The Mathematical Principles of Natural Philosophy

Newton's *Principia* is perhaps the second most famous work of mathematics, after Euclid's *Elements*. Originally published in 1687, it gave the first systematic account of the fundamental concepts of dynamics, as well as three beautiful derivations of Newton's law of gravitation from Kepler's laws of planetary motion. As a book of great insight and ingenuity, it has raised our understanding of the power of mathematics more than any other work.

This heavily annotated translation of the third and final edition (1726) of the *Principia* will enable any reader with a good understanding of elementary mathematics to grasp easily the meaning of the text, either from the translation itself or from the notes, and to appreciate some of its significance. All forward references are given to illuminate the structure and unity of the whole, and to clarify the parts. The mathematical prerequisites for understanding Newton's arguments are given in a brief appendix.

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Isaac Newton

Translated and annotated by C. R. Leedham-Green

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Translator's Preface

Having purchased, on a whim, a second-hand copy of the Koryé and Cohen (1972) variorum edition of Newton's *Principia*, I also acquired the Cohen & Whitman (1999) translation of *The Principia* into English. This translation cried out to me, and perhaps to others, that another was urgently called for. It is my primary aim, in this introduction, to explain why I do not believe that the Cohen and Whitman translation should be allowed to stand unchallenged as the canonical English version of this great work.

Let me first say of translating *The Principia* what Newton so memorably said of Descartes' theory of vortices, namely that it is pressed with many difficulties. Different readers will wish to read *The Principia* in different ways, and will wish for translations made with different principles. Indeed the serious reader may profit from different translations. The translator faces many acute problems to which there are no single satisfactory solutions. Some of the issues that I have with the Cohen & Whitman translation are not that poor choices have been made; but rather that a translation based on other choices is also needed. To some extent I regard my translation and that of Cohen & Whitman as complementary.

In brief, at the risk of trivialising a complex matter, whereas Cohen & Whitman aim at linguistic precision, at the risk of losing or obscuring the scientific or mathematical meaning, I have consistently aimed to express that meaning, even at the cost of linguistic imprecision.

Even to a mathematician Cohen & Whitman (1999) can be, at best, very obscure. In their preface they write 'We were not so vain that we were always sure that we fully understood every level of Newton's meaning'. Translating mathematics without an adequate understanding can be hazardous; and if an argument defeats the translator then I think the translator needs to be very specific, and say 'I cannot make sense of the following, but this is what the Latin seems to say.'

While Cohen does not presume to understand the whole text, he does make a very gentle boast at the end of his guide, where he needs a sentence to summarise his attitude. This is what he writes: 'Whoever studies the *Principia* in awareness of the works of Newton's predecessors will share the high value assigned this work ever since its first publication in 1687 and will rejoice that the human mind has been able to produce so magnificent a creation.' Obviously Cohen's claim to being aware of the works of Newton's predecessors was well founded; and I am not so vain as to make a similar claim myself. However, my appreciation of *The Principia* owes much more to my having tried to unravel the precise meaning and significance of the text than from my (admittedly inadequate) reading of the works of his predecessors. It is unlikely that two readers of *The Principia* such as Cohen and myself, with such opposed attitudes to the text, should agree on how it should be translated.

It is important to realise, in the context of understanding the text, that Latin in general, and *The Principia* in particular, can be given to ambiguities of many kinds. Some arise from the vast extent of Latin literature and the consequent multiplicity of meanings of individual words; some arise from grammatical ambiguities in Latin; and some arise from Newton's style of writing. The response of Cohen and Whitman has often been to retain rather than resolve these ambiguities, sometimes abandoning English grammar for English written within the context of Latin grammar, and generally replacing Latin words by the etymologically closest English word. Thus Cohen writes, on p. 299 of their translation, 'For example, rather than impose a strict modern dichotomy between a vector "velocity" and a scalar "speed", we have generally followed Newton and have translated Newton's "velocitas" as "velocity", and "celeritas" as "speed".' We might permit ourselves to add 'regardless of meaning' to their sentence, and pay due attention to the introductory 'For example', warning us that they maintain this style of translation throughout. What Newton in fact means by *velocitas* is discussed here in Appendix G.

It is inevitable that a translation of *The Principia* that in any way clarifies Newton's arguments will be attacked as an attempt to modernise Newton's thought. As far as the mathematics is concerned, there is no need for clarification to do anything but clarify. Newton understood what he was doing. There are more modern concepts that he might have used, had they not been beyond the scope of his mathematics: such concepts are not used in our translation, and rarely in the notes. When it comes to physics the situation is less clear. For example, Newton had a partial understanding of energy, as discussed in Appendix F, but no word for the concept.

A more awkward issue, particularly towards the beginning of Book 1, is the relation between Newton's dynamics and pre-Newtonian dynamics. One could assert that Newton was the first to formulate the concept of mass; that mass and force are inextricably entwined concepts; and that therefore Newton was the first to have a modern definition of force. Having come to this conclusion, and accepting that Newton's concepts of force and mass are equivalent to the modern definitions (which they are, at least within the confines of Newtonian dynamics) there is no modernising to do. This is, in a sense, a reasonable attitude. However, mass is related to weight, and force is a concept that, in a less well-defined sense, goes back to the beginning of time.

So Newton has some historical baggage to carry with him; in particular pre-Newtonian equivalents of mass, with their pre-Newtonian terms, such as *vis inertiae*, and *vis insita*. The acceleration of a body is proportional to the force applied to it, and Newton effectively defines the *vis insita* to be the inverse of the constant of proportionality, in line with the modern definitions of mass and force, in Definition 3. He follows up this definition by asserting that the *vis insita* is proportional to the mass, and only differs from the mass in the way that it is thought about. But here he uses *corpus* for 'mass' at its first appearance, and *inertia massae*, or 'inertia of mass' at its second appearance. He then defines the inertia (the same word in English or Latin) of matter in exactly the same way (with elegant variation) that he has just defined *vis insita*. He then deduces that *vis insita* may be given the very significant name *vis Inertiae*, the significance being perhaps emphasised by an unexpected upper case 'I'.

So now we have *corpus*, *massa*, *vis insita*, *inertia*, *vis Inertiae*, and *inertia massae*, all with identical meanings, and with only one word, namely 'mass', that conveys this meaning in contemporary English. There is a case for leaving these terms untranslated; there is no English expression that begins to convey both the meaning (which is unambiguously 'mass') and that gives the historical connotation. However, if one does translate these terms as 'mass' one is not in any way whatsoever imposing modern ideas on Newton. What is the modern idea? The danger is not of imposing on Newton modern concepts that had escaped him, but rather of depriving him of some of his connections with the past.

As a further twist, towards the beginning of Book 1 he makes use of *conatus*, or 'endeavour'; so the *vis insita* of a body is expressed as a *conatus* to travel at a uniform velocity. Cohen takes a very strong view on such issues. In his attack on Cajori's edition (Cajori, 1934), Cohen writes, on p. 32 of Cohen & Whitman (1999), 'A fault of a much more serious kind is the attempt to modernize Newton's thought and not just Motte's translation. . . The same tactic of making Newton's physics seem more correct may be seen in the Motte–Cajori substitution of the word "continues" for Motte's more volitional "perseveres" in the statement of the first law of motion, where "Every body perseveres in its state. . . " is modernized to "Every body continues in its state. . . " Newton's Latin version, in all three versions, uses the verb "perseverare".'

Cohen is carried away by his enthusiasm. The first law, in unison with the whole of *The Principia*, states that bodies move according to mathematical laws, and not by their own volition. One might argue that these are not incompatible; so let us emphasise the supposed free will of bodies, and translate the law as "Every body always decides. . . " This would be ridiculous. Newton asserts explicitly, in his concluding general scholium, that he is concerned with the laws that bodies obey, and not with the reasons that they obey them. If *perseverare* is to be taken to have a volitional nuance then clearly this is pure metaphor; so at the very worst Cajori has modernised Newton's idiom. Idioms do change with time and in translation. The best that can be said of the English weather on most days is 'The sun is trying to break through'. It is a convenient metaphor, and not easy to replace with an equally snappy expression; but the reader may have trouble in translating this sentence into Latin, or any other language, without losing the volitional aspect of the metaphor. Does *perseverare* have a volitional nuance? I believe that Cohen & Whitman will have translated *perseverare* as 'perseveres' because they almost always take the etymologically closest translation. While the translation of *perseverare* here as 'perseveres' or 'continues' might seem of no significance, I should like to consider the matter briefly, as it touches on a central issue of translation. Let us take the Cajori translation, and translate it back into Latin. We need a verb for 'continues'. I see that Cassell's Latin dictionary has thirteen suggestions, appropriate in different contexts. These include *perseverare* and *persistere*, the others being inappropriate here. Both of these verbs have a volitional nuance, *persistere* suggesting a certain obstinacy. So Cohen is wrong, in that *perseverare* is the least volitional verb that Newton had at his disposal. Against that, one might argue that *perseverare* does still have a volitional nuance.

At this point the argument becomes more and more subtle, and the outcome more and more uncertain, and less and less important. Those who wish to pursue the nuances of Newton's Latin are hoeing a difficult row, and need to rely on better evidence than an assumption that the etymologically closest word to Newton's Latin bears the same nuance in contemporary English that the Latin word did to Newton.

In this context, Cohen & Whitman translate *coire* as 'to coincide' throughout the proof of the corollary to Lemma 18, Section 5, Book 1, where it surely means 'to converge'. Here it is not a question of whether bodies move according to their free will, but of whether lines and points are moving or stationary. They are moving, *ire* means 'to go', and to translate *coire* as 'to coincide' weakens the rigour of Newton's proof. It is only too easy to make such mistakes, and unwise to reject other translations so easily. But I should not be too dogmatic here; in Whiteside (1974) *coire* is also translated as 'to coincide', and perhaps Cohen & Whitman follow this lead.

So there is a major issue that runs through *The Principia*, which is not modernising Newton's thought, but modernising the way that Newton expresses his thoughts. As far as mathematics is concerned, Newton regards Euclid as the gold standard. Thus Newton believes that he can (or that he should) reduce all his mathematics to Euclidean geometry. In the same way, modern mathematicians believe that they can (and that someone else should) translate their arguments into the language of first-order set theory. All that Newton really needs as the foundation for his many arguments that do not sit well with Euclidean geometry is an understanding of real numbers; and he has a very clear and modern understanding of real numbers.

