Some Reflections on Newton's 'Principia'

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Abstract

We examine the text of Principia to discover the extent to which Newton's claims about his own contribution to it were justified. We argue that the general Scholium, written twenty six years after the first edition, substantially misrepresents the main body of the text for polemical reasons. We discuss papers of Wallis, Wren and Huygens that use the third law of motion, as stated by Newton in Book 1. We also argue that Newton's use of induction has been misunderstood by many twentieth century commentators, who have confused it with the very different notion due to Hume.

1 Introduction

This paper has two related goals. The first is to discuss comments that Newton made in Principia about his own contribution and about the contributions of others. There are not many of these, but they reveal remarkable inconsistencies between the main text, the General Scholium, and what is known on the basis of Newtonian scholarship since 1960. We also discuss a variety of comments about Principia by some later scientists and philosophers, which indicate the degree to which they were reacting to each other rather than to the original text.

The relationship between Newton's comments about his own contribution to Principia and what is revealed by careful analysis is complicated, and it is best to distinguish between the main text and the General Scholium, composed twenty six years after the first edition had appeared. We consider that for the most part Newton represents 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. We describe in some detail articles of Wallis and Huygens, whom he names along with Wren in the Axioms preceding Book 1, [P, p 424]. We also argue that the General Scholium was a polemical work, which substantially misrepresents the contents of the main text.

We avoid using the terms ‘Newton’s theory/method/style’. Books 1 and 3 discuss two different topics, which we will call his Dynamics and his Gravitation. These are treated in very different manners, and it does not make sense to talk about ‘Newton’s method’ as if he applied the same method to both of them.

We use the word ‘Commentators’ below to refer to people in the twentieth century who should have read Principia before commenting on it (but might not have), but who did not have access to the huge flowering of Newtonian scholarship since 1960. We also include some later people who had not fully absorbed the implications of this Newtonian scholarship. The Commentators in our sense do not include Poincaré and Einstein, who remarked that many nineteenth scientists were misled by a continental tradition that presented Newtonian mechanics in a more formal and less experimentally based style than did Principia; [8, 12]. Among the Commentators one must include Popper, still cited by scientists as one of the most important philosophers of science in the twentieth century. (Philosophers in general are less enthusiastic about his contributions.) Another Commentator, the astrophysicist Chandrasekhar, rewrote Books 1 and 3 in 1995 in modern notation, providing his own proofs for the benefit of ‘the common reader’ – a scientist who did not wish to wade through the archaic language and highly geometric arguments in Principia; [1]. We discuss Chandrasekhar’s comments about ‘Newtonian’ relativity below.

The first language of many of the Commentators was by no means always English, so one cannot assume that they used the Motte translation of 1729. Accordingly we feel free to use the recent translation by Cohen and Whitman (pp. 371-944 in [3]) in the hope that it is the closest to the Latin original. We use the letter ‘P’ to denote this translation. We distinguish between what might be concluded by a conscientious reader of Principia who did not have access to Newton’s private papers and the results of recent Newtonian scholarship by confining many of our comments on the latter to footnotes.

2 Newton's Dynamics

We use this term to mean 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'; [P, p 416,417]. Newton calls this rational mechanics [P, p382] and states unambiguously [P, p424 top] that he is not claiming credit for these laws, which he adopts from earlier authors; we discuss this is more detail below.[1]

One of the most 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 ... [P, p405].

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. In the General Scholium he states

> 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. [P, p 943]

In other words Newton was separating explanation from description. This was a regressive approach by the standards of the time, and was wholly rejected by a number of his contemporaries,[2] but it proved extremely important in the future development of physics.[3]

Newton justifies his discussion of the concept of force in [P, p. 419,420]. He states

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[1] 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', [17, pp. 409-420], one would conclude that this was extraordinary generosity on the part of Newton, otherwise one of the most ungenerous individuals imaginable. But it appears that others accepted his account. For example when Halley reviewed Newton's Principia, he passed over the three laws of Motion as 'necessary Praecognita' without comment.[9] This suggests he did not regard Newton's contribution to these as major.

