The Newtonian Myth

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Abstract

We contrast the contents of Newton's Principia with a variety of accounts of his inductive methods, particularly by Karl Popper. We demonstrate that many statements made by philosophers about the Principia have been factually incorrect, and that Newton's actual methods were much more flexible than admitted even by him. We argue that his use of induction was methodologically sound, or at least immune to criticisms often made of it.

Introduction

The scientific and philosophical literature relating to Isaac Newton's theory of gravitation provide us with a rare opportunity to observe the creation and development of a myth over a period of almost three centuries. The myth, briefly, is that in the Principia Newton presented a rigorous deduction of his universal laws of gravitation from Kepler's laws without invoking any hypotheses. The work was sufficiently difficult that many people took at face value Newton's own statement in the General Scholium that he had followed the (Baconian) inductive method, and did not appreciate the wide variety of different types of arguments to be found in the text itself. Newton was an extremely complex individual, and not to be trusted even when describing his own work! Indeed, later in his life he claimed to have derived most of the results in the Principia originally by using calculus, a claim for which no documentary evidence exists and which is almost certainly false, [CoWh, p 122−124].

Our goal here is not to make a contribution to the huge literature on Newton himself, but rather to point to the superficial accounts of 'Newton's method' made by some much more recent philosophers and scientists. Of course it is easier to take someone else's word about what is in a technically difficult text than to read it oneself. But this particular text is of such central importance to the philosophy of science that it remains surprising that factually incorrect claims about its contents were still being made over two centuries after it was written.

We focus particularly on Karl Popper, because of his strong rejection of the inductive method. While Popper was right to insist that theories could only be regarded as scientific if they were subject to testing and possible refutation, other aspects of his scheme are less convincing. His view that the process of formulation of a theory could not be discussed scientifically and was only of psychological interest was probably heavily influenced by comments of Einstein, [Pop1, p 31−2]. The Principia shows that, in some cases at least, far more can be said. Newton's theory was indeed derived from phenomena using induction, and, as long as one accepted that this process was fallible and needed further tests to confirm the conclusions, there need be no objection to this. We mention that Newton's use of induction in 'Opticks' has been discussed by Worrall, and the present paper may be regarded as complementing his conclusions, [Wo]. In addition Norton has argued that, in spite of his own comments, Einstein did follow a recognisable method when searching for his general theory of relativity, involving a sequence of eliminative inductions, [Nor].

It is hardly likely that the present author will interpret correctly all aspects of the very complex and diverse structure of the Principia. Nor can we follow the entire development of the myth within the scope of this brief document. We have chosen instead to concentrate on a few particularly famous scientists and philosophers of science, mostly from before the discovery of relativity theory and quantum theory. These discoveries forced a radical re−evaluation of Newton's work, and have eventually enabled us to make a more measured assessment of what he actually did. But we shall see that the myths surrounding his work survived well into the twentieth century.

When one writes in detail about the contents of the Principia, it is necessary to be careful about which version one is referring to. It went through three Latin editions which were substantially different from each other, and was translated into English by Motte in 1729, as well as into several other languages. I will be using the new translation by Cohen and Whitman, which includes a substantial commentary and notes on the variations between the earlier editions. Large parts of Books 1 and 3 have been reworked in modern notation by Chandrasekhar, [Ch], one of the twentieth century authorities on astronomy, but his work is not regarded as being authoritative as historical scholarship, [CoWh, p 295]. When referring to later scientists and philosophers, I am forced by my own limitations to rely upon the English translations.

Methodological Rules

Newton starts the third edition of Book Three of the Principia with a section entitled 'Rules for the Study of Natural Philosophy'. There are four such rules, of which I quote the last, [CoWh, p796]:

> 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.

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.

See [Wo] and [Ma] for discussions of these rules and of their historical origins. We claim that they are methodological rules in exactly the sense described by Popper in 'The Logic of Scientific Discovery', [Pop1, p 53].

> Methodological rules are here regarded as *conventions*. They might be described as the rules of the game of empirical science. They differ from the rules of pure logic as do the rules of chess, which few would regard as part of *pure* logic.

There are three arguments in support of the view that Popper is describing ideas which Newton had been using two and a half centuries earlier. The first is the structural similarity of Newton's Rule 4 to the following rule of Popper

> Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without 'good reason'. A 'good reason' may be, for instance: replacement of the hypothesis by another which is better testable; or the falsification of one of the consequences of the hypothesis.