Unfortunately his zeal for Euclid persuades him to translate his arguments at least part of the way into Euclidean geometry, even when this is inappropriate. I have translated these arguments as he wrote them, and whenever I have felt that this might be helpful, I have given, in the notes, a version in 'modern' notation; that is, in Leibniz' notation. Most of these arguments are carried out by writing down a simple differential equation and solving it, exercises that Newton will have carried out instantaneously in his head. The idea that these easy solutions somehow escaped him, and that the contorted geometrical translations of these arguments that he gives represent his line of thought, is unbelievable. The contortions that he has to go through are often rather serious, and it is not easy to appreciate, or even to follow some of these arguments without the simple solution before one. But there are some arguments in *The Principia* that are purely geometric, such as the two proofs of the law of the ellipses, Proposition 11 of Section 3, Book 1, in contrast to those in which the obvious proof by calculus has been translated into rather ungainly geometry. But what is one to make, for example of the proof of Proposition 41 in Section 8 of Book 1? This is a routine proposition, of some importance, that is stated in terms of integrals. However, the solution is given geometrically, the integrals being areas. The proof takes Newton two full pages, including a diagram reproduced on each page. When we examine the proof, re-writing it in a note in more familiar notation, we find that more than thirty steps have been taken. But, as we observe, there is an entirely elementary proof that takes three lines. We leave the reader to draw whatever conclusions seem most probable.

No difficulty arises if it is felt that a rewriting of a proof, in a note, is necessary for a proper appreciation of the original; but there is a constant problem with single words that Newton uses with undefined, or unexpected meanings. I have very seriously avoided following the practice of Cohen & Whitman. I will not translate *constanter* as 'constantly' when Newton means 'monotonically'. However, I do tell the reader that 'monotonically' is a translation of *constanter*, I explain how the subsequent argument requires this meaning, and I explain why Newton is content to consider monotonic functions in the given context. Any serious mathematician could have worked all this out, but it does need to be worked out. It seems to me that Cohen & Whitman were 'not so vain' to decide what Newton meant by *constanter*, so they translated it as 'constantly'. So this brings me to the central issue. Is it the responsibility of the translator, or of the reader, to make sense of what the translator writes? If Cohen &

Whitman succeeded in decoding what they wrote (where this is possible), why did they leave no trace of the massive effort required to carry this out?

Part of the problem that Newton has with his exposition is his reluctance to multiply quantities. Thus we define work as a product of a distance and a force, and may measure work in foot–pounds. One measures the distance in feet, the force in pounds, and the product is the work in foot–pounds. This, of course, would not have been regarded as a new idea by Newton; he does as much himself, measuring work in inch-ounces at the end of Section 6, Book 2. However, he regards this as poor style, so he would have defined work as a quantity that is jointly proportional to the force and the distance through which it acts. From a modern perspective this is a clumsy circumlocution; and the modern style is sometimes used by Newton; so to reword Newton in this way would not be to modernise his thought, but rather to modernise his style.

It is worth noting that Newton, in the scholium after Lemma 10, in Section 1, Book 1, explains this use of proportion (which he has been using for some thirty pages) in terms of multiplying and dividing quantities. So he assumes that his readers will regard the multiplication and division of quantities as more natural than dealing in ratios.

I have refrained from trying to improve on Newton's style up to the point where I have felt that the reader will otherwise get lost in the fog. If, as is often the case, a translation that I regard as acceptably clear is unacceptably far from what Newton wrote, I simply produce what I regard as a faithful translation, and give a paraphrase in the notes. My test of whether a translation is close enough to what Newton wrote is quite simple, at least to state. I take what I have written and ask myself, if I were required to translate what I have written back into Newton's Latin, whether I would regard what Newton wrote as a plausible retranslation.

I hope that the result of my labours is a translation that is much easier to read than that of Cohen & Whitman, and that is more reliable on the main mathematical and physical issues. Against this, their translation is supported by an incomparably better knowledge of Newtonian criticism, and of the historical context. *The Principia* has a mathematical context, a physical context, and an historical context. The mathematical context asks primarily how Newton's mathematics sits within mathematics as a whole, and similarly for the physical context. It is not for me to say what is meant by historical context; but whatever is meant is far better addressed by Cohen & Whitman, with Cohen's lengthy and learned guide, than is my translation, the work of a mathematician.

So far, in this preface, we have discussed generalities. I now turn to detail and examples. So I invite the reader to consider Cohen and Whitman's translation of Corollary 8 to Proposition 66 in Book 1, reproduced below. I choose this corollary, and the next, since this proposition and its corollaries are central to *The Principia*. The enthusiastic reader may wish to try to understand our translation of the corollary; the technical terms used are explained in Appendix E. But here is their translation.

Corollary 8 Since, however, the advance or retrogression of the apsides depends on the decrease of the centripetal force, a decrease occurring in a ratio of the distance TP that is either greater or less than the square of the ratio of the distance TP, in the passage of the body from the lower to the upper apsis, and also depends on a similar increase in its return to the lower apsis, and therefore is greatest when the proportion of the force in the upper apsis to the force in the lower apsis differs most from the ratio of the inverse squares of the distances, it is manifest that KL or $NM - LM$, the force that subtracts, will cause the apsides to advance more swiftly in their syzygies and that LM, the force that adds, will cause them to recede more slowly in their quadratures. And because of the length of time in which the swiftness of the advance or slowness of the retrogression is continued, this inequality becomes by far the greatest.

Whether or not the reader has chosen to accept the challenge of understanding our translation, I expect that it will be accepted that the above translation is obscure. Moreover the reader is likely to conclude that the one clear statement, namely that $KL = NM - LM$, is false.

In defence of Cohen & Whitman, this is, of course, linguistically true to the original, and they may lay fault at Newton's feet. However, the object of the exercise is to penetrate the fog, rather than to apportion blame. My own view is that the translator has to work out what the original means, and either produce a translation that reflects this meaning clearly, or, if this is impossible, produce a translation like the above and then give a paraphrase. Whether Cohen & Whitman understood this corollary I cannot tell; they certainly have problems with the subsequent one. That corollary is long and rambling, the statement and the proof being intertwined. It contains a clause, repeated with elegant variation, which they mistranslate on its first appearance in a way that destroys the meaning of the corollary, but which they translate correctly on its second appearance. Rather than repeat the lengthy corollary here, I refer the reader to the text of this translation.

All the defects with Newton's style of exposition are typical of his time. The reader may recognise the following sentence by another genius of the seventeenth century.

> And me perhaps each of these dispositions, as the subject was whereon I entered, may have at other times variously affected; and likely might in these foremost expressions now also disclose which of them swayed most, but that the very attempt of this address thus made, and the thought of whom it hath recourse to, hath got the power within me to a passion, far more welcome than incidental to a Preface.

To make the comparison fair it too is out of context, being the second sentence of Milton's Areopagitica.

Before looking at the problems that arise from Newton's style of writing, it may be worth pointing out that The Royal Society had adopted a stylistic policy summarised by one of its founders, Bishop Thomas Sprat, in 1667, and quoted from Moessner (2009), as follows:

> a constant Resolution, to reject all the amplifications, digressions, and swellings of style: to return back to the primitive purity, and shortness, when men deliver'd *things*, almost in an equal number of *words*. They have exacted from all their members, a close, naked, natural way of speaking; positive expressions; clear senses; a native easiness: bringing all things as near the Mathematical plainness, as they can: and preferring the language of Artizans, Countrymen, and Merchants, before that, of Wits, or Scholars.

Newton paid insufficient attention to this resolution, and a serious difficulty, already mentioned, is his constant use of elegant variation; he not only uses different words for the same meaning, but also uses the same word with different meanings; sometimes in the same sentence. As an example, consider Proposition 23, Section 5, Book 2. Here Newton is considering the pressure exerted by a fluid, and he has three words, namely *compression*, *pressio*, and *vis*, and two concepts, namely force and pressure, for these three words to describe. It is vital to distinguish correctly between these concepts. He uses *compression* twice, meaning 'pressure' on each occasion; *pressio* ten times, meaning 'pressure' twice and 'force' eight times; and *vis* three times to mean 'force'. Cohen & Whitman translate both *pressio* and *compression* as 'pressure', and *vis* as 'force'. But, to be fair, none of the translations I have consulted has addressed the issue in this proposition. As another example, the bucket in the great scholium at the end of The Definitions is sometimes denoted by *vas*, and sometimes by *situla*; but it is surely the same bucket.

Other types of variation occur in the form in which Newton frames his assertions. Thus we find in Book 1, Section 1:

Lemma 1 *Quantities . . . become equal in the limit.* (Present indicative.)

Lemma 2 *If . . . I say that . . .* (*Dico quod* with the indicative.)

Lemma 3 *If . . . it will shrink for ever, and will finally vanish.* (Future.)

And in Section 2, Proposition 1: *Areas . . . to be proportional to the times*. (Infinitive.)

Consider also Newton's use of *aequatio* in two correct senses; firstly in the standard current usage of the word 'equation', and also in the sense of 'correction'; what one adds to one thing to make it equal another. The English word 'equation' may also be used both senses, though the latter sense is now obsolete, except in the 'equation of time' for a sundial. Cohen & Whitman translate *aequatio* as 'equation' in all cases according to their principles, and we translate it as 'equation' or 'correction' according to ours.

The critic who wishes to read subtleties into Newton's use of language has to make very sensitive judgements, based on the work as a whole, and needs a firm grasp both of the extent of Newton's elegant variation, and of issues of Latin usage that may lie below the surface. These issues are complicated by that fact that Newton's Latin is informed both by Cicero and other classical writers on the one hand, and by the scientific usages of his time (such as his occasional use of *plus minus* to mean 'more or less') on the other.

Related to this issue is the fact that Latin is not English. Latin is a highly inflected language, English is not. So for example, given a point P and a point Q, Newton asserts that a line is to be drawn joining the point P to the point Q. Nothing odd there, you might think; but an English text would simply join P to Q, without pointing out the obvious fact that P and Q are still points. Why does Newton do this? He needs P to be in the accusative and Q to be in the dative or ablative (depending on how he has constructed the sentence). So we will have *punctum* P and *puncto* Q, if you leave out 'point' in Latin the sentence collapses. (As always, matters are not so simple. So in Book 2, Section 7, Proposition 34, the proof contains the expression *in* bE rather than *in lineam* bE for 'onto the line bE'.) Not so in English, and so one should write simply P and Q. But conversely there are contexts in which grammatical gender resolves ambiguities in the Latin that have to be resolved by other means in English. I have made some effort to remove such latinisms from my translation.

