothesis 1:

> *The center of the system of the world is at rest*. No one doubts this, although some argue that the earth, others that the sun, is at rest in the center of the system.[58]

The dangers of imposing one's own viewpoint onto historical texts are well illustrated by S. Chandrasekhar's comments on this passage in his *Newton's Principia for the Common Reader*.[59] While the work has merits as an exposition of some of the more technical passages in *Principia*, it was criticized by Cohen for 'disdainfully and cavalierly dismiss[ing] the whole corpus of historical Newtonian scholarship'.[60] Chandrasekhar enunciates a principle of relativity – that inertial frames are undistinguished.[61] One frame will serve as well as any other. He associates this principle with Newton and Galileo, but then suggests that there is no historical basis for giving Galileo credit for it. Chandrasekhar is unable to account for the discrepancy between his view of Newtonian relativity and Hypothesis 1, and comes to the conclusion that Newton's retention of the hypothesis is probably a lapse.[62] Unfortunately, for this theory, if one examines the Scholium after the 'Definition' section of *Principia* one finds that Newton has very strong views about the existence of absolute space.[63] Newton admits that it is

> very difficult to find out the true motions of individual bodies and actually differentiate them from apparent motions, because the parts of that immovable space in which bodies truly move make no impression on the senses. Nevertheless the case is not utterly hopeless.

The Scholium as a whole makes it quite clear that Newton did believe his Hypothesis 1, in spite of the fact that his laws did not distinguish between relative and absolute motion. R. S. Westfall has discussed the reasons for this, which include Newton's rejection of the relativism of Cartesian physics and his strong association of relativism

57 Since the Sun is a thousand times the mass of Jupiter, one would expect the 'constant' value of a^3/P^2 in Kepler's third law to vary by no more than one part per thousand from one planet to another.

58 Cohen and Whitman *Principia*, op. cit. (1), 816; original italics.

59 S. Chandrasekhar, *Newton's Principia for the Common Reader*, Oxford, 1995 (hereafter Chandrasekhar *Principia*).

60 Cohen and Whitman *Principia*, op. cit. (1), 295.

61 Chandrasekhar *Principia*, op. cit. (59), 41.

62 Chandrasekhar *Principia*, op. cit. (59), 377.

63 Cohen and Whitman *Principia*, op. cit. (1), 408–15.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

with atheism.[64] Chandrasekhar's outlook on this issue is a typical projection of someone who had grown up under the influence of Einstein's relativity theory, and wanted to believe that Newton shared his own views. Following his Hypothesis 1, Newton adjudicates on the earlier disputes about the relative merits of the geocentric and heliocentric theories of the solar system. Technically speaking, both sides in this dispute were wrong, but Copernicus was far closer to the truth, and for most purposes one can treat the Sun as being the centre of the system of the world.

The General Scholium

It is not possible to understand the General Scholium without remembering that the second edition was published in 1713, twenty-six years after the first edition had appeared. When he wrote the first edition, Newton had no experience of public life and shunned contacts with others. At the start of 1713 Newton was seventy years old, and had been Master of the Mint for seventeen years and president of the Royal Society for ten years. These experiences greatly increased his self-confidence, but did not make him any more pleasant an individual. Indeed, his disputes with Leibniz, Flamsteed and others proved that he was very willing to use his position of authority to manipulate situations to his own advantage. The General Scholium appears in the second and third editions and must be regarded as polemical. The same can be said of Cotes's Preface to the second edition. Newton starts by summarizing the inadequacies of Descartes's vortex theory, continues with a long religious passage, and then goes on to defend his own theory of Gravitation. He admits the philosophical weakness of any theory that depends upon action at a distance, but tries to distinguish between the predictive success of his laws and discussions about whether his theory of Gravitation is a final explanation of the phenomenon. At this point Newton is reacting to the criticisms of Leibniz and Huygens, the latter by now dead. After the appearance of the first edition of *Principia* they had insisted that bodies could only act on each other while in contact. For them, Descartes's scheme of vortices was plausible even if currently inaccurate, while Newton's ideas about Gravitation had no prospect of providing the physical explanation of gravity that they considered necessary. Newton's attack on Leibniz would have been much more obvious at the time than it is to the modern reader.[65] The most famous passage in *Principia* occurs in the General Scholium. He writes,

> Thus far I have explained the phenomena of the heavens and of our sea by the force of gravity, but I have not yet assigned a cause to gravity … I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses.[66]

In this passage Newton was making explicit what had only been implicit in the first edition: that he was interposing the concept of mathematical explanation between pure

64 Westfall, op. cit. (4), 415.
65 Westfall, op. cit. (4), 749, 750.
66 Cohen and Whitman *Principia*, op. cit. (1), 943.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

description (as in Kepler's laws) and physical (or agent-causal) explanation. This was a regressive approach by the standards of the time and had been rejected by a number of his contemporaries. Yet it proved extremely important for the future development of physics.[67] Newton's further statement that 'hypotheses … have no place in experimental philosophy' is at odds with the fact that the main text of *Principia*, written long before this, contained many hypotheses, some of which we have already discussed. I have argued elsewhere that his methodology is much more diverse than is represented by this sentence.[68] This conflict is mentioned explicitly by Cotes in his Preface to the second edition.[69] In private correspondence with Cotes about this sentence, Newton wrote that

> in Experimental Philosophy [the word Hypothesis] is not to be taken in so large a sense as to include the first Principles or Axiomes which I call the laws of motion … the word Hypothesis is here used by me to signify only such a Proposition as is not a Phaenomenon nor deduced from any Phaenomena but assumed or supposed without any experimental proof.[70]

Much misunderstanding would have been avoided had Newton simply explained the difference between hypotheses of the type that he was using and those of Descartes. His immediate audience would have been in no doubt that he was criticizing Descartes's philosophical methodology, but the subtext was so well concealed that later generations did not appreciate his intentions. Newton's only mention of Dynamics in the General Scholium occurs in the same paragraph:

> In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility and impetus of bodies, and the laws of motion and the law of gravity have been found by this method.

It is possible to defend this passage by reference to the contents of *Principia*, in so far as it relates to Gravitation. The phenomena in question are those listed as such at the head of Book 3:[71] the main propositions of Book 3 are indeed deductive in form. The inductions are based on Rules 3 and 4, which involve fallible generalizations and approximations. So, therefore, do any conclusions based on them. It is much more difficult to explain away the reference to the laws of motion, because Newton had not attempted to deduce them from the phenomena in the same manner as he had deduced the Law of Gravitation. It can hardly be the result of a lapse of attention, since the exact wording of this passage was altered following correspondence with Cotes, and the original version did not refer to the laws of motion.[72] Newton did not state that he himself had found the laws of motion by this method, but only that they had been

67 Berkeley, Kant, Laplace and others later decided that physical explanations were entirely unnecessary. Almost no physicist today thinks that we have a physical explanation of the laws of quantum mechanics while many do not even think that we should expect to get one in the future. Nevertheless our mathematical description of how it works is extremely detailed and highly successful.

68 Davies, op. cit. (40).

69 Cohen and Whitman *Principia*, op. cit. (1), 393.

70 Newton to Cotes, 29 March 1713, cited in Cohen and Whitman *Principia*, op. cit. (1), 277.

71 Cohen and Whitman *Principia*, op. cit. (1), 797–801.

72 Cohen and Whitman *Principia*, op. cit. (1), 276.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

found. However, Newton knew that the route by which he and others had arrived at the laws of motion was extremely roundabout, and bore little resemblance to the experimental philosophy that he was advocating. This applies in particular to Corollary 2 of the Axioms, as noted above. He might well have felt that his authority by that time was so great that nobody would dare to challenge him on this point.

Conclusions

Einstein and Poincaré pointed out that many nineteenth-century scientists such as Laplace were impressed by the ever-increasing successes of Newton's theory, and presented it as a logically exact, deductive subject which would last for all time.[73] A number of early twentieth-century commentators interpreted *Principia* through the spectacles of these nineteenth-century myths. They had little interest in, or basis for, relating *Principia* to other developments in the seventeenth century, let alone earlier periods. When Einstein's new theory of relativity appeared, these commentators were forced to question these assumptions, but most did not go back to examine exactly what *Principia* claimed. In particular, they did not distinguish between a 'logical/probabilistic' definition of induction and Newton's earlier and more subtle formulation of the concept, which acknowledged the possibility of later corrections and modifications. Newton's experimental philosophy is frequently contrasted with medieval science, substantial parts of which were concerned with examining the logical consistency of various ideas via thought experiments. However, this should not be taken as implying that every contribution to scientific understanding fitted perfectly into the dominant attitude of the relevant period, nor that there was a sharp transition between the two at some particular date. Galileo did not consider it necessary to drop different weights from a height to compare how rapidly they fell (in spite of what his biographer Viviani alleged about experiments at the Tower of Pisa). Yet Philoponus might already have done so in the sixth century AD.[74] Newton concealed many of his unorthodox views in *Principia*, in order to present it as the inevitable result of a rational process of enquiry. The attempts of some later philosophers to reconcile Newton's philosophical views, as expressed in the General Scholium, with what he had written in the main text of *Principia* twenty-six years earlier were always doomed to fail. The fact that these philosophers focused on the General Scholium rather than on the main text was, no doubt, associated with the fact that the General Scholium was easy to read while the main text was very difficult, because Newton wished it to be so. Although the General Scholium doubtless represented his later views, it was also polemical in nature. It contained a counterattack by Newton against the Cartesians, made without mentioning the

73 A. Einstein, 'Newtons Mechanik und ihr Einfluss auf die Gestaltung der theoretischen Physik', *Naturwissenschaften* (1927), **15**, 273–6 (English version in *idem*, *The World as I See It*, New York, 1934); H. Poincaré, *Science and Hypothesis*, New York, 1952.

74 S. Drake, *Essays on Galileo*, 2 vols., Toronto, 2000, ii, 185. In his 'Corollary on void', Philoponus states that if one actually drops two substantially different weights, they fall at equal speeds. *Place, Void and Eternity. Philoponus: Corollaries on Place and Void*, tr. D. Furley, with Simplicus, *Against Philoponus on the Eternity of the World*, tr. C. Wildberg, London, 1991, 59.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press

names of his opponents. Under its influence some later commentators did not appreciate that in the main text of *Principia*, Newton used whatever method seemed most appropriate for the matter being discussed, including hypotheses, thought experiments and idealizations (simplification of a problem to one that he could actually solve). Eventually Newtonian scholarship and the philosophy of science are converging, but the process has taken far too long.

https://doi.org/10.1017/S000708740800188X Published online by Cambridge University Press