Negotiating the *Principia*:

the failure of Newton's arguments to persuade his readers, 1684–94

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Abstract

When Isaac Newton's *Principia Mathematica* was published in the summer of 1687, it met with immediate acclaim. Through a close examination of contemporary reading notes, this thesis aims to establish the extent to which that acclaim was the result of his peers' assent to the arguments contained in the book.

It will demonstrate that, so far as can be reliably inferred from the extant documentary evidence, early readers were generally not persuaded by the demonstrations in the *Principia*. Newton's peers commonly didn't scrutinise the arguments in his book; when they did scrutinise them, they commonly didn't understand them; and when they did understand them, they commonly didn't agree with them. They frequently disputed the composition of his proofs, the validity of his methodology, and the articulation of his foundational concepts. When circumstances allowed, they communicated these misgivings back to Newton, who often altered his text in response, re-working, re-phrasing and re-structuring his demonstrations. They questioned both the formulation of his method of first and last ratios and his mathematisation of force, and none of the readers for whom there is reliable evidence assented to the entirety of Newton's proof of the inverse-square law. To the extent that they were persuaded by the correctness of Newton's conclusions, it was either because they were successfully able to reconstruct his arguments within their pre-existing conceptual frameworks; or because they held face-to-face conversations with the author in which they were able to query, contest and negotiate the composition of his text.

In other words, the book was very ineffective at persuading readers of the validity of the arguments it contained. This means that the acclaim *Principia* received at the moment of publication was unwarranted: it must have had some cause other than readers’ assent to its demonstrations.

Detailed Abstract

This thesis concerns early readings of Isaac Newton's *Principia Mathematica*. It argues that the immediate acclaim the book received when it was published in the summer of 1687 was not founded on readers' assent to the arguments printed on its pages. So far as can be inferred from the extant primary evidence, early readers were in general not persuaded by the arguments they found in the book.

Newton's peers generally did not scrutinise the details of his mathematical demonstrations; when they did, they often did not understand them; when they did understand them, they often did not assent to their validity. Notwithstanding the immediate acclaim it received at the moment of publication, there is no evidence that any of Newton's readers assented to all the arguments contained in the *Principia* in the years following publication. The text did not convince its readers of the validity of the mathematical methods it employed, and it did not convince its readers of the physical assumptions on which it was based. Readers commonly re-interpreted Newton's arguments in terms of their own pre-existing conceptual frameworks, providing their own reconstructions of Newton's proofs using alternative notation, logic, concepts and methodologies. When readers were able to communicate these reconstructions back to Newton, he often incorporated them in subsequent editions of the book. To the extent that they agreed with Newton's conclusions, they did so either by successfully reconstructing his arguments in their own terms, or by holding face-to-face conversations with the author in which they queried, refined and negotiated the validity of his proofs. The text that was printed in the book was

not sufficient to persuade its readers of the correctness of the arguments it contained.

These arguments are founded on a close analysis of a number of primary sources currently unexamined by the secondary literature. Specifically, this thesis will contain new analyses of: the editorial comments Edmond Halley made when he read early drafts of Newton's text; the marginal notes John Flamsteed wrote in his presentation copy; the extensive marginalia and reading notes composed by Nicolas Fatio de Duillier when he began preparing a second edition; the letters Gilbert Clerke sent to Newton a few months after publication; the entries John Locke recorded in his commonplace books on the three occasions that he read the *Principia*; the scattered notes made by Christiaan Huygens when he studied a selection of Newton's proofs. It is also based on a detailed examination of the opening sections of the two hundred pages of *Notae* composed by David Gregory during his methodical study of the text, and draws on existing studies of Gottfried Leibniz's response to his early engagement with the book. It will also make frequent reference to the alterations to the text Newton accumulated during the years after the first edition was published, which he recorded in two copies kept expressly for that purpose (Cambridge, Trinity College, NQ.16.200; Cambridge University Library, Adv.b.39.1). The changes collected in these copies formed the basis for Cotes' second edition of 1713, and while some edits merely record typographical slips and superficial tweaks to the published wording, many comprise significant modifications of the original demonstrations. Moreover, Newton often made these changes in direct response to the suggestions and criticisms of his readers. In many instances an individual alteration of the text can be identified with the specific communication from a reader that provoked it. These edits therefore provide a direct record of Newton negotiating the validity of his mathematical arguments with his peers, and show that the new mathematical knowledge in the *Principia* was made by means of a dialogue between the author and his readers, who refined the articulation of the arguments given in the original text.

The extant sources show that Newton's peers commonly read his book piecemeal, examining only isolated passages in idiosyncratic sequence. Most early readers left most of the book untouched. Only Gregory appears to have completed a close reading of the complete text, although the evidence suggests that some others (Locke, Fatio, Halley and possibly Leibniz) skim-read the whole thing. The only passages that were routinely examined by early readers were the Definitions and Laws, the results on orbital forces in Sections 1–3 of Book 1, and Book 3. However, even within these passages the level of scrutiny varied, and individual steps in the logical structure of a proof were frequently omitted. Evidence exists of only a very small number of readers (Gregory, Fatio, Leibniz, plus possibly Halley and Clerke) who can be reasonably said to have closely studied the entirety of Newton's demonstration of the inverse-square law. It is also noteworthy that Newton's proofs concerning mutual attraction (as opposed to single, centrally directed forces) at the end of Book 1 received almost no attention before Gregory studied them at the end of 1692. Of the readers for whom evidence is available, only Leibniz read these passages, and even he appears not to have scrutinised the mathematics. This leads to the important observation that there is no evidence that any readers undertook a detailed examination of Newton's mathematical demonstrations involving universal gravitation until at least five years after the *Principia* was published.

There was no one way of engaging with the text of the *Principia*, and different readers read different sections in different ways. A crude but useful distinction can be made between uncritical skim-reading and close scrutiny of Newton's arguments. The latter necessitated verifying the mathematical demonstrations in the text, which almost always involved the reader picking up a pen and reconstructing Newton's connected prose in symbolic form. They did this either in the margin of the book, or in separate reading notes. That is, reading the *Principia* was active process of reconstruction rather than a passive process of absorption. As they reconstructed Newton's arguments in order to verify them, they generally did so using their own individual pre-existing conceptual frameworks. Few of Newton's readers were persuaded to adopt the mathematical and physical framework in which he presented his arguments. Instead, most readers reconstructed (or attempted to reconstruct) his demonstrations in their own terms. Their reconstructions commonly employed different mathematical methodologies, different physical assumptions, and a different logical structure. They assented to his claims to the extent to which they were successfully able to reconstruct arguments in their support within their own framework. Verifying Newton's proofs thus often involved deriving his conclusions by different means. Readers therefore often assented to Newton's claims while disputing the validity of the arguments he provided in their support.

As they reconstructed Newton's arguments in their own terms, readers often thought that they had improved them. The act of recreating his demonstrations caused them to – as they saw it – simplify, clarify and restructure the original explanations, and sometimes extend them to produce new results. In this sense, editing the text was integral to the act of reading it. Because readers were obliged to recreate Newton's arguments in order to verify them, they were automatically alerted to ways in which they could be improved. When circumstances allowed, readers often communicated these alternatives back to Newton. Clerke did so by letter, while Fatio, Gregory and (probably) Halley all did so in person. And as the edits he collected in his personal copies show, Newton frequently altered his proofs in direct response to this feedback, often adopting the proposed alterations verbatim. Some of the changes he made comprised small typographical or grammatical corrections, but many did not, and Newton refined some of the most important passages in the book in direct response to suggestions from his peers. The final text of the second and third editions therefore represents more of a group effort than the secondary literature has previously acknowledged.

Instances when readers straightforwardly verified Newton's proofs – that is, when they were successfully able to reconstruct his arguments in the terms in which he presented them – are extremely rare in the primary documentation. There were many qualities of Newton's text that prevented such independent reconstruction. Sometimes the prose formulation of his demonstrations was difficult to reconstruct symbolically, because his explanations were too convoluted to understand, and his printed diagrams too unclear. Sometimes Newton left gaps in his proofs that readers were unable to fill in. Sometimes, the length of the book discouraged or prevented close scrutiny of his arguments. And sometimes the conceptual framework within which a given reader was operating could not accommodate Newton's argument. In addition, Fatio, Gregory and (probably) Halley sought face-to-face meetings with Newton in which they queried and challenged his proofs. That is, even for Newton's most careful and attentive peers, the text itself was not sufficient to persuade readers of the validity of the arguments it contained.

Readers often disputed the validity of Newton's innovative mathematical methodology. Some reinterpreted his geometrical limit proofs in terms of infinitesimals, which often led them to misunderstand or deny their validity. Even those readers who attempted to engage with Newton's proofs in the terms that they were written frequently disputed their articulation or denied that they were sound. In his presentation of the method of first and last ratios in Section 1 of Book 1, readers objected to his limit definition in Lemma 1 because Newton's phrasing made the logic of the demonstration unclear; and they denied the need for the unusual style of proof he deployed at the heart of his exposition in Lemmas 6–9. Newton rewrote these proofs in direct response to his readers' feedback, along with many others. Readers also disputed the validity of his innovative physical methodology. Some denied his foundational assumptions, with Halley prompting Newton to make significant alterations to the phrasing of the Definitions and Laws, and Leibniz preferring his own metaphysics to Newton's. Different readers were used to mathematising orbital motion in different ways, and so a number either opposed Newton's use of centripetal rather than centrifugal forces, or explicitly asserted their ambivalence. Probably following a conversation with the author, Gregory noted that Proposition 6 required rewriting owing to a flaw in its logic: the *Principia*'s most careful reader thus judged that all the orbital force theorems in the opening sections of the book were invalid. And as stated above, there is no evidence of any readers closely scrutinising the mathematical proofs concerning universal gravitation in the first five years after publication.

The most that can therefore be said about the response of Newton's peers to his book is that a very small number of readers gave heavily qualified assent to a very small number of the arguments it contained. They were routinely unpersuaded by the arguments in the form that they were provided in the first edition, and often only agreed to their own reconstructions of Newton's arguments, or to modified versions of the text. Even this often only occurred after a face-to-face conversation with Newton. It is not even sustainable to make the narrow claim that his peers accepted his solution to the planetary orbit problem Halley had presented him with in 1684, because the archive does not contain evidence of any individuals who assented to the proof of the inverse square law that appeared in the first edition. Every reader for whom we have reliable evidence denied the validity of one or more steps in Newton's argument.

This thesis is therefore an internalist history that reaches an externalist conclusion. By a close examination of the reception of Newton's mathematical arguments, I aim to show that his book met with approval for some reason other than assent with his mathematical arguments. Public confidence must have been generated by alternative means. What these means were, is beyond my remit: my purpose here is merely to argue that at the time of publication Newton's peers were not persuaded of the correctness of the arguments they found printed in his book.

Contents

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Prologue

One day during the summer of 1684, probably in August, Isaac Newton received a visitor to his rooms in Trinity College, Cambridge.

The story has often been told. There is little documentary evidence to substantiate the facts of the occasion, but we may imagine Edmond Halley walking through the Great Gate of Trinity and turning right towards the Lucasian Professor's chambers. He ascended a staircase on his right, knocked on the door, was shown into a room, and sat down.

He was there to discuss the mathematics of elliptical orbits. Back in January he, Christopher Wren and Robert Hooke had been debating the problem of astronomical motion one evening after a meeting of the Royal Society. Halley wanted to know whether Newton could prove mathematically that a force that followed an inverse-square law could account for the elliptical shape of planetary orbits.

Newton, so the story goes, claimed that he had already produced a proof but was unable to find it. He sent Halley on his way, vowing to deliver him a copy in due course. In November of that year Newton made good on his promise, and Halley received a short tract containing his derivation. Halley shared it with the Royal Society, and then encouraged Newton to expand the tract into a book. That book would be published in July 1687.

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Almost exactly ten years later, Newton received another visitor.

This story is less often told. In May 1694, the Scottish mathematician David Gregory walked through the Great Gate, turned right and right again, and ascended the staircase. He knocked on the same door, was admitted to the same room, and maybe even sat in the same chair as Halley had a decade earlier.

He, too, was there to discuss the mathematics of motion in elliptical orbits. He had just finished working through the *Philosophiae Naturalis Principia Mathematica*, and at that moment knew its contents better than anyone other than its author. He had carefully examined every page, worked through every proof, and weighed up every one of Newton's arguments. He had been impressed by many of its technical achievements, and sought further instruction on many of its methods. But he also had questions.

These weren't fussy questions about trivial details. These were direct challenges to some of the most fundamental ideas in the book. He wasn't convinced by Newton's rejection of centrifugal forces. He couldn't see the purpose of the style of proof Newton had used in his method of first and last ratios. He suspected there was a major logical flaw in his derivation of the inverse-square law. He was there to query, contend and haggle over the validity of the book's most important demonstrations.

A full decade had passed since Halley asked Newton to explain the problem of orbital motion, but Gregory was not yet convinced by the solution he had given. Ten years after Halley had posed the question, Newton's most attentive reader was still not satisfied with his answer. Gregory had paid careful attention to the arguments printed on the pages of the *Principia*, and he thought that many of them were faulty.

And he wasn't the only one.

General Introduction

Motivation of this thesis

The secondary literature on Isaac Newton's *Principia Mathematica* contains two distinct narratives about how the first edition was received when it was published in July 1687. On the one hand, it reports that Newton's book met with immediate acclaim. According to Richard Westfall's biography,

> Newton's book took Britain by storm. Almost at once it became the reigning orthodoxy among natural philosophers.[1]

The success of the *Principia* was both instant and spectacular:

> Almost from the moment of its publication, even those who refused to accept its central concept of action at a distance recognized the *Principia* as an epoch-making book.[2]

Niccolò Guicciardini corroborates this view:

> The book won immediate fame for its author and was adopted by most Fellows of the Royal Society as containing the right answer, as well as the right method of obtaining it, to the problems concerning cosmology and planetary motions which had occupied people such as Hooke, Halley and Wren in the early 1680s.[3]

In intellectual circles, "the fame and influence of the *Principia* spread quickly."[4] Scholars across England, Scotland, Ireland and the European continent scrambled to access copies. Laudatory reviews appeared in the *Philosophical*

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[1] Westfall, *Never at Rest*, 472.
[2] Ibid., 469.
[3] Guicciardini, *Reading the Principia*, 170.
[4] Westfall, *Never at Rest*, 470.

Transactions, the *Journal des Sçavans* and the *Acta Eruditorum*,[5] and it provided the stimulus for the first Boyle Lectures, given by Richard Bentley in 1692. In his personal life, too, the book's publication marked a watershed, as Newton's raised intellectual status was reflected in his changing social circumstances. He began to visit London more frequently, dining with the likes of the Duke of Devonshire and William of Orange. Westfall wrote that "The Newton of 1689 was a different man from the Newton of the 1670s. The completion and publication of the *Principia* and his own realization of its significance gave him a new confidence."[6] 1689 was the year in which Newton was elected to represent Parliament, was proposed for the Provostship of King's College, and sat for portrait by Sir Godfrey Kneller. The speed with which his personal renown spread matched that with which his book was acclaimed. Unlike, for example, Copernicus in the sixteenth century or Mendel in the nineteenth, there was no delay before Newton's work was approved. There was no long period of reflection during which the book's credibility was considered and assessed. The *Principia* met with "immediate" success, and was established as an epoch-making book "from the moment of publication." This happened "almost at once." It took Britain "by storm." As soon as the *Principia* was published in the summer of 1687, according to Rob Iliffe, its effect was to "quickly cement Newton's reputation as the greatest natural philosopher of his time."[7]

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[5] Cohen, *Introduction*, 145-57; Westfall, *Never at Rest*, 469-72. All three reviews were unsigned, although that in the *Philosophical Transactions* was composed by Halley, and Cohen suggests that the review in the *Acta* was possibly written by its editor, Otto Mencke.
[6] Westfall, *Never at Rest*, 488-9.
[7] Iliffe, "Newton: The Making of a Politician."

But alongside this narrative runs a parallel story. This tells that, when it was published, almost no-one read Newton's book. One anecdote occurs repeatedly in the literature:

> A well-known legend says that a student in Cambridge, while Newton was passing by, was heard to utter, 'There goes the man who has writ a book that neither he nor any one else understands'[8]

The *Principia* was notoriously difficult to read, according to this story. It consisted of a series of impenetrably complicated geometrical demonstrations, and at over five hundred pages, was intimidatingly long. Not only that, but its proofs depended upon radically novel and unfamiliar mathematical techniques. Almost no-one was capable of understanding such a forbidding, technical, specialised work. Everybody agreed that this new book was revolutionary in its importance, but "Even in the seventeenth century, few essayed to read it."[9]

A very basic question therefore emerges when these two narratives are set side-by-side: if so few people read it, how did the credibility of the *Principia* come to be established so immediately? Given that it was celebrated as containing the right answers to the problem of planetary motion almost as soon as it was published, how was this authority so rapidly conferred? How did its dense thicket of geometrical proofs – based as they were on a new and unusual mathematical methodology – come to be validated so quickly? The gap between these two narratives is the *explanandum* for this thesis.

It is not impossible for both narratives to be simultaneously true. They can be easily reconciled, so long as it can be shown that the technical details of

[8] Guicciardini, *Reading the* Principia, 170, quoting King's College, Cambridge, MS Keynes 130.5, no. 2.
[9] Curtis Wilson, writing in Densmore, *Newton's* Principia*: The Central Argument*, xiii.

Newton's text were carefully verified by a small number of contemporaries whose judgment was widely trusted. If a few recognised authorities worked through and checked his mathematical proofs, then their approval would have sanctioned wider assent. This mechanism would constitute the seventeenth-century equivalent of the modern process of peer-review. The purpose of this thesis would therefore appear to be extremely straightforward: to identify this handful of trusted readers, and demonstrate that they quickly assented to the arguments they found in Newton's text.

The secondary literature has given surprisingly little direct attention to this question. There have been many studies of individual responses to Newton's text, but no focussed attempt to identify the group of individuals whose quick assent to the mathematical arguments of the *Principia* caused it to be acclaimed so immediately upon publication. Furthermore, I will argue below that much of the literature on the reception of Newton's book employs language and tropes which assume that such an investigation is not required, either because it is assumed that it has already been established which of Newton's peers verified his proofs, or – which is worse – because it is assumed that Newton's proofs did not need verifying. Here, for example, are Steven Shapin's most detailed published views on Newton's early readers:

> The book that is said to have marked the culmination of the Scientific Revolution and to have changed the way "we" think about the world – Isaac Newton's *Principia Mathematica* – was probably read in its entirety by fewer than a hundred contemporaries, of whom no more than a handful were competent to understand it.[10]

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[10] Shapin, *The Scientific Revolution*, 123.

These lines are from a general survey written twenty-five years ago, but even so the imprecision of Shapin's claim is striking. "Fewer than a hundred" leaves a generous margin of error for the number of people who read the book "in its entirety." It is not specified who the "handful" were, how it has been established that they were "competent to understand it," and why even Shapin was only "probably" confident in the supporting evidence. Just as notable is his lack of alarm at this uncertainty, which I do not share. The dissonance within the secondary literature between the assumption of immediate acclaim and the assumption of a vanishingly small readership appears to have gone unnoticed.