Also Latin is a grammatically more rigid language than English, and the distinction between transitive and intransitive verbs should be more strictly observed in Latin. Thus a planet 'is moved' round the sun, *movetur*; it does not move round the sun. This would invite the question 'What is it moving round the sun, a wheelbarrow?' In English there is no such problem, so the translator should have a planet moving round the sun rather than being moved. Much ink has been vainly spent in discussing this matter. The alternative view is that Newton is implying the existence of some agent that is causing the planet to move, as may have been the case with some other authors. I have no doubt that this is not Newton's intention. His knowledge and concern for Latin grammar would compel him to use the passive. He uses the same verbs in the same way when discussing purely theoretical situations. In discussing celestial mechanics, he sometimes uses the verb *pergere* for 'to move' as an active intransitive verb, removing any possible suggestion of a causing agent from the scene. When writing in English he uses the active.

There are other delicate issues, such as the use of the singular rather than the plural, the question of ambiguities that arise or are resolved in translating naively between the two languages, and so forth. Thus the reader is warned that my translation does not follow Newton's grammatical constructions as closely as do Cohen & Whitman; and that conversely nuances in their use of language may, on occasion, need to be interpreted in the context of the nuances of Latin. It is impossible to draw a satisfactory line between stylistic features of Newton's writing, that one would wish to preserve, and stylistic features of Latin, as opposed to English, that one would expect a translation to remove.

Serious issues arise with ambiguities in the Latin that need to be resolved. Latin is generally written without articles, definite or indefinite. Thus 'a dog bites a man', and 'the dog bites the man' could both be translated into Latin in the same way. Of course the ambiguity between 'a' and 'the' can be resolved in Latin in many ways; but it may be left to the reader to resolve the ambiguity from the context. Cohen & Whitman often leave out an article against normal English usage. I am more decisive; but there are risks in resolving ambiguities, illustrated dramatically by the wrong resolution in their translation of Corollary 3 to Proposition 4, Section 1, Book 2. Here a parabola is described in terms of its *vertex D*, *diametros*, and *latus rectum*. It is to be expected that these are, respectively, the vertex of the parabola, the principal axis, which passes through D, and the *latus rectum*, the chord that passes through the focus and that is orthogonal to the principal axis. However Newton uses all these terms in more general senses, so we need to be on our guard. Later in the proof of the corollary, Cohen & Whitman (1999) states that '... it must go forth from the same place D along the same straight line DP in order to describe a parabola... ' But, the logical structure of the argument requires 'the parabola', not 'a parabola'. But then DP would have to be the tangent to the parabola at D, so D would not be the vertex (in the modern sense) of the parabola, and the proof collapses. The problem is resolved by observing that Cohen & Whitman have made a second mistake. The word that they have translated as 'along' is *secundum*, which means 'following' in all the many meanings of this word. Their error is entirely natural, in that Newton frequently uses *secundum* in the sense of 'in the direction of' or 'along'; for example in the proof of Proposition 46, Section 10, Book 1. But here this is completely wrong, and what Newton means here is 'of the same length as'. What has happened is that Newton is representing speed in terms of distance. So the particle is to set off from D with a speed defined by the length of the straight line DP, but does not move in the direction of this line. Cohen & Whitman have been undone, not by a lack of sensitivity to Newton's Latin, but by two ambiguities in the text that they have resolved incorrectly. The corollary is important. Newton is measuring speed by the length of the *latus rectum* of the parabola that a body would execute if it moved horizontally at that speed off a cliff and into a vacuum. This second mistake is repeated three times in their translation of Section 2, Book 2, where they would have been saved from error by checking the length of the *latus rectum* in question. Again, in the same section, they translate *hyperbola conjugate* as 'conjugate hyperbola', where the mathematics implies that Newton meant 'the other branch of the same hyperbola'. This is a linguistic error on Newton's part, an error that he made twice. The first error was picked up by Pemberton, the editor of the third edition, but this second error was missed.

So far I have selected translations by Cohen & Whitman of rather obscure passages, and I have presented them out of context. To give the reader a more balanced view I now give an example of a translation of a very easy result; in fact of what one might regard as fifth-form mathematics. In Book 2, Section 2, Newton finally gets round to proving the formulae for the derivative of the product of two functions, and for the derivative of an arbitrary power of a function. He combines these in the unnecessarily complicated Lemma 2. To follow the text one must understand Newton's calculus notation. For example, in Leibniz' notation one would write $d(f(t)g(t))/dt = f(t)dg(t)/dt + g(t)df(t)/dt$. Newton, roughly speaking, illegitimately multiplies throughout by dt and obtains $d(f(t)g(t)) = f(t)dg(t) + g(t)df(t)$. So now one may think of $d(f(t))$ as the increment in $f(t)$ in an infinitesimal moment of time, and try to find an adequate formulation of this concept. We are not concerned here with the resulting difficulties. Newton calls $df(t)$ the 'moment' of $f(t)$, and the statements we shall come across can trivially be replaced by formally correct statements by taking ratios of moments. Returning now to Lemma 2, it has three corollaries. The first of these, as translated by Cohen and Whitman, is as follows.

Corollary 1 Hence in continually proportional quantities, if one term is given, the moments of the remaining terms will be as those terms multiplied by the number of intervals between them and the given term. Let A, B, C, D, E, and F be continually proportional; then, if the term C is given, the moments of the remaining terms will be to one another as $-2A$, $-B$, D, $2E$, and $3F$.

Those familiar with Cohen & Whitman (1999) (or the original) will know out that 'continually proportional' means 'in a geometric progression', that 'given' should usually be replaced by 'constant', and that 'as' means 'proportional to'. So we may take the nth function to be $Cf(t)^n$ for some differentiable function $f(t)$, where n takes any (presumably finite) set of consecutive integer values that includes 0, which corresponds to C. Then $d(Cf(t)^n)/dt = nCft)^{n-1}df(t)/dt = nCf(t)^n \times f(t)^{-1}df(t)/d(t)$, and the main result follows. The second statement is simply an example; but let us work through it in Newtonian notation. So $CE = D^2, C^2F = D^3$, and $BD = AE = C^2$. Now write a, b, d, e, and f for the moments of A, B, D, E, and F respectively. (The moment of C is zero.) So (by Lemma 2) $Ce = 2Dd$, $C^2f = 3D^2d$, $bD + Bd = aE + Ae = 0$, and one finds that $a = (-2A)(d/D)$, $b = (-B)(d/D)$, $d = D(d/D)$, $e = 2E(d/D)$, $f = 3F(d/D)$, as required.

We now leave Corollary 2 to the reader. It states, in Cohen and Whitman's translation:

> *Corollary 2* And if in four proportionals the two means are given, the moments of the extremes will be as those same extremes. The same is to be understood of the sides of any given rectangle.

This is one of the most elementary results in *The Principia*, stated without proof, as was the first corollary, as being obvious. We challenge the reader to decode it.

To some extent, such problems with the Cohen & Whitman translation may arise because they are more interested in other aspects of *The Principia*. Their main objections to the Motte–Cajori translation are given on pp. 33–37 (and so as part of Cohen's guide). Cohen gets quite excited by some of these errors. 'An error so gross that I find it difficult to believe it can be attributed to a scholar of Cajori's stature.' 'A final example is so ridiculous. . . ' The 'gross error' is a mistranslation of *sacris literis* in a brief discussion of the distinction between time and space as relative concepts (as in Scripture) and as absolute concepts (as in physics). (The unusual spelling of *literis* with a single 't' follows Newton.) The 'ridiculous error' is the development of Professor Machin (Gresham professor of astronomy) into two professors, Professor Machin and Professor Gresham. I have no difficulty in believing that a distinguished scholar has made such mistakes. In my experience they happen frequently. I too will have made mistakes, and all I ask is that readers who find mistakes should not be astonished. Perhaps the most 'ridiculous error' that I found in the Cohen and Whitman translation is in Book 2, where they have Newton hanging an iron bob on a wire, and letting it oscillate in mercury. Unfortunately, iron is less dense than mercury, and the bob would float. However, I am not astonished, having almost made the same blunder myself.

It is, of course, important to move towards a reliable translation. To this end I have compared my translation in detail with Cohen & Whitman (1999), and in less detail with various other translations, and I have mentioned, in my notes, every place where I have a substantive disagreement with Cohen and Whitman. This includes all clear errors in their translation that I have found, as well as places where there is room for more than one interpretation. In such situations I have recorded the views of other translators. If a reader observes a substantive disagreement between my translation and that of Cohen and Whitman that I have not pointed out, they should suspect that I am in error; I have at least made the error of failing to notice the disagreement. When I note a disagreement I give my reasons; if it is a simple misprint in their translation, I hope that I can be trusted to have exercised adequate care. I have generally ignored problems with their diagrams. These are based on the diagrams of Motte's translation, and reproduce almost all of his digressions from the original. These digressions appear to be errors, rather than deliberate emendations, and they have little effect, except perhaps, for the omission of the diagram for Proposition 36, Book 3; an omission that is corrected by Cohen and Whitman.

This detailed comparison with Cohen & Whitman (1999) has been of great value to me. I have made my comparisons from the earliest stages of the translation, and I have no doubt that, as well as saving me much time, they have also saved me from many errors. On the other hand, there are over 150 places where I challenge the correctness of their translation. That corresponds to one error in every three or four pages. Opportunities for error arise at every step, and in many forms, and while I hope to have done better, the ease with which I have found errors that I have had to correct in my own translation leads me to fear that I may have done worse.

It is clear that further translations should be made. We have modern translations into English by historians and now by a mathematician. Perhaps the next translation should be made by an astronomer. I hope that the later translators into English will compare their translations with Cohen & Whitman (1999) and mine, and will list errors that they find. It is to be hoped that, in this way, the number of errors will reduce with subsequent translations, and that the level of understanding will increase.

In addition to the translation of Cohen and Whitman, I have consulted the translations Motte (1729) and of du Châtelet (1756), which gain credit from their early dates, and of Schüller (1999). I have also consulted partial translations by Donahue in Densmore & Donahue (1995), by Whiteside (1974), and by Rossi in Brackenbridge (1995). (The latter includes a translation of the first three sections of Book 1, and of Halley's Ode, from the first edition, by Dr. Rossi, the wife of Professor Brackenridge, who records that 'Long hours were spent as I, the physicist, argued for what I thought was Newton's intent, and she, the Latinist, argued for what Newton's Latin actually said.') Having spent long hours having these arguments with myself I warm to this insight. I have not considered translations into languages that I can only read with a dictionary. I have had problems enough with the English of Cohen and Whitman. For example they translate *quam proxime* as 'very nearly', rather than 'to a good approximation'. I understand the statement that a mountain is very nearly three thousand feet high as meaning that it is slightly less; but their interpretation would be different. This simple divergence in English usage would have caused me many difficulties with their translation if I had not had access to the original.