The second is that all of Newton's rules and the above rule of Popper are expressed using modal verbs, and are not the direct statements which would be appropriate for laws of nature or of logic. The third is the fact that Newton originally called the first two hypotheses and only later renamed them rules, adding the third and fourth; clearly his use of the word 'rule' was carefully considered.

There is evidence in the 'Rules' section of Book 3 that Newton's Rule 1:

> No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena.

was associated with a logically quite distinct *belief* about the real world. This is Newton's statement 'For nature is simple and does not indulge in the luxury of superfluous causes'. Strictly speaking science does not need such a belief, since research will ultimately establish whether or not it is valid. In the biological sciences the *belief* is definitely false, but the *rule* is still used (scientists were right to try to explain inheritance in terms of genes even though the idea of a gene has become far less easy to pin down since the unravelling of the genetic code in DNA). However, it may safely be assumed that Newton, in common with almost all current physical scientists, did actually hold the belief.

It appears that Newton did not regard induction as a law of nature or of logic, since he stated that conclusions reached using Rule 4 were subject to later reconsideration. Popper put himself in an exposed position by comparing the methodological rules of science with those of chess. He could then not criticise Newton's reference to induction in Rule

4, any more than a chess player could criticise the rules of checkers, unless he were able to prove that it involved a logical inconsistency (such as a putative rule in chess which stated that the game must start with White moving his queen). The fact that conclusions reached using Rule 4 were stated to be provisional overrules a criticism of induction used by Popper in a different context, [Pop1, p 253−4]. Nor could Popper easily argue that Newton's rules were less fruitful than his own for the development of science: Newton used his rules extensively and explicitly in the Principia, and they were, to say the least, highly successful.

Popper's Misinterpretation of the Principia

Popper's rejection of the inductive method is well known, as is the fact that Newton claimed to base his 'Principia Mathematica' upon this method, following the general custom of members of the early Royal Society of London. One might have thought, therefore, that Popper would have subjected Newton's claim to careful analysis. If one looks through two of Popper's major books, 'The Logic of Scientific Discovery' and 'Conjectures and Refutations',[1] one finds remarkably few references to Newton. Sometimes Popper refers to Newton's theory and sometimes he attacks statements or beliefs which he attributed to Newton. He identifies Kant's error concerning the absolute truth of Newton's theory, but follows Kant in many other respects. His references to Newton have a lack of sharpness, and we argue that during most of his life he relied mainly on second−hand sources such as Hume and Kant.[2]

Although this may be surprising at first sight, it has to be remembered that professional Newtonian scholarship did not exist in 1934, when Popper wrote the original German version of 'The Logic of Scientific Discovery' and only started to develop in a serious way from the end of the 1950s, after he had written most of his major works. His knowledge of the details and historical background of the texts was probably similar to that of most of his contemporaries.

Popper rarely mentioned the Principia even indirectly, and we will see that he made demonstrably incorrect statements about what it contained.[3] Let us consider what he wrote in Chapter 8 of 'Conjectures and Refutations', published in 1958. The most important section surrounds the following passage, [Pop2, p 185]:

> Newton himself asserted that he had wrested its functional principles from experience by *induction*. In other words, Newton asserted that the truth of his *theory* could be logically derived from the truth of certain *observation−statements*. Although he did not describe these observation−statements precisely it is nevertheless clear that he must have been referring to Kepler's laws, the laws of the elliptic motions of the planets.

It is possible to claim that the context indicates that Popper was summarising Kant's views in this paragraph, rather than stating his own. In this interpretation my comments

[1] First published in 1934 and 1963 resp.
[2] The latter wrote two scientific books, 'The General Natural History of the Heavens' and 'Physical Monadology' in the 1750s, prior to his main philosophical period.
[3] We will discuss the very different conclusions to be drawn from 'The Self and Its Brain' below.

below would have to be read as a criticism of Kant (as interpreted by Popper) rather than of Popper himself. But Popper made no attempt to contrast the passage with the actual contents of the Principia. In the following pages he did criticise the idea that Newton's theory was based upon induction, but only by emphasising the parts played by Copernicus, Brahe and Kepler in its generation. I will adopt the simpler, direct interpretation of the passage as expressing Popper's own beliefs about the Principia.