Outline of Sources

This lack of attention has allowed significant evidence concerning the early reception of the *Principia* to be overlooked, a close examination of which will be the foundation of this thesis. Specifically, this study will contain new analyses of the following primary sources:

- the editorial comments Edmond Halley made when he read early drafts of Newton's text;
- the marginal notes John Flamsteed wrote in his presentation copy;
- the extensive marginalia and reading notes composed by Nicolas Fatio de Duillier when he began preparing a second edition;
- the letters Gilbert Clerke sent to Newton a few months after publication;
- the entries John Locke recorded in his commonplace books on the three occasions that he read the *Principia*;
- the scattered notes made by Christiaan Huygens when he studied a selection of Newton's proofs;

- the two hundred pages of *Notae* composed by David Gregory during his methodical study of the text.[11]

None of these sources has only recently been identified, and all have been either published or available for study in prominent libraries for at least a century. All are listed in Cohen's *Introduction to Newton's Principia* of 1971. Yet at the time of writing all of them remain largely unexamined by the secondary literature. Halley's, Flamsteed's and Fatio's have not been studied at all; those of Huygens, Gregory and Clerke have been only partially examined; and Locke's notes have been misdescribed. The only primary source relevant to this thesis that has already been fully accounted for is the documentation that records Leibniz's response to his readings of the *Principia*: this is the subject of an existing monograph and papers by Bertoloni Meli, on which I will frequently draw.[12]

A detailed examination of these primary sources will refute any unspoken assumption that Newton's peers successfully verified his proofs when the *Principia* was published. On the contrary, they will show that – so far as can be reliably inferred from the extant evidence – early readers were in general not persuaded by the arguments they found in the book. They demonstrate that large sections of the *Principia* were not studied by early readers; that when they did study them, they often did not understand them; and when they did understand them, they often did not agree with them. Readers were generally not persuaded by Newton's innovative mathematical and physical methodologies, and instead re-interpreted Newton's arguments in terms of their

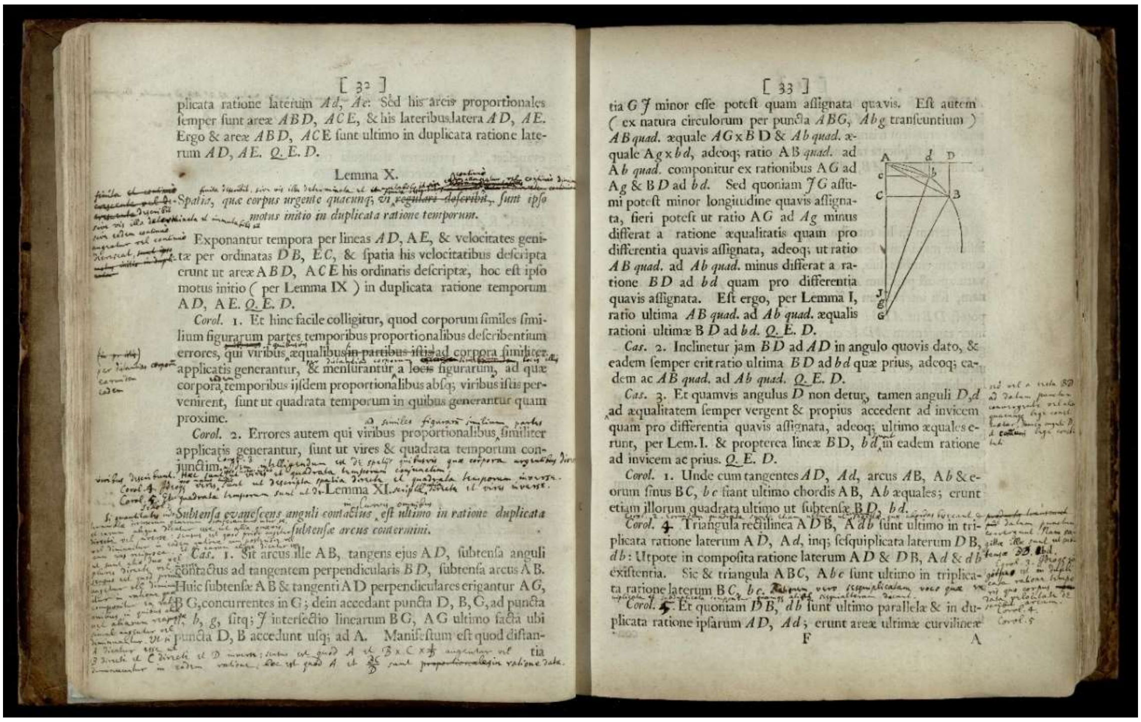
[11] Gregory's *Notae* is the only one of these sources that I cannot claim to have examined in full: I have only been able to examine parts of this document, as will be explained in Chapter 1.
[12] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*; Bertoloni Meli, "Leibniz's Excerpts from the *Principia Mathematica*."

own pre-existing conceptual frameworks. Notwithstanding the immediate acclaim it received at the moment of publication, there is no evidence that any of Newton's readers assented to all the arguments contained in the *Principia* in the years following publication. The best that can be said is that some of its readers gave qualified assent to some of its claims.

One further primary source will play an important role in this analysis: the set of alterations to the text that Newton accumulated over the years after the first edition was published. He recorded these edits in two copies of the book kept expressly for that purpose, both now in Cambridge. One (the "annotated" copy) is in the Wren Library at Trinity College, a page of which is shown below; the other (the "interleaved" copy) is in the University Library.[13] The changes collected in these copies formed the basis for Cotes' second edition of 1713, and to the extent that they are mentioned in the secondary literature at all – as I shall discuss below – they are described as "corrections". An important aim of this thesis is to argue that this label mischaracterises their contents. Some edits do indeed merely record typographical slips and superficial tweaks to the published wording, but many comprise substantive alterations to the original arguments. They represent significant modifications of the printed demonstrations. Moreover, Newton often made these changes in direct response to the criticisms of his readers. In many instances it will be possible to identify an individual alteration of the text with the specific communication from a reader that provoked it. These edits therefore provide a direct record of Newton negotiating the validity of his mathematical arguments with his peers. They comprise a valuable historiographical resource that details how assent to

[13] Cambridge, Trinity College, NQ.16.200; Cambridge University Library, Adv.b.39.1.

the claims in the *Principia* was established by means of a dialogue between the author and his readers.



Pages from Section 1 of Book 1 of Newton's annotated copy of the first edition, Cambridge, Trinity College, NQ.16.200.

Methodology

My analysis of these sources will draw on the secondary literature of the last two decades examining reading practices and the reception of scientific and mathematical texts. There will be obvious parallels between this thesis on readings of Newton's *Principia*, and similar studies of readings of Copernicus' *De Revolutionibus*, Galileo's *Discorsi*, and Euclid's *Elements* by Owen Gingerich, Renée Raphael, and Benjamin Wardhaugh, Philip Beeley and Yelda Nasifoglu.[14] My approach has been informed by studies of note-taking and

[14] Gingerich, *An Annotated Census of Copernicus'* De Revolutionibus; Gingerich, *The Book Nobody Read*; Raphael, *Reading Galileo*; Wardhaugh, "Defacing Euclid: Reading and Annotating the *Elements of Geometry* in Early Modern Britain"; Wardhaugh, "Rehearsing in the

marginalia in early modern culture, and there will be a particular resonance with Anthony Grafton's view of texts as unstable artefacts that emerge from a collaborative process involving authors, editors and correctors, and which are therefore "more social than individual products." [15] The primary evidence considered in this study will reinforce these scholars' conclusions that different readers can approach the same book in a variety of ways, and engage with the same text using a range of methods, tools, frameworks and categories. It will provide support for the argument prominently articulated by Grafton and Lisa Jardine that a single text is capable of inducing a plurality of responses, and my analysis will share their assumption that reading is an active rather than a passive pursuit.[16]

Specifying the nature of the activity associated with contemporary readings of the *Principia* will not be straightforward. In his study of practical geometry, Jim Bennett has borrowed the term "operative knowledge" from Robert Hooke to describe how objects such as astrolabes were "instruments of doing rather than knowing," and Boris Jardine has employed this vocabulary in his recent studies of instrument books as sites of mathematical practice.[17] By "practice" Bennett means activities such as map production, canal construction, bridge building, but with a little elasticity the knowledge in the *Principia* can also usefully be understood as "operative," and the margins of the book likewise be

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Margins: Mathematical Print and Mathematical Learning in the Early Modern Period"; Beeley, Nasifoglu and Wardhaugh, eds. *Reading Mathematics in Early Modern Europe*.

[15] Yeo, *Notebooks, English Virtuosi, and Early Modern Science*; Blair, "The Rise of Note-Taking in Early Modern Europe"; Jackson, *Marginalia: Readers Writing in Books;* Grafton, *The Culture of Correction in Renaissance Europe*. The quotation is from Grafton, "Editing Technical Neo-Latin Texts: Two Cases and Their Implications," 175.

[16] Jardine and Grafton, "'Studied for Action': How Gabriel Harvey Read His Livy"; see also Frasca-Spada and Jardine, eds. *Books and the Sciences in History*.

[17] Jardine, "More than a Manual: Early-Modern Mathematical Instrument Books"; Jardine, "The book as instrument: craft and technique in early modern practical mathematics"; Bennett, "Practical Geometry and Operative Knowledge."

seen as sites of mathematical practice. The primary evidence will certainly show that engaging with the *Principia* could entail doing as well as thinking, as readers recapitulated Newton's techniques in their notes and annotations. Wardhaugh has used the term "rehearsing" to describe this process, and it will become apparent that contemporary readers of Newton engaged with his book by supplementing, editing, correcting, and translating the printed text, just as Early Modern readers did with Euclid.[18]

However, the emphasis of my approach will differ from those of Gingerich, Raphael and Wardhaugh in two important ways. The first is that I will not aim to draw conclusions about the modes of reading of large groups, since the extant evidence of early readers of the *Principia* does not comfortably divide into categories. For example, I will not be able to make claims analogous to Raphael's conclusion that an important group of Galileo's readers engaged with his text using traditional bookish methods,[19] because among the remaining evidence of Newton's early readers, modes of reading varied too widely to be able to make such generalisations. We will see that Locke's engagement with the text, for instance, was very clearly determined by the humanist tradition of commonplacing: but I cannot claim that he represented other individuals who engaged with the text in the same way, because there is no clear evidence that there were any. Likewise, Gregory's *Notae* sit firmly within the commentary tradition, but no other early reader seems to have processed Newton's book in this way. Leibniz's *Excerpts* have something in common with the

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[18] Wardhaugh, "Rehearsing in the Margins: Mathematical Print and Mathematical Learning in the Early Modern Period"; Wardhaugh, "Defacing Euclid: Reading and Annotating the *Elements of Geometry* in Early Modern Britain." For an articulation of the view that mathematics is simultaneously practical as well as propositional, see Netz, *The Shaping of Deduction in Greek Mathematics*.
[19] Raphael, "Reading Galileo's *Discorsi* in the Early Modern University."

commonplacing format, but his *Notes* do not; and Flamsteed's marginalia and Huygens' notes do not appear to be consistent with any identifiable mode of reading. I will draw generalisations in this thesis, but those generalisations will be about how early readers of the *Principia* responded to the arguments they found printed on its pages, and not about the reading traditions they represented. My primary aim is to understand contemporary responses to the claims in Newton's book, and the reading habits via which these claims were accessed are only a means to that end. While my sources are very similar to those studied by Raphael, Gingerich and Wardhaugh, therefore, unlike them I will not be drawing conclusions about historical modes of reading or reading practices.

The second difference is more positive. In discussing the "reception" of scientific texts, I wish in this thesis to distinguish between two component aspects of the process: on the one hand, the means by which the methods and conclusions of a new text come to be approved, validated and trusted; and on the other, the means by which those methods and conclusions are then transmitted to a wider audience. I do not claim that these two components are absolutely distinct and easy to disentangle, and acknowledge that in any given case they are likely to be interconnected in complicated ways. But I propose that this is a useful distinction to make in analyses of the "reception" of a scientific publication, since it allows for a more detailed examination of the different ways in which such a text can be read. In modern practice these two components commonly follow in sequential order, for example. It might reasonably be expected that the first phase of the reception of a modern text comprises its approval and validation, perhaps by a small group of trusted gatekeepers, which is ordinarily followed by a second phase in which the contents of the text are transmitted to the wider community, probably via established institutional structures. It might also be expected that these two phases entail different modes of reading, and that readers judging the validity of a newly published text engage with its contents differently from readers assimilating an established text they already have some prior reason to trust. The modern reader might expect the latter to take place in an educational context, possibly under the guidance of a teacher, whereas the former is maybe more likely to take outside an instructional setting, and perhaps more likely to be solitary. It might therefore be a valuable goal of a historical investigation to establish the extent to which these differences were, or were not, exhibited in contemporary readings. It would be profitable to examine the extent to which modern expectations about the relationship between these two phases were also exhibited in historical practice. At the very least, I suggest that it is helpful to recognise that these two components are usually subsumed in the literature under the single word "reception".

Following this distinction, the studies referred to above have focussed almost exclusively on the second, "transmission" component. Raphael's output is dominated by readings of the *Discorsi* in educational institutions in the decades following publication, and even in her paper on its publication through the collaborative efforts of agents and printers, she does not discuss whether Galileo had to battle to overcome the incredulity of his correspondents or struggle to win their trust.[20] Gingerich similarly establishes patterns of

[20] Raphael, "Printing Galileo's *Discorsi*: A Collaborative Affair"; Raphael, *Reading Galileo*, 11–15.

ownership and annotation of the first two editions of the *De Revolutionibus* in the decades after their publication, but the closest he comes to assessing the means by which readers validated the arguments in the text is his examination of the marginalia of Erasmus Reinhold, who recapitulated some of Copernicus' analysis.[21] The studies of readings of Euclid in early modern Europe are likewise concerned with the transmission of a text whose authority had already been established. In strong contrast to these, my concern in this thesis is solely with the initial "validation" component of the reception of the *Principia*. Newton made a series of surprising claims in his book, and justified them with a new and unusual methodology: I am interested in the means by which his readers judged the validity of those novel claims, and either did or did not come to trust in his conclusions. I am not concerned with how Newton's ideas were subsequently disseminated more widely. That is why my focus is on the text's earliest readers in the years immediately after publication, and why I have chosen to frame my investigation with reference to the speed with which it "took Britain by storm." In other words, for the purposes of this thesis I am viewing the *Principia* as a rhetorical rather than a didactic tool, and am interested in Newton's book only as a technology of persuasion, and not as a technology of instruction.

This study is not the only existing attempt to apply the methods of histories of reading practices to the reception of the *Principia*. Most obviously, this thesis is indebted to Guicciardini's published work over the last three decades, on which it will frequently draw. Two particular themes of his work will resonate with those explored here: the "equivalence" of outwardly dissimilar

[21] Gingerich, *An Annotated Census of Copernicus'* De Revolutionibus, 268–78.

mathematical methodologies, and the role of scribal publication in Newton's network of correspondents.[22] The ways in which my analysis differs from his will become apparent in due course. I have examined different primary sources and am exclusively interested in early readers of the text, and so our conclusions emphasise different aspects of the reception of Newton's book. On the rare occasion that I disagree with Guicciardini on an individual point of interpretation, I will state as such and make my case with a suitable degree of caution. My work is similarly intended to complement Mordechai Feingold and Andrej Svorenčík's forthcoming census of first editions, which promises to be a valuable aid in extending the research presented here.[23] What is not yet clear is the extent to which Feingold and Svorenčík have been able to examine the detail of the marginalia they have discovered, and in particular assess readers' responses to Newton's mathematical demonstrations. Steffen Ducheyne's recent analysis of Adriaen Verwer's study of the first edition, while providing much useful contextual information about the reception of the *Principia* in the Dutch Republic, requires the reader to take on trust its assertions about Verwer's response to Newton's proofs, and it may well be that Feingold and Svorenčík's forthcoming publication is likewise not intended to answer the questions my study addresses.[24] But it will at the very least enable patterns of ownership and families of annotations to be identified, and in that sense this

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[22] See in particular Guicciardini, *Reading the Principia*, 250–60; Guicciardini, *Isaac Newton on Mathematical Certainty and Method*, 339–64; Guicciardini, "Isaac Newton and the publication of his mathematical manuscripts"; and Guicciardini, "David Gregory's manuscript 'Isaaci Neutoni Methodus fluxionum' (1694): A study on the early publication of Newton's discoveries on calculus."

[23] Feingold and Svorenčík, "A preliminary census of copies of the first edition of Newton's *Principia* (1687)."

[24] Ducheyne, "Adriaen Verwer and the First Edition of Isaac Newton's *Principia* in the Dutch Republic."

study is intended to complement Feingold and Svorenčík's work, no less than it hopes to be able to accompany Guicciardini's.

Summary of Conclusions

A close examination of the primary sources points towards a conclusion that deserves to be stated boldly: notwithstanding the praise it was afforded in public, there is no evidence that in the handful of years following publication the *Principia* succeeded in persuading any of Newton's peers of the arguments it contained. The acclaim it received at the moment of publication was not founded on assent to its demonstrations. So far as can be reliably inferred from the extant documentation, his contemporaries often didn't engage with the details of his proofs, and when they did they often disagreed with them. The text did not convince its readers of the validity of the mathematical methods it employed, and it did not convince its readers of the physical assumptions on which it was based. To the extent that they agreed with Newton's conclusions, they did so either by reconstructing his arguments in their own terms, using alternative concepts and methodologies, or by holding face-to-face conversations with the author in which they queried, refined and negotiated the validity of his proofs. The text that was printed in the book was not sufficient to persuade its readers of the correctness of the arguments it contained.

Specifically, this thesis will make the following claims.

Who read it

There are only eight individuals for whom reliable documentation exists proving detailed engagement with the text before the summer of 1694: Edmond Halley, David Gregory, Gilbert Clerke, John Flamsteed, John Locke, Christiaan

Huygens, Nicolas Fatio de Duillier and Gottfried Leibniz. The list of individuals for whom there is specific reason to infer some sort of study, but the record of which is no longer extant, is just as short: Robert Hooke, Robert Boyle, John Wallis, John Craig, Richard Bentley, Colin Campbell, William Molyneux, William Petty, Abraham de Moivre and Edward Paget. Feingold and Svorenčík propose a print run of 600–650 copies of the first edition, of which they have located 387. Evidence of other engagement with the text will have been lost, but a meaningful estimate of the quantity and type of early readings beyond these named individuals will have to await the publication of their census.

Of these eight, only Leibniz's response has been adequately analysed by historians. The other primary sources this thesis will examine are listed above, and I will use them to construct a detailed chronology of known early readings of the *Principia*, such as is currently lacking in the secondary literature. This will reveal that Fatio and (probably) Halley paid close attention to substantial portions of Newton's text, and so deserve to be considered among his most important readers. Huygens, on the other hand, made no sustained attempt to study the text in any structured way, and only examined a handful of isolated passages relevant to his own research interests. Locke's three readings of the book have been misdescribed by the secondary literature, in ways that have important implications for Locke scholarship: the pervasive story that Locke asked Huygens whether he could trust the mathematical demonstrations of the *Principia* needs to contextualised by the evidence strongly suggesting that Locke scrutinised the text more closely than Huygens did. Flamsteed and Clerke only engaged with a few sections of the book, but their reactions are consistent with those of their peers, and so provide valuable case studies of the ways in which the book was read. Gregory provided the most thorough scrutiny of Newton's text when it was published, working through the entire book sequentially in two separate phases. However, we will see that Gregory, Fatio and (probably) Halley all required face-to-face meetings with Newton to process the details of his arguments, in addition to their independent study.

In short, this thesis will make the following new claims:

- the importance of Halley and Fatio as early readers of Newton's text has been significantly under-stated;
- the importance of Huygens has been significantly over-stated, and there is no evidence that he deserved to be trusted when Locke sought his opinion as to the validity of Newton's mathematics;
- Locke's three readings of the book have been misdescribed by the secondary literature;
- Gregory is the only early reader for whom there is good evidence of a detailed study of the entire book.

Which passages they read

The extant sources show that Newton's peers commonly read the text piecemeal, examining only isolated passages in idiosyncratic sequence. Most early readers left most of the book untouched. Only Gregory appears to have completed a close reading of the complete text, although the evidence suggests that some others (Locke, Fatio, Halley and possibly Leibniz) skim-read the whole thing. The only passages that were routinely examined by early readers were the Definitions and Laws, the results on orbital forces in Sections 1–3 of Book 1, and Book 3. However, even within these passages the level of scrutiny varied, and individual steps in the logical structure of a proof were frequently omitted. There exists evidence of a very small number of readers (Gregory, Fatio, Leibniz, plus possibly Halley and Clerke) who can be reasonably said to have closely studied the entirety of Newton's demonstration of the inverse-square law.

It is also noteworthy that Newton's proofs concerning mutual attraction (as opposed to single, centrally directed forces) at the end of Book 1 received almost no attention before Gregory studied them at the end of 1692. Of the readers for whom evidence is available, only Leibniz read these passages, and even he appears not to have scrutinised the mathematics. This leads to the important observation that there is no evidence that any readers undertook a detailed examination of Newton's mathematical demonstrations involving universal gravitation until at least five years after the *Principia* was published.

To repeat:

- most readers engaged with the text piecemeal, studying isolated passages out of sequence;
- the only passages routinely examined by early readers were the Definition and Laws, Sections 1–3 of Book 1, and Book 3;
- most readers left most of the rest of the book unread;
- there is no evidence that any readers scrutinised the mathematical arguments involving universal gravitation during the first five years after publication.

How they read it

The primary evidence shows that there was no one way of engaging with the text of the *Principia*. Different readers read different sections in different ways. A crude but useful distinction can be made between uncritical skim-reading and close scrutiny of Newton's arguments. The latter necessitated verifying the mathematical demonstrations in the text, which almost always involved the reader picking up a pen and reconstructing Newton's prose in symbolic form. They did this either in the margin of the book, or in separate reading notes. That is, reading the *Principia* was active process of reconstruction rather than a passive process of absorption.

In addition, Newton's peers approached his demonstrations from within a variety of conceptual frameworks. Different readers were accustomed to using different mathematical and physical methodologies, which meant that as they reconstructed Newton's arguments in order to verify them, they generally did so using their own individual pre-existing conceptual frameworks. Few of Newton's readers were persuaded to adopt the mathematical and physical framework in which he presented his arguments. Instead, most readers reconstructed (or attempted to reconstruct) his demonstrations in their own terms. The proofs readers thus generated were often different from those Newton gave in the text. Their reconstructions commonly employed different mathematical methodologies, different physical assumptions, and a different logical structure. They assented to his claims to the extent to which they were successfully able to reconstruct arguments in their support within their own framework. Verifying Newton's proofs thus often involved deriving his conclusions by different means. Readers therefore often assented to Newton's claims while disputing the validity of the arguments he provided in their support.

As they reconstructed Newton's arguments in their own terms, readers often thought that they had improved them. The act of recreating his demonstrations caused them to – as they saw it – simplify, clarify and restructure the original explanations, and sometimes extend them to produce new results. In this sense, editing the text was integral to the act of reading it. Because readers were obliged to recreate Newton's arguments in order to verify them, they were automatically alerted to ways in which they could be improved. When circumstances allowed, readers often communicated these alternatives back to Newton. Clerke did so by letter, while Fatio, Gregory and (probably) Halley all did so in person. And as the edits he collected in his annotated copy show, Newton frequently altered his proofs in direct response to this feedback, often adopting the proposed alterations verbatim. Some of the changes he made comprised small typographical or grammatical corrections, but many did not, and Newton refined some of the most important passages in the book in direct response to suggestions from his peers. The final text of the second and third editions therefore represents more of a group effort than the secondary literature has previously acknowledged.