[2] Huygens, Leibniz and others objected to the very idea that one might accept the existence of remotely acting forces, and insisted that bodies could only act on each other while in contact; for them this made Descartes' scheme of vortices plausible even if currently inaccurate, and Newton's ideas about gravitation a non-starter.

[3] Almost no physicist today thinks that we have an explanation of the laws of quantum mechanics, and 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.

> [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.

He enumerates applications to a variety of machines. These fall within the field that we call Statics, and have no connection with Law 2, which is a part of Dynamics.

There are extensive commentaries on the first two laws,[4] but much less has been written about Law 3. If one examines Principia one finds a statement that appears not to have been discussed in any detail by Cohen or Whiteside.[5] In [P, p 424, bottom] 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 and then published them. He is surely referring to articles that they submitted to the Philosophical Transactions of the Royal Society in November 1668, December 1668 and January 1669 respectively; see [16, 10]. None of these contains experimental data: they are summaries of the results of even earlier investigations. Wallis uses the laws, without writing them down, to perform a series of calculations algebraically. In item 4, he writes the formula $VT = PL$ for the momentum[6] V of a single body, where T is the time, P the weight[7] 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 $V = PC$. In

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[4] A version of Law 1 appears in Descartes' 'Principia Philosophiae' of 1644, but Newton went to some lengths not to mention Descartes' name in Principia. Cohen states that Newton learned it from Descartes [3, p109], but this does not imply that Descartes was the first to understand the law. Similar statements can be traced into antiquity, becoming less related to experimental test as one goes back in time.

[5] Cohen's discussion of Law 3 in [3, p 117,118] is very brief and starts with a quotation of Mach's 'The Science of Mechanics'. Cohen appears to accept Mach's claim that this Law was discovered by Newton himself, and makes no comment about the attribution by Newton. In [4, p. 68] Cohen repeats the comment about Mach, does now refer to Newton's comments concerning Wallis and Huygens, but does not mention Wren nor that Newton gave a precise reference to the journal in which he claimed that all three used Law 3. Cohen does not attempt to assess whether Newton's claim is justified.

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; see [18, p.148, n.152]. Whiteside also states in [18, p.149, n.153] that Newton derived the law of conservation of momentum under the hypothesis of perfect elasticity. He mentions Newton's reference to Wallis, Wren and Huygens in [18, p.148/149 n.152] but does not discuss it.

[6] He actually uses the word 'Vis', which we take to mean 'Vis Insita'. This was a well-known term at the time, [3, p. 96], and was used by Newton in his Definition 3 ([P, p. 404].

[7] In the context it is tempting to translate 'Pondus' as mass rather than weight. A distinction between the two concepts was made in Principia's Definitions [P, p. 404], but it had not been well understood before that.

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 conservation of the total momentum $(1+mn)V$ without comment to deduce that their common speed afterwards is $\frac{1+mn}{1+m}C$. 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. Huygens' Laws of Motion contains 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. [P, p. 420].

Newton's Corollary 3 does not make any reference to the need to restrict attention to hard bodies or elastic collisions. Indeed Newton states explicitly on page 427 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. He does not mention that Wallis had used the conservation of momentum for inelastic impacts without suggesting that there was any novelty in this.

None of the above people uses the word 'momentum' anywhere. 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. Nevertheless Newton's Corollary 3 and Huygen'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 instantly if this was 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; [P, pp. 425-428]. On page 428 he even describes an experiment that indicates that the Law 3 applies to magnetic interactions. The similarity between Newton's Law 3 and Huygens' Law 5 is apparent, but the replacement of the word 'collision' by 'action' expands the range of applicability of the law to include gravity. Newton does not provide any evidence for believing that Law 3 applies to gravity, an idea that he only came to while writing 'De Motu' in 1685; [2, p. 583]. The idea is intimately bound up with his description of gravity as a centripetal force in Definition 5, since if gravity resulted from the interactions of bodies with the aether, there would be no reason to suppose that Law 3 applied to it. The application of Law 3 to gravity is a clear example of an hypothesis whose justification depends on the predictive success of his entire theory.