I have made an effort always to see a little below the surface. I have checked almost all Newton's calculations, finding a few minor slips, and have tried to ensure that I understand all his arguments in adequate detail, considering not just the validity of his proofs, but what lies beneath them. When he approximates a real number by a rational, has he used continued fractions? Does his argument conceal some concept, such as potential energy? When he plots the course of a comet across the skies, what are the stars to which he refers? What are the towns to which he refers? This last consideration led to an unexpected discovery. All translations that I have seen inform us that Picard measured the distance from Sourdon, which lies a little South of Amiens, to Malvoisine; both in degrees of latitude and in French toises. But the town Newton refers to as Malvoisine is not Malvoisine, which is in completely the wrong place, but rather Touquin. Presumably Touquin changed its name at some date from Malvoisine to avoid having two towns with the same name.

I have also commented very briefly on the lives of the less famous people whom Newton mentions.

On the other hand I should acknowledge that this is nothing more than an annotated translation. I have not entered into any of the issues that arise in serious depth. Many great and central parts of *The Principia* consist of heroic attacks on problems that are hopelessly beyond the compass of Newton's mathematics. The central examples are his work on the three-body problem, which is principally an attempt to understand and predict the motion of the moon, and his work on hydrodynamics and aerodynamics, which is principally an analysis of and attack on Descartes' theory of vortices. Here Newton, necessarily, replaces proof by heuristics. The strengths and weaknesses of these heuristic arguments have been the subject of much expert analysis. It is not appropriate for me, as an algebraist, to enter into these discussions, or to assess their conclusions, and I do not claim any understanding of these arguments beyond what should suffice for an adequate translation. I have given a simple account of the main astronomical and tidal issues in Appendices C and D, and have added comments in my notes. These observations will, of course, be of no interest to the experts; and I hope that they do not lay bare too obviously my own lack of expertise.

Some defects in Newton's style, due mainly to the fashions of his time, are passed over without comment in the translation, and are worth a brief mention as follows.

In acknowledging the work of others Newton is sometimes careless, and sometimes (in view of his priority dispute with Leibniz and others) dishonourable. I have not attempted to rectify the situation in any consistent way. The development of calculus in the seventeenth century is a complex matter, involving a large number of mathematicians.

A less serious problem is his tendency to calculate physical quantities to a totally unrealistic accuracy. This practice has not entirely died out. Looking up the diameter of the earth in feet on the Web I found an answer to a fraction of an inch. There is sometimes a virtue in this vice, in that the superfluous accuracy allows the reader to deduce what arithmetical process gave rise to the given result. I do not inform the reader whenever such bogus precision occurs, leaving it to good sense. Similarly, I do not always comment when Newton makes a small correction that will be overshadowed by other errors.

I have followed Newton's calculations, but I have not considered the extent to which he fudges his data. Nowadays one expects to be able to analyse data to great precision, and to acquire the data with precision equipment. But when Newton is, for example, considering the motion of the moon, he knows that one of the problems that he faces is the imprecision of his theoretical calculations. So when these problems multiply, Newton cannot so much ask 'what does my theory predict?', but rather 'is my theory compatible with observation?' One might interpret this as asking 'Can my calculations be fudged to fit the data?' I regard the resulting problems as being beyond the scope of this translation.

For a further discussion of Newton's style, and the problems of translation, see Appendix F.

A careful analysis of *The Principia* requires one to be aware of how Newton uses the various results that he states and proves. I have therefore recorded, with each result, forward references to where the result is quoted. I hope that these references will help the reader, as they have helped me, to a better understanding.

Finally, I would draw attention to the appendices. Appendices A, C, D and H may be helpful to the reader in understanding the text. Appendix J consists of three illustrations that are intended to give a visual impression of the technology of Newton's time. St. Paul's cathedral, where Newton and Desaguliers carried out aerodynamic experiments as described in Book 3, was built by Christopher Wren after the fire of London, and Newton might have contributed to the complex mathematics involved. It was the greatest feat of engineering of its time, and cannot be adequately described in a few words. The other two illustrations relate to the larger refracting telescopes of the time. The first *reflecting* telescope, constructed by Newton, created a stir, and was very compact.

Acknowledgements

It was Carl Murray who persuaded me many years ago to take this translation seriously, and to develop it into a publishable work. He has produced the diagrams and star maps. The revised diagrams were produced by using the originals as templates, taking account of the text so that the figures or constructions were produced more precisely wherever possible. The star maps were produced using MATHEMATICA. Wolfram Neutsch has read the entire work, and has saved me from many painful errors. Julian Barbour, whose (2001) book gives a brilliant account of the scientific context of *The Principia*, spent a day explaining to me the basic issues, as seen by a physicist who is a leading expert on the foundations of physics. Nicholas Kollerstrom, whose (2000) book is essential reading for anyone with an interest in Newton's lunar theory, has given me some much needed advice on these technical issues. My sister Elisabeth Leedham-Green has been a constant source of information on anything from Byzantine ligatures to the Cambridge pyxides. My daughter Sarah Williams has advised me on Anglo-Saxon. I am also indebted to Joseph Bemelmans, Antoine Dethier, Underwood Dudley, Niccolò Guicciardini, Rob Iliffe, Michael Jewess, Scott Mandelbrote, the late Michael Nauenberg, Paolo Palmieri, Lawrence Paulson, Tim Penttila, Wilhelm Plesken, and Francis and Judith

Roads, for encouragement, advice, computational support, information, and corrections to the text. I also thank David Tranah for persuading the Syndics of the Cambridge University Press to agree to the publishing of this translation, and for his help and encouragement in bringing this about. I am grateful to the referees, whose variegated and not always flattering comments on a partial early draft have helped me to reform my ways. I was particularly impressed by the firm and contrary opinions expressed on the virtues and vices of the Cohen and Whitman translation. There does appear to be a divide between mathematicians and physicists on the one hand, and historians and philosophers on the other, as to how mathematics and physics should be regarded; and while many views are better than one, there is a need for better mutual understanding. I have been mildly rebuked, by the reader appointed to check a later draft, for showing an inclination towards the Whig attitude to history. As far as the history of mathematics is concerned I must plead guilty. These are interesting questions. Let us pursue them with mutual respect.

Notes for the Reader

There are two specific issues that we would draw to the reader's attention. The first concerns Newton's use of 'Q.E.D.', and 'Q.E.I.', and 'Q.E.F.', and 'Q.E.O.' The first three stand for *Quod Erat Demonstrandum*, or 'What was to be proved, and *Quod Erat Inveniendum*, or 'What was to be discovered', and *Quod Erat Faciendum*, or 'What was to be done' respectively. According to Cohen and Whitman,the last stands for *Quod Erat Ostensum*, or 'What was to be proved'; we believe it more probably that it is, *Quod Erat Optandum*, or 'As desired'.

Newton's propositions are either theorems to be proved or problems to be solved. Typically a theorem consists of a single result, which is repeated in some form at the end of the end of the proof, and is terminated with Q.E.D. Sometimes a theorem consists of more than one statement, and the proof of each statement may lead to a 'subsidiary' Q.E.D. And sometimes the proof does not conclude with the statement to be proved, and no Q.E.D. appears. A proposition that is a problem may end with a statement of the solution and be terminated with Q.E.I. or Q.E.F., followed by a proof that the solution is correct and terminated with Q.E.D. So these terms are not always terminators, though they often serve that purpose.

The second issue is the question of dates. In Newton's time the old Julian calendar was in general use in England, while the new Gregorian calendar was in use on the continent. Newton often specifies which he is using, but his default is to use the Julian calendar. When it seems appropriate I have commented on which is being used.

ON THIS WORK

OF MATHEMATICAL PHYSICS

the outstanding ornament of our age and race

BY THAT MOST DISTINGUISHED MAN

ISAAC NEWTON.

Lo yours is the rule of the sky, you have measured the weight of the stars,
Lo Jupiter, placed in the balance; you too have uncovered the laws
Ordained to be ever obeyed, the foundation of all of his works,
By God the father of all, when first he created the world.
The innermost secrets of heaven are yielded up to our sight,
Nor are those forces concealed that spin the outermost spheres.
The sun resides on its throne, compelling all planets towards it,
Nor will it suffer these stars to travel directly through space,
But each must bend in its path that is fixed in the heaven for ever,
Pulled in unceasing rotation about the centre of all.
Now must the terrible comet turn in a path that is known,
Now we shall not be astonished when these stars with their beards come again.
Here we may learn for what cause the silver-faced Phoebe on high
Advances with unequal steps, and why she will never obey
The laws that were set in their place by the mathematicians of old.
We learn why the nodes must regress, and the apses must always advance.
We learn too the strength of the force that wandering Cynthia wields,
Sea-weed appears on the strand as the ebbing tide recedes,
And shallow waters are formed that sailors have learnt to fear.
And then the tide returns, to strike the heights of the shore.
Questions that puzzled the minds of all the wise men of old,
That scholars still put to debate, in vain and clamorous strife,
Have found an answer at last, mathematics dispelling the dark.
Error need no more confuse, nor press on the faltering mind.
You have found the pathway that leads to the realm of the gods above,
By the sharpness of genius divine you ascend the steeps of high heaven.

Away with all earthly concerns, Oh mortals, now lift up your hearts;
For here you may truly discern a mind that is godlike and great;
That followed a long and arduous path, far from the common herd.
He who, in tablets of stone, condemned all murderous acts,
Adultery also, and theft, and the telling of lies under oath;
Or he who first around cities threw up strong walls for his tribe;
Or he who first gave the world the life-giving present of bread;
Or he who first from the vine pressed the soother of all our cares;
Or he who turned sounds into sight, making his paper from reeds,
And gave to mankind the skill of expressing in writing his speech;
These gave some solace small, but did not ennoble our fate.
They gave us comforts to weigh against grief in the balance of life.
But now to the feast of the gods we may come, and here may discuss
Laws that the worlds obey. Now before us we see
Secrets the blind earth hides, and the order of nature unchanged,
And things that the bygone ages of creation have kept concealed.

Sing with me of the man who brought such things to light,
Oh you who rejoice to drink the nectar prepared for the gods,
Praise NEWTON, the man who opened the chests of hidden truth,
Praise NEWTON, the darling of muses, the man whom pure Phoebus approached,
Filling the mind of a man with the higher spirit divine:
Nor has a mortal the right to come any nearer the gods.

EDM. HALLEY

Translator's Notes

'The outstanding ornament of our age and race' is the book, and not the man, though various translations are at best ambiguous on this point.