In short:

- different readers read different passages in different ways;
- a helpful distinction can be made between cursory skim-reading of passages and detailed scrutiny of arguments;
- verification of the mathematical proofs entailed an active process of reconstruction:
    - Newton's connected prose had to be reconstruction in symbolic form;
    - readers often reconstructed Newton's proofs in their own terms, using their individual, pre-existing conceptual framework;

    - their reconstructions were therefore often substantively different from the arguments Newton supplied in the text;
    - readers assented to Newton's conclusions according to whether they were successfully able to reconstruct arguments in their support in this way;
    - they thus often assented to Newton's conclusions while disputing the validity of the arguments he used to prove them;
- editing the *Principia* was therefore integral to the act of reading it:
    - readers automatically produced what they thought were improved versions of Newton's demonstrations;
    - the act of reconstruction in some instances led readers to extend Newton's proofs to generate new mathematical results;
    - when these alternatives and additions were communicated to Newton, he often edited his text in response, adopting his readers' suggestions verbatim.

What they thought of it

The primary evidence shows that occasions when readers straightforwardly verified Newton's proofs – that is, when they were successfully able to reconstruct his arguments in the terms in which he presented them – were extremely rare. We shall see that there were many qualities of Newton's text that obstructed such independent reconstruction. Sometimes the prose formulation of his demonstrations was difficult to reconstruct symbolically, because his explanations were too convoluted to understand, and his printed diagrams too unclear. Sometimes Newton left gaps in his proofs that readers were unable to fill in. Sometimes, the length of the book discouraged or prevented close scrutiny of his arguments. And sometimes the conceptual framework within which a given reader was operating could not accommodate Newton's argument. In addition, we shall see that Fatio, Gregory and (probably) Halley sought face-to-face meetings with Newton in which they queried and challenged his proofs. This demonstrates that, even for Newton's most careful and attentive peers, the text itself was not sufficient to persuade readers of the validity of the arguments it contained.

Readers often disputed the validity of Newton's innovative mathematical methodology. Some reinterpreted his geometrical limit proofs in terms of infinitesimals, which often led them to misunderstand or deny their validity. Even those readers who attempted to engage with Newton's proofs in the terms that they were written frequently disputed their articulation or denied that they were sound. In his presentation of the method of first and last ratios in Section 1 of Book 1, readers objected to his limit definition in Lemma 1 because Newton's phrasing made the logic of the demonstration unclear; and they denied the need for the unusual style of proof he deployed at the heart of his exposition in Lemmas 6–9. Newton rewrote these proofs in direct response to his readers' feedback, along with many others.

Readers also disputed the validity of his innovative physical methodology. Some denied his foundational assumptions, with Halley prompting Newton to make significant alterations to the phrasing of the Definitions and Laws, and Leibniz preferring his own metaphysics to Newton's. Different readers were used to mathematising orbital motion in different ways, and so a number either opposed Newton's use of centripetal rather than centrifugal forces, or explicitly asserted their ambivalence. Either during or soon after his initial reading

Gregory objected to the logic of Proposition 6: the *Principia*'s most careful reader thus thought that all the orbital force theorems in the opening sections of the book were invalid. And we have seen that there is no evidence of any readers closely scrutinising the mathematical proofs concerning universal gravitation in the first five years after publication.

The most that can therefore be said about the response of Newton's peers to his book is that a very small number of readers gave heavily qualified assent to a very small number of the arguments it contained. Even then, this often only happened after a face-to-face conversation with Newton. It is not even sustainable to make the narrow claim that his peers accepted his solution to the planetary orbit problem Halley had presented him with in 1684, because the archive does not contain evidence of any individuals who assented to the proof of the inverse square law that appeared in the first edition. Every reader for whom we have reliable evidence denied the validity of one or more steps in Newton's argument.

In summary:

- readers were often unable successfully to reconstruct his proofs:
    - his prose was often too convoluted to recreate symbolically;
    - Newton sometimes left gaps in his explanations;
    - the length of the book prevented close scrutiny of all its arguments;
    - the conceptual frameworks in which some readers operated could not always accommodate Newton's arguments;
    - even very careful readers sought clarification from Newton in face-to-face meetings;

- early readers were in general not persuaded by Newton's innovative mathematical methodology:
    - they often tried to reconstruct his arguments in terms of infinitesimals;
    - this often led them to deny or misunderstand Newton's results;
    - they were not persuaded by his exposition of the method of first and last ratios: they found the logic of the limit definition in Lemma 1 unclear, and did not see the need for the unusual style of proof he used in Lemmas 6–9;
    - Newton edited many of his limit-based proofs in response;
- early readers were in general not persuaded by Newton's innovative physical methodology:
    - they disputed his foundational assumptions and disagreed with how he articulated his underlying concepts;
    - many either denied Newton's rejection of centrifugal forces, or remained explicitly ambivalent;
    - Halley made significant alterations to Newton's phrasing of the Definitions and Laws;
    - Gregory disputed the logic of Proposition 6, on which all the orbit theorems depended;
    - there is no evidence that any readers closely scrutinised the proofs concerning universal gravitation in the five years after publication;
- the most that can be said is that a very small number of readers gave qualified assent to a very small number of arguments in the *Principia*;

- even the narrow claim that Newton's readers accepted his proof of the inverse-square law is not supported by the evidence.

Negotiating the *Principia*

I have tried to distil these conclusions into the title of this thesis. The phrase "Negotiating the *Principia*" is intended to convey that assent to the arguments in the book was established only to the extent to which readers could successfully reconstruct its demonstrations in their own terms. In the cases of those readers who had direct contact with Newton, this negotiation is embodied in the archive as the edits Newton recorded in his annotated copy of the first edition, some of which – as I will demonstrate – were made in direct response to specific, identifiable feedback from his peers. Assent was in these instances established by means of a dialogue between author and readers, and the process by which the text was validated necessitated alterations to the text itself. But even those readers who did not have direct contact with Newton are to be understood as having undertaken their own private negotiation, in that they reconstructed alternative demonstrations that were substantively different from those in the book. In both cases Newton's peers routinely felt unable to assent to his arguments in the form that they were printed in the first edition, and could only agree to modified versions of the text they themselves generated. Irrespective of whether they communicated these modifications back to Newton, readers assented not with the demonstrations as they appeared on the page, but with their own, alternative reconstructions. This is the sense in which I mean that the validity of the arguments in the *Principia* had to be negotiated with its readers.

The subtitle of my thesis, "the failure of Newton's arguments to persuade his readers," conveys the same information in much stronger language. So far as we can tell from the extant evidence, in general readers did not assent to the proofs as they appeared in the printed version of 1687. The book's readers were not persuaded by them. The *Principia* generally failed to persuade its readers to adopt its innovative mathematical methodology, generally failed to persuade its readers to adopt its innovative physical methodology, and failed to persuade many of its readers of the validity of many of its proofs. It failed to persuade any of its documented readers of the correctness of its derivation of the inverse-square law. At best, they gave qualified assent to their own reconstructions of Newton's arguments.

I wish to clarify that I do not dispute that when it was published in 1687 the *Principia* "took Britain by storm." The book did indeed win "immediate fame for its author"; it did "quickly cement Newton's reputation as the greatest natural philosopher of his time." I merely wish to demonstrate that successful verification of the proofs in the text was not the cause of this acclaim. Newton's contemporaries did not grant their approval because they agreed with the arguments that were printed in his book. On the contrary, this thesis aims to show that Newton's contemporaries frequently disagreed with the arguments that were printed in his book. The acclaim Newton received was, in this specific sense, unwarranted. The extraordinary event in the summer of 1687 that requires explanation by historians was not the publication of the *Principia*, but the fact that his peers so immediately approved it when they had no good reason to do so. Newton's peers did believe that he had written "an epoch-making book," and they did trust that it contained important truths about the natural world and the way it should be interrogated, but that trust did not arise from their study of the text.

This thesis is therefore an internalist history that reaches an externalist conclusion. By a close examination of the reception of Newton's mathematical arguments, I aim to show that his book met with approval for some reason other than assent with his mathematical arguments. Public confidence must have been generated by alternative means. What these means were, is beyond the remit of this thesis: I will make some suggestions in my Conclusion, but these will be unevidenced, and intended to guide further research. My purpose here is merely to argue that at the time of publication Newton's peers were not persuaded of the correctness of the arguments they found printed in his book.

Six tropes in the Newton literature this thesis will address

These conclusions have a number of implications for the secondary literature on the reception of the *Principia*. These consequences are best conveyed with reference to six tropes that frequently occur in published studies of the reception of Newton's book. There is value in all of these tropes, and I do not wish to make the strong argument that any of them is false. However, it is an aim of this thesis to draw attention to their habitual and reflexive use. I wish to address the latent assumptions on which they are based, and to encourage critical examination of the evidence in their support. These tropes are connected and interdependent, but can usefully be specified as follows.

The *Principia* was self-evidently "difficult"

The trope that the *Principia* was a "difficult" book saturates the secondary literature on the reception of Newton's text. Feingold and Svorenčík refer to its "vaunted incomprehensibility" and "recondite nature" in the opening lines of their preliminary census.[25] Elizabethanne Boran introduces her and Feingold's collection of papers on eighteenth-century readings of the book by characterising it as "abstruse" in the first sentence, and explaining that "Many early readers (able mathematicians included) complained that it was too difficult."[26] Iliffe reports that "the *Principia* became a byword for impenetrability" and that anyone attempting to read it faced the overwhelming challenge of "mastering the work's incredibly abstruse contents."[27] Newton's text is so routinely described as "difficult" that it is hard to find any studies that do not employ this adjective. An extreme instance is Stephen Snobelen's paper on the subject, which opens with the sentence "Isaac Newton's *Principia* was a notoriously difficult book to read," is predicated on the book's reputation for being "impenetrable" and "the perceived difficulties of abstraction and mathematics," and concludes that "For most, it remained a veiled and difficult treatise, replete with mathematical abstractions and incomprehensible geometrical diagrams."[28]

There is, of course, a great deal of truth in this trope. This thesis will present clear documentary evidence that many early readers found the *Principia* difficult to read, and the above accounts are right to emphasise the important implications this had for the ways in which Newtonianism was subsequently disseminated. But the secondary literature is much less effective at examining what this label means. Statements that the book was difficult are not usually accompanied by attempts to identify the qualities of Newton's text that were

[25] Feingold and Svorenčík, "A preliminary census of copies of the first edition of Newton's *Principia* (1687)," 253.
[26] Boran and Feingold, eds. *Reading Newton in Early Modern Europe*, 1.
[27] Iliffe, *Newton: A Very Short Introduction*, 102, 105.
[28] Snobelen, "On reading Isaac Newton's *Principia* in the 18th century," 159, 161–3.

responsible for its difficulty, or evidence showing how this difficulty affected readers' ability to process individual passages. That the book was mathematical need not automatically have rendered it all incomprehensible, for instance, as much as Snobelen's phrasing suggests otherwise. Mathematics is no more intrinsically difficult than any other area of knowledge. If historians mean to say that some but not all of the mathematics in the book was difficult – that Lemma 28 of Book 1 on the non-integrability of ovals was particularly complicated, for example, or the scholium following Proposition 34 of Book 2 on the solid of least resistance – then they should be challenged to identify the offending passages, and explain why those particular sections had such a disproportionate effect on the reception of the text. The novelty of Newton's claims is certainly not sufficient reason to single out his work as uniquely difficult, since many texts contain new ideas without being labelled as inherently difficult to understand. If the *Principia*'s difficulty is thought to be a consequence of the style in which it was written, this needs to be reconciled with Newton's decision to eschew contemporary symbolic notation and present his arguments in classical, geometrical terms, which on the face of it would have made the book more accessible and more easy to understand. And if the book's difficulty is understood by historians to have been a result of its length, that distinction should be made clear. But such a distinction would need to acknowledge that many of the most eye-catching results – including the proof of the inverse-square law – are in the first fifty pages, which means that the important parts of the *Principia* are not particularly long at all. It would be useful to establish whether readers who studied only the first fifty pages still found it difficult.

The secondary literature contains only scattered gestures at such an analysis. Tom Whiteside's view is that "Quite bluntly, the logical structure of Newton's book is slipshod, its level of verbal fluency none too high, its arguments unnecessarily diffuse and repetitive, and its very content on occasion markedly irrelevant to its professed theme."[29] But as I understand it, this judgement is based on his own readings of Newton's book rather those of Newton's peers, and as such is anachronistic: the literature lacks a systematic examination of the documentary evidence to establish which passages contemporary readers found difficult, and how they responded to these difficulties. Furthermore, it is yet to explain how, in spite of its incomprehensibility, the book nevertheless came to be acclaimed so quickly when it was published. The above studies explain very persuasively how Newton's ideas were disseminated in the eighteenth century, but do not address how early readers – those individuals who would write rather than read the commentaries and popularisations that subsequently spread its ideas more widely – overcame the challenges the secondary literature claims were inherent in the text. In short, I do not share many historians' confidence that the book's difficulty requires no further explanation: on the contrary, a detailed deconstruction of this term is an important aim of this thesis.

It could only be read by "experts"

The narrative that Newton's text was self-evidently difficult sustains a second trope, that it could only be understood by a very small number of "experts" who were sufficiently "competent" to read it. Iliffe, for example, reports that

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[29] Whiteside, "The Mathematical Principles Underlying Newton's *Principia Mathematica*," 116.

> In its own day, it was couched in a language that few were able to begin to read; it was hard going even for the most adept of contemporary practitioners, and the number of people who could offer expositions of its most abstruse sections was miniscule.[30]

Andrew Warwick agrees that "When Isaac Newton published the 'Principia' three centuries ago, only a few scholars were capable of understanding his conceptually demanding work" because "the esoteric demonstrations of advanced mathematics were directly accessible only to a tiny and close community of experts."[31] Snobelen affirms that "only a handful of exceptionally competent scholars could comprehend its involved mathematical physics." Yet the distinction between this tiny, elite group and the mass of common readers has never been articulated, and its membership list not specified. Two names appear in almost every account of the reception of the *Principia*, but they are only ever included to emphasise their lack of expertise: John Locke, who, unable to get very far with the book himself, is said to have asked Huygens whether Newton's mathematics was correct, and Gilbert Clerke, who is characterised as an "exasperated" "ageing former Cambridge mathematician" who was reduced to writing to Newton to ask for help.[32] But Clerke's letters have never been fully examined by the secondary literature, and neither have the notes Locke made on the three separate occasions that he read the book. Many of the documents recording Huygens' readings of the *Principia* have also been overlooked, which means that the extent to which he should or should not have been trusted to provide assurance is far from clear.

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[30] Iliffe, "Butter for Parsnips," 34.
[31] Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics*, 30.
[32] Guicciardini, *Reading the* Principia, 176; Snobelen, "On reading Isaac Newton's *Principia* in the 18th century," 159.

Whiteside at least attempted to identify this exclusive band of experts:

> The *Principia* was – and is – accessible in its detail only to the mathematically sophisticated. In Newton's own lifetime only a handful of his contemporaries working without distraction at the frontiers of current research – maybe only the Dutch scientist Christiaan Huygens, the German *uomo universale* Leibniz, the able Swiss mathematician Johann Bernoulli, the French priest Pierre Varignon, the Huguenot expatriate Abraham De Moivre and Newton's most able editor Roger Cotes – had, each in his own way, achieved a working knowledge of the *Principia*'s technical content.[33]

In this passage Whiteside says he is listing individuals who achieved a working knowledge of the book "In Newton's own lifetime." This lifetime lasted for forty years after publication, and Whiteside provides six names, meaning that on average one person with the required expertise to access Newton's book came along every six or seven years. That is a remarkable claim for the undisputed scholarly authority on Newton's mathematics to be making. It is difficult to reconcile with the fact that the *Principia* "took Britain by storm" at the moment that it was published. Taking their words at face value, Whiteside and Westfall cannot both have been right. Of Whiteside's six, I have been able to find documentary evidence that only two – Huygens and Leibniz – engaged in any serious study of the text before 1694. Bernoulli and Varignon, so far as I can tell, only began to study the book later in the decade.[34] Cotes celebrated his fifth birthday the week the book was published. It is noticeable that Whiteside has quietly slipped in a "maybe" to modify the strength of his assertions, indicating that even he hadn't convinced himself of the evidence of early readings of the

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[33] Whiteside, "The Mathematical Principles Underlying Newton's *Principia Mathematica*," 117.
[34] Guicciardini, *Reading the Principia*, 195–249.

book: like Shapin's "probably" in the passage quoted above, this is a disconcerting word to find used by the pre-eminent scholar in his field on a matter of such significance.

Shapin invokes this trope using different language by impugning the "competence" of any individual not capable of understanding the book. This uncomfortably whiggish habit of blaming readers for not understanding Newton's arguments still persists in the literature, in large part because Cohen and Whiteside – on whose work all subsequent studies have depended – often employed such present-centred language. Whiteside described Halley's reading notes as a "not very percipient critique" of Newton's text, and Clerke's criticisms as a series of "tiresome and near-trivial 'scruples'."[35] Cohen declared that "Huygens had not really got the message of the *Principia*" because he "failed to discern" the difference between Newton's mathematical construct and physical reality, which "was a distinction that Huygens himself was not able to make, or was not willing to make."[36] In a similar way, Hall reported that Fatio "possessed mathematical abilities that were considerable though not of the highest order."[37] Even Bertoloni Meli describes Leibniz's misgivings with one of the *Principia*'s most important proofs using the words "The extent of Leibniz's failure here is stunning: obviously he read the text superficially."[38] Taken together, these judgements raise uncomfortable questions about the reception of the text: if the secondary literature reports that neither Halley, Huygens, Fatio nor Leibniz could not be trusted to read Newton's book properly, it might reasonably be

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[35] Newton, *The Preliminary Manuscripts for Isaac Newton's 1687* Principia*, 1684–1686*, xvii; Newton, *The Mathematical Papers of Isaac Newton*, 6:xxi.
[36] Cohen, *The Newtonian Revolution*, 81–2.
[37] Hall and Hall, *Unpublished Scientific Papers of Isaac Newton*, 205.
[38] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 244.

asked which of his peers was capable of reaching an informed verdict. It might legitimately be questioned how, if the text was so intrinsically difficult and its readers so reliably incompetent, the *Principia* came to be trusted at all.

In this thesis I will turn this trope on its head: rather than take the correctness of the text for granted and ask whether readers understood it, my aim is to establish how readers came to trust the results in the text. The *Principia* was capable of being put to many uses – as a teaching textbook, as a repository of empirical data, as a signifier of social status – but in this study I choose to prioritise its role as a rhetorical device. I wish to assess the extent to which it successfully persuaded the author's peers of the validity of the arguments it contained. If the documentary evidence reveals that Newton's intended readers did not fully grasp the full meaning of his arguments, then I will assume the balance of responsibility for that failure of communication to lie with the author, not the reader. I start from the view that it was the purpose of Newton's book to persuade his readers, and not the duty of his readers to understand the book.

"Understanding" was the only obstacle to assent

The unexamined assumptions that the *Principia* was self-evidently difficult and could only be read by experts leads directly to a third trope which pervades much of the secondary literature: the ability to "understand" the text was the only obstacle to agreeing with it. If he was successfully able to "master" the mathematical demonstrations in the book, according to this narrative, the reader automatically agreed with them. No other barrier to assent with Newton's mathematics is considered. The possibility that a reader might understand but still object to the validity of his mathematics is not acknowledged. When the primary evidence inconveniently shows that Newton's arguments were sometimes not approved by his peers, the disparity is explained away – as Cohen and Bertoloni Meli's examples above show – as a failure of understanding on the part of the reader.

This trope is manifested in accounts of the reception of the *Principia* in two distinct ways. The first is the characterisation of the act of engaging with the text as a passive attempt to understand it, and the routine use of the word "master" as a synonym for "read." Feingold, for example, reports that "Craig and other early readers of the *Principia* certainly persisted in their attempts to master the book, despite initial difficulties."[39] Ducheyne writes that Verwer "spent most of his time mastering the mathematical technicalities of the *Principia*."[40] Iliffe goes further, describing "the Herculean task of grasping the contents of the book" in stark terms:

> If Newton's apparently superhuman labor had led to the book's creation – and it was his own "industry and patient thought" that he himself repeatedly stressed – then potential disciples had to work just as hard to master its contents. Accordingly, much of the evidence from the period points to the sheer toil required to excavate its treasures and young acolytes were not lacking to undergo this supreme test.[41]

Iliffe's characterisation of readers' engagement with the *Principia* would appear to be a specific instance of his more general view that "the force of the demonstration of a mathematical text exerts something like an irresistible compulsion on those that are held to comprehend it" a claim which appears

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[39] Feingold and Svorenčík, "A preliminary census of copies of the first edition of Newton's *Principia* (1687)," 262.
[40] Ducheyne, "Adriaen Verwer and the First Edition of Isaac Newton's *Principia* in the Dutch Republic," 504.
[41] Iliffe, "Butter for Parsnips," 51–52.

explicitly to exclude the possibility that a reader might ever understand a mathematical argument but deny its validity.[42] I do not disagree that “mastering” the details of Newton’s arguments was an important component of the reception of his book, as the evidence even from its most attentive readers will make clear. But care should be taken not to imply that it was the only or even the determining factor, or that act of reading the book was always one of passive, uncritical assent to the arguments printed on the page. The connotations of the word “master” would seem to preclude the possibilities, for example, that a particular demonstration might be disputed, or half-understood, or misunderstood. Neither is it clear how the vocabulary of “mastery” would categorise a reader who agreed with Newton’s conclusions but not the arguments given in their support, or one who processed the conclusions without scrutinising the supporting arguments at all. A close examination of the primary sources will be required to establish whether any contemporary readers did in fact read any passages in this way.