In [P, p. 425] Newton refers to a book by the 'eminent Mariotte' on the subject of pendulums. This must be 'Traité de la percussion ou choc des corps', published in 1673, which became a standard reference on elastic and inelastic collisions of bodies.[8] In [P, p. 452] Newton refers to Huygen's excellent treatise 'Horologium Oscillatorium', also published in 1673. 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 aware that many of the ideas in the Axioms 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 his 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'.[9] The earlier contributions of philosophers such as Philoponus, Buridan, Oresme and others are not mentioned by Newton.

Newton's main contribution to Dynamics lies in the clarity of his 'Definitions' section, and the fact that his *mathematical development* of Dynamics in Principia is much deeper than 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 mathematical theorems proved to the Solar System; [P, p. 793]. 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.

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[8] Somewhat surprisingly Newton did not acknowledge the considerable influence that Wallis's two books 'Arithmetica Infinitorum', 1656, and 'Mechanica: sive, De Motu, Tractatus Geometricus', 1671, had on his own work; see [15].

[9] Cohen states that Newton was not in fact familiar with Galileo's work; [3, p. 113,146].

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 30th diagram.

Book 1 is mainly developed using a geometrical formalism, but there are several places in which Newton reveals his mastery of 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; [P, p. 529,532], and his discussion of the motion of the apsides in Book 1 Proposition 45, [P, p. 539-543], where he uses 'our method of converging series' twice. Book 2 is less geometrical than Book 1, with many passages relying on the use of algebra, logarithms and even fluxions; see for example pages 647, 662, 749.

3 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 Rule 4 relates to the use of induction.

> 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; [P, p. 796].

A 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.

For Newton Rule 4 is not a law of logic, nor is it an infallible route to the truth. It uses a modal verb and is a rule of procedure which still lies at the heart of all scientific activity. The rule does not appear in Book 1, because it has no place in a mathematical treatment. It is not related to what is now called mathematical induction, nor is it the same as Popper's definition of induction 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.[13, p 147]

See [5] for several similar references to induction by Popper and by Duhem. There is a fundamental difference between saying that something is logically true or true with probability, and saying that it is approximately true. For example, the probability that the length of an carefully machined metre rule is exactly a metre is vanishingly small, but its length may approximate to a metre with great accuracy. Almost every proposition in Principia Book 3 is approximate in this sense. In Rule 4 the phrase 'more exact' makes it clear that the word 'exact' chosen by Cohen and Whitman is not a logical term. (The original latin is '*pro veris aut accurate aut quamproxime haberi debent*'.)

Popper and Duhem failed to recognize that they were using the word in a different sense from Newton, and that their criticism of other Commentators could not be a criticism of Newton. On the other hand, Einstein wrote

> Newton's fundamental principles were so satisfactory from the logical point of view that the impetus to overhaul them could only spring from the demands of empirical fact. Before I go into this I must emphasize that Newton himself was better aware of the weaknesses inherent in his intellectual edifice than the generations of learned scientists who followed him. This fact has always aroused my deep admiration ...[8]

The misunderstanding of Newton's Rule 4 may have started with David Hume's famous arguments against the use of induction. Hume thought of it as a rule purporting to allow one to draw universal conclusions about the future from particular observations in the past, and drew the correct conclusion that no logical evidence for its truth could be provided. Newton, however, had not made any such logical claim about his use of induction. It is better regarded as characterizing scientific activity – it appears to work, so one uses it, aware that the conclusions so reached may have to be modified at any time.

The section on Rules is followed by a section called ‘Phenomena’; [P, pp. 797-801]. 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 interpretations of evidence. If one considers that the statement that the Earth is round is an 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.