Translating poetry is no doubt a specialist task, and one for which the present translator is not fit. The poem has also been translated by Mary Ann Rossi in Brackenbridge (1995). The discrepancies between various translations may be laid at the door of poetic licence, though it is not so easy to discern whether some translations are translations into verse or prose. 'Lo Jupiter, placed in the balance' translates *Computus en Jovis*, literally, 'Lo, the calculation (singular) of Jupiter'. So either the planet is being calculated or the god is calculating. Cohen & Whitman (1999) appear to have decided on the god, rendering *Jovis* as 'Jove', while Rossi retains the ambiguity with 'the computations of Jupiter'. The gravitational field of Jupiter is the first topic (after the rules of scientific argument) considered in Book 3. 'Silver-faced Phoebe' and 'the wandering Cynthia' are both, of course, apotheoses of the moon. There is an issue with Phoebe refusing to obey old laws. *Hactenus . . . numerorum fraena recuset*. The last three words unambiguously means 'she rejects the restraints of the numbers', that is, does not obey the laws. The problem is with *hactenus*, which could mean, according to Lewis & Short (1879), either 'thus far' or 'till now'. The former might imply that the motion of the moon is still not understood, and the latter that it now is. The former interpretation is chosen by Rossi, but we, with Cohen & Whitman (1999), regard the context as demanding the latter. 'Sea-weed' translates *ulva*. Modern dictionaries give *ulva* to mean 'sedge', but Adam Littleton's Latin dictionary Littleton (1673), which Halley may have used, gives the meaning as: 'Reet, or weed of the sea, Sea-grass, or weeds growing in pools and standing water'. 'Reet' is a very rare and obsolete variation of 'reed', and 'weed of the sea' must be sea-weed. At about the time that Halley wrote his ode, *ulva* came into English as a botanical term for laver, a type of sea-weed. The word is translated as sea-weed in Brackenbridge (1995) and sand in Cohen & Whitman (1999). The question arises as to whether the shallows are mistrusted by sailors, as in our translation, and in Cohen & Whitman; or esteemed by sailors, as by Rossi. Though either would seem possible, the weight of evidence in Lewis & Short (1879) supports our interpretation. Judgement on the grounds of context we leave to sailors. The first two great benefactors of mankind that are mentioned are Moses and, perhaps, Aeneas. Whether Halley will have had in mind specific mythological heroes who gave us bread, wine, and writing, and whether this choice of five great benefactors was original, I do not know. One might have expected the giver of fire. 'Did not ennoble our fate' is a translation of *Humanam sortem minus extulit*. The crux here is the meaning of 'minus'. We take this, in the words of Lewis & Short (1879), to be softened negation; but Cohen & Whitman (1999) take it to mean 'less'; that is to say, less than did Newton, which sends them in a very different direction, causing difficulties avoided by Rossi in Brackenbridge (1995) where *minus* is essentially ignored. Phoebus (as opposed to Phoebe) is the sun, or here more plausibly Apollo.

The ode is written in the metre of The Aeneid, whose opening lines introduce Aeneas, the founder of Rome . . . *dum conderet urbem* . . . *atque altae moenia Romae*. The Aeneid was held in the highest esteem in Newton's time. Charles I had used the *sortes Virgilianae* (or looking at random excerpts from the Aeneid) to foretell the result of the battle of Naseby.

An attempt has been made to preserve this metre, though, as Latin verse is based on quantity rather than stress, whatever poetic value the original may have has certainly been lost in translation.

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Frontmatter
More Information

The Author’s Preface to the Reader

Since the ancients (according to Pappus*) greatly esteemed* mechanics[*] *in the investigation of natural science; and since more recent philosophers, having discarded the concepts of the essential nature*[†] *and occult properties of objects, have taken steps to subject the phenomena of nature to mathematical laws, it seemed fitting to develop* mathematics *in this work, in so far as it applies to* philosophy. *The ancients in fact divided* mechanics *into two disciplines; the* theoretical, *that proceeds by rigorous proofs, and the* practical. *All manual skills belong to the practical branch, and the term* ‘mechanics’ *has evolved from this meaning. But since craftsmen usually work with little accuracy, so all* mechanics *is distinguished from* geometry, *in that anything precise is described as* geometry, *and anything that is less precise as* mechanics. *But the errors do not lie in the craft but in the craftsman. He who works less accurately is the poorer craftsman; and he who could work most accurately would be the most perfect craftsman of all. For the construction of straight lines and circles, on which* geometry *is founded, belongs to* mechanics. Geometry *does not teach but assumes the construction of these lines. For it assumes that the beginner has learnt how to construct these lines accurately before he approaches the threshold of* geometry, *and then teaches how problems are solved using these constructions; the constructions of straight lines and circles are problems, but not geometrical problems. From* mechanics *it is assumed that a solution to these problems arises; in* geometry *the use of these solutions is taught. And* geometry *boasts that it can achieve so much from so few principles from a different discipline. So* geometry *is based on practical mechanics; and is nothing other than that part of* general mechanics *that proposes and describes the art of accurate measurement. While moreover the manual crafts depend chiefly on the movement of objects, so, in general,* geometry *is based on magnitude, as is* mechanics *on motion. In this sense,* theoretical mechanics *will be the science of motion arising from forces of any kind, and of the forces that are required to produce motion of any kind, accurately stated and proved. This part of* mechanics *was developed by the ancients as* five forces *of the manual crafts; and they hardly considered gravity (since it is not a manual force) except when weights are moved by these forces. But*

[*] Though *mechanica* comes from the Greek for a machine, the term ‘mechanic’ in English referred originally to manual work; hence Shakespeare’s ‘rude mechanicals’.

[†] The essential nature of an object was a central concept in Scholastic Physics .

being concerned not with craft but with philosophy, and writing about forces that are not manual but natural, we have considered mostly those things that concern gravity, lightness, elasticity, the resistance of fluids, and other such forces, whether attractive or repulsive. And so we offer this our book as the mathematical principles of philosophy. For all the difficulty of philosophy is seen to turn on the problem of investigating the forces of nature from the phenomena of motion, and then explaining the other phenomena in terms of these forces. And this is the object of the general propositions that we have set forth in books one and two. But in the third book we have set forth an example of this with an exposition of celestial mechanics. For in this book, from celestial phenomena, using propositions proved mathematically in the earlier books, the gravitational forces by which bodies are attracted to the sun and to the individual planets are calculated. Then, from these forces, again using mathematical propositions, the movements of the planets, comets, moon, and sea are deduced. Would that the other phenomena of nature[‡] *could be derived from the principles of mechanics by arguing in the same style. For many things persuade me to have some suspicion that all natural phenomena may depend on certain forces by which the particles of bodies, through causes not yet known, either pull together, and cohere in symmetrical shapes, or fly from each other and recede: these forces being unknown, philosophers have, until now, studied nature in vain. But I hope that the principles put forward here will shed some light, either on this way of philosophising, or on something nearer the truth.*

[‡] 'Would that the other phenomena . . .' In Densmore & Donahue (1995) 'Would that' is replaced by 'In just the same way'; this translates *Utinam*.

As to the publishing of this work, that most intelligent and in all matters of literature most learned man Edmond Halley *worked with energy; he not only proof-read the text and took care of the wood cuts, but it was through his agency that I set about writing it. Indeed, when I had given him a proof of the shape of the heavenly orbits he did not desist from asking me to communicate the result to the* Royal Society, *which then, by its exhortations and kind auspices, caused me to begin considering publishing these matters. But after I had considered the variations in the motions of the moon, I had also begun to consider other matters concerning the laws and measurement of gravity and of other forces, and the orbits of particles attracted by forces obeying any given laws, and the mutual motion of many bodies, and the motion of bodies in resisting media, and the forces, densities, and flows of media, and the orbits of comets, and so forth, and I considered that the publication should be deferred so that I could consider these other matters and publish them all together. As for results on the motion of the moon (imperfect as they are) I put them together in the corollaries to Proposition 66, Book 1, so that I would not have to set them forth individually in a more prolix way than the matter was worth, nor prove them separately, interrupting the flow of the other propositions. I have preferred to insert a number of results that*

I discovered late in somewhat unsuitable places, rather than changing the numbering and cross-referencing of the propositions. I earnestly beg that everything should be read with an open mind, and that defects in such difficult material should not give rise to blame, but rather should be investigated, and fruitfully expanded, by new efforts of readers.

Signed in *Cambridge*, at the College of
The Holy Trinity, May 8, 1686.
IS. NEWTON.

The Author's Preface to the Second Edition

In this second edition of The Principia *many errors, scattered through the text, have been corrected, and a number of additions has been made. In Section 2 of the first book the calculation of the forces by which particles may revolve in given orbits has been simplified and extended. In Section 7 of the second book the theory of the resistance of fluids has been investigated more precisely, and confirmed by new experiments. In the third book the theory of the moon, and the precession of the equinoxes, have been deduced more fully from the relevant principles, and the theory of comets has been confirmed by computing more of their orbits, and to a greater accuracy.*

Signed in *London*,
Mar. 28, 1713.

IS. NEWTON.

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Frontmatter
More Information

The Editor's Preface to the Second Edition

We present to you, kind reader, a new and long desired edition of Newton's philosophy, with many additions and corrections. The main contents of this most celebrated of works you may gather from the adjoined indexes: what has been added or changed from the author's preface. It remains to add a few words concerning the methodology of this philosophy.

Those that undertake the study of Physics may be divided into three classes. There are those who seek to endow various kinds of things with corresponding occult properties, on which they would then have the operations of these bodies to depend, for unknown reasons. Such is all the scholastic teaching, derived from *Aristotle* and the Peripatetics. They assert that specific effects arise from specific properties of bodies; but since they do not say whence these properties come, they teach nothing. And since they are solely concerned with the names of things, and not with the things themselves, they should be considered to have invented a certain philosophical language, and should not be considered to have taught philosophy.

So others have hoped to attract praise for being more diligent by rejecting this useless hodge-podge of words. Thus they assert that all matter is of the same kind, and that all types of variation that are perceived in bodies arise from very simple and easily understood aspects of the component particles. And they are right to progress from the simpler to the more complex, provided that they do not endow these fundamental aspects of particles with more than nature herself endowed. But when they take it upon themselves to ascribe whatever unknown shapes and sizes they wish to the parts, with unknown structures and motions, and indeed to hypothesise certain occult fluids that permeate the pores of bodies very freely, endowed with an overwhelming subtlety, and stirred by occult forces, they are now floating off into dreamland, and have lost contact with reality; a reality that will be sought in vain with false conjectures, and that can scarcely be investigated by even the most certain observations. It must be said of those who base their speculations on hypotheses, even if they then proceed very precisely according to mechanical laws, that they have constructed a fable, which, however elegant and beautiful, remains a fable.