The second indicator of the latent assumption that understanding was the only obstacle to assent is that histories of the reception of the *Principia* are disproportionately concerned with the means by which the book was rendered comprehensible after publication. Newton’s expert intermediaries (they are often described as “followers”, “disciples”, or even “acolytes”) first toiled to master the

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[42] Ibid., 33. This view also entails the assumptions that every mathematical publication is “succinct, rigorous, and highly prescriptive with regard to what it expects of its audience” and includes “definitions or axioms that are taken for granted” by its readers. Iliffe’s language in this paper appears to derive directly from Steven Shapin and Simon Schaffer’s *Leviathan and the Air-Pump*, which reports that in the seventeenth century “geometry yielded irrefutable and incontestable knowledge” and provided “the kind of certainty that compelled absolute assent” because “the obscure and abstract mathematical format compelled assent… from readers who understood the demonstrations” (Shapin and Schaffer, *Leviathan and the Air-Pump*, 100, 23, 35). A similar characterisation appears in Shapin, *The Scientific Revolution*, 112–120; I am not confident that it is sustained by the evidence, either in general or with specific reference to the *Principia*.

proofs themselves, according to these studies, and then set about communicating the results to others. Such narratives thus bypass the stage in which those intermediaries came to trust in the validity of Newton's arguments in the first place, because it is taken for granted that understanding the proofs was sufficient to be persuaded by them. Both Snobelen and Iliffe's accounts focus in this way on popularisers and disciples who "began early on to produce more digestible renditions of the *Principia*"[43] and who could "mediate between the meaning of the text and wider publics"[44]; and Boran and Feingold's edition of papers likewise emphasises the role of textbooks and popularisations, framing the reception of Newtonianism it in the context of William Molyneux's call for "a second edition which would make the text more accessible for general readers."[45] As explanations of the later dissemination of Newtonianism these studies are persuasive, but none of them acknowledge the possibility that there might have been obstacles to assent with Newton's mathematics other than the ability to understand it. Their only concern is to establish the means by which the *Principia* came to be understood, and they overlook the possibility that Newton's demonstrations could ever have been contested. By framing the reception of the *Principia* exclusively through efforts to render it comprehensible, such narratives unquestioningly assume that once Newton's mathematical arguments were understood, they were automatically agreed with.

The changes Newton made after publication were "corrections"

The prevalence of the foregoing tropes has led to an important artefact being mischaracterised in the secondary literature: the changes Newton made to the

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[43] Snobelen, "On reading Isaac Newton's *Principia* in the 18th century," 159–60.
[44] Iliffe, "Butter for Parsnips," 52.
[45] Boran and Feingold, eds. *Reading Newton in Early Modern Europe*, 3.

text in the years immediately after publication are routinely described as "corrections". A standard reference on this topic is still Hall's article from over sixty years ago, "Correcting the *Principia*." This paper discussed the abandoned plans for a second edition during the 1690s, and reported that by way of preparation Newton "noted corrections in an interleaved copy of the first." By this Hall meant the edition in the Wren Library referred to above; but having acknowledged this document's existence, he was not able to closely examine its contents or explore its history. He merely reported that "In remedying numerous errors of the first edition" various sections were revised, in an attempt "to see the errors… corrected." After all, "it was not astonishing that mistakes should have been made, in so difficult an investigation." Hall also noted that "Fatio de Duillier prepared a long list of emendations," which is true: Fatio did indeed collect a list of changes to the text in the reading notes identified above. But Hall was not able to inspect this document, and so did not establish its provenance or identify its relationship with the changes in Newton's interleaved copy. The secondary literature still continues to follow Hall in describing both Fatio's list and the changes in the Wren copy as "corrections", even though their contents have not been systematically examined.[46]

Here is the passage in which Hall reflects on why the edits might have been necessary:

> Four chief criticisms were directed against the *Principia*, one philosophical and three mathematical. The first was, of course, aimed at the Newtonian concept of gravity with its apparently esoteric idea of attraction. The other three concerned the obscurity of Newton's language, the mistakes of principle in certain of his demonstrations, in

[46] Hall, "Correcting the *Principia*," 291–3.

> Book II especially, and the vast number of minor slips, some due to the author's, some to the printer's carelessness. Many points of the last kind were such as would cause little difficulty to readers skilled in both Latin and mathematics (others would be unlikely to perceive them), but they marred the perfection of a great book, while a few were faults of consequence.[47]

Hall's claims here deserve close examination. His first suggestion – that the *Principia* elicited criticism for its esoteric idea of attraction – is not one to which I object. But his reference to "the obscurity of Newton's language" invites further questions: it would be helpful to establish which readers found this to be a problem, in which passages, and – most importantly – whether this prevented them from understanding what Newton was trying to say. Hall also mentions a "vast number of minor slips," which he elsewhere describes as "an infinity of errors to correct."[48] The primary evidence will need to be examined to confirm that these errors were indeed "minor" and caused as little difficulty to his readers as Hall suggests, and careful analysis will be required to distinguish such inconsequential slips from the "mistakes of principle in certain of his demonstrations," which would appear to pose a much more significant problem. Mathematical proofs are conventionally presented in a deductive sequence, which means that one weak link automatically threatens the logic of the whole chain. Identifying these flaws and locating the conclusions they undermine would appear to be an urgent task for any historian of the reception of the *Principia*. Having done so, it might then be questioned how the text managed to retain the confidence of its readers if, as Hall asserts, it contained so many mistakes.

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[47] Ibid., 293.
[48] Ibid., 292.

A close examination of these changes to the text is of central importance to my thesis. I will show that there is a clear link between these edits and the documentation that records readers' reactions to the text: in many instances, Newton altered his proofs in direct response to feedback from his readers. They not only highlighted minor slips and objected to the obscurity of his language, but also refined his terminology, disputed his logic, and suggested significant changes to his wording. This happened repeatedly, in critically important passages, throughout the book. Newton's readers encouraged him to rewrite, reorder and restructure some of his most important proofs. They haggled and negotiated over the validity of his arguments. Newton's interleaved copy thus comprises a precious resource that provides a detailed record of how the new mathematical knowledge in the *Principia* was forged by means of a dialogue with his readers. I suggest that the word "corrections" gives a misleading impression of the changes they contain: the edits Newton made to the text in the years following publication provide direct evidence of readers contesting the mathematical arguments in the *Principia*.

The mathematics can only be understood relative to the Leibniz "debate"

Hall's "Correcting the *Principia*" also exhibits our penultimate trope, which is the central idea of the most famous passage in his article:

> Cotes was a competent mathematician, but [when preparing the second edition] it was beyond his power to remedy the greatest single defect of the *Principia*, its outmoded mathematical texture, even had he wished to do so. By 1713 the ascendancy of the Leibnizian calculus among the mathematicians of the continent was assured; the geometrical demonstration, familiar to Huygens and others as well as to Newton in 1687, was already obsolete… The *Principia* was to remain a classic

> fossilized, on the wrong side of the frontier between past and future in the application of mathematics to physics.[49]

It is not the assumption of a clearly demarcated, impermeable "frontier" between the Newtonians and the Leibnizians I wish to subject to critical scrutiny here, because this has already been done very successfully by Guicciardini[50]: it is the assumption that the Newton-Leibniz debate is a necessary component of a study of the reception of the *Principia* at all. The subject of this thesis is the means by which the book came to be approved when it was published, and I can see no *a priori* reason why the disputes which occupied the opening years of the eighteenth century provide a uniquely valuable context from which to view events that took place over a decade earlier. I wish to suggest that the strong narrative pull of the subsequent rivalry between Newton and Leibniz – to which Hall contributed by normalising its depiction as a "War" – has had a distorting effect on studies of the reception of the *Principia*.

This trope is particularly emphasised by historians of mathematics, who share a training in the development of the methods of the calculus during the seventeenth and eighteenth centuries. According to the received narrative, the reception of the *Principia* was part of a debate between two competing mathematical schools. On one side, Newton led a group of English and Scottish mathematicians who championed the classical methods of proof used in the *Principia*, which were based on Newton's conception of a geometrical limit. On the other, a group of continentals led by Leibniz employed a set of modern, analytical heuristics that were based on algebraic infinitesimals. The reception of the *Principia* is thus seen through the prism of two competing mathematical

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[49] Hall, "Correcting the *Principia*," 301.
[50] Guicciardini, *Reading the* Principia, 250–60.

methodologies, and contemporary readings of Newton's book are framed within this context. An important component of such histories is the ongoing programme of "translation" of Newton's geometrical demonstrations into the modern method of analysis by the Leibnizians during the years following publication. According to these narratives, in the decades after 1687 continental scholars "translated" the obsolete style of the *Principia* into the new terminology of the calculus.

The most prominent examples of this approach are the two monographs by Guicciardini and Bertoloni Meli, both of which situate the reception of the *Principia* within the context of the later debates. Bertoloni Meli's work on Leibniz's reading of the book can hardly avoid a comparison between the two methodologies, but he consciously frames the relationship as one of binary opposition. The sub-title of his book is "Newton versus Leibniz," and its opening sentence announces that his subject is "the competing world systems put forward by Newton and Leibniz in the late 1680s." Guicciardini also frames his study firmly within the context of the debate between Newtonians and Leibnizians, dividing his book into chapters on the two schools that emerged following the publication of the *Principia*, one in Britain in which "Newton surrounded himself with a small group of mathematicians with whom he shared his discoveries," and another on the continent who had "a clearly stated programme: Newton's demonstrations had to be translated into Leibnizian language." To be clear, I do not wish to challenge the overwhelming majority of Bertoloni Meli's and Guicciardini's conclusions. In particular, I acknowledge Guicciardini's important argument that the division into two schools was far from absolute, and that they had many methods and aims in common. It is merely their perpetuation of the trope that the reception of the *Principia* can only be viewed from within the context of the Newton-Leibniz dispute to which I wish to draw attention. I suggest that this is a choice, not a requirement. Historians are not obliged to examine early readings of the book exclusively with reference to events that took place ten, twenty or thirty years later.[51]

The reason for drawing attention to this trope is that it encourages two assumptions that this thesis aims to challenge. The first is the that only Leibniz and his followers were sanctioned to disagree with the mathematical arguments in the *Principia*. The Leibnizians were Newton's only worthy disputants, this trope suggests, and only they were allowed to deny the validity of the claims in his book. In mathematical terms, the trope implies that the only reason for disagreeing with Newton's proofs was an ideological commitment to infinitesimals. It allows historians to overlook the possibility that individual readers who were sympathetic to Newton's geometrical approach might nevertheless think that some of the proofs in the *Principia* were faulty. And the second danger of the trope is that its emphasis on a programme of "translation" misrepresents the way in which the book was read. As the documentary evidence will show, many early readers did indeed process the *Principia*'s geometrical proofs by reformulating them in analytical terms. But they did not read Newton's proofs in his terms first and then afterwards translate them into infinitesimals. Rather, they read Newton's geometrical limit theorems in terms of infinitesimals. They responded to the geometrical demonstrations printed on the pages of the book by reconstructing them in analytical terms on the pages of

[51] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 1; Guicciardini, *Reading the Principia*, 3, 169, 195.

their notes. These readers did not translate the proofs into infinitesimals as a point of ideological principle, but because that was the only way they were able to process the arguments in the text. Translation was thus integral to the act of reading: it was a function of the way the book was read, not a result of the ideologies of the competing schools.

My approach in this thesis will be much more aligned with that articulated in Guicciardini's most recent paper on Gregory's assimilation of Newton's fluxional methods: there were not two ways in which contemporary readers could process the arguments in Newton's text, but many.[52] My goal is not to situate the reception of the *Principia* within the context of the Newton-Leibniz debate, but to examine whether its earliest readers were persuaded by the arguments it contained. The ways in which they engaged with the text were naturally influenced by the wider communities within which they operated, but I will avoid the vocabulary of a debate between two competing schools, and focus solely on whether the text persuaded its individual readers as and when they sat down to read it. My aim is to establish who those early readers were, and the extent to which they were or were not persuaded by its arguments they found printed its pages. It is not my aim to retell the story of the publication of the *Principia* in 1687 from the retrospective viewpoint of the *Commercium Epistolicum* of 1712.

The *Principia* was a "masterpiece"

The final trope in the secondary literature to which this thesis seeks to draw attention is also the most deeply embedded: the routine description of the *Principia* as a "masterpiece." Like the adjective "difficult", this epithet is so

[52] Guicciardini, "David Gregory's manuscript 'Isaaci Neutoni Methodus fluxionum' (1694): A study on the early publication of Newton's discoveries on calculus."

ubiquitously applied to Newton's book that examples are unnecessary; and like "difficult" I do not wish to make the strong argument that this word is never appropriate. I merely wish to draw attention to its habitual and reflexive use. I wish to encourage critical evaluation of what Newton scholars mean when they automatically describe his book in this way. Used in the historical sense of a piece of work produced to gain admittance to a guild-like group of peers, it may just about be defensible that the *Principia* was Newton's "masterpiece," since its publication successfully raised the status of its author and advanced his professional prospects (although this leaves unanswered the questions of who the guild and what the criteria of admittance were). However, I do not think this is the meaning most historians intend. I think they mean the word to be understood less narrowly, and simply wish to convey the outstanding brilliance of Newton's book. The importance of Newton's discoveries between 1684 and 1687 and the developments they stimulated over the subsequent decades and centuries are hardly deniable. But to unthinkingly describe the book itself as a "masterpiece" is, I suggest, to ignore the *Principia*'s many shortcomings. It was poorly structured and confusingly written. Very few contemporary readers were able to access its contents, and even then only in part. They were habitually forced to reconstruct its arguments in terms different from those that appeared in the text, and a full understanding of many of its passages does not appear to have been possible without a face-to-face conversation with the author. Readers commonly objected to much of what it contained. Critical passages had to be rewritten because they were faulty or unclear. Its most important readers thought its most important arguments were faulty. As a rhetorical tool it was extremely ineffective, since it successfully persuaded only a tiny group of individuals of a very small number of the claims it contained. It is not clear to me that "masterpiece" is the right word to describe a book with these properties. Historians who adopt it should at the very least be challenged to articulate why they think it is the appropriate term to use: this thesis will not shirk from emphasising the many defects of Newton's book, and aims to critically examine the effect these had on its reception.

Structure of Thesis

The structure of my thesis will be thematic. In Chapter 1 I will attempt a complete chronology of known readings of the *Principia* before 1694, detailing all the extant evidence that Newton's book was studied during these years, and identifying which sections of the text were read, by whom, and when. Chapter 2 will examine in detail what the act of reading the *Principia* involved: I will argue that there were many different ways in which readers engaged with the text, but that they all entailed reconstructing Newton's prose in symbolic form, often using different notation or different foundational concepts. Chapter 3 will interrogate the trope that Newton's book was difficult to understand, and will use examples from contemporary reading notes to specify those qualities of the text that rendered it ineffective as a tool of persuasion. Having thus provided a general characterisation of ways in which the book was read, the two final chapters will detail the lack of assent to the novel claims Newton made in his book. Chapter 4 will show that readers were not persuaded by the new mathematical methods presented in the *Principia* (which is to say, Newton's geometrical conception of limit as formulated in his method of first and last ratios) and either demanded that he re-write his proofs, or generated their own using alternative methodologies. And Chapter 5 will show that readers were not persuaded by the new physical claims made in the *Principia* – they disputed the articulation of his foundational concepts, and denied that he had provided the only acceptable mathematisation of force.

Guidance for the Reader

As is appropriate for a study of a text in which the quantity of difficult mathematics both was and is perceived to be an obstacle to comprehension, I have given careful thought to how I present my mathematical analysis in this thesis. A close reading of mathematical manuscripts will be central to my overall argument. However, I have deliberately structured this thesis so that it can be profitably studied by any reader who is not confident engaging with the analytical details. This General Introduction, Chapter 1, the Introductions to Chapters 2, 3, 4 and 5, and the Conclusion are all entirely free of mathematical content. Taken together, these passages are intended to comprise a coherent summary of the conclusions of my study, and may be read without difficulty by any non-mathematical reader.

That said, I have tried very hard to render my discussion of the mathematics in this thesis as clearly and accessibly as possible. As discussed above, I do not wish to suggest that the demonstrations in the *Principia* are all trivially easy. But I am strongly of the view that the perceived difficulty of the book should not be allowed to act as an excuse for historians not to engage with any of its mathematical detail. I have taught mathematics in UK schools for twenty years, and it seems to me that many of the important demonstrations in the *Principia* – including most of the results discussed in this thesis – are accessible to A Level students, and often require no knowledge beyond GCSE. I have accordingly pitched my mathematical explanations at the level of an alert sixteen-year-old. The reader who has not previously engaged with the mathematics of the *Principia* will find a sympathetic guide in Densmore's *Newton's* Principia*: The Central Argument*, whose commentary is warmly recommended (with the important caveat that it is based on the text of the third edition, not the first). I have often re-written seventeenth-century proportions using modern notation so that they may more easily be understood, but the role of such reconstructions in interpreting the printed text is a central theme of this thesis, and the extent to which they threaten to obscure significant distinctions will be explicitly addressed. There will be some instances when the reader who has received an undergraduate education in the epsilon-delta definition of a limit will be at an advantage, but I have not taken this knowledge for granted.

I have assumed throughout that my reader has to hand a modern translation of the third edition such as that by Cohen and Whitman, which I will often advise them to consult.[53] Whenever the text of third edition significantly differs from the first, I will highlight this in my discussion. I will also make frequent references to the annotated copy in which Newton recorded changes to the text after publication, which has been made available to view online by the Wren Library, Cambridge.[54] A clean copy of the first edition without the annotations, which the reader may also occasionally wish to view, has been provided online by the Library of Congress, Washington.[55] The images of diagrams and text I have used in this document are generally taken from this copy.

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[53] Newton, *The* Principia*: Mathematical Principles of Natural Philosophy*.
[54] https://mss-cat.trin.cam.ac.uk/manuscripts/uv/view.php?n=NQ.16.200
[55] https://www.loc.gov/resource/rbc0001.2013gen20872/?sp=11

I have not always provided English translations of quotations from primary sources, but have done so whenever I have judged it helpful for the reader, particularly with longer passages. It is important to emphasise that these translations are never meant to be elegant paraphrases of their author's intended sense, but literal renderings of the text on which they are based. The aim of my translations, where I have provided them, is to support the reader in their comprehension of the original, in parallel with which they are always intended to be read.

Finally, I wish to thank the prospective reader of this thesis for their time and consideration. I welcome constructive discussion of its contents, and look forward to being able to develop those ideas that are considered to be valuable, and acknowledge the faults in those that are not. In other words, I will gladly accept the opportunity to correct – or negotiate – the arguments as they appear in this text. Indeed, I cannot do better than to quote the words Newton used to end his own preface to the 1687 edition, presented here alongside Cohen and Whitman's translation:

> Ut omnia candide legantur, & defectus, in materiatam difficili non tam reprehendantur, quam novis Lectorum conatibus investigentur, & benigne suppleantur, enixe rogo.
>
> I earnestly ask that everything be read with an open mind and that the defects in a subject so difficult may be not so much reprehended as investigated, and kindly supplemented, by new endeavors of my readers.

1. A chronology of early readings of the *Principia*

1.1 Introduction

The purpose of this chapter is to provide a detailed chronology of known early readings of the *Principia*. I will delay an examination of responses to the arguments in the text until later: my present aim is to establish in as much detail as possible which readers read which passages, in what way, and when, and to specify the primary sources on which the analysis in subsequent chapters will be based. Such a chronology is necessary here for the simple reason that none currently exists in the secondary literature. The two standard works on this subject are Guicciardini's *Reading the Principia* of 1999, and Cohen's *Introduction to Newton's Principia* of 1971, to both of which this research is heavily indebted. Nevertheless, both Cohen and Guicciardini contain misdescriptions and omissions that have not been identified by other scholars in the decades since their studies were published.

The primary sources I have examined for evidence of engagement with the book are of four types. The first are personal reading notes. These take many forms: Leibniz, Flamsteed and Fatio left notes in the margins of their copies; Huygens and Gregory worked through individual propositions on separate pieces of rough paper; Halley and Fatio wrote out sheets of editorial feedback to be read by Newton; and Locke made notes in commonplace books. In whatever form they appear, such notes are the most valuable source of evidence of early readings, since they provide a direct and detailed record of how individuals engaged with the text. In Chapter 2 the contents of these documents will be examined; my aim here is merely to identify them in the archive and place them chronologically.

Second, evidence of engagement with the text can often be found in contemporary correspondence. In their exchanges with each other – or with the author directly – Newton's peers discussed both general themes of the *Principia* and specific results in the text. It is rare that letters contain detailed reference to the validity of individual mathematical proofs, and in that sense correspondence provides a less precise record of engagement with the text than reading notes: when the topic of *Principia* arises in contemporary correspondence, it is generally to discuss the implications of Newton's conclusions rather than the validity of the arguments which led to them. Nevertheless, such references will provide important clues as to which sections of the book were read, and when.

Third, engagement with the text of the *Principia* can sometimes be inferred from treatises, tracts and articles (either published or unpublished) subsequently composed by Newton's peers. In many ways this is the most important type of evidence for a history of the reception of the *Principia* to consider, but it is by far the most complex. It is also highly individualised, and the extent to which Newton's ideas were developed in the later work of his readers varies widely. Leibniz's response to Newton's book is the theme of Bertoloni Meli's *Equivalence and Priority*; Fatio appears to have gone no further on his own than proposing a physical mechanism for gravity; the impact of Newton's physics on Halley and Flamsteed's astronomy is not considered to be of great importance by their biographers; Clerke and Locke produced no mathematical physics of their own; Huygens had little time to develop the ideas he read about in the *Principia* before he died in 1695; and the extremely significant effect of the *Principia* on the subsequent work of Gregory undoubtedly merits further research, but his archives are too large to be encompassed by this thesis.