4 Newton’s 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 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.[10] In contrast to Book 1, his Book 3 contains substantial quantities of observational data. He attributes these to a variety of people, including Flamsteed, the Astronomer Royal; he also describes experiments that he had carried out himself.

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[10] The idea that gravity might satisfy laws of this general type was not completely novel. In 1670 Hooke proposed similar ideas, but in much less detail, in a Cutler lecture, published in 1674, [11, p 84,85] and 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’; [11, p 290]. Very few Commentators could have known this because Newton removed almost all references to Hooke from Principia before its publication.

The main laws governing gravity are stated and proved in Book 3 Propositions 1, 2 and 3, [P, p. 802], dealing with the satellites of Jupiter, the planets and the Moon respectively. The fact that gravity acts on the planets towards the Sun is deduced from Book 1 Proposition 2, [P, p. 446], and the equal area law, Book 3 Phenomenon 5, [P, p. 801]. Newton gives two proofs of the inverse square law of gravitation. The first relies on Book 3 Phenomenon 4, [P, p. 800], and Book 1 Proposition 4, [P, p. 449], which applies to bodies moving in circular orbits. 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.[P, p. 802]

Kepler's law of elliptic orbits is not mentioned in this context, contrary to claims by Popper that it was a key ingredient in Newton's theory of gravitation; see [14, p. 185].

Newton argues that the 'force by which the Moon is kept in its orbit is the very one that we generally call gravity' in a Scholium on page 805. 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, [P, p. 806], but Newton de-emphasizes his contribution to this. He reports his own very ingenious pendulum experiment which confirms this law to within one part in a thousand; [P, p. 807]. He also reports calculations of the relative gravitational attraction of the Sun and Jupiter on the moons of Jupiter which confirm the equality of inertial and gravitational mass in that context to within one part in a thousand; [P, p. 808]. In the General Scholium he refers to Boyle's observation that 'a tenuous feather and solid gold fall with equal velocity in such a vacuum'; [P, p. 939]. 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, starting on page 888, is devoted to the study of cometary orbits. This part contains more observational data than any other, because very little was know about the orbits before Newton's time.[11]

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[11] Galileo rejected the view of Brahe and other astronomers that they were material bodies, arguing in 'Il Saggitiore' 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; [11, p 81]. It is a comment of the breakdown of their relationship that Newton makes no mention of Hooke's 'Cometa', published by the Royal Society in 1678. This

In Book 3 Proposition 40, [P, p. 895], Newton deduces that comets move in conical orbits. Corollary 2, [P, p. 895], states

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

This is quite clearly an hypothesis – the orbits could quite 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 an elliptical orbit; [P, p.911]. On page 928 he states that 'comets are a kind of planet revolving about the Sun in very eccentric orbits'. 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 [7, p. 192] and Popper [14, p. 185] say, correctly, that Newton's inverse square law of gravitation cannot be deduced from Kepler's laws because it actually contradicts them, but this is not an interesting conclusion – Newton knew that he was drawing his conclusions from approximate observations, and that he was using his fallible induction Rule 4 to obtain the simplest general law which explained them.

5 Hypothesis 1

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. See [P, p. 816]

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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.

The dangers of imposing one's own viewpoint onto historical texts are well illustrated by Chandrasekhar's comments on this passage in 'Newton's Principia for the Common Reader'; [1]. This has many merits as an exposition of some of the more technical passages in Principia, but was criticized by Cohen in the following terms.

> Readers should be warned that Chandrasekhar disdainfully and cavalierly dismisses the whole corpus of historical Newtonian scholarship, relying exclusively on (and quoting extensively from) comments by scientists, many of whose statements on historical issues are long out of date and cannot stand the scrutiny of critical examination. [3, p. 295]

On page 41 Chandrasekhar enunciates a principle of relativity – that inertial frames are undistinguished: one frame will serve as equally as any other. He associates this 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; [1, p. 377].