I will take these sentences in reverse order, starting by comparing the final sentence with what is actually written in the Principia. There was in fact a very substantial amount of explicit data in Book 3. For example in the opening Phenomena section the periods of the four satellites of Jupiter are given together with four different methods of estimating their distances from the centre of Jupiter. These are compared with the results calculated according to Newton's theory. There is also a table of the orbital periods of the six planets and determinations of their mean distances from the Sun according to both Kepler and Boulliau. These are again compared with the predictions of his theory. Not only is Book 3 full of tables of data, but the text contains many discussions of the degree of relationship between the data and his theory. This is particularly full in the case of the Moon, where the theory did not fit very exactly with the predictions of the inverse square law and Newton considered various reasons for this. In the end he had to make the *judgement* that the discrepancy was a technical one which would eventually be explained within the scope of his general theory.[4]

An apologist might argue that most astronomical data are not observation−statements in the sense of Popper, but rather the conclusions of chains of theoretical arguments combined with observations. A typical example of this, announced by Roemer in 1676, was that a substantial anomaly in the apparent motions of the satellites of Jupiter needed to be corrected by reference to the finite speed of light and the variation in the distance between the Earth and Jupiter. But almost all scientific facts can be criticised in this way, until one ends up arguing that one cannot regard the contents of a letter as referring to facts: one only has inferences from the patterns of black and white light entering one's eyes. This is of course correct, but such comments are not of great interest precisely because of their universality. Science progresses by developing increasingly sophisticated views of what should be regarded as laws and as observations; both necessarily involve layers of interpretation accepted from the work of previous generations, and both may need later reconsideration.

Popper repeats the myth that Kepler's law of elliptic orbits was a key ingredient in Newton's derivation of the inverse square law of gravitation, a myth which is still widespread at the present time. In fact, as Chandrasekhar and others have remarked, he does not use it at all in Book 3 of the Principia, [Ch, p 355]. The inverse square law is, indeed derived from the ellipticity of *hypothetical* orbits in Book 1, Proposition 11, but this is the purely mathematical part of the Principia. In Book 3, which considers the *actual* planetary orbits, the law of elliptical orbits is *derived* in Proposition 13, but only after the inverse square law has been proved by quite different methods.[5] Nor does

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[4] An adequate explanation had to wait until the 1750s and the more systematic use of calculus.

[5] The derivation is based upon Book 1 Proposition 13 Corollary 1, but the validity of this argument has been seriously questioned, [CoWh, p 135−136]. Opinions vary between considering the proof wholly fallacious to maintaining that it has a minor flaw of omission. Whichever is the case, Johann Bernoulli

Newton attach any great importance to this proposition. The reason is clear: measuring the differences between circles and ellipses of very small eccentricity would not provide the sharp physical evidence which Newton sought. So as a matter of physics, Newton did not depend on Kepler's law of elliptic orbits.

Let us next consider Popper's claim that Newton purported to give a logical proof of his theory from the observational facts. In fact as Rule 4 above and the whole text of the Principia shows, Newton was always highly aware of the need to consider discrepancies between his theory and observations very thoroughly. Nowhere is there a suggestion that the use of the inductive method leads *logically* to the laws which he derives as Popper suggests. In fact I have failed to find any trite syllogistic arguments in the Principia of the type which Popper quotes from Hume; Newton's thoughts operated at a far higher level. It appears likely from text later in Chapter 8 that when Popper wrote the second sentence of the above quotation he was in fact relying upon the writings of Hume and Kant.

It is of course true that Book 1 of the Principia is deductive on style, but this is hardly surprising since it is basically pure mathematics. In the General Scholium added to the third edition, Newton criticises those who feign (i.e. unnecessarily adopt) hypotheses. Among such he surely included the questionable status of the ether and an explanation of how forces could act at a distance, which he himself never managed to resolve. Newton was entirely justified in trying to separate the mathematical and predictive success of his theory from hypotheses concerning the justification for action at a distance. Many of his critics could not accept any theory which involved such an action, [Co, p 32, 131], [CoWh, p149−155], and Newton himself tried in vain to find a mechanical explanation for it. He chose his deductive *style* to make it more difficult for readers to reject his conclusions, [CoWh, p 195−6], and in this he was (eventually) extremely successful.

The first statement in the quotation from Popper is a reference to an assertion in the General Scholium, discussed below. In the remainder of this section I will provide evidence that Newton employed a wide variety of different scientific methods, and that his reliance on induction is much less exclusive than is usually believed and than Newton himself claimed. When it came to the main text of the Principia, Newton used whatever argument was most likely to convince his readers.[6] In many places Newton considered alternative explanations and then ruled out all except one on the basis that they did not fit the astronomical data, exactly as Popper would like him to have done.

As a typical example Newton considered the effect of several different simple central force laws (Book 1 Proposition 45, Examples 1−3) on small perturbations of circular orbits. He then showed that the apsides move from one orbit to another by an amount which depended very sensitively on the particular force law considered. In Book 3 Proposition 2 he finally inferred that the planets move under an inverse square law of force by using the fact that their apsides do not move. He clearly knew that Proposition 45 only considered some particular, simple force laws and that Proposition 2 was not an ab initio logical deduction but the choice of one out of a few simple alternatives. This

considered it desirable to publish his own correct proof in 1710.

[6] In a footnote to 'Conjectures and Refutations' on page 107, Popper did state that Newton indeed made many hypotheses, referring particularly to his 'Opticks', but he did not expand on this comment.

was available for anybody to see. His other proof of the inverse square law in Proposition 2 was based upon Kepler's 3/2 law and Proposition 4 of Book 1, *which dealt only with circular orbits*. Newton regarded this as less compelling, but he was not beyond drawing approximate conclusions from approximate descriptions of the planetary orbits. As an approximation it is completely convincing, but he could not have thought that it was a logically conclusive proof. Cohen and Smith have argued that the Principia consists of a series of successive approximations on the way to his final universal law of gravitation rather than a process of logical deduction, [Sm].

Having established to his satisfaction that there was a universal inverse square law of gravitation, Newton next turned to the question of whether the gravitational force was proportional to the masses of the bodies concerned. Proposition 6 of Book 3 states [CoWh, p 806]:

> *That all bodies gravitate toward each of the planets, and at any given distance from the centre of any one planet the weight of any body whatever toward that planet is proportional to the quantity of matter which the body contains.*
>
> Others have long since observed that the falling of all heavy bodies toward the earth (at least on making an adjustment for the inequality of the retardation that arises from the very slight resistance of the air) takes place in equal times, and it is possible to discern that equality of the times, to a very high degree of accuracy, by using pendulums. I have tesed this with gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I got two wooden boxes, round and equal. I filled one of them with wood, and I suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes, hanging by equal eleven−foot cords, made pendulums exactly like each other with respect to their weight, shape and air resistance. Then, when placed close to each other [and set into vibration], they kept swinging back and forth together with equal oscillations for a very long time. Accordingly, the amount of matter in the gold (by book 2, prop. 24, corols. 1 and 6) was to the amount of matter in the wood as the action of the motive force upon all the gold to the action of the motive force upon all the [added] wood – that is, as the weight of one to the weight of the other. And so it was for the rest of the materials. In these experiments, in bodies of the same weight, a difference of matter that would be even less than a thousandth part of the whole could have been clearly noticed.

The above passage is worthy of careful analysis. Newton was well aware of the difference between the mass (quantity of matter) and weight of a body, but his Definition 1 of Book 1, [CoWh, p 403], is not very revealing, and has been much discussed, [CoWh, p89−95]. He had already investigated the speed of oscillations of pendulums in Book 2, Section 6, [CoWh, p 700] and conducted a series of experiments to determine the effect of air resistance. It was common knowledge since the time of Galileo that, if one could neglect the effects of air resistance, bodies of different masses fell together and the period of a pendulum depended only on its length. These facts established that the inertial and gravitational masses were equal. So what is the purpose of the last part of the above paragraph? It appears that Newton was facing the following issue: it was possible that the gravitation constant G in the universal law

$$F=Gm_1m_2 / r^2$$

depended upon the material of which the bodies concerned are made, and that this did not show up in the astronomical data because all the planets and satellites have more or less the same composition. The above experiments provided evidence that for a range of materials G is indeed a constant.

Everything written in the quoted paragraph matches the modern scientific method of forming a hypothesis, considering how to test it, and then carrying out the experiment. Newton started from a generally agreed rule, discovered by Galileo, that bodies fall under gravity at a rate which does not depend on their composition apart from the effects of air friction. He then devised the *most critical test of the relevant law which he could*, compensating for air resistance by putting two different materials in boxes of exactly the same size and shape. Since he was not able to measure time very accurately he used these boxes as weights at the ends of pendulums of equal length. The experiment was beautiful in that no timing was needed −− the null result that the two pendulums oscillated in step with each other proved that the two materials had the same ratio between their inertial and gravitational masses. He finally repeated the experiment with several different materials obtaining the same null result on each occasion. In further support of the claim that this was an instance of hypothesis testing, it should be mentioned that the very special apparatus used would only yield results accurate to one part in a thousand in the particular case in which the pendulums oscillated in step, that is in which the inertial and gravitational messes were indeed equal.

Of course Newton only carried out a finite number of comparisons and only demonstrated the equality to one part in a thousand. He knew that he was not giving a logical proof and on many such occasions referred explicitly to his rule of inductive generalization. Scientists do exactly the same today, in the twenty−first century. The equality of the inertial and gravitational masses is assumed in Einstein’s theory of relativity, which been tested as a whole, but the possibility that technetium (say) might deviate extremely slightly from the usual law is simply disregarded for obvious reasons: once one has a certain amount of evidence for a belief there are more interesting things to do than to simply keep on seeking more. It is, of course, entirely possible that we may one day regret this omission.

In Proposition 4 of Book 3 Newton performed some fairly convincing calculations to prove that the centripetal force which keeps planets in their orbits is the same as the gravity which controls the motion of projectiles and pendulums on the Earth. In the present age of satellites and aircraft it takes an effort for us to realize that this was by no means obvious, but people in the seventeenth century had no evidence that gravity extended further than a few hundred metres above the ground, while the centripetal forces which kept heavenly bodies in their orbits operated at distances of between hundreds of thousands and hundreds of millions of kilometres. Following this proposition

Newton inserted a Scholium, [CoWh, p 805], in which he discussed the identity of the two forces more fully. He imagined several further moons of the Earth, one of which was orbiting just above the tops of the mountains. By the universal inverse square law which he had discovered it would be subject to his new centripetal force. Since it was very close to the Earth it would also be subject to the normal force of gravity. If these were not the same then they should be added together to give a combined force twice as great as gravity. If now the moon were deprived of its orbital motion, it would fall to Earth with twice the speed of normal objects, a conclusion which he regarded as absurd. The above argument is clearly a *thought experiment*, a genre which became very popular in the twentieth century as a means of teasing out paradoxical consequences of laws. My point in mentioning it here is to emphasise that it bore no relationship with his stated philosophy of sticking strictly to what could be deduced from phenomena without the use of hypotheses.

Newton's sophistication of thought was well demonstrated when he discussed the explanation for the tides, [CoWh, p 877]. He discussed the problems associated with the flows of large bodies of water into and out of channels, and explained his criteria for selecting the locations which were likely to provide the most accurate evidence of the effect of the moon and sun. These arguments involved careful *judgements* about which data to accept and which to reject, judgements which were not supported by any propositions or inductions, because the complexities of geography did not permit this.

I have written above that Newton did not depend upon Kepler's law of elliptical orbits when considering the planets. After he had established the inverse square law for gravitation and proved that bodies, in particular comets, must move in conical orbits, [Book 3, Proposition 40], he devised a method for determining the trajectory of a comet from three given observations, assuming that the orbit was a parabola, [Book 3, Proposition 41]. This was a rather weakly stated hypothesis at this stage since it was a priori possible that most comets move in hyperbolic orbits; Newton alluded to it on page 895, writing

> Unless I am mistaken, comets are a kind of planet and revolve in their orbits with a continual motion … Hence if comets revolve in orbits, these orbits will be ellipses … so close to parabolas that parabolas can be substituted for them without sensible errors.

But he did not prove anything here. *After* applying his method of determining the supposedly parabolic orbit to the comet of 1680, with data provided by Flamsteed, he wrote (p 909)

> Finally, in order to establish whether the comet moved truly in the orbit thus found, I calculated – partly by arithmetical and partly by graphical operations – the places of the comet in this orbit at the times of certain observations, as can be seen in the following table. (The table confirms his predictions to within a few minutes of arc.)

What better illustration could anyone ask of the method of testing a conjecture (that the orbit was indeed a parabola) by comparing predictions based on the conjecture with observations not already used to determine the free parameters of the equations? Not yet satisfied, he then quotes further observations and calculations by his ‘fellow countryman Halley’, confirming yet again the predictions of his theory. This must be regarded as a major success of his theory, since the orbits of the comets are extremely eccentric, unlike those of the planets.

As a final example of Newton’s flexibility of approach one may look at the last pages of Book 3, where he discussed the nature of cometary tails. Here Newton abandoned the deductive style, and instead launched on a long discussion of various opinions about their nature, and the difficulties involved in each of them. The discussion is full of references to observations by various people, and a remarkable amount of detail is recorded. This section clearly shows how Newton thought about a subject which was still incomplete, as opposed to how he chose to present a theory when it had all been worked out. One finds an exactly parallel schism between the way scientists talk to each other today and how they choose to write up their work for publication.