The final category of primary evidence to be considered are the changes Newton made to the text of the *Principia* in response to suggestions from his peers. As related in the Introduction above, it will be a theme of this thesis that as a natural outcome of the process of studying it, many of the book's readers – Halley, Clerke, Gregory, Fatio – proposed changes to the text, which were often accepted by Newton for inclusion in subsequent editions. He recorded these edits in two copies of the first edition he kept for this express purpose: the "annotated" copy now in the Wren Library,[56] and the "interleaved" copy (so-called because it was bound with specially inserted blank pages for additional notes) in the Cambridge University Library.[57] Newton's usual habit was to mark changes in both, and so although there are many differences, the edits in the two copies are generally extremely similar. And although the changes are individually undated, some chronological information can be inferred because when Newton gave out copies of his book in the years after publication, he often transferred whatever edits had been collected up to that point into the presentation copy. This happened, for example, when he gave a copy to Locke in 1690. When combined with the reading notes referred to above, these changes will reveal which passages were read and by whom, and will in subsequent chapters enable detailed examination of readers' responses to Newton's arguments.

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[56] Cambridge, Trinity College, NQ.16.200.
[57] CUL, Adv.b.39.1.

Of course, it cannot be claimed that the sources listed in this chapter constitute a complete record of all readings of the text before 1694. For example, correspondence strongly suggests that Hooke, Wallis, Petty, Boyle, Paget, de Moivre, Craig, Bentley, Campbell and Molyneux all engaged with the *Principia* to some degree in the years immediately following publication. I have, however, been unable to uncover any documentation recording the details of their study, and for this reason they are not represented here.[58] An unknown number of other contemporary readings will either not have generated any material evidence, or generated material evidence that has since been lost. Feingold and Svorenčík's forthcoming publications will be the best source of an estimate for their quantity and character. However, this thesis claims to attempt a fuller analysis of the extant primary sources than is currently available in the secondary literature. With the exception of Leibniz's reading notes (for which I have relied on Bertoloni Meli's transcription and analysis) and Gregory's *Notae* (of which I have only been able to study the opening sections, as detailed below), I have closely examined all the primary evidence concerning early readings of the *Principia* of which I am aware.

A summary of the chronology provided in this chapter is presented in the table below. It makes no claim to completeness or absolute precision, but it is intended to be usefully indicative.[59] Its purpose is to discourage historians from thinking of "reading the *Principia*" as a single, continuous act conducted in one sitting at the moment of publication, and to focus attention on which passages

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[58] See Feingold and Svorenčík, "A preliminary census of copies of the first edition of Newton's *Principia* (1687)," 256–8; Iliffe, "Butter for Parsnips," 48–56.

[59] I am grateful to Niccolò Guicciardini for alerting me to one omission from this table, a commentary in Fatio's hand (mostly dated 1691) held at the Royal Society in London, MS/64. I was unaware of the existence of this document while conducting my doctoral research.

were studied by which readers, in what way, and when. It highlights the overall conclusion of this chapter, which is that early readers almost always engaged with Newton's text piecemeal. They read an assortment of passages, in different ways, in a variety of orders, over a number of years after publication. I have chosen to present the contents of this chapter chronologically to emphasise exactly this point. Early readings of the text were usually scattered and disjointed. It will become apparent that readers commonly engaged with the opening fifty pages of Book 1 (containing the Definitions, Laws, the method of first and last ratios in Section 1, and the main orbital force theorems in Sections 2 and 3), along with the entirety of Book 3. All the evidence suggests that these passages were the most frequently read. This is consistent with the advice Newton gave in a letter to Bentley in 1691 that "When you have read the first 60 pages, pass on to the 3d Book," and also the instruction to the reader at the start of Book 3.[60] But I have found no written record of this instruction before the letter to Bentley, and cannot see how a reader was supposed to know that before doing anything else they needed to turn to page 401 to find out how the author intended his book to be read: perhaps this instruction was communicated by word of mouth. It was anyway far from an established pattern. Huygens, for example, appears initially to have dipped in to examine individual results relating to his particular research interests, before being stimulated to examine other passages when he came across Leibniz's references to them in the *Acta Eruditorum*. Flamsteed, I will argue, set out to work through the opening sections in detail, but gave up and skipped ahead to the climax of the inverse-

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[60] *NC*, 3:155–6.

square law. Importantly, we will see that even when readers paid attention to the details of a particular mathematical proof, it was rare that they examined every step in the logical chain leading up to its conclusion. The only reader for whom there is reliable evidence of a complete and careful scrutiny of the entire text is Gregory. Halley may have done so too, although the balance of evidence suggests otherwise, and it is likely that Fatio began such a project but did not see it through. Leibniz appears to have read most of the book, but – so far as we can tell from the extant evidence – did so without checking all the proofs. This puts him in the same category as Locke. Huygens comes out very badly from my analysis, which finds little evidence that he engaged with many of Newton's proofs in any detail, along with strong indications that he studied much less of the book than the secondary literature is willing to admit. The responses of these individuals to the substance of Newton's arguments will be the subject of later chapters: my aim here is simply to demonstrate that anyone who wishes to explain how the *Principia* came to be acclaimed so quickly when it was published in the summer of 1687 needs to account for the fact that early readings of the text by Newton's peers were generally haphazard, non-continuous, unsystematic and incomplete.

Who read what, when, and how

While every effort has been made to make this information as accurate as possible, it makes no claim to precision or completeness. It is intended to show that the readers examined in this thesis read different passages, in different ways, at different times.

		Halley	Clerke	Locke	Flamsteed	Gregory	Huygens	Locke	Huygens	Leibniz	Leibniz	Huygens	Leibniz	Leibniz	Fatio	Locke	Fatio	Gregory
		Editorial notes	Letters to Newton	1st reading notes	Marginalia	*Notae*	Calculations for shape of the earth	2nd reading notes	Notes on centres of gravity	*Marginalia*	*Notes*	Reading notes	First set of *Excerpts*	Second set of *Excerpts*	Editorial notes	3rd reading notes	Marginalia	*Notae*
		1685–86	Sep – Nov 87	Sep 87	Oct – Dec 87	Sep 87 – Apr 88	by Nov 87	Mar 88	by 1688	autumn 1688	autumn 1688	Feb – Apr 89	Apr – Nov 89	Apr – Nov 89	probably before 13 Mar 1690	1691	maybe Mar 90; definitely by 29 Apr 92	Dec 92 – Jan 94
	Definitions																	
	Laws																	
Book 1	Section 1												Lemmas 9–11					
	Section 2									Props 4, 10	Props 1, 3	Props 6, 9	Prop 6					
	Section 3									Prop 11								
	Section 4																	
	Section 5										Lemma 22							
	Section 6									Lemma 28	Lemma 28			Lemma 28. Prop 31				
	Section 7										Prop 39						Prop 38, 39	
	Section 8													Prop 40				
	Section 9										Prop 45			Prop 44				
	Section 10																	
	Section 11												Intro	all except Props 62–64				
	Section 12																	
	Section 13																	
	Section 14																	
Book 2	Section 1																	
	Section 2									Props 5–9	Prop 10	Props 5 and 8, Lemma 2		up to Prop 8				
	Section 3																	
	Section 4																	
	Section 5												Prop 23					
	Section 6																	
	Section 7												Prop 40 Scholium					
	Section 8												Scholium after Prop 50					
	Section 9									Prop 51			Prop 52-53					
Book 3							Prop 19			Props 6–10	Prop 40		All		Lemma 4 onwards			

Hard evidence of detailed study	
Hard evidence of cursory reading	
Reasonable assumption of (at least) cursory reading	
If a particular result if named, only this result was studied in that Section	Prop 40

1.2 Halley's editorial notes, 1685–86

The story of Halley's role as midwife to the *Principia* has often been told. He prompted the book's composition, funded its printing, broadcast its importance among his network of correspondents, and managed the mood-swings of its famously truculent author. Uniquely qualified to undertake such a forbidding task, Halley was improbably well-equipped with the disparate skills the work required. He had experience of dealing with printers and booksellers, was at the centre of intellectual life in London, had established academic connections on the continent, could win the trust and then handle the ego of his notoriously irascible author, was one of the few men in the country capable of understanding the mathematical proofs, and possessed the youth, energy and ambition to drive the job to completion. We may well agree with De Morgan that "But for him in all probability, the work would not have been thought of, nor when thought of written, nor when written printed."[61] However, historians have been curiously uninterested in Halley's role as the *Principia*'s first reader. His response to the content of the text, and his acceptance of Newton's arguments – on the occasions that it has been considered at all in the secondary literature – has been uncritically taken for granted.

This silence is not due to a lack of available evidence. It is clear that Halley read various draft sections of the *Principia* as it was being composed, because in the Portsmouth Collection at the Cambridge University Library there are six sheets of commentary located at MS Add. 3965, 94–99. These were first identified by Cohen in his *Introduction* of 1971, having received confirmation

[61] De Morgan, "Halley," in The Cabinet Portrait Gallery of British Worthies, 11:12, London: Charles Knight, 1847, quoted in Cook, *Edmond Halley: Charting the Heavens and the Seas*, 178.

from Whiteside that they were by Halley. From them Cohen inferred the existence of a now-lost early draft of the book "containing Definitions, Laws of Motion, and either all or great parts of the three books comprising the *Principia* as we know it." Westfall's biography referred to the notes only in passing; Whiteside dismissed them as a "not very percipient critique by Halley"; and Cook's brief report stated that "Halley contributed nothing to [the *Principia*'s] composition. Although he was very impressed by it, he was clearly surprised by each new development as Newton unfolded it, and probably did not fully understand it at the time."[62]

The notes occupy six folios, all written on the recto side only. The sheets are discontinuous and self-contained, with no overflow from one page to the next. Between them, their commentary covers draft versions of the Definitions and Laws, the first four sections of Book 1, the start of Book 2, parts of Book 3, and various other passages. In total, the text covered represents just under half of the final book, which strongly suggests that these six extant leaves were once part of a larger batch of notes. Assuming that Halley saw drafts of all the remaining sections, and that he produced a volume of commentary roughly in proportion to the number of pages he read, we can speculate that Halley originally produced a total of a dozen or fifteen pages of notes, of which those in MS Add. 3965 represent the incomplete remains.

The various draft sections of the *Principia* from which Halley made these notes mostly no longer exist. Only two substantial parts remain, both of which

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[62] Cohen, *Introduction*, 122–124; Westfall, *Never at Rest*, 436; Cohen, *Introduction*, 337; Newton, *The Preliminary Manuscripts for Isaac Newton's 1687* Principia, xvii; Cook, "Halley, Edmond (1656–1742)."

are now in the University Library at Cambridge.[63] The first is a set of drafts of parts of Book 1, at MS Dd.9.46. These documents were submitted to the University archives by Newton himself, since under the terms of his Lucasian Professorship he was obliged to deposit the text of his weekly lectures to the Library. For this reason they are usually referred to as the "Lucasian Lectures." They are not a record of his teaching, however, but rather a collection of working drafts of the beginning of the *Principia*, retrospectively dressed up and dated to look as if they had been delivered as lectures. They are in the hand of Newton's amanuensis Humphrey Newton, and although some parts may have been dictated, the majority are likely to have been copied out from earlier drafts long since discarded.[64] In many locations Newton has subsequently written on or edited Humphrey's text himself. The whole is an incomplete and disordered jumble of just over one hundred pages covering the Definitions, Laws of Motion, and perhaps the first two-thirds of Book 1. Cohen demonstrated that their contents had originally been grouped into two successive drafts of the opening sections of the *Principia*, which he labelled LL_α and LL_β. Some pages of the first draft LL_α were transferred with small revisions into the second draft LL_β, while others were completely re-written, and yet more newly composed.[65] The final manuscript of Book 1, which Halley took to the printer and is now held in the library of the Royal Society, is very similar to LL_β, which therefore represents a very late stage in the editorial process. Whiteside transcribed and translated

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63 The University Library at Cambridge holds a small number of other draft sections of the *Principia* which do not concern us here, because they have no bearing on Halley's notes. They mostly comprise short bundles of pages pertaining to individual propositions. Chapter IV of Cohen's *Introduction* contains a full catalogue.

64 See Cohen, *Introduction*, 77n4.

65 Cohen tracked down a further eight leaves archived at CUL, MS Add. 3965, 7–14 that also belong to the first draft LL_α.

sections of the manuscripts in Volume 6 of his *Mathematical Papers*, and then published facsimiles in 1989 under the title *Preliminary Manuscripts for Isaac Newton's 1687* Principia*, 1684-1685*.

The second set of manuscripts is an early version of what formed the basis of Book 3, now at MS Add. 3990. It bears the title "De Motu Corporum, Liber Secundus," indicating that it was composed before Newton decided to split the first book in two. In fact, in can fairly confidently be dated to the autumn of 1685, since it contains data obtained from Flamsteed in September of that year. Furthermore, the following summer Newton told Halley that his theory of comets was still unfinished and that "In Autumn last I spent two months in calculations to no purpose for want of a good method, wch made me afterwards return to the first Book & enlarge it wth divers Propositions."[66] An extract of MS Add. 3990 appeared in Whiteside's *Mathematical Papers* in 1974, and images of the bound manuscript are now available to view online; the document was also published in full by John Conduit in 1728, soon after Newton died.[67] Like MS Dd.9.46, MS Add. 3990 is in Humphrey's hand, with revisions and emendations inserted by Newton. Unlike MS Dd.9.46, however, Halley did not see the pages of MS Add. 3990: he read a later draft of the same book.

These documents enable us to date Halley's readings of the drafts quite precisely. The proposition numbers of LL_{α} match MS Add. 3990 but are different from those in LL_{β}, suggesting that Newton wrote MS Add. 3990 with or just after LL_{α}, which he subsequently expanded into LL_{β}. LL_{α} grew out of the *De Motu* Newton prepared towards the end of 1684, which is why Whiteside placed LL_{α}

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[66] Newton to Halley, 29 Jun. 1686, *NC*, 2:437. For corroboration of this date, see also Westfall, *Never at Rest,* 433–34 and 437, and *NMP*, 6:xxxii, 481.
[67] *NMP*, 6:481–96. Accessed 24 June 2021 at https://cudl.lib.cam.ac.uk/view/MS-ADD-03990/9.

in “winter/early spring 1684–5” or “early summer,”[68] and LL_{β} soon after MS Add. 3990 in the autumn. The six pages of Halley’s notes in MS Add. 3965 ff. 95–99 refer to a variety of drafts, most of which no longer exist. The notes on f. 95 refer to a now-lost draft of the Laws of Motion, but must have been made prior to the second draft LL_{β}, because Newton in many cases incorporated Halley’s suggestions into the text of LL_{β}. The notes on f. 99 relate to a non-extant draft of later sections of Book 1, but must also date from before LL_{β}, because the proposition numbers Halley uses are different. And Halley’s notes on f. 97 can be dated with certainty to between LL_{α} and LL_{β}, because his page and line references match LL_{α}, and because Newton implemented most of Halley’s suggestions directly onto the pages of LL_{α}, after which they were copied into LL_{β}. It is also clear that Halley made the notes on f. 94 after reading LL_{β}, because the page and line numbers match, and because Newton inserted many of his suggested alterations onto their pages. The same is true of the final two notes on f. 95, which were inserted later than the other comments on that page. The comments on f. 96 refer to a non-extant preliminary version of what was ultimately published as Book 2. Newton had originally planned the work to comprise only two books, but at some unknown point as it expanded during composition, decided to split the first book into two. The notes on f. 96 refer to the proposition numbers after this division had been made, which means they must have been made after 20 June 1686, when Newton informed him of the change. And finally, the notes on f. 98 refer to a draft of what was ultimately published as Book 3, but because the manuscript on which it was based is no

[68] *NMP*, 6:xxvi, 21.

longer extant, and because there is nothing to indicate whether they were made before or after the first book was split into two, they cannot be dated with confidence. All that can be inferred from the page and line numbers Halley used is that the version he saw must have been after MS Add. 3990, the format and numeration of which indicate that it was composed to accompany LL_{α}. This suggests that Halley was likely to have made these notes after those on f. 97, which are based on sections of LL_{α}. But there is no firm evidence for this: the notes on f. 98 could have been made at any stage in the process.

To summarise:

	LL_{α} composed (spring 1685?)
f. 95	the MS on which these notes are based is lost, but Halley's comments are marked into LL_{β}
f. 99	also based on a now-lost MS, but the proposition numbers show it pre-dates LL_{β}
f. 97	Halley read LL_{α} and Newton inserted corrections into LL_{α} (and, in two places, LL_{β})
	LL_{β} composed (autumn 1685?)
f. 94	Halley read LL_{β} and Newton inserted corrections into LL_{β}
f. 95	the two final notes on f. 95 are based on LL_{β}, and Newton corrected LL_{β}
	Newton tells Halley there will be three books not two (20 June 1686)
f. 96	based on a non-extant draft of the final book, but the proposition numbers date it after the first book was split in two
	It is not possible to date f. 98, which is based on a non-extant draft of the final book.

The details of this chronology are much less important than its overall shape, which contains valuable information about Halley's editorial role. The most significant revelation is that Halley must have made his notes on at least three distinct occasions: once after the spring of 1685, again after the autumn, and for a third time following June 1686. It is not the case – as Cohen and Cook loosely imply[69] – that Newton invited feedback from a single provisional manuscript, which was read sequentially over the course of a few days or weeks. Instead, Halley's advice was sought piecemeal, presumably over a number of months, section by section, a few bundles of pages at a time. If it is assumed that the six extant folios in MS Add. 3965 represent perhaps one half or one third of the total feedback Halley provided, a protracted back-and-forth of draft texts between Newton and Halley over the course of 1685 and 1686 may be reasonably inferred.

There is one other important conclusion to draw. In a number of instances, Halley commented on text which Newton had added to Humphrey's original fair copy. So Halley must have read what was at the time the definitive, most up-to-date version of the text, rather than a redundant, old copy. It is also clear from f. 94 and f. 97 that Newton's habit was to make his corrections directly onto the manuscript Halley had just read. Newton handed over a few bundles of pages to read, Halley made his notes and gave them back, whereupon Newton made corrections directly onto the original, returned manuscript. It is inconceivable that this exchange could have involved long-distance delivery. Newton would have been reluctant to let the "live" version of his precious work leave his own study, let alone Cambridge: Halley must therefore have travelled up from London to visit him and read the proof sections there. This must have happened on a minimum of three separate occasions, and, depending on the extent to which the extant notes are an incomplete record of the process, maybe as many as ten. The surviving correspondence provides evidence of only one visit

[69] See Cohen, *Introduction,* 336–7.

by Halley to Newton during the printing of the *Principia*, in a letter to Flamsteed dated 3 September 1686, when Newton makes a passing reference to "Mr Halley (who was lately here)". These notes indicate that there must have been perhaps a handful more, and that Halley was a semi-regular visitor to Cambridge during these years. Halley's proof-reading duties extended over many more months, and entailed significantly more travel, than Cohen, Westfall or Cook have acknowledged.

The content of Halley's notes will be examined in detail in subsequent chapters. I will show in Chapter 2 that the majority of his comments constitute small alterations to Newton's grammar or phrasing, and that the largest part of his feedback concerns syntax, morphology, and the clarity of Newton's wording. That said, in some instances the precise phrasing of the text is of considerable importance: Chapter 4 will discuss Halley's proposed tweaks to the wording of various limit results, and in Chapter 5 I will demonstrate how he also made alterations to the definitions of mass, inertia, centripetal force, and relative and absolute space. These are critically important passages, in which the foundational methods and concepts of modern science were being articulated: and yet the evidence unambiguously demonstrates that Halley was granted editorial access to them by his usually diffident author.

The key question of the extent to which Halley worked through and verified Newton's mathematical proofs is harder to judge. I will present clear evidence that he must have followed at least some of the technical arguments very closely, since he picked up on occasional errors. But on the other hand, a comparison of his notes with those of Gregory and Fatio – who both set out to achieve complete readings of the text – suggests it is unlikely that he subjected every demonstration to close scrutiny. Gregory took the best part of two years to work his way through the whole book; but Halley only had a few months. Gregory made hundreds of pages of dense notes, and Fatio made detailed annotations on dozens of pages; assuming that the extant sheets comprise very roughly one half of his total feedback, Halley produced only twelve or fifteen pages of observations in total. Furthermore, if he did process every result, Halley did so in a way that failed to detect the many errors that passed through to be printed. And there is no sign in the correspondence that Gregory, Fatio or anyone else thought that Halley had worked through all the proofs. If they had done so, they might well have asked him for help in their own endeavours, or spread the word that he knew the text as well as Newton. At the very least they might have found an opportunity to express their admiration. While it is possible that Halley fully verified every one of the proofs as they were produced, therefore, for him to have done so in the time available would have been not much less of an achievement than Newton's in composing them.

I suggest that Halley superficially looked through all the mathematics for obvious errors, but did not subject all of it to critical analysis. He read the text, but probably without checking all the mathematics. If this inference is correct, then it is revealing something very important about the reception of the *Principia* – because it indicates that Halley had already placed his trust in the correctness of Newton's claims before he read his text. His notes at MS Add. 3965 are not the initial responses of a sceptical reader being convinced for the first time of the surprising results the book contains. They are, for the most part, superficial tweaks in the late stages immediately before publication. They are the comments of an editor who has already chosen to attach his professional reputation to the central argument of his author. If it is right to assume that Halley endorsed the surprising and revolutionary assertions Newton was making in the second half of the 1680s, these notes show that it was not the act of studying his geometrical proofs that caused him to be persuaded. Halley's assent to Newton's claims was not contingent on the validity of the mathematical arguments contained in his book.