There remains a third class that acknowledges that philosophy is an experimental subject. They too wish the causes of all things to be derived from the simplest possible axioms; but they do not allow anything the status of an axiom except in so far as it has been checked by experiment. Hypotheses are not devised, or accepted into physics, unless their truth is investigated. This can be done in two ways; the analytic and the synthetic. The forces of nature, and the simpler laws that they obey, are deduced analytically from certain selected phenomena, and then the remaining laws are deduced from these simpler ones by synthesis. This is by far the best way of philosophising, which, in the opinion of our most celebrated author, should deservedly be favoured above others. He considered this to be the only method worthy of being developed and embellished in the organisation of his work. He gave a very famous example of this methodology by developing celestial mechanics, in the most elegant way, from the theory of gravity. That gravity was an innate property of all bodies was suspected or hypothesised by others: but he was the first and only person who could prove this, from observable phenomena, and, by extraordinary intellectual effort, place gravity on the firmest foundations.

Now I know of a number of men of great fame who are unduly swayed by certain prejudices, and are unwilling to accept this new principle, preferring the uncertain to the certain. It is not my intention to impune their reputation; I prefer to set forth these few words to you, kind reader, so that you yourself may pass a proper judgement.

So in order to take up the exposition of the argument from the simplest and nearest points, let us discuss briefly the nature of gravity on earth, so that we may then progress more safely when we come to the heavenly bodies, far removed from where we dwell. It is now agreed by all philosophers that all bodies at the surface of the earth gravitate downwards. That there are no bodies that truly levitate has for a long time been confirmed by many experiments. What is called relative levity is not true but only apparent levity, and arises from the greater gravitational forces on the contiguous bodies.

Now as all bodies gravitate to the earth, so conversely does the earth gravitate equally to the bodies; and that gravity acts as an equal and opposite force is shown as follows. Let the earth be divided in any way into two masses, equal or unequal. Now if the gravitational forces acting on the two parts were not equal, the lesser force would give way to the greater, and the two parts together would move off to infinity in the direction of the greater force, entirely contrary to experience. So it will have to be admitted that gravitational forces acting on the two parts are in equilibrium; that is to say, are equal and opposite.*

* This argument is based on the scholium that concludes *The Laws of Motion*.

The gravitational forces acting on bodies, at the same distance from the centre of the earth, are proportional to the quantity of material in the bodies. This can indeed be deduced from the equal acceleration of all bodies falling from rest under the force of gravity. For the forces by which unequal bodies accelerate equally must be proportional to the quantities of material to be moved. Now it is clear that all bodies do in fact accelerate equally, because in a *Boyle* vacuum they describe equal distances in equal times as they fall [from rest], once the effect of air resistance has been allowed for. But the law may be tested more accurately by experiments with pendulums.†

† The period of a pendulum does not depend on the material from which the bob is made. See Cor. 7 to Prop. 24, §6, Bk 2.

The gravitational fields of bodies, at equal distances, are proportional to the quantities of material in the bodies. For since bodies gravitate to the earth, and the earth conversely gravitates towards the bodies, with equal force, it follows that the gravitational attraction of the earth towards any body, that is, the force by which the body attracts the earth, will be equal to the gravitational attraction of the same body towards the earth. Now this gravitational force was proportional to the quantity of material in the body, so the force by which any body attracts the earth, that is, the absolute force of the body, will be proportional to the same quantity of material.

Hence the gravitational attraction of the whole body arises from and is composed of the attractive forces of the parts. If the mass of material is increased or reduced in some way, it is clear that its gravitational force will increase or decrease in the same proportion. And so the action of the earth will be seen to arise from the combined action of the parts, and so all terrestrial bodies must attract each other with absolute forces that are proportional to the amount of material. This is the nature of gravity on the earth. Let us now examine its nature in the heavens.

Every particle remains in its state of rest or of uniform motion in a straight line, except when it is forced to change its state by forces acting on it.‡ This law of nature is accepted by all philosophers. Now it follows from this that all bodies that move in curves, and hence move off continually from the straight lines that are tangents to their orbits, are kept in their curved paths by some continually acting force. So with the planets, as they revolve in their curved orbits, there must be some force by whose repeated actions they are continually deflected from the tangents.

‡ Newton's first law is quoted exactly, with one trivial change, from the second edition's version.

Now it must be equally agreed that the following result has been deduced by mathematical arguments, and most rigorously proved. Every particle that moves in any curved planar path, and that, with a radius drawn to some point that is either stationary or moving in any way, describes areas about that point that are proportional to the times, is acted on by a centripetal force acting towards that point.

> This is Newton's Proposition 2, Section 2, Book 1; but with the centre of attraction moving in any way, rather than moving with a uniform linear velocity. Cotes wishes to apply this law to the satellites of the planets, where the centre in question does not move uniformly. The result holds in this greater generality, as Newton proves in Proposition 3 of the above section, provided that the forces that act on the centre act equally (when measured by the acceleration they induce) on the particle.

Since therefore it is agreed amongst astronomers that the primary planets as they revolve around the sun, and the secondary planets as they revolve around their primaries, describe areas proportional to the times, it follows that the force by which they are continually deflected from the tangents and are forced to revolve in curved orbits, is directed towards the bodies that lie at the centres of the orbits. And so it is not inappropriate to call the force as it acts on the revolving body 'centripetal', and as it acts on the central body 'attractive', whatever the cause that may be supposed to give rise to it.

And indeed the following results must also be accepted, and are proved mathematically. If many bodies revolve in concentric circles at constant speeds, and the squares of the periods are proportional to the cubes of the distances from the common centre, the centripetal forces will be inversely proportional to the squares of the distances.[§] Or if bodies revolve in orbits that are almost circular, and the apsides of the orbits do not rotate, the centripetal forces of the revolutions will be inversely proportional to the squares of the distances. It is agreed by astronomers that both these conditions are satisfied by the planets. And so the centripetal forces of all the planets are inversely proportional to the squares of their distances from the centres of the orbits. If anyone should object that the apsides of the planets, and particularly of the moon, are not completely at rest, but that they advance with a slow motion, one could reply that, even if we were to concede that this very slow motion does arise from a small deviation of the centripetal force from the inverse square law, then this deviation could may be computed mathematically, and would be completely undetectable. For this exponent for the centripetal force of the moon, which is the case when the disturbance is greatest, will be slightly more than two, and will be almost sixty times closer to two than to three. But it would be a truer answer if we were to assert that this advance of the apsides did not arise from a deviation from the inverse square law, but arose directly from a different cause as is demonstrated very well using this theory. It follows then that the centripetal forces by which the primary planets tend towards the sun, and the secondary planets tend towards their primaries, are precisely proportional to the inverse squares of the distances.

[§] This is Cor. 7 to Prop. 4, §2, Bk 1, reworded.

From what has been said so far, it follows that the planets are kept in their orbits by some force that acts on them continuously: it follows that the force is always directed towards the centres of the orbits: it follows that this force increases as one approaches the centre, and decreases as one moves away from it: and that it is increased as the square of the distance is reduced, and is reduced as the square of the distance is increased. Now let us see, by comparing the centripetal forces of the planets with the force of gravity [on the surface of the earth] whether they are of the same kind. They will in fact be of the same kind if they obey the same laws and have the same effects here and there. So let us first examine the centripetal force of the moon, which is nearest to us.

The distances that bodies describe, when they are dropped from rest, in a given period of time, when they are acted on by various forces, are proportional to those forces: this follows from mathematical arguments. Therefore the ratio of the centripetal force of the moon, revolving in its orbit, to the force of gravity at the surface of the earth, will be the ratio of the distance that the moon would fall to the earth in a very short period of time, if it were assumed to be deprived of all circular motion, to the distance through which a heavy body would fall in the same very short period of time when falling near the earth under its own weight. The first of these distances is equal to the versed sine of the arc described by the moon in the same period of time; that is, the distance that the moon would fall away from the tangent, caused by the centripetal force; and thus it may be calculated, given the moon's period and its distance from the centre of the earth. The second distance may be computed with experiments with pendulums, as *Huygens* taught. When this calculation has been carried out, the ratio of the former distance to the latter; that is, of the centripetal force of the moon revolving in its orbit to the force of gravity at the surface of the earth; will be found to be the ratio of the square of the radius of the earth to the square of the radius of the moon's orbit. It follows, from what we have shown above, that this is equal to the ratio of the centripetal force of the moon, as it revolves in its orbit, to the centripetal force of the moon near the surface of the earth. So the centripetal force near the surface of the earth is equal to the force of gravity. So they are not different forces, but are one and the same force. For if they were different forces bodies would fall to the earth under the combined forces twice as fast as if they were solely under the influence of gravity. So the centripetal force by which the moon is continuously pulled or pushed from its tangent, and is kept in its orbit, is the same as the terrestrial force of gravity, extending as far as the moon. And it is consonant with reason to suppose that this force extends for immense distances, as there is no measurable reduction in the force; not even at the summits of the highest mountains. So the moon is drawn by gravity to the earth, and indeed by a mutual action the earth conversely is equally attracted to the moon, as is abundantly clear when we use this theory to consider the tides of the sea and the precession of the equinox, which are caused by the action of the moon and of the sun on the earth. And by this we finally learn with certainty the law by which the force of gravity decreases as the distance from the earth increases. For since the force of gravity is no different from the centripetal force of the moon, and since this is inversely proportional to the square of the distance, it follows that the force of gravity reduces in the same ratio.

Now let us move on to the remaining planets. Since the revolutions of the primary planets around the sun, and of the secondary planets about

Jupiter and Saturn, are phenomena of the same type as the revolution of the moon around the earth, and since it has been shown that the centripetal forces of the primary planets are directed towards the centre of the sun, and of the secondary planets towards the centres of Jupiter and of Saturn, in the same way that the centripetal force of the moon is directed towards the centre of the earth; and also since all these forces are inversely proportional to the distances from the centres, in the same way that the force of the moon is [inversely] proportional to the square of the distance from the earth; it will be concluded that this is a universal phenomenon. Thus, as the moon gravitates towards the earth, and the earth conversely gravitates towards the moon, so all secondary planets gravitate towards their primaries, and the primaries conversely gravitate towards the secondaries; and thus all primaries gravitate towards the sun, and the sun conversely towards them.

Therefore the sun gravitates towards all planets, and all they towards the sun. For the secondary planets are borne round with their primaries, and revolve about the sun with them. And by the same argument planets of either kind gravitate towards the sun, and the sun towards them. That the secondary planets do in fact gravitate towards the sun follows very clearly from the lunar variations; of which a very accurate theory, made clear with an admirable insight, is expounded in the third volume of this work.