Other Comments of Popper

In 1977 Popper and Eccles published ‘The Self and Its Brain’, a work in which the contributions of the two authors are fortunately kept quite distinct. In this book Popper refers twice to the Principia. I quote a passage which is remarkable for its inconsistency with my previously quoted, and earlier, passage of his, [PoEc, p 190].

> (a) (Newton) believed that his laws of motion were obtained by induction from the phenomena.
> (b) He admitted that induction was not a valid proof.

Popper’s words ‘admitted’ and ‘valid proof’ in (b) suggest that Newton had been driven back to a defensive position with which he was not really satisfied. But the evidence and historical scholarship do not support this. Newton was very cautious not to suggest that data of limited accuracy could yield a *logical* proof of the truth of his laws, and even emphasised that mathematics is exact but nature is not, [Book 2, Prop 48, p 772], [Co, p92]. Perhaps (b) should be redrafted as ‘Popper admitted that Newton had not claimed that induction led to a logically irrefutable proof’.

Popper’s statement (a) would be accurate if the word ‘believed’ were changed to ‘claimed’. It is dangerous to suggest that someone as brilliant as Newton was not aware that the third edition of the Principia contained three explicitly labelled hypotheses, and that the actual methods used in the Principia were much more varied that he admitted in the General Scholium. This Scholium has been much discussed, [CoWh, p 274−292], and the idea that Newton held mistaken beliefs about the contents of his own work, [PoEc, p 175], is the least plausible explanation for what he wrote. Most likely he was contrasting his working methods as strongly as possible with those of the Cartesians. By their standards he did indeed ‘not feign hypotheses’, [Co p100, 106], [Cl, p270]. Newton was also declaring his position in the bitter disputes which arose within the Royal Society after the publication of the first edition of the Principia between the ‘naturalists’ (for whom the patient collection of observations was the primary goal) and the ‘mathematicians’ (for whom observations were only a stage on the way to a mathematical theory of the world), [Fe, p 95]. Certainly he was not writing to avoid all possible misinterpretations by nineteenth and twentieth century philosophers of science.

Perhaps the most extensive comments of Popper about the Principia are to be found in ‘Realism and the Aim of Science’, published in 1983, but written in the 1950s. I reproduce two quotes, [Pop3, p 140, 147].

> For this reason it is impossible to derive Newton’s theory from either Galileo’s or Kepler’s, or both, whether by deduction of by induction. For neither a deductive nor an inductive inference can ever proceed from consistent premises to a conclusion that formally contradicts these premises. I regard this as a very strong argument against inductivism.
>
> 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’.

The first passage echoes comments of Duhem, and is discussed in the next section. In the second Popper fails to comment that his meaning is quite different from that of Newton, explained in the latter’s Rule 4. It is probable that the meaning Popper gives is that of many twentieth century philosophers, but no modern scientist would accept it for a moment, and nor did Newton. It presents induction as a logical principle (possibly involving some probabilistic component), rather like deduction but involving an extra step which Hume proved to have no logical justification. Popper has no right to criticise Newton for using the word in a different sense, particularly when Newton’s use was much more cautious, and free of logical claims.

Duhem

It is impossible to determine all of the sources which might have misled Popper when he was writing about Newton, but it is instructive to compare his views with those of two philosophers of science from early in the twentieth century who would have had access to the same sources: Duhem and Poincare. At that time it had long been considered that Newton’s laws were *exactly true*; finite human beings had somehow acquired final knowledge of the laws governing the world. We will consider the reasons for this briefly in the next two sections.

Duhem wrote at some length about what he called the Newtonian method: that of proceeding deductively from established fact to general laws ‘without any use having been made of any fictive hypotheses’, [Du, p191]. Newton’s clear statement that the Sun is mobile and that Kepler’s laws were only approximate was represented by Duhem as follows.

> That posited, let us follow Newton’s reasoning.

> Newton first took the sun as the fixed point of reference; he considered the motions affecting the different planets by reference to the sun; he admitted Kepler's laws as governing these motions, and derived the following proposition: if the sun is the point of reference in relation to which all forces are compared, each planet is subjected to a force directed toward the sun, a force proportional to the mass of the planet and to the inverse square of its distance from the sun. Since the latter is taken as the reference point, it is not subject to any force. [Du, p192].[7]

The word 'first' above might have two meanings. If it means 'from the very beginning and without reservation' then it is certainly incorrect, as my comments above show. If, however, it means 'as an initial approximation, to be revised subsequently in favour of Newton's own much more accurate theory' then Duhem's further criticisms of Newton become much more difficult to understand. Following the above passage Duhem correctly explained that Kepler's and Newton's laws could not both be exactly true.

> if, on the contrary, we admit the absolute exactness of Kepler's laws, we are compelled to reject the proposition on which Newton based his celestial mechanics …Therefore, if the certainty of Newton's theory does not emanate from Kepler's laws, how will this theory prove its validity? It will calculate, with all the high degree of approximation that the constantly perfected methods of algebra involve, the perturbations which at each instant remove every heavenly body from the orbit assigned to it by Kepler's laws; then it will compare the calculated perturbations with the perturbations observed by means of the most precise instruments and the most scrupulous methods. [Du, p193]

This passage cannot correctly be directed against Newton, who fully recognised that Kepler's laws were only approximately true. In Proposition 12 of Book 3 Newton wrote that the Sun is not fixed, but must move appreciably as it orbits around the common centre of mass of the entire solar system. Following Propositions 12 and 13 he even estimated the magnitude of this movement. He also discussed corrections to Kepler's laws due to the gravitational forces of Jupiter and Saturn on each other [CoWh, p817 and 818], and claimed that such effects had been observed by astronomers.[8] Nor did Newton rely exclusively upon Kepler's laws: his sharpest test of the truth of the inverse square law for the planets depended on the measurement of the positions of the apsides.

Not only did Newton not consider Kepler's laws to be exact, but, at the time Duhem wrote, nobody else had thought this for more than a hundred years. By and large Duhem came to reasonable conclusions concerning the status of Newton's theory, but he could have done so much more easily by referring directly to what Newton himself wrote in the main body of the Principia. He was, in fact, criticizing Newton the myth rather than Newton the man.

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[7] 'The Aim and Structure of Physical Theory' was originally published in French in 1914.

[8] Unfortunately it now appears that this could not have been correct, since the effect is much smaller than any instruments could have detected at that time, [CoWh, p206−217]. Chandrasekhar tactfully fails to mention this section of Principia, [Ch, p 379].

Poincare

Even in the third edition of the Principia there is an unfortunate Hypothesis 1 which declares that the centre of the world (i.e. the centre of mass of the solar system) is at rest, in line with his belief in the existence of absolute space, [CoWh, p 816, 231]. Newton discussed the difference between absolute and relative space at some length. The famous astrophysicist Chandrasekhar supported the modern view that choosing the centre of mass of the solar system to be the origin is just a convenient convention, and suggested that Newton may have retained the word 'hypothesis'; in error, [Ch, p 377]. But there is little to support this interpretation, and it seems likely that Newton's belief in absolute space was genuine, [CoWh, p 107]. It may possibly have had religious overtones.

In his book 'Science and Hypothesis' Poincare made clear how strongly he disagreed with this concept of absolute space, [Poi, p 114]. In fact at that time (1902) he was struggling towards a theory of relativity, as of course was Einstein. I am, however, not concerned with Poincare's scientific beliefs, which changed sharply between 1902 and 1905, but what he had to say about the Newtonian myth. Unfortunately his books concentrate heavily on nineteenth century mathematics and physics, and one has to look carefully for hints of his attitudes towards Newton. Even when he criticises the idea of 'constructing the world with the aid of the smallest possible amount of material borrowed from experiment', [Poi, p xxii], he did not mention Newton, but 'many men of science a hundred years ago'; in fact the quotation is a paraphrase of a statement which Laplace made in the preface to his Celestial Mechanics'; see below. As a major contributor to the development of mechanics, it is not surprising that the few historical comments which Poincare made were much better informed than those previously mentioned. One fascinating passage throws substantial light on how the myths were sustained.

> The English teach mechanics as an experimental science; on the Continent it is taught always more or less as a deductive and *a priori* science. The English are right, no doubt. How is it that the other method has been persisted in for so long; how is it that Continental scientists who have tried to escape from the practice of their predecessors have in most cases been unsuccessful? On the other hand, if the principles of mechanics are only of experimental origin, are they not merely approximate and provisory? May we not some day be compelled by new experiments to modify of even abandon them? These are the questions which naturally arise, and the difficulty of solution is largely due to the fact that treatises on mechanics do not clearly distinguish between what is experiment, what is mathematical reasoning, what is convention and what is hypothesis. [Poi, p 89].

These criticisms of the post−Newtonian scientific community were echoed by Einstein, writing in Die Naturwissenschaften on the two hundredth anniversary of Newton's death, 1927.

> 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 …

Laplace

The myth that Newton derived his inverse square law of gravitation from the ellipticity of the planetary orbits may derive from the fact that this was exactly what Laplace did do in his 'Celestial Mechanics', [Lap1, p239].[9] In four massive tomes, written between 1798 and 1827, Laplace set himself the task of reworking Newton's theory making systematic use of the calculus and taking advantage of the much more accurate astronomical data then available. By the time he wrote, the truth of Newton's laws was not in doubt, so he abbreviated the proof of Newton's inverse square law as far as he could in order to spend his time calculating detailed corrections to the planetary orbits. In his Preface to Volume 1 he wrote 'It is very important to reject every empirical process, and to complete the analysis, so that it shall not be necessary to derive from observations any but indispensible data.' True to his intentions, the tomes are full of detailed and forbidding calculations, with a minimum of references to the real world. The following text, [Lap1, p 256], is one of the rare concessions which he makes to the limitations of the deductive method.

> We have shown that this law follows the inverse ratio of distances. It is true, that this ratio was deduced from the supposition of a perfect elliptical motion, which does not rigorously accord with the observed motions of the heavenly bodies. But we ought to consider that the most simple laws should always be preferred, until we are compelled by observation to abandon them. It is natural at first to suppose that the law of gravitation is inversely as a power of the distance; and we find by calculation that the slightest difference between this power and the square would become extremely sensible in the position of the perihelia of the planetary orbits, in which, however, no motions have been discovered by observation, except such as are very small, the cause of which will be explained hereafter. In general we shall see, in the course of this work, that the law of gravitation, in the inverse square of the distances, represents with the greatest precision, all the known inequalities of the motions of the heavenly bodies; and this accordance, taken in connexion with the simplicity of the law, authorises the belief that it is rigorously the law of nature.

Without in any way detracting from Laplace's greatness, one has to admit that the Principia reveals the underlying physics more clearly, and that Newton grappled much more openly and thoroughly with the limitations of the scientific method. In the last

[9] He had some justification for doing this, since by his time much more data relating to the highly elliptical orbits of the comets existed; he frequently referred to the planets and comets as a single group of entities.

sentence Laplace seems to be taking a different position from Newton in one important respect. Namely he considered that a theory *may be justified as a whole*, that is in terms of its overall agreement with observations, and that one does not need to verify each law separately. He did not however, claim that the tests logically proved the truth of the theory, but only that they authorised such a belief.

In addition Laplace was responsible for the following famous passage, written in a passage arguing that randomness appears in our description of events solely as a result of our ignorance of the relevant laws or data.

> An intellect which at any given moment knew all the forces that animate Nature and the mutual positions of the beings that comprise it, if this intellect were vast enough to submit its data to analysis, could condense into a single formula the movement of the greatest bodies of the universe and that of the lightest atom: for such an intellect nothing could be uncertain; and the future just like the past would be present before our eyes. [Lap2]

This Laplacian determinism was sometimes incorrectly called Newtonian determinism, and the phrase 'forces which animate Nature' was taken to be a reference to Newton's laws of motion. I do not wish to get involved in an analysis of Laplace's *thoughts* here, but Laplace was aware of the existence of electricity, magnetism and chemistry, even though they were hardly understood at that time, and it must have been clear that any final laws of the universe would have to take them into account. Nevertheless, this statement of Laplace certainly contributed to the nineteenth century view that Newton's laws were exact and fully explained the motion of bodies.

Conclusions

We conclude from our discussion that Popper, like many others of his generation, had not read the Principia sufficiently carefully and was misled by the myths which surrounded it. In spite of what he wrote in the introductory sections to the Principia, Newton followed a wide variety of different methods, as seemed appropriate in the circumstances. Sometimes he formulated alternative hypotheses which he then tested against the evidence, and sometimes he even based his arguments on thought experiments. The technical difficulty of the Principia, combined with its stunning predictive success for over two centuries, led people to idealize the process which led to his discovery of the law of gravitation. This was partly a result of Newton's own propaganda, but just as much a result of the increasing adulation which he received as his theory kept on delivering accurate predictions. Perhaps we can at last realize that Newton was greater than some of his twentieth century philosophical critics have allowed him to be, and that his scientific philosophy can hardly be faulted even by today's standards.

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