1.3 Clerke's letters to Newton, September – November 1687

Among the earliest readers of the book after its publication in the summer of 1687 was the retired theologian Gilbert Clerke. The son of the headmaster of Uppingham School, Clerke had studied at Sidney Sussex, Cambridge, in the 1640s, where he claimed to have worked alongside Isaac Barrow. He had subsequently been elected to a fellowship, but was forced to resign in 1655 owing to his Unitarian views. He returned to Northamptonshire, and was taken into employment by the politician (and early member of the Royal Society) Sir Justinian Isham, tutoring his children and running his estate at Lamport Hall. Here he composed a handful of short tracts about natural philosophy, theology and mathematics, and made observations of the Great Comet of 1680–81. At the time of publication of the *Principia*, he was sixty one years old.

Otherwise of no importance to historians, Clerke exchanged a series of letters with Newton between September and November of 1687, in which he sought clarification of various matters in the opening sections of the book. This correspondence comprises five letters, all now held by the Dibner Library and housed in the Smithsonian, Washington. All five were published in Volume 2 of

The Correspondence of Isaac Newton, and are also accessible online.[70] Four of them were sent by Clerke to Newton, dated 26 September, 3 October, 7 November, and 21 November, and the fifth is a draft of Newton's reply to the first. They were subsequently kept by Newton, from whose archive they ultimately made their way to the Burndy collection. Newton did not reply to the final two letters, and his response to that of 3 October has been lost.

Clerke's queries allow a fairly precise reconstruction of his engagement with the book. In his first letter he reports that he has been studying the first three sections of Book 1, and asks for help with one particular line of algebra and some of Newton's terminology. In his October reply he commits to embarking on Book 3, and in November he reports attempting to tackle Section 11 and "some of the foregoeing sections." By the time of his final letter, he has had the opportunity to study Section 1 of Book 1 on the method of first and last ratios, and raises a number of objections to Newton's limit proofs. In total Clerke therefore read the first sixty pages, Book 3, and possibly also some other passages. It will become clear in subsequent chapters that he studied many of the proofs in meticulous detail, paying close attention to the validity of Newton's mathematical arguments.

To the extent that these letters are mentioned in the secondary literature at all, it is generally to dismiss Clerke as a naïve and ill-informed old man. Sylla reports that he was "a mathematician of modest ability," and Whiteside that he was an "aged correspondent" who pestered Newton with "tiresome and near-trivial 'scruples' regarding the opening pages of his book."[71] Clerke's tone is

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[70] *NC*, 2:485–96.
[71] Sylla, "Compounding ratios," 12; *NMP*, 6:xxi.

indeed humble and apologetic throughout the exchange, and he admits to what appears to be a very basic misunderstanding in his first letter. But I shall argue in Chapter 3 that Clerke's complaints were far from atypical among Newton's contemporaries. He was not the only reader of the *Principia* who had difficulty recreating its prose demonstrations in algebraic form, and not the only reader who contested its terminology. Furthermore, his final letter has received almost no attention from the secondary literature, despite it containing his most important criticisms. I will show in Chapter 4 that Clerke's objections to the method of first and last ratios were among a range of complaints raised by Newton's readers against the formulation of his limit arguments, and Newton amended his text in direct response to Clerke's comments. I shall argue that the condescension with which Clerke is treated is undeserved: the queries he raises are much closer in substance to those of his better-known peers than historians have been prepared to acknowledge.

1.4 Locke's first reading, September 1687

John Locke has always played a very particular role in histories of early readers of the *Principia*. For three centuries he has been presented as the exemplar of a well-intentioned but technically incapable class of individual who tried but failed to understand it. Even during Newton's lifetime, the story spread that Locke had attempted to read Newton's book, but could not follow the mathematics. He did his best to understand Newton's physics – so the rumour went – but had to ask others to confirm the validity of his geometrical proofs. After Newton's death, Desaguliers said that he had been told "several times by Sir Isaac Newton himself" that

> The great Mr. Locke was the first who became a Newtonian Philosopher without the help of Geometry; for having asked Mr. Huygens, whether all the mathematical Propositions in Sir Isaac's *Principia* were true, and being told he might depend upon their Certainty; he took them for granted, and carefully examined the Reasonings and Corollaries drawn from them, became Master of all the Physics, and was fully convinc'd of the great Discoveries contained in that Book: Thus he also read the *Opticks* with Pleasure, acquainting himself with every thing in them that was not merely mathematical.[72]

Some years earlier, Conduitt had also recorded that "Lock took his prop[osition]s for granted on hearing Hugens say that he had proved them," crediting De Moivre as his source. Throughout his work, Locke held up geometrical reasoning as an abstract ideal to which all forms of knowledge should aspire, but the contemporary evidence suggests that he lacked the technical skills required to understand Newton's proofs.[73]

It was not until Axtell's studies in the late 1960s that a more complete picture emerged. Axtell demonstrated that Locke made three separate attempts to read the *Principia*: soon after publication in 1687; the following March, in order to compose a review for the *Bibliothèque Universelle*; and again in 1691. Axtell praised Locke's "intellectual determination" as he sustained his "earnest assault on the *Principia*," concluding that "the emphasis should be placed on the positive aspects of Locke's achievement, the mastering of 'all the Physicks' *in spite of* his mathematical handicap." He attributed Locke's rejection of Cartesianism to his readings of the *Principia*, after which he replaced his

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[72] Quoted in Locke, *The Educational Writings of John Locke*, 307.

[73] King's College Library, Keynes MS 130, quoted in Cohen, *Introduction*, 147. See also Yolton, *Locke and the Compass of Human Understanding*, 87, and Gibson, "Locke's Theory of Mathematical Knowledge and of a Possible Science of Ethics."

previous Baconian view of science with an outlook based on Newton's mathematical approach.[74]

All histories of the reception of the *Principia* since then have been based on Axtell's study. Cohen did not dwell on Locke's first or third readings, and only referred to the second in passing.[75] Westfall retold the traditional story:

> Since he was not a mathematician, he found the demonstrations impenetrable. Not to be denied, he asked Christiaan Huygens if he could trust the mathematical propositions. When Huygens assured him he could, he applied himself to the prose and digested the physics without the mathematics.[76]

Anstey bases his study of Locke's epistemology of natural philosophy on Axtell's conclusions; and Guicciardini pays very little attention to Locke.[77] What it might mean for a reader of the foundational text of mathematical physics to "digest the physics without the mathematics," no recent scholar has felt the need to explore.

I do not seek to challenge Axtell's conclusion that Locke did not follow Newton's geometrical demonstrations. However, I will demonstrate in Chapter 2 that this emphasis on Locke's mathematical inability mischaracterises Locke's relationship with the text. I will show that the details of all three of Locke's encounters with the *Principia* can be accounted for by his lifelong habit of making notes in commonplace books, as described by Richard Yeo. Notwithstanding the fact that Locke bypassed the book's mathematical content, Yeo's description of Locke's commonplacing routine accounts exactly for the

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[74] Axtell, "Locke, Newton and the two cultures," 175–7; Locke, The Educational Writings of John Locke, 74. The emphasis is Axtell's.
[75] Cohen, *Introduction*, 146.
[76] Westfall, *Never at Rest*, 470.
[77] Guicciardini, *Reading the Principia*, 176.

way he processed the text, and provides a much more complete understanding of Locke's relationship with Newton's work than Axtell was able to supply.

The primary evidence shows that Locke read the *Principia* three times. On each occasion he skipped all the mathematical sections, but read through everything else in order, making careful notes. Those three sets of notes are now in the Lovelace Collection at the Bodleian Library, and they are all are dated. In September 1687, shortly after publication, he made the notes now at MS Locke c. 33, 19–20; in March 1688, he took the notes in MS Locke c. 31, 99–100 and used them to write a review in the *Bibliothèque Universelle*; and in 1691, during the early years of his friendship with Newton, he made notes in MS Locke d. 9. On this final occasion, he also had access to a four-page simplified proof of the inverse-square law (at MS Locke c. 31, 101–4) and an updated copy of the *Principia* (now in the Wren Library of Trinity College, Cambridge, at Adv.b.1.6), both of which had been given to him by Newton.

The circumstances of his second and third readings will be considered below. On the occasion of the first, he was living in Rotterdam, having fled London in 1683 owing to suspicions about his involvement in the Rye House Plot. By 1687 he was staying with the merchant Benjamin Furly, whose copy of Newton's book we may assume Locke read in September, making notes on individual sheets that are now mounted in MS Locke c.33, 99–100. Their structure and contents, and what they reveal of Locke's process of engagement with Newton's text, will be examined in Chapter 2; the circumstances of his second reading six months later will be discussed below.

1.5 Flamsteed's marginalia, October – December 1687

Newton and Flamsteed had known each other for over fifteen years when the *Principia* was published in the summer of 1687. The Astronomer Royal had provided Newton with data for the book during its preparation, so it was natural that Flamsteed would be presented with a copy upon publication. This copy is now in the library of the Royal Society (RCN 18577), to which it was presented in 1853 by John Baily from the collection of his late uncle, Francis Baily. It contains the marginal notes Flamsteed made when he read various parts of Book 1 and Book 3 in the autumn of 1687. Mentioned only in passing by Cohen and Flamsteed biographers, these marginalia have never been the subject of a historical study.

Flamsteed's correspondence usefully provides a timeline of his engagement with the *Principia*. As it was going through the press he referred to its imminent publication in letters to William Molyneux and Richard Towneley, reporting that it contained mathematical proofs of the planetary orbit laws "which Kepler found out first but could assigne no reason for but what was drawn from Experiment."[78] It is not known exactly when and how Flamsteed received his copy from Newton, but in October 1687 he wrote to Gottfried Kirch, recommending the book as containing "many most useful propositions and geometrical theorems" in which "the author demonstrates what the most wise Kepler once discovered by means of lengthy toil."[79] It is unclear from this letter whether Flamsteed had yet had the opportunity to undertake any study of book, or was merely reporting what he understood to be its contents as he had to

[78] Flamsteed, *The Correspondence of John Flamsteed*, 2:350, 2:297–8.
[79] Flamsteed, *The Correspondence of John Flamsteed*, 2:363.

Towneley. Helpfully, his next letter to Molyneux, on 19 December, is much more specific:

> I should be glad to heare what opinion your friends have of Mr Newtons booke I have mastered 60 pages. The rest I have gone onely cursorily through. When our dayes lengthen I shall set upon the rest.[80]

And he repeats this assertion to Molyneux the following June:

> I have onely read some 60 pages of Newton. after which I found the most material parte of that booke beeing mastered the rest would be easy. but it cost me many dayes paines to get through them. and yet I thinke my time well requited.

The marginal annotations in his first edition broadly support Flamsteed's claim that he read most of the first 60 pages. Or, at least, he attempted to: he made detailed mathematical notes alongside Corollary 2 to Law 3 (concerning the resolution of forces into components); Proposition 4 (the centripetal force required to maintain a circular orbit); Proposition 10 (the force required to maintain an elliptical orbit if directed towards the centre); Proposition 11 (the force required to maintain an elliptical orbit if directed towards a focus); Lemma 14 (a technical result about parabolas); and Proposition 13 (the force required to maintain a parabolic orbit). I will show in Chapter 2 that in each case he either verified or attempted to verify Newton's demonstration. In addition, there are smaller annotations next to Lemma 11, Proposition 14, Proposition 16, which provide technical results, respectively, about the method of first and last ratios and orbits under inverse square forces. His last detailed marginal notes

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[80] Ibid., 2:373.

were made on page 57, and there are no subsequent verifications of mathematical proofs.

It is noticeable that Flamsteed appears only to have paid attention to the proofs concerning circular, elliptical and parabolic orbits, along with their supporting geometrical theorems. He left no annotations next to Newton's results about hypothetical, non-real orbits, such as the logarithmic spiral or hyperbolic orbits considered in Propositions 9 and 12, or circular orbits for which the centripetal force is directed towards a point other than the centre, as in Propositions 7 and 8. We may readily surmise why the Astronomer Royal chose to ignore these passages. Moreover, his annotation alongside Corollary 2 to Law 3 – for a variety of reasons that will be considered in detail in Chapter 3 – shows that Flamsteed was unable to verify Newton's proof, and gave up half-way through. This is the first substantial mathematical demonstration in the book, and the opening dozen or so pages – containing the Definitions and first two Laws – are non-mathematical, so can be read without writing anything down. I therefore suggest that Flamsteed's original plan had been to work through the opening sections in order, but that as a result of his early setback with Corollary 2 to Law 3, he chose instead to skip ahead to the results in which he was most interested – which is to say, the theorems relating to real astronomical orbits in Sections 2 and 3.

Importantly – as will be discussed further in Chapter 5 – Flamsteed does not appear to have scrutinised the proofs in Section 1 on the method of first and last ratios. There is only one annotation in these pages, a cursory note verifying the manipulation of ratios in Lemma 11, which I suggest Flamsteed made when he found the result cited in Proposition 4. But there is no evidence of any sustained engagement with Newton's arguments in Section 1. One other omission is noteworthy: Flamsteed does not appear to have verified the proof of Proposition 6. This result is of critical importance, since it establishes Newton's general method for quantifying centripetal force, which he subsequently applies in each of Propositions 7 to 13. It is clearly cited in each of these proofs, and yet the margins on the pages of Proposition 6 are blank. It is possible that Flamsteed worked through this result on separate sheets which have since been lost, but given that it is no more complicated than the other proofs he verified in the margins, I suggest this is unlikely. However, the remaining proofs about centripetal forces all depend on Proposition 6, and it constitutes an indispensable link in the logical chain that leads to the inverse square law. This means that Flamsteed's trust in the central claims of the *Principia* – like Halley's – cannot possibly have been founded on his assent to the mathematical arguments provided in the book, for the simple reason that he did not he did not study them all.

1.6 The first part of Gregory's *Notae*, September 1687 – April 1688

By far the most substantial written response to the *Principia* – the only detailed commentary on the whole text, composed by one of the author's most trusted and influential disciples – has yet to be fully examined by the secondary literature over three centuries after it was composed. In the mid-1950s Gregory's two hundred pages of *Notae* were identified by Wightman and Turnbull in the library of the Royal Society, where they have in all likelihood been for very much longer. There are also three contemporary duplicates of the manuscript, in the libraries of Christ Church, Oxford, the University of

Edinburgh, and the Gregory Collection at the University of Aberdeen. Furthermore, Kirsanov suggested in 1992 that Gregory’s annotated first edition is now at Moscow University, having been brought to Russia by Robert Erskine, chief physician to Peter the Great, in the early eighteenth century. That document, too, has not been closely examined.[81]

Gregory’s *Notae* record the only known complete reading of Newton’s text from the time of publication. Gregory worked through the whole book, verifying (or attempting to verify) each proof in turn. His notes follow Newton’s results in the order that they are printed, although in some instances entries refer to later propositions, which suggests that they were copied up from some earlier document (which may or may not have been the annotated Moscow copy). The first thirty pages cover Sections 1 to 9 of Book 1, and are dated September 1687 to April 1688; the remainder are dated December 1692 to January 1694. Some of the pages have extra slips of paper attached with paste, containing additional later notes. Many of these record the discussions that took place when Gregory visited Newton in May 1694, but he continued to add to the document throughout his life, and the last is dated 1708. Gregory ultimately wished for the *Notae* to be published as a running commentary on the *Principia*, and the Christ Church copy appears to have been prepared for the press, since the notes are numbered as footnotes. The first serious scholarly engagement with the *Notae* appeared in Eagles’ PhD thesis of 1977, but her research covered the entirety of Gregory’s mathematical career and so her study of the

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[81] Wightman, “Gregory’s ‘Notae in Isaaci Newtoni Princpia Philosophiae’”; Wightman, “David Gregory’s Commentary on Newton’s *Principia*”; Kirsanov, “The Earliest Copy in Russia of Newton’s *Principia*: is it David Gregory’s Annotated Copy?”

manuscript was only partial. Guicciardini's analysis is also based on a reading of selected passages.[82]

Over the following chapters, I will provide analyses of those sections of the *Notae* I have so far been able to examine. The resources available to me in this doctoral project – as well as the COVID restrictions which shut the library at the Royal Society during 2020–21 – have allowed me to study in detail only his notes on the opening fifty pages of Newton's book, up to and including his proof of the inverse square law. The close scrutiny to which Gregory subjects Newton's arguments in these passages is obvious from even a partial examination. Nevertheless, although the quantity of his response easily surpasses that of his contemporaries, its quality was in some respects typical of the *Principia*'s other readers. In Chapter 2 I will show that Gregory engaged with Newton's mathematical demonstrations – as others did – by reconstructing his connected prose in symbolic form, and Chapter 3 will examine the obstacles this created when attempting to verify the proofs. In Chapters 4 and 5 his response to Newton's geometrical limit methods and his analysis of force will be assessed. In general Newton's peers engaged with these two topics not by processing Newton's arguments in the terms in which he presented them, but by reconstructing his proofs within their own conceptual framework. However, as will be explained in these last two chapters, Gregory was unique among known early readers in that he was able to process the book's arguments within a variety of conceptual frameworks. He was thus able not only to engage with Newton's demonstrations in the terms that they were presented, but also to

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[82] Eagles, "The Mathematical Work of David Gregory, 1659–1708"; Guicciardini, *Reading the* Principia, 179–84.

consider how they might more profitably be accommodated within others: his assent to the claims in the *Principia* was founded on an awareness that more than one argument could be provided in support of Newton's conclusions.

1.7 Huygens' first references to the *Principia*, December 1687

As with Gregory, there are important omissions in the secondary literature's account of Huygens' engagement with the text of the *Principia*. His objection to the reality of universal gravitation due to the absence of a physical mechanism is an established part of the historical narrative, but the assumption that his view was formed after sustained study of the text has not been critically examined. Although the story is frequently repeated that Locke took Huygens' word that Newton's mathematics was correct, no study has interrogated the extent to which Huygens' judgment deserved to be trusted on the matter, nor (assuming that the anecdote is true) on what basis Huygens was making that claim. Cohen discussed Huygens only briefly in his *Introduction*; Guicciardini was not able to examine all the available documentation; and the *Oeuvres Complètes* on which Cohen and Guicciardini based their studies contains inaccuracies.

In fact, the extant evidence indicates that Huygens did not engage with the text of the *Principia* to a particularly high degree. I will show below that he read the Laws, Definitions and some parts of Book 3, but worked through the details of only a handful of Newton's mathematical proofs (in Section 2 of Book 1 and Section 2 of Book 2), and only when prompted to do so by Leibniz's publications in the *Acta* at the start of 1689. On the occasions that he did pay close attention to the text, Huygens mined it for information on specific topics he was already interested in – primarily the shape of the earth, and motion in resisting media.

Unlike Halley, Fatio, Gregory and Leibniz, at no point did he attempt a complete reading of the text; instead, like Flamsteed, Huygens engaged with the sections that were directly relevant to his research interests. The rest of the book, so far as we can reasonably infer from the available evidence, he left unexamined. He did not seem unduly interested in the proofs concerning planetary orbits or comets, for example. Nor did he pay detailed attention to Newton's method of first and last ratios (as will be discussed in Chapter 4), instead choosing to interpret Newton's geometrical limit proofs in terms of infinite series (see Chapter 2). It is also noticeable that Huygens frequently referred to the difficulty of the book, asserting that "la chose ne soit pas sans cette grande difficultè" and describing it as containing "beaucoup de choses obscures." He wrote that its demonstrations were "perobscura," and were presented "obscure admodum."[83] In short, Huygens studied the proofs of the *Principia* in less detail than either Gregory, Leibniz or Fatio, and read the book less completely than Halley or Locke. In terms of the number of proofs that there is evidence he worked through, Huygens engaged with Newton's work to approximately the same extent as Flamsteed and Clerke. In *Reading the Principia*, Huygens is one of only two individuals (the other is Leibniz) to whom Guicciardini devotes an entire chapter: I suggest that this overstates his involvement with the text.

The primary sources relating to Huygens are exceptionally difficult to navigate, although this is not acknowledged by Newton scholars. The standard reference work is the 22-volume *Oeuvres Complètes* compiled between 1888 and 1950, which is mostly based on manuscripts housed in the Codices

[83] Huygens to Leibniz, 18 Nov. 1690, *OC*, 9:536–40; Huygens to Fatio, 18 Dec. 1691, *OC*, 10:209–12; CH, HUG 7, 15v (=*OC*, 21:418); CH, HUG 26, 85r (=*OC*, 21:420).

Hugeniani at the University of Leiden. A visit to this archive has been beyond the resources of this project, which is therefore based on the material published in the *OC*. Its pages have been digitised and uploaded online, although because the cross-references still refer to page numbers in the print version, and because the layout on the website does not distinguish clearly between manuscript transcription and editorial comment, it is very hard to use. Moreover, much of it is arranged thematically, which means that – despite its dense critical apparatus, containing countless cross-references to other volumes – it is impossible for any scholar using it to be confident that they have found all the material relating to a particular topic. It also means that notes made by Huygens on the same sheet of paper are not necessarily adjacent or even near each other in the *OC*, but may instead appear in separate chapters or volumes. This situation has been mitigated by Joella Yoder's recent *Catalogue of the Manuscripts of Christiaan Huygens*, which provides a concordance between the entries in the *OC* and the manuscripts in Leiden. However, Yoder's book draws attention to the fact that many of the manuscripts have only been partially published, or even not published at all: the *Oeuvres Complètes* are therefore not reliably *complètes*. My analysis here is subject to these important qualifications.[84]

The first reference to the *Principia* in the Huygens correspondence is in a letter from Fatio dated 24 June 1687, which reports its imminent publication and outlines its contents. It is not clear when Huygens first accessed a copy, although a much later letter to his brother indicates that he was given one by

[84] In the closing stages of this doctoral project the digitised contents of the Codices Hugeniani were published online, at https://primarysources.brillonline.com/browse/codices-hugeniani. However, I have been unable to access this resource freely: at the time of writing the University of Oxford has not taken out a subscription.