One may learn very explicitly that the attractive influence of the sun is propagated in all directions and for vast distances, and that it extends to all parts of the surrounding space, from the motion of comets. Having set forth through immense intervals of space, they are carried into the neighbourhood of the sun, and sometimes approach so close to its sphere, when they are in perihelion, that they seem almost to touch it. Astronomers in the past sought in vain a theory to explain this; but such a theory has happily been found in our age, and proved beyond doubt by observations, thanks to our most illustrious author. So it is clear that the comets move in conic sections with a focus in the centre of the sun, and that, with a radius drawn to the sun, they sweep out areas proportional to the times. From these phenomena it is clear, and proved mathematically, that those forces by which the comets are restrained in their orbits act towards the sun, and are inversely proportional to the squares of the distances from its centre. And so the comets gravitate towards the sun; and the attractive force of the sun extends not just to the bodies of the planets, at their fixed distances, lying approximately in the same plane, but also to the comets placed in the most diverse regions of the heavens, and at the most diverse distances. It is therefore the nature of gravitating bodies that they extend their forces at all distances to all gravitating bodies. From this indeed it follows that all the planets and comets attract each other, and gravitate towards each other, as is confirmed by the perturbation of

Jupiter and Saturn, a perturbation that is not unknown to astronomers, and which arises from the actions of these planets on each other, and is indeed confirmed by the very slow motion of the apsides, as mentioned above, and which arises from a similar cause.

Thus we come at last to the point where we may assert that the earth, the sun, and all the celestial bodies that accompany the sun, attract each other. So every very small part of each of these bodies has its attractive force, with a strength proportional to the quantity of material; as was shown above of terrestrial matter. Moreover at diverse distances these forces will be inversely proportional to the squares of the distances. For from this law of attraction for particles, the same law of attraction follows for globes, as is proved mathematically.¶

¶ This is Prop. 74, §12, Bk 1.

The above conclusions are based on an Axiom that no philosopher rejects, namely that effects of the same kind – that is, whose known properties are the same – arise from the same causes, and their as yet unknown properties are the same. For who would doubt that if gravity is the cause for a stone to fall in *Europe* then it should be the cause of its fall in *America*? If the gravitational force between a stone and the earth is mutual in *Europe*, who would deny that it is mutual in *America*? If in *Europe* the attractive force between a stone and the earth is composed of the attractive forces of the parts, who will deny that it is similarly composed in *America*? If the attraction of the earth is propagated to all kinds of bodies at all distances in *Europe*, why should we not say that it is propagated in the same way in *America*? All philosophy is based on this rule, and if it is taken away we can make no universal assertions. The form of individual objects is discovered by observations and experiments; but without this rule we can make no judgements concerning universal objects.‖

‖ This is Rule 2 of *The Rules of Scientific Argument*, Bk 3, re-written and expanded, with the same examples, also expanded.

Now since all bodies, on earth or in heaven, on which experiments may be carried out or observations made, have gravity, it must be said in general that gravity is a property of all bodies. And just as no bodies may be conceived that do not have extent, mobility, and impenetrability, one should not conceive of bodies that lack gravity. The extent, mobility, and impenetrability of bodies cannot be made known without experiment, and gravity is made known in the same way. All the bodies of which we have observations have extent, and mobility, and impenetrability; and hence we conclude that all bodies, including those of which we have no observations, have extent, and mobility, and impenetrability. Similarly all bodies of which we have observations have gravity, and hence we conclude that all bodies, including those of which we have no observations, have gravity. If someone states that the bodies of the fixed stars do not have gravity, on the grounds that their gravity has not yet been observed, then by the same argument one could say that they do not have extent, or mobility, or impenetrability, since these properties of the fixed stars have not yet been observed. What is the use of words? Either gravity is one of the primary properties of all bodies, or extent, mobility, and impenetrability are not. And either the nature of things may be correctly explained by using the gravity of bodies, or it cannot be correctly explained using the extent, mobility, and impenetrability of bodies.[††]

[††] The properties of extent, mobility, and impenetrability are discussed in this context at the beginning of Bk 3, and again in its final scholium.

I hear some people rejecting this conclusion, and muttering I know not what about occult properties. Indeed they prattle perpetually about gravity being an occult property; and that occult causes should be far removed from philosophy. There is an easy answer. Occult causes are not those whose existence has been very clearly proved by observations, but only those whose existence is occult, imagined, and not yet proved. So gravity will not be an occult cause of the celestial motions, provided that it can be demonstrated from phenomena that this property truly exists. These people prefer to take refuge in occult causes; I know not what; such as vortices; and consider that various types of matter that have been imagined, and are completely unknown to the senses, are the causes that control these motions.

So will gravity be called an occult cause, and for that reason be rejected from philosophy, because the cause of gravity is occult and has not yet been discovered? Those who assert this should see that they do not make absurd assertions by which the foundations of the whole of philosophy would be undermined. For causes in general may be traced in a continual chain, from the more complex to the simpler: but when you arrive at the simplest you can go no further. So it is impossible to give a mechanical explanation of the simplest causes: if such were given these causes would no longer be the simplest. You wish to call these simplest causes occult, and to exclude them? At the same time you will also exclude those most nearly dependent on them, and also those that depend on these, until all causes have been removed, and philosophy is duly purged.

Some say that gravity is against nature, and call it a constant miracle. And so they wish to reject it, because supernatural causes have no place in physics. It is hardly worth while spending time to refute this absurd objection, which would undermine all philosophy. For they will either deny that gravity is a property of all bodies, which I have already asserted to be impossible; or they will assert that it is against nature because it does not have its origin in aspects of other bodies; that is, in mechanical causes. There are indeed primary effects of bodies, which, being fundamental, do not depend on others. Let them see therefore whether all these are equally supernatural, and so, equally, should be rejected, and then see what kind of philosophy will result.

There are some who dislike this celestial mechanics because it seems to be in opposition to the dogmas of *Descartes*, and can be scarcely reconciled with them. These should be allowed to enjoy their opinion; but they must behave equitably, and not deny to others the freedom they would demand for themselves. And so we are entitled to hold and embellish the NEWTONIAN philosophy, which we consider to contain the greater truth, and to follow the causes that have been established from phenomena, rather than those that have been invented, and are not yet established. It is the duty of true philosophy to deduce the nature of things from true, existing causes; and indeed to seek out those laws by which, by his will, the great architect established this most beautiful order of the universe, and not those by which he could have established it had it seemed to him good. For it is consonant with reason that the same result could follow from many different causes; but the true cause will be that from which the result, truly and actually, does follow; the other possible causes have no place in true philosophy. In a mechanical clock, the same motion of the hour hand could arise from a hanging weight or from an internal spring. So, when a clock is brought forth that is in fact driven by a weight, anyone who supposes that there is a spring, and undertakes to explain the motion of the hand from this over-hasty supposition, will be laughed at. It was necessary to examine the internal structure of the machine more carefully to obtain in this way a true and certain basis for the displayed motion. The same judgement, or one that is not dissimilar, should be passed on those philosophers who would have us believe that the heavens are filled with some very subtle material, and that this moves endlessly in vortices. For if phenomena can be explained, even very precisely, from their hypotheses they will not be said to have been true philosophers, and to have found the true causes of the heavenly motions, until they have proved either that these things exist, or at least that there are no other [possible] causes. So if it has been shown that the universal attraction of bodies has a true place in the nature of things; and if it has also been shown how the motion of all celestial bodies follows from this theory; then it would be a vain objection, and worthy of ridicule, if anyone were to say that the same motions could be explained by vortices, even if we were to concede that this were possible. But we do not concede that this is possible; for the phenomena cannot be explained in any way by vortices, as our author has proved by many very clear arguments. So they that devote their useless labour to patching up their most inept imaginings, and adorning them with new commentaries, are indulging their dreams beyond measure.

If the bodies of the planets and comets are carried round the sun by vortices then the bodies that are carried and the nearest parts of the vortices will have to have the same speed and orbit[‡‡], and the same density, or the same force of inertia per unit mass. It is agreed that planets and comets, when they are in the same regions of the heavens, move with differing speeds and orbits. So it would necessarily follow that those parts of the celestial fluid at the same distance from the sun at the same time move in differing directions with differing speeds: so there will be one direction and speed for the passage of the planets, and another for the passage of

‡‡ Here and below 'orbit(s)' translates *determinatione cursus*, or 'boundary of the flow'.

the comets. Since this cannot be explained, it will have to be admitted that not every heavenly body is carried round by the material of the vortex, or it must be said that their motions are carried round not by one and the same vortex, but by many that are different from each other, and that pervade the same space around the sun.[§§]

[§§] The fact that the vortices would have to have the same density as the planets they carried round follows from the fact that the planets follow closed orbits with respect to the sun. This is Prop. 53, §9, Bk 2.

If many vortices are supposed to be contained within the same space, and to penetrate each other, and to revolve with differing motions, then since these motions must agree with the motions of the bodies they carry along, and which obey very precise laws, moving in conic sections; in one case with large eccentricities and in the other approximating a circular shape; it may rightly be asked how it is possible that these orbits are precisely preserved, and are not in any way perturbed, in so many centuries, by the action of the material they come up against. Indeed, if these fictitious motions are more complex and hard to describe than the actual motions of the planets and comets, then it seems to me that they are accepted into philosophy in vain; for every cause should be simpler than its effect. If one is permitted to invent things, let someone assert that all the planets and the comets are surrounded by atmospheres, as in the example of our earth, which seems to be a hypothesis that is more consonant with reason than the hypothesis of vortices. Let him then assert that these atmospheres, by virtue of their nature, move round the sun and describe conic sections; which indeed is a motion that is much easier to imagine than the corresponding motion of vortices that penetrate each other. Then let him assert that one should believe that these planets and comets are borne around the sun by their atmospheres, and let him triumph in having found the causes of the celestial motions. But anyone who might consider that this invention should be rejected will also reject the other invention; for one egg is not more like another than is the hypothesis of atmospheres to the hypothesis of vortices.

Galileo taught that the deflection from a straight path of a stone that had been thrown and moved in a parabola arose from the weight of the stone towards the earth, caused by some occult property. Now it is possible that another philosopher, with a sharper nose, might have ascribed a different cause. So let him invent a subtle material, that cannot be seen, or touched, or detected by any sense, and that exists in the regions nearest to the surface of the earth. Now this material, in diverse places, is carried by differing and frequently contrary motions, and strives to describe parabolic arcs. Thereupon, he will arrange the deflection of the stone beautifully, and the applause of the crowd will be deserved. The stone, he will say, swims in this subtle fluid, and following its course, cannot but follow the same path. But the fluid moves in parabolic arcs; therefore the stone, of necessity, moves in a parabola. Now who will fail to be astonished at the sharpness of the mind of this philosopher, who, by mechanical causes; that is, by matter and motion; has deduced the phenomena of nature with great clarity, so that even the common crowd can understand? Who indeed will not laugh at good old *Galileo*, who reintroduced occult properties, happily excluded from philosophy, with a great heap of mathematics? But I am ashamed to dwell any longer on this nonsense.