Unfortunately, for his theory, if one examines the Scholium after the 'Definition' section of Principia, [P, pp. 408-415], one finds that Newton has very strong views about the existence of absolute space. He admits

> 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.[12] 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 very close to the truth, and for most purposes one can treat the Sun as being the centre of the system of the world.

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[12] Westfall expands on the reasons for this, which include Newton's rejection of the relativism of Cartesian physics and a strong association of relativism with atheism, [17, p. 415].

6 The General Scholium

It is not possible to understand the General Scholium without remembering that it was published in 1713, twenty six years after the first edition 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, 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 first appears in the second edition, and must be regarded as partly polemical and partly explanatory.[13] Separating the two is not easy. Newton starts by summarizing the inadequacies of Descartes' vortex theory, continues with a long religious passage, and then goes on to discuss his own theory of Gravitation. He admits the philosophical weakness of any theory that depends upon action at a distance, but tries, in response to the criticisms of Leibniz and others,[14] to distinguish between the predictive success of his laws and discussions about whether his theory of Gravitation is a final explanation of the phenomenon.

The most famous passage in Principia occurs on page 943, in the General Scholium. We start with the sentence

> I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses. [P, p. 943].

His 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, contains many hypotheses, some of which we have already discussed. We have argued elsewhere that his methodology is much more diverse than is represented by this sentence; [5]. Lacking knowledge of the historical context, many Commentators did not realize that his primary purpose here was to distinguish his approach from that of Descartes. This conflict is mentioned explicitly by Cotes in his preface to the second edition; [P, p. 393]. Nor would they have known of private correspondence between Newton and Cotes about this very sentence, in which Newton wrote

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[13] The same can be said of Cotes' Preface to the second edition. The attack on Leibniz would have been much more obvious at the time than it is to the modern reader; [17, pp. 749,750].

[14] One of his most important critics, Huygens, was by now long dead.

> 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. [3, p. 277]

Much misunderstanding would have been avoided if Newton had simply explained the difference between the type of hypotheses that he was using and those of Descartes. His immediate audience would have been in no doubt that he was criticizing Descartes' 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 in the section with that name on pages 797-801, and the main propositions of Book 3 are indeed deductive in form. The inductions are based on Rule 4, which involves approximations and fallible generalizations. So, therefore, do any conclusions based on it.

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; [3, p. 276]. Newton did not state that he himself had found the Laws of Motion by this method, but only that they had been found. This is consistent with his statements in the first edition that the three Laws of Motion were the work of others which he was simply adopting and developing mathematically in Book 1. 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. He might well have felt that his authority by that time was so great that nobody would dare to challenge him on this point. We are not likely to find out more about his thoughts than is contained in his letter to Cotes, because people do not generally reveal their deepest motives to others in writing.

7 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; [8, 12]. A number of 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, the 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 Hume's logical notion of induction and Newton's much more subtle formulation of the concept.

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, [6, p. 185], (in spite of what his biographer Viviani alleged about experiments at the Tower of Pisa) but Philoponus did do precisely that in the sixth century AD. Newton concealed many of his unorthodox views in Principia, in order to present it as the inevitable result of a rational process of enquiry.

With one exception, Newton was not responsible for the later misunderstandings of Principia, but he wrote in such a forbidding style that most of his contemporaries and the later Commentators were unable or unwilling to assess Principia themselves. That exception was the general Scholium, written in response to criticisms made over the twenty six years between the publication of the first and second editions. This was polemical in nature, and contained a counterattack by Newton against the Cartesians, made without mentioning the names of his opponents. Their objections to action at a distance were later forgotten, and a different construction was put on the paragraph. Under the influence of the General Scholium many later Commentators did not appreciate that Newton's methodology, as revealed by the body of the text, was startlingly modern, and gradually rediscovered many of his ideas without realizing that they were doing so. Eventually Newtonian scholarship and the philosophy of science are coming together, but the process has taken far too long.

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