Newton. It is not until six months after publication that concrete evidence of his engagement with the text appears, when on pages of his notebook HUG 1 dated 3 December he calculated the shape of the earth based on Newton's "channel" method from Proposition 19 of Book 3. Huygens performed this calculation differently from Newton, because he assumed that gravity remains constant within the interior of the earth, rather than (as Newton had proved in Section 12 of Book 1) increasing in direct proportion to the distance from the centre. He therefore arrived at a different value for the extent to which the earth is flattened at the poles. Huygens had made a study of the shape of the earth in the spring of 1687, and so the suggestion from the editors of the *OC* that when the *Principia* was published "Ce furent, paraît-il, les considérations de Newton sur la forme de la terre qui attirèrent en premier lieu son attention" is surely correct. Huygens' first interaction with Newton's book therefore seems to have been to look through it for new techniques to inform his previous work.[85]

Over the following twelve months, the Huygens archive contains only a few scattered references to the content of the *Principia*. In a letter to Hudde dated 24 April 1688, Huygens refers to Newton's hypotheses about gravity, "die ick niet en kan approberen" ("which I do not and cannot approve").[86] On 14 December 1688, he returned to a set of notes on the shape of orbits he had made in the early 1680s to insert the following comment:

> Hasce omnes difficultates abstulit Clar. vir. Neutonus, simul cum vorticibus Cartesianis; docuitque planetas retineri in orbitis suis

[85] Fatio to Huygens, 24 Jun. 1687, *OC*, 9:167–171; Huygens to Constantyn Huygens, 30 Dec. 1688, *OC*, 9:304–5. His calculations on the shape of the earth are at *OC*, 21:398–402, and he went back to insert a note recording their outcome at *OC*, 21:391; his study from in earlier in the year is *OC*, 21:375. See *OC*, 21:385 for the editors' commentary.
[86] Huygens to Hudde, 24 Apr. 1688, *OC*, 9:267.

> gravitatione versus solem. Et excentricos necessario fieri figurae Ellipticae.
>
> The famous man Newton removed all these difficulties, together with Cartesian vortices; and he showed that the planets are kept in their orbits by gravity towards the sun. And that these eccentric orbits necessarily become elliptical figures.[87]

And in a brief note that can be dated to some time in 1688, he wrote that "Theorema nostrum de centri gravitatis quiete vel aequali progressu perseverante demonstrare conatur Newtonus, et recte in corporibus ante concursum, sed non post."[88] The result that the common centre of gravity of a system does not change its state of motion appears as Corollary 4 of Law 3 in the *Principia*, and Huygens had arrived at an equivalent statement in the 1660s. This note, then, provides evidence that Huygens had at least come across it in Newton's book by that time. But his phrasing "demonstrare conatur" is revealing: the extent to which Huygens had engaged critically with Newton's proof is not clear, nor is it apparent whether he found it to be convincing.

Huygens thus gained some familiarity with the results of the *Principia* over the eighteen months following its publication. However, this awareness could have been obtained by superficial browsing, or by reading reviews, or from second-hand reports from correspondents. With the exception of Proposition 19, the historical record contains no evidence of detailed scrutiny of any passages in the book before the end of 1688. The absence of evidence does not in itself constitute the evidence of absence. Nevertheless, I shall argue below that there is good reason to believe that Huygens had the book on his

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[87] CH, HUG 1, 8 (=*OC*, 21:143).
[88] CH, HUG 7A, 10r (=*OC*, 21:415).

shelf for almost two years before he paid significant attention to anything in Books 1 or 2: it was not until the spring of 1689 that he made any direct attempts to verify Newton's mathematical arguments.[89]

1.8 Locke's second reading and review, March 1688

Locke read the *Principia* for the second time in March 1688. Still in the Dutch Republic, he did so for the explicit purpose of writing a review in the *Bibliothèque Universelle et Historique*, a periodical established in 1686 by Jean le Clerc. Locke had previously contributed to the *Bibliothèque* an article on his method of note-taking (which will be discussed in Chapter 2), and in January 1688, it carried an abridged version of his *Essay Concerning Human Understanding*, which at that time had yet to be published. Two months later, Le Clerc published his review of the *Principia*, although Locke's name was not attached. Fourteen pages long and written in French, this was the first review of the book to appear outside England, and Axtell argued that it played an important role in in spreading Newtonianism on the continent.[90]

That this anonymous review was indeed written by Locke was established by Axtell's studies of the 1960s, building on earlier work by Colie.[91] Axtell demonstrated that the words and structure of the review very closely match the contents of two sheets of Latin notes dated 1688, now at MS Locke c. 31, 99–100. There are a few differences between the two documents, but the review is otherwise a direct translation of the Latin notes into French. However, the two

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[89] I am grateful to Niccolò Guicciardini for pointing out that Huygens also devoted sustained attention to the Scholium after Newton's Definitions in Leiden MS H 7A, edited by Gianfranco Mormino in *Penetralia motus: La fondazione relativistica della meccanica in Christiaan Huygens, con l'edizione del* Codex Hugeniorum *7A*, Florence: La Nuova Italia Editrice, 1994. I was unaware of the existence of this source while conducting my doctoral research.
[90] Axtell, "Locke's Review of the *Principia*," 159.
[91] Axtell, "Locke's Review of the *Principia*"; Colie, "John Locke in the Republic of Letters."

documents have an unusual structure, for which Axtell was unable to account. The parts covering Books 1 and 2 consist almost entirely of the section headings transcribed directly from the *Principia*, and to represent Book 3 Locke provided an assortment of remarks about astronomical bodies, comets, heat, light and the tides. The review (although not the Latin notes) also has a long section in which Locke presents Newton's argument against Cartesian vortices. Cohen was equally puzzled in his *Introduction*:

> The presentation of the *Principia* in the *Bibliothèque Universelle* (it is hardly a 'review') consists primarily of a translation into French of the Latin headings of the successive sections of Books I and II, followed by the reviewer's own summary of Book III, together with an introductory paragraph… The result is curious indeed.[92]

On the contrary, we will see in the next chapter that these two documents are easily accounted for by Locke's note-taking habits, as described by Yeo. Prompted (we may assume) by Le Clerc, Locke read the *Principia* in full, for a second time, in March 1688. He again skipped all the mathematical proofs, but given his method of extracting notes, the review in the *Bibliothèque* and the preparatory sheets at MS Locke c. 31 appear exactly as we would expect them to, as will be presently explained.

1.9 Leibniz's *Notes* and *Marginalia*, autumn 1688

Leibniz's reading of the *Principia* is the subject of Bertoloni Meli's *Equivalence and Priority*, on which my discussion of Leibniz within this thesis is entirely reliant. Bertoloni Meli demonstrates that, rather than first studying the book and

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[92] Cohen, *Introduction*, 146.

then returning to his desk to formulate a separate scheme, Leibniz's study of the *Principia* was integrated continuously into the development of his own ideas:

> it is impossible to isolate two separate phases of Leibniz's thought, namely that of the interpretation [of the *Principia*] and that of the development of his own theory. Rather, I wish to stress that these two moments are present at the same time: Leibniz's *Notes* and essays on planetary motion form a continuum.[93]

There are four documents that record Leibniz's direct engagement with the printed text: the marginalia he inserted in his copy of the first edition in late 1688; a set of notes he made at the same time; and two sets of excerpts he made the following year. Between the notes and the excerpts – in the first two months of 1689 – he published in the *Acta Eruditorum* his "Schediasma" and "Tentamen", which address topics covered by Newton in the *Principia*.[94]

Between November 1687 and June 1690 Leibniz was undertaking his Italian journey, the official purpose of which was to research the origins of the house of Este. In both the *Schediasma* and *Tentamen* Leibniz stated that had not at that time seen a copy of the *Principia*, and had only read Christopher Pfautz's review of the book in the *Acta Eruditorum*, but Bertoloni Meli has shown that these claims were disingenuous.[95] In fact, Leibniz began his study of the text as he was staying in Vienna in the autumn of 1688, when he made his *Marginalia* and *Notes*. The *Marginalia* are contained in Leibniz's personal copy of the first edition, now in the Bodmer Library in Geneva. It was identified as such by

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[93] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 95.
[94] Following Bertoloni Meli, I will hereafter refer to these sources as the *Marginalia*, *Notes* and *Excerpts, Schediasma* and *Tentamen* respectively.
[95] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 7–8.

Fellmann in 1973, who published transcriptions and a brief analysis.[96] The annotations are fairly brief and are scattered throughout the book, but concentrate on Section 1, Propositions 4, 6, 10, and 11, and Lemma 28 of Book 1; Section 2 of Book 2; and the opening twenty pages of Book 3. The *Notes* comprise two sheets of paper folded in quarto in the Leibniz Library in Hannover, classmark LH 35, 10, 7, 32–5. These were first identified by Bertoloni Meli, who has provided some images, complete transcripts, and a detailed analysis.[97] The *Notes* contain a commentary on the Definitions and Laws, Section 1, and the first three pages of Section 2. Bertoloni Meli dates the document to "the autumn of 1688," partly because the paper on which they are written matches the paper Leibniz used in other documents at that time in Vienna. He further suggests that most of the *Marginalia* were composed at the same time as the *Notes*, arguing that their contents correspond, and also that a short list of references in the *Notes* matches the annotations in his first edition. I find this match less compelling than he does, but have no reason to object to his overall claim that the two documents were contemporaneous.[98]

Equivalence and Priority demonstrates that as Leibniz read the *Principia* he "interpret[ed] the concepts and vocabulary of the *Principia* into the scheme of his own terms and ideas,"[99] absorbing parts of Newton's work into his own analysis of orbital motion. It will be shown in Chapter 4 that he reconstructed Newton's geometrical limit results in terms of infinitesimals, and in Chapter 5 that he formulated central forces very differently. While – as I will demonstrate

[96] Leibniz, *Marginalia in Newtoni Principia Mathematica*.
[97] Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 220–249.
[98] Ibid., 237. Fellmann had dated the *Marginalia* to the summer of 1689.
[99] Bertoloni Meli, "Leibniz's Excerpts from the *Principia Mathematica*," 478.

in Chapter 2 – Leibniz was not the only reader who processed Newton's arguments by re-interpreting them in terms of his own pre-existing conceptual framework, the extent to which he used the *Principia* to stimulate the development of his own ideas is unique. This is evident even within the eight pages of *Notes*, in which statements and discussions of Newton's results are intermixed with his own explorations. Bertoloni Meli demonstrates how Leibniz's ideas on central forces developed from the *Notes*, through other documents composed in the autumn of 1688, and then into the *Tentamen*. This was published in the February 1689 edition of the *Acta* under the full title "Tentamen de Motuum Coelestium Causis" ("An essay on the causes of celestial motions").[100] The previous month's issue of the *Acta* had contained his "Schediasma de Resistentia Medii et Motu Projectorum Gravium in Medio Resistente" ("A sketch on the resistance of a medium and on the motion of heavy projectiles in a resisting medium"). The contents of this article overlap with some sections of Book 2 of the *Principia*: the passages that correspond are not examined in the *Notes*, but there are a small number of *Marginalia* against them. Unlike the *Tentamen*, Bertoloni Meli does not present evidence that the *Schediasma* was influenced by Leibniz's study of Newton's text. Nevertheless, the publication of these two articles did prompt another of Newton's peers to examine some of the arguments contained in the *Principia*.

1.10 Huygens' reading notes, February – April 1689

Other than studying the method of Proposition 19 of Book 3 to inform his calculations on the shape of the earth, there is no evidence that Huygens

[100] Bertoloni Meli provides a complete translation of the *Tentamen* in *Equivalence and Priority: Newton versus Leibniz*, 126–142.

engaged in detail with any passages of the *Principia* in the eighteen months after it was published. It was only at start of 1689 that he worked through any of Newton's mathematical proofs, when he read and made notes on various proposition that are now in his archive at HUG 7 (Book G), 15–16. These notes are themselves not dated, but they are preceded by pages marked 20 December 1688 and followed by notes on the April 1689 issue of the *Acta Eruditorum*.[101] Some further notes at HUG 26 (Chartae Mechanicae), 85 are very likely to date from the same time. The content and the timing of the notes reveal the purpose of Huygens' study: he read a series of results about orbital motion from the start of Book 1 and a small number of sections about motion in resisting media at the start of Book 2, because he had read about them in Leibniz's *Schediasma* and *Tentamen*, which had just been published in the *Acta*. Just as he had in November 1687, Huygens again went to the *Principia* to mine it for specific information about research topics in which he was already interested.

In the *Tentamen* Leibniz analysed orbital motion, and it prompted Huygens to work through some of Newton's proofs from Section 2 of Book 1. The notes he made when he did so are in HUG 7 (Book G). His notes on Proposition 6 (the central result establishing Newton's measure for centripetal force) are on folio 15r, and those on Proposition 9 (the force required to maintain a body in a logarithmic spiral) are on the reverse, at folio 15v. Surprisingly, there is no evidence that Huygens worked through Newton's proof of Proposition 11 (the inverse-square law) nor Proposition 4 (uniform circular motion, which Huygens

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[101] CH, HUG 7, 7, 28–30.

had himself analysed in his *Horologium* of 1673). On folio 12r there is a short note acknowledging the result of Proposition 10 (elliptical motion with the force directed towards the centre), but no verification of the proof. He queries one small step in Corollary 2, writing that “Il devoit avoir montrè auparavant quelle raison il y doit avoir entre les celeritez du corps à l'endroit ou aboutissent les grands diametres des Ellipses,” a phrasing which strongly suggests that he hadn’t worked through all the preceding results.

Leibniz’s *Schediasma* concerns the motion of bodies in resisting media, and it directed Huygens to read the corresponding passages in the *Principia* at the start of Book 2. The *Schediasma* was published in January, one month before the *Tentamen*, but Huygens seems to have worked through Newton’s results on resisting media after those on central forces. On folio 16r of HUG 7 (Book G), immediately after his notes on Proposition 9 of Book 1, he analysed Lemma 2 and Proposition 8 from Section 2, which concerns the motion of bodies acted on by a resistance force proportional to the square of the velocity. Then, on folio 85r of HUG 26 (Chartae Mechanicae) he worked through Proposition 5. Unlike the pages of HUG 7, the loose sheets collected together into HUG 26 are not dated, but it is reasonable to assume that these sets of notes are contemporaneous. The same may be said of a brief note that appears on folio 84v of HUG 26. This sheet contains Huygens’ own calculations about motion in a resisting medium that the editors of the *OC* suggest date from the late 1660s, to which Huygens has later appended the words “Sic fere Neutonus Propos. 2 lib. 2.”

All of these documents are published in the *Oeuvres Complètes*.[102] They have not been carefully examined by Newton scholars, however: the fullest study is by Guicciardini, who goes into detail on the notes on Proposition 6 of Book 1, and follows the *OC* in mis-labelling the notes on Lemma 2 of Book 2. Close analyses of all of Huygens' notes will be provided over the following chapters, where I shall establish the general pattern that, rather than engaging with Newton's proofs as they appeared in the text, he instead reconstructed them on his own terms, using different notation, different conceptual bases, and different logical arguments. He assented to some conclusions and disputed others, but in each case based his judgment not on Newton's original argument, but on his own reconstruction of it.

These notes, composed between February and April 1689, provide the only evidence of Huygens' sustained engagement with the detail of the text of the *Principia*. He spent the summer of 1689 in London with Newton, Fatio, Flamsteed and other members of the scientific and political elite, at the end of which trip Newton gave him two more propositions on motion on a resisting medium. Huygens made further notes on this topic in late 1690 at HUG 7, 75–9, explicitly referencing Proposition 9 of Book 2. He also hypothesised about the cause of gravity, most notably in his *Discourse de la Cause de la Pesanteur* of February 1690. But otherwise, references after 1689 to specific results in Newton's book are infrequent. When Fatio visited the Hague in June 1690 he brought his notes with him, but there is no evidence that Huygens used these

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[102] *OC*, 21:417–422. It is possible that the Leiden archive contains more relevant material that has not been published in the *OC*, and Yoder suggests that there is a second derivation of Proposition 5 and further commentary on Proposition 8 on f.85v of HUG 26, whose contents have not been published. It has been beyond the resources of this project to establish whether this is indeed the case.

documents to undertake further study of the book. Indeed, while labouring to prepare what he thought was going to be a second edition in May 1692, Fatio asked Huygens if he would be willing to contribute his manpower:

> Si de votre côté M^{r}. vous entrepreniez quelqu'une des autres Sections il ne seroit pas difficile de venir bien tot à bout de tout le livre. Et nous pourrions nous rendre conte l'un à l'autre des difficultez que nous aurions rencontrées et nous faciliter reciproquement l'etude d'un livre qui est assurement fort excellent mais en mesme temps fort obscur.[103]

> If on your side, sir, you were undertaking one of the other Sections it wouldn't be difficult to get through the whole book quickly. And we could relate to each other the difficulties that we encountered and reciprocally facilitate the study of a book which is certainly very excellent but at the same time very obscure.

Huygens' reply has been lost, but there is no evidence that he provided Fatio with the help he requested. In his correspondence with Leibniz he frequently engaged in general discussion about the cause of gravity, and towards the end of 1690 the pair discussed Newton's treatment of tides, universal gravitation and comets in very general terms, but when Leibniz posed direct questions about specific passages in the book, Huygens avoided answering. And in his correspondence with l'Hôpital in the early 1690s, Huygens was impatient to learn about Newton's integration methods, but did not raise the question of how they might be applied to specific propositions. The general pattern, therefore, is that while Newton's work is a recurrent theme of his correspondence over the

[103] Fatio to Huygens, 9 May 1692, *OC*, 22:158.

final years of his life, Huygens discussed the implications of the *Principia*'s assertions much more than he interrogated its arguments.[104]

The central narrative in the secondary literature concerning Huygens' response to the *Principia* is his objection to the reality of universal gravitation, which he described as "une hypothese si peu probable et si hardie."[105] His view that the mathematical system Newton built on it was therefore fundamentally compromised finds clear expression in this letter to l'Hôpital dated 29 December 1692:

> Un scavant Anglois vient de me dire que la seconde edition des Principes de Mr. Newton, de la quelle Mr. Fatio devoit avoir soin, ne se fera pas encore si-tost. Il y a une infinité de fautes à corriger et quelques unes qui sont de l'autheur, comme il reconnoit luy mesme. J'estime beaucoup son scavoir et sa subtilitè, mais il y en a bien de mal emploiè à mon avis, dans une grande partie de cet ouvrage lors que l'autheur recherche des choses peu utiles, ou qu'il batit sur le principe peu vraisemblable de l'attraction.[106]

> An English savant has just told me that the second edition of Mr Newton's *Principia*, which Mr Fatio should take care of, will not yet be complete soon. There is an infinity of faults to be corrected, some of which are the author's, as he recognises himself. I greatly appreciate his knowledge and subtlety, but in my opinion there is a lot of misuse in a

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[104] For Huygens' evasiveness, see the exchange with Leibniz between October and November 1690 at *OC*, 9:516–572, especially 523 and 533. See also Huygens to Leibniz, 29 May 1694, *OC*, 10:609–11, where they discuss the bucket experiment at the end of the Definitions. Huygens and l'Hôpital discuss Newton's quadrature techniques at *OC*, 10:481–5, 518–24, for example, but I cannot find any references to specific passages in the *Principia* anywhere in their correspondence.

[105] *OC*, 16:250.

[106] Huygens to l'Hôpital, 29 Dec. 1692, *OC*, 10:348–55. See also Huygens to Leibniz, 18 Nov. 1690, *OC*, 10:538.

> large part of this work, when the author searches for things of little use, or when he builds on the unlikely principle of attraction.

It will be shown below, however, that the "infinity of faults" had been tracked down not by Huygens but by Fatio. So far as can be inferred from the primary sources, therefore, Huygens dismissed the possibility of universal gravitation without examining Newton's mathematical arguments in favour of its existence. He appears to have formed his views of Newton's claims after minimal close analysis of the text. There is little evidence to show that Huygens ever subjected many of the demonstrations of the *Principia* to sustained scrutiny.

1.11 Leibniz's *Excerpts*, April – November 1689

In 1988, Bertoloni Meli published details of two manuscripts he had discovered recording Leibniz's second phase of study of the *Principia*, which he labelled the first and second set of *Excerpts*. These documents – of which Bertoloni Meli provided a transcription and brief commentary – are in the Leibniz Library in Hannover at LH 35, 14, 2, occupying folios 37–8 and 31–6 respectively.[107] They demonstrate that Leibniz subsequently re-examined some of the passages he had remarked upon in the *Notes*, and also read many later sections of the book. Based on the paper he used, Bertoloni Meli concludes that they were "probably written in Rome in 1689,"[108] where Leibniz stayed from April to November.

Both documents largely comprise excerpts from Newton's text, either transcribed verbatim or slightly rephrased. Very occasionally these are

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[107] Bertoloni Meli, "Leibniz's Excerpts from the *Principia Mathematica*."

[108] Ibid., 477.

accompanied by Leibniz's own observations, but such remarks are far less frequent and substantial than they were in the *Notes.* He did not at any stage in the *Excerpts* work through any of Newton's mathematical proofs, although he did in some instances comment on them. Taken at face value, this strongly suggests that this second phase of reading was significantly less detailed than the first phase represented by the *Notes*. Only in a few instances is there evidence that Leibniz subjected Newton's arguments to fresh critical scrutiny; overwhelmingly he merely wrote out paraphrases of the text. The impression is of a cursory reading in order to gain an overview of the contents of the book.

In the first set of *Excerpts*, Leibniz began by writing out brief paraphrases of Newton's Definitions and Laws, followed by Lemma 11, Lemma 10, Lemma 9 and Proposition 6 (in that order). He then skipped a long way ahead to the end of Book 2, where he made a few notes concerning fluids and resistance drawn from Sections 5, 7, 8 and 9. These happen to be precisely the four sections in the *Principia* that have the word "fluid" in the title, so it may be that Leibniz flicked through the pages searching for passages relating to that topic. He ended with a couple of dozen notes from Book 3 concerning planetary orbits and gravity. The second set of *Excerpts* is three or four times as long. He again began by copying out the Definitions and Laws. He then skipped the method of first and last ratios, and paraphrased Propositions 1–4 and 7–10, in such a way that the two sets of *Excerpts* between them cover all of Sections 1 and 2. This was followed by Lemma 28, Proposition 40, Proposition 44, and most of Sections 10–14 of Book 1. He then embarked on Book 2, until immediately after Proposition 8 he came to an abrupt halt with the words "Nondum perrexi" ("I have not yet gone further").

If Leibniz did resume further systematic study of the text on a later occasion, there is no evidence of it in the documentary record. Naturally, the themes of the *Principia* continue to be a topic of frequent discussion in his correspondence over the following years, especially with Huygens. In due course he revised both the *Tentamen* and the *Schediasma*, and in the *Acta* of 1690 he published a tract on the cause of gravity. But there is no further evidence of detailed study of the book. Bertoloni Meli has fully evaluated the influence of the *Principia* on the development of his thought: to his analysis I only add the observation that, so far as we can tell from the extant evidence, from the entire five hundred pages of text, Leibniz only scrutinised closely the method of first and last ratios in Sections 1, and the handful of main orbital proofs in Sections 2 and 3. For the remainder of the book – other than a few isolated exceptions – he appears to have read through but not checked the proofs. He skimmed through the pages but did not stop to verify the mathematics. Although this fact does not impact upon Bertoloni Meli's investigation, it is very important to mine: it is striking that even the man presented as the author's greatest rival paid little detailed attention to the overwhelming majority of Newton's book.

1.12 Fatio's reading notes, spring 1690

As with Halley, Huygens and Gregory, significant evidence concerning the readings of the *Principia* by Newton's Swiss acolyte Nicolas Fatio de Duillier has been overlooked by the secondary literature. Newton scholars have known for almost two centuries that Fatio was considered as a possible editor of a new version of the book in the early 1690s, and his annotated first edition has been in the Bodleian since 1755. His reading notes were published in Huygens'

Oeuvres Complètes in 1905, and Cohen contextualised this evidence for the convenience of modern scholars in his *Introduction* of 1971. But these documents have not yet been fully examined, and mentions of Fatio in the secondary literature are often accompanied by insinuations about his technical expertise, Hall describing him as "possessed by mathematical abilities that were considerable though not of the highest order." My priority in the following chapters is to provide accurate account of his engagement with Newton's text rather than a judgement of his ability.[109]

There are two sources of primary evidence detailing Fatio's readings of the *Principia*. The first is a set of notes Fatio took with him to on his trip to the Netherlands from June 1690 to September 1691, most of which he spent with Huygens in the Hague. He initially left the notes with Huygens, but soon after arriving back in London asked Huygens to return them.[110] Before he did so Huygens made a copy, which is published in the *Oeuvres Complètes*, 10:147–55. Fatio's original has been lost. After Huygens died the copy eventually came into the hands of Johannes Groening, who reordered and published the notes as a supplement to his 1701 *Historia Cycloeidis* under the heading "Christiani Hugenii Annotata posthuma in Isaaci Newtoni Philosophiae Naturalis Principia Mathematica," a title which gives the misleading impression that they were composed by Huygens.[111]

The original document from which Huygens took his copy reflects the outcome of Fatio's reading of parts of the *Principia* and his subsequent discussions with Newton. It comprises three distinct sections. The first is a set

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[109] Rigaud, *Historical Essay on the First Publication of Sir Isaac Newton's* Principia, 89; Hall and Hall, *Unpublished Scientific Papers of Isaac Newton*, 205.
[110] Fatio to Huygens, 18 Sep. 1691, *OC*, 10:145–6.
[111] Groening, *Bibliotheca Universalis*.

of notes Fatio made during his initial study of the book. These cover the first sixty pages and Book 3 fairly thoroughly, and also make detailed reference to a few other passages. In many instances these notes record typographical or grammatical errors or suggest small re-phrasings, but in others Fatio extended Newton's mathematics to generate new results, as will be discussed in Chapter 2. He also interrogated the method of first and last ratios very closely, as we shall see in Chapter 4. It is clear that Fatio considered himself to be preparing a second edition of the text, because he suggested alterations using phrases such as "pro 'mensuratarum' legi velim 'quantitatum mensuratarum'."[112] Moreover, Newton must have seen these notes, because – as Huygens recorded when he took his copy – in some instances Newton tweaked Fatio's proposals in his own handwriting. It is also clear from the pages of the annotated copy in Cambridge that Newton often altered his text in accordance with Fatio's suggestions. This first section of the document therefore provides clear evidence that Fatio was preparing a second edition in collaboration with Newton.

The second – completely distinct – section of Fatio's document is headed "Ex Newtoni codice" and is a list of nine changes Newton made to the text that Fatio transcribed directly from the annotated copy. Three of these changes had been recorded by Newton in the main body of the book, and six in the endpapers at the back, from where Fatio copied them verbatim. Importantly, many of the edits in the "Ex Newtoni codice" are responses to Fatio's editorial notes, since the changes are in some instances extremely similar to Fatio's

[112] *OC*, 10:147. Fatio is here suggesting an alteration to the Scholium after the Definitions, on page 10 of the first edition.

suggestions. Newton must therefore have altered his text in response to the commentary in the first section of the document, and these changes were then recorded by Fatio in the second.

The third section is titled “Alia Errata ex Newtoni mei codice,” and is a much longer list of corrections compiled by Newton that Fatio copied out from the back of the annotated copy on a separate occasion. In the annotated copy these edits start on the Errata page and spill over onto the subsequent endpapers, from where Fatio copied them out verbatim. Helpfully, Fatio’s transcription is dated “Londini. 13 Mart. die 1689/90.” It is also the case that the “Ex Newtoni codice” edits are written on the endpapers of the annotated copy directly underneath those listed in the “Alia Errata.” The edits in the second section of Fatio’s document therefore post-date those in the third. The dates of the editorial notes in the first section are less clear. On the one hand, it is intuitively plausible that he copied the “Alia Errata” from the annotated copy in preparation for his own study of the text, which would suggest that he began his editorial notes soon after 13 March. But on the other, his notes frequently pick up misprints that are also recorded in the “Alia Errata,” which shows that if he did have these edits in his possession when he undertook his study, he did not pay them much attention. On balance I prefer this latter interpretation, and so suggest that Fatio probably made his editorial notes before copying the “Alia Errata” on 13 March (while acknowledging the strong possibility that he made them immediately afterwards).

The Trinity College Exit and Redit book shows that Newton was away from Cambridge between 10 March and 12 April 1690, and this final piece of information confirms a relatively clear narrative. Newton must have spent this month in London, much of it with Fatio. Huygens had just sent Fatio a copy of his *Treatise* to give to Newton, and Westfall's suggestion that Newton spent these weeks studying Huygens' *Treatise* is plausible.[113] Newton must have taken his annotated copy with him to London, because on 13 March Fatio copied out Newton's list of corrections at the back (the "Alia Errata"). Very soon afterwards Fatio discussed with Newton the notes he had made on the opening sixty pages and Book 3 of the *Principia*, which he had made on the understanding that he was preparing a second edition. He probably made these notes over the weeks or months prior to Newton's visit, but might have made them once he had arrived. As a result of their discussion Newton made a number of changes to his text, some of which were written into the body of the annotated copy, and others listed in the endpapers. Fatio then took another copy of the changes ("Ex Newtoni codice"), before Newton returned to Cambridge in mid-April, and Fatio travelled to the Netherlands in June. The nature of Fatio's comments, and the extent to which he was persuaded by Newton's mathematical arguments, will be the subject of subsequent chapters; he resumed his study of the text very shortly afterwards, as we shall presently discover.

1.13 Locke's third reading, 1691

Locke returned to England on 12 February 1689, on the same ship that carried Princess Mary over from Briel to join her husband, the new King William III. Until that year, Newton and Locke had neither met nor corresponded, yet a coincidence of timing in the wake of the Revolution saw the two reach the peak

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[113] Westfall, *Never at Rest*, 496.

of their renown almost simultaneously. Both were ambitious and had recently been released from years of self-imposed isolation; each was eager to have his own public status reflected and amplified by the other. Locke scholars emphasise the impact the *Principia* had made on him in the Netherlands, and for Newton the publication of his book had been a "psychological watershed in his life." Westfall described how his new confidence was reflected in his burgeoning relationship with Locke, writing that "Each recognized in the other an intellectual peer."[114]

Locke appears to have succeeded in tracking Newton down within a few months of arriving back in England, probably at the intellectual salon of the Earl of Pembroke. The pair quickly exchanged tokens of their mutual admiration. Locke's *Essay Concerning Human Understanding* was finally published soon after his return to England, and he made sure to give Newton a presentation copy. Soon after, Newton sent Locke the amended copy of the *Principia* now in the Wren Library at Adv.b.1.6. He also supplied him with a simplified proof of the inverse-square law for elliptical orbits, dated March 1690.[115]

Locke read the *Principia* for the third time some time during 1691, as shown by his dated notes in MS Locke d. 9. Cohen did not mention this document, Axtell reported that on his third reading Locke "filled several pages of a notebook called 'Adversaria Physica' with detailed notes," and Guicciardini writes that "Locke wrote a notebook called *Adversaria Physica* in which he commented on several propositions."[116] These descriptions give a misleading

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[114] Axtell, "Locke's Review of the *Principia*," 154; Westfall, *Never at Rest*, 500–1, 488–9.
[115] Westfall, *Never at Rest*, 488; Axtell, "Locke, Newton, and 'The Elements of Natural Philosophy'," 235; Milton, "Locke, John (1632–1704)". The proof of the inverse-square law is at Bodleian, MS Locke c. 31, 101–4.
[116] Axtell, "Locke, Newton and the two cultures," 176; Guicciardini, *Reading the Principia*, 176.

impression of Locke's note-taking process, because MS Locke d. 9 is a commonplace book, which he had originally begun in 1660. He titled it "Adversaria Physica" not because it was a coherent tract on Newtonian physics, but because it contained his collected notes on medical and natural philosophical subjects – "Physica" as opposed to "Ethica". The notes on the *Principia* are scattered throughout. The manner in which he engaged with the text was exactly the same as on the first two occasions that he read it, as we shall see in Chapter 2.

This third reading must have been from the presentation copy now in the Wren Library, because it contains a large number of manuscript amendments to the text, and Locke's notes quote the altered version, not the original.[117] Axtell wrote of these updates that "The majority of [Newton's] notations were made for the sake of understanding rather than strict accuracy and obviously reflected his estimate of Locke's ability to follow the *Principia*,"[118] but an inspection of the source shows this not to be true. All of the edits in Locke's copy are also in his annotated copy, which shows that Newton merely transcribed all the changes to the text he had thus far collected. The alterations were not made specifically for Locke – he just sent his new friend a copy which reflected the most up-to-date version of the text.

Moreover, these edits provide further chronological information of value. Locke's copy is undated,[119] but the changes it contains can profitably be

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[117] See, for example, the figures Locke records under his note headed 'Sonus' in MS Locke d.9, which are taken from the altered text in Adv.b.1.6 but were different in the original.
[118] Axtell, "Locke, Newton and the two cultures," 176.
[119] Cohen, *Introduction*, 202, dates Newton's gift to Locke to "the early 1690s"; Axtell, "Locke, Newton and the two cultures," 176, asserts "sometime in 1691," but provides no evidence for this year. Perhaps he assumed that the notes in MS Locke d. 9 were made immediately upon receipt.

compared with those Fatio suggested during his two readings of the text. Many of the edits Fatio made in his first phase of study (considered in the previous section) have also been transferred into Locke's copy; but those recorded in his second phase (to be examined in the next section) have not. Locke's copy must therefore have been prepared between the two. Furthermore, the simplified proof of the inverse-square law Newton gave to Locke is dated March 1690, which exactly coincides with Newton's visit to Fatio in London. Given that Locke's copy cannot have been prepared before that moment, there is a strong possibility that it was marked up during Newton's stay, and then delivered to Locke along with the simplified proof of the inverse-square law at the same time.

1.14 Fatio's marginalia, 1691–92

The second primary source documenting Fatio's reading of the *Principia* is the extensive set of marginal notes in his copy of the first edition, now in the Bodleian. Although listed by Cohen, these have not been examined in detail in the secondary literature, and are generally not mentioned at all.

These annotations vary widely in size and complexity. Small edits and textual corrections appear throughout the book, although these cannot be taken as reliable evidence of close study, because many are likely to have been either transcribed from external sources, or spotted during cursory reading. However, some passages are accompanied by substantial marginal commentary, and these form a more meaningful pattern. There are almost no long marginalia before page 69, which marks the end of Section 4 of Book 1. Fatio's longest and most dense annotations are very noticeably concentrated in Sections 5 and

9 in Book 1, with some further commentary alongside Proposition 30 and the Scholium in Section 6, Proposition 38 in Section 7, and Lemma 2 in Book 2. His handwriting in these notes is in general extremely tidy and there are very few crossings-out, but there are seven loose sheets of scrap paper tucked between the pages on which the writing is much more messy, suggesting that Fatio on occasion made rough reading notes which he then copied up neatly into the margins. He also has placed a small tick above the number at the top of many of the pages. With a handful of exceptions, he has ticked off all the pages in Sections 1–5 and Section 9, along with Propositions 32 and 33 in Section 7, the first two pages of Book 2, and Lemma 2 in Book 2. This would be an unreliable guide to his reading were it not that they exactly match a letter he wrote to Huygens on 29 April 1692, in which he said he had "still only studied in depth the first five sections, the ninth, and the treaty of comets. Elsewhere I have only studied thoroughly a few propositions here or there."[120] They may therefore reasonably be taken as a helpful indicator of the extent of his study.

When combined with the evidence from the document containing his earlier reading notes, a clear narrative emerges about how and when Fatio engaged with the *Principia*. As explained above, he studied the Definitions, Laws, Sections 1 to 4 of Book 1, and Book 3 shortly before (or possibly during) Newton's stay in London in the spring of 1690. Newton made a number of changes to his text as a result of their discussions. Some time after the visit, Fatio set out to work through more sections, probably making notes on rough sheets before copying them up into the margin of his copy. This further study encompassed the entirety of Section 5 and 9, along with Proposition 30 and the

[120] Fatio to Huygens, 29 Apr. 1692, *OC*, 22:158–9.

Scholium in Section 6, some of Section 7, and Lemma 2 in Section 2 of Book 2. He must have subsequently discussed Sections 5, 6 and 9 and Lemma 2 with Newton face-to-face, because many of his suggested edits appear in the annotated copy. It is also the case that – as I shall show in Chapter 3 – some of the annotations record conversations in which Fatio asked Newton for clearer explanations of the proofs.

Establishing a precise timeline for this second phase of reading is difficult. The marginalia themselves are not dated, and Fatio continued to make additions throughout his life, including dozens based on the third edition of 1726. He departed for the Netherlands in June 1690, returning in September of the following year. Newton visited him in London almost as soon as he got back, from 12 to 19 September 1691. On 18 September Fatio asked Huygens to return the documents on which *OC*, 10:147–55 was based, which suggests that he had some pressing need to use them at this time. Three months later Huygens and Fatio corresponded about the prospect of a second edition, Fatio reporting that "Le catalogue des Errata du livre de Mr. Newton grossit sensiblement entre mes mains à mesure que j'avance dans la lecture que je fai de ce livre et qui est tout à fait rigoureuse et severe."[121] Fatio's letter of 29 April 1692 (quoted above) provides a *terminus ante quem* for most of the annotations. This leaves three possibilities for his reading of Sections 5, 6, 7, 9 and Lemma 2. He either studied and discussed them when Newton was staying with him in March 1690; or he read them in the Netherlands, possibly even with Huygens, before reviewing them with Newton on his return; or he set to work soon after he got back, finishing before April 1692. The second possibility

[121] Fatio to Huygens, 28 Dec. 1691, *OC*, 10:215.

strikes me as the least likely, since there is no hint of it in Fatio's correspondence with Huygens, and he would have had a long wait before sharing his thoughts with Newton. There is little clear evidence to inform a decision between the first and the third: owing to his request that Huygens return the documents he had lent him on 18 September, the absence of most of the edits in Fatio's copy from Locke's, and the immediacy of the sentence "the catalogue of Errata grows appreciably in my hands, as I advance in the reading that I do of this book," I suggest that he made them once he was back in England. It is possible that he and Newton spent his visit to London at the start of September 1691 engaged in intense editorial discussion just as they had in March 1690.

What is beyond doubt is that Fatio made a detailed reading of in total about half the book, working through the proofs with Newton as part of his efforts to prepare a second edition. The content of his deliberations, and how they fit into the overall pattern of the reception of Newton's mathematical arguments, will be discussed in subsequent chapters. What I hope to have made clear, however, is that the secondary literature significantly understates Fatio's importance as an early reader of the *Principia*. The judgement that his time with Newton in March 1690 was spent passively "listing corrections to the *Principia*"[122] misrepresents the active role he took in challenging, rewriting and augmenting the text. Fatio easily exceeds Huygens, probably exceeds Halley, equals Leibniz, and is second only to Gregory in terms of the quantity of detailed scrutiny to which he subjected the text. He was one of the three most important readers of Newton's book, and deserves to be recognised as such.

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[122] Iliffe, "Newton: The Making of a Politician."

1.15 The second part of Gregory's *Notae*, December 1692 – January 1694

In 1691 Gregory was appointed Savilian Professor of Astronomy at Oxford, thanks in large part to the recommendation of the Lucasian Professor of Mathematics at Cambridge. At the end of the following year, for reasons not revealed by the historical record, he chose to resume his close scrutiny of Newton's book, which had been suspended in the spring of 1688. The final 180 pages of his *Notae* in the Royal Society – by far the greater part, covering Section 10 of Book 1 until the end of Book 3 – are dated December 1692 to January 1694. Gregory visited Newton in Cambridge for five days in May 1694, during which stay Newton told him that he was planning a second edition and shared his proposed revisions. Gregory expressed his desire that his own notes be included in the new publication, although there is no evidence that Newton considered this proposal seriously; nevertheless, the existence of the *Notae* was not kept secret, because both Huygens and Malebranche subsequently referred to them, and in 1713 Saunderson discussed the possibility of their publication. More importantly for our present purpose, Gregory returned to the *Notae* after the visit and attached small slips of paper to individual entries, recording the outcome of his discussions with Newton.[123]

The contents of both the original notes and the later additions will be examined over the following chapters. But let it first be noted what the fact of Gregory's visit tells us about the reception of the mathematical arguments in the *Principia*. Like Newton's meetings with Fatio in March 1690, and – probably – with Halley in 1685, it provides another clear instance in which solitary reading

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[123] Guicciardini, *Reading the* Principia, 179–80.

of the *Principia* was not sufficient to persuade its readers of the validity of the arguments it contained. Even the book's most attentive readers could not verify the proofs without a face-to-face discussion with their author. Its arguments were only considered convincing when supplemented by a personal submission from Newton himself. A decade after Halley had set him the challenge of constructing a mathematical explanation of planetary motion, Gregory was sitting in the same room querying his solution. Almost seven years after it was printed, the *Principia*'s most attentive reader was still unable to verify its central demonstrations. If the narrative in the secondary literature is to be believed that the moment it came off the press "Newton's book took Britain by storm," and that "Almost at once it became the reigning orthodoxy among natural philosophers," then this is telling us something very important about the reception of the *Principia*: the acclaim the book received at its moment of publication cannot have been based on its readers' acceptance of the mathematical arguments that were printed in it.

2. Practices of reading the *Principia*

2.1 Introduction

The previous chapter provided a chronological narrative of readings of the *Principia* over the half-decade after its publication, specifying who read which sections of the book, in what order, and when. My aim in this chapter is to describe what the act of reading on these occasions involved, based on a close examination of the documentary evidence. It will quickly become apparent that different individuals read the book in different ways. Newton's peers had a variety of expectations about how his book should be processed, and brought to the text a range of reading habits, prior knowledge, and preconceptions about mathematical concepts. The act of reading was a multifarious, compound, heterogenous activity.

One crude but useful distinction I will make in what follows is between mathematical and non-mathematical readings of the text. In the first, readers carefully studied the individual steps in Newton's mathematical arguments in an attempt to verify his proofs, whereas in the second passages in the book were read, considered, and their contents meaningfully processed, but without checking the mathematics. Individuals were commonly able to switch between these two modes, reading some passages mathematically and others non