In summary, things stand as follows: the number of comets is vast; their motions are highly regulated, and they follow the same laws as the motions of the planets. They move in orbits that are conics of high eccentricity. They are carried away into all parts of the heavens, and pass very freely through the regions of the planets, and often move against the order of the signs. These phenomena are most certainly confirmed by astronomical observations, and cannot be explained by vortices. On the contrary, they are inconsistent with planetary vortices. No place may be found for the motion of comets unless this fictitious matter is completely removed from the heavens.

Now if the planets are carried round the sun in vortices, then those parts of the vortices that most closely encircle any given planet will be of the same density as the planet, as has been mentioned above. So all the material that is contiguous to the earth's orbit will have the same density as the earth, while the material that lies between the orbit of the earth and the orbit of Saturn will have an equal or greater density. For in order that the structure of the vortex remain stable, the less dense parts should occupy the centre, and the denser parts should go out further from the centre. Now since the periods of the planets are proportional to their distances from the sun, raised to the power $3/2$, the periods of the parts of the vortex will have to obey the same law. From this it follows that the centrifugal forces of these parts [per unit mass] are inversely proportional to the squares of the distances. Those parts therefore that are at a greater distance from the centre strive to recede from the centre with a lesser force; so if they were less dense they would give way to the greater force by which the central parts strive to rise. So the denser parts would rise, and the less dense parts would sink, and they would interchange places, and so the fluid material of the whole vortex would have been ordered and arranged in this way in order that it could now rest in equilibrium. If two fluids, of different densities, are contained in the same vessel, it will certainly happen that the denser fluid, under the stronger force of gravity, will seek the lower place; and by a not entirely dissimilar argument it must be asserted that the denser parts of the vortex will seek a higher place, on account of the greater centrifugal force. So the whole part of the vortex that lies beyond the earth, which is by far the greater part, will have a density, and force of inertia per unit mass, that are not less than the density and force of inertia [per unit mass] of the earth. From this there will rise an enormous resistance to the passage of comets, which would be very easy to detect, not to say that it would rightly seem capable of absorbing their motion, and bringing them to a complete halt. But it follows from the fact that the motion of comets obeys very precise laws that they suffer no resistance that can be detected at all. And so they do not come into contact with any material with any force of resistance and hence with any density or force of inertia. For the resistance of media arises from the inertia of the fluid material, or from viscosity. That which arises from viscosity is exceedingly small; and indeed it can scarcely be observed with commonly known fluids, unless they are very sticky, like oil and honey. The resistance that is felt in air, water, quicksilver, and fluids of this kind that are not sticky, is almost all of the first kind, and cannot be reduced by any amount of refining, provided that the density or force of inertia of the fluid is held constant, to which this resistance is always proportional, as has been proved very clearly by our author in his excellent theory of resistance, which is now expounded a little more accurately in this second edition, and confirmed more fully through experiments with falling bodies.

Bodies, by advancing, gradually impart their momentum to the surrounding fluid, and by imparting lose it, and by losing it they are slowed down. And so the slowing down is proportional to the momentum imparted, and the momentum imparted, when the speed of the advancing body is fixed, is proportional to the density of the fluid; so the slowing down, or resistance, is proportional to this density of the fluid; nor can this slowing down be reversed in any way unless the lost momentum is restored by the fluid pushing forwards on the rear parts of the body. But this may not be said to take place, unless the force exerted by the fluid on the body in the rear parts is equal to the force exerted by the body on the fluid in the front parts; that is, unless the relative speed with which the fluid strikes the body from behind is equal to the speed with which the body strikes the fluid; that is, unless the absolute speed of the fluid pushing forwards is twice the absolute speed of the fluid that is being pushed, which cannot be. So the resistance of fluids that arises from their density and force of inertia cannot be removed in any way. So the conclusion will be as follows: the celestial fluid has no force of inertia as it has no force of resisting; there is no force by which momentum could be communicated because there is no force of inertia; there is no force that could induce any change in bodies, either individual or multiple, since there is no force that transfers momentum; there is no effect whatsoever, since there is no means of causing change of any kind. So why should one not be able to say that this hypothesis, which is clearly without foundation, and which is not the slightest use for explaining anything about the nature of things, is completely absurd, and totally unworthy of a philosopher? Those who would have the heavens filled with a fluid material, but assert that it has no inertia, remove a vacuum with words, and replace it with matter. For since a fluid material of this kind cannot be distinguished in any way from empty space, the whole argument is about the names of things, not about their natures. But if anyone is so insistent on matter that they do not wish to believe that any space void of body should be allowed, let us see where this leads.

For such people will either assert that the nature of the universe to be everywhere full, an idea they have invented, has originated through the will of God, to the end that an omnipresent aid to the operations of nature could be obtained from a very subtle ether, permeating and filling everything; which cannot be maintained since it has already been shown, by the phenomenon of comets, that this ether would be of no effect; or they will say that it originated through the will of God for some unknown end; which also should not be maintained, because a different nature of the universe could equally well be established by the same argument; or, finally, they will not assert that it originated from the will of God, but rather from some necessity of nature. So finally they will have descended into the filthy dregs of a dirty flock. These are they that imagine that the universe is ruled by fate, not by providence; that matter, from its own necessity, exists always and everywhere, without bounds, and eternal. With these assumptions everything everywhere will be the same; for a variety of form is incompatible with necessity. Nothing will move; for if motion, of necessity, takes place in some predetermined direction, with some predetermined speed, then, by the same necessity, the motion will take place in another direction, with another speed; but it is impossible to move in different directions with different speeds; so there can be no motion. For surely the universe, with its most beautiful forms and diversity of motion, could not have arisen, except by the most free and total will of God, the provider and ruler.

From this source therefore everything that is called a law of nature proceeds; in which laws very many signs of a most wise council appear, but no sign of necessity. We need to seek these laws, not from uncertain conjectures, but by proving them through observation and experiment. He that trusts that he can find the true physical principles and laws of things depending purely on force of mind, and the light of internal reason, will have to assert that either the universe exists of necessity, and that the proposed laws arise from the same necessity, or, if the order of nature was instituted by the will of God, that he, a poor mortal, has the insight to understand how things should best be. All good and true philosophy is founded on phenomena: and if these phenomena lead us, perhaps unwillingly and reluctantly, to principles of this kind, in which one may discern very clearly the most excellent council and high dominion of an all-wise and powerful Being; then those principles will not be rejected on the grounds that they might be displeasing to certain persons. These people call the laws that displease them either miracles or occult qualities. But names given maliciously should not be turned into a fault of the things themselves, unless these people wish to go so far as to say that philosophy should be founded on atheism. Philosophy will not be overthrown by these people, provided that the order of things refuses to change.

So honest and unbiased judges will approve the best type of philosophy, which is based on experiments and observations. One can scarcely express in words the amount of light that has been shone on this method, or the authority with which it has been endowed, by this famous book of our most illustrious author. His great genius in solving any very difficult problem, and stretching out to those up to which it had not been hoped that the human spirit could soar, has rightly been admired and is respected by all who have some expertise in these matters. So the door being opened, we are shown the entry to the most beautiful secrets of nature. The most elegant structure of celestial mechanics is laid bare and presented for a deeper understanding, that even King *Alfonso*, if he were to come to life again, could hardly wish it to be simpler or more harmonious. And so the majesty of nature may now be looked at more closely, and enjoyed in the sweetest contemplation, and the creator and lord of the universe may be more deeply worshipped and adored, which is much the best fruit of philosophy. For one would have to be blind not to see at once the infinite wisdom and goodness of an omnipotent creator in the most excellent and wise structures of nature; and be mad not to acknowledge them.

Alfonso X of Castile (1221–1284) was known as 'The Wise' for his interest in astronomy and other intellectual matters. A famous quotation, attributed to him, upon hearing an explanation of the extremely complicated mathematics required to demonstrate Ptolemy's theory of astronomy, was 'If the Lord Almighty had consulted me before embarking on creation thus, I should have recommended something simpler.'

And so this superb work of Newton will rise up as a most well-defended stronghold against the attacks of atheists: nor could anything be happier than to draw a dart against that impious band from this quiver. This was understood long ago and set forth in very learned discourses in English and Latin, and first most excellently set forth by that man, famous in all forms of literature, and excellent patron of good arts, Richard Bentley, a great ornament to this his age, and to our academy, the most worthy and virtuous master of our college of *The Holy Trinity*. I must admit my debt to him in various ways; and you, kind reader, will not deny him your thanks. For he it was who, after enjoying a long friendship with our most famous author (for which he expects to be held in as high regard as for his own writings, which delight the world of letters) he took care of the fame of his friend, and at the same time the development of the sciences. And so, since copies of the first edition were very rare and expensive, he persuaded, with frequent requests, which almost amounted to harassment, that most distinguished of men, no less famous for his modesty as for his vast intellect, to allow a new edition of this work, with all blemishes removed, and enriched with brilliant additions, to be published at his expense, and under his auspices; and he called on me, under his authority, to carry out the not unpleasant duty of seeing that this was carried out as well as possible.

Cambridge, May 12, 1713.
Roger Cotes, fellow of the College of *The Holy Trinity*,
Plumian professor of astronomy
and experimental philosophy.

The Author's Preface to the Third Edition

In this third edition, edited by Henry Pemberton M.D.*, a man of the greatest experience in these matters, a number of things in the second book, concerning the resistance of media, has been explained in a little more detail than before, and new experiments have been added concerning the resistance of heavy bodies that fall through air. In the third book the argument by which it is proved that the moon is retained in its orbit by gravity is expounded in a little more detail, and new observations concerning the ratio of the diameters of Jupiter, made by Mr.* Pound*, have been added. Also we have added some observations of the comet which appeared in* 1680*, that were made by Mr.* Kirch *in the month of November when he was in* Germany*, and that recently came into our hands, and by the help of which it is confirmed that the orbits of the comets approximate very closely to parabolas. And the orbit of this comet, by a calculation of* Halley*, is determined a little more accurately than before, and shown to be an ellipse. And it is shown that this comet follows this elliptical orbit, through nine signs of the zodiac, no less accurately than the planets follow their elliptical orbits, as calculated by astronomy. And the orbit of the comet that appeared in the year* 1723*, as calculated by Mr.* Bradley*, the professor of astronomy at* Oxford*, has been added.*

IS. NEWTON.

Signed in *London*
Jan. 12, 1725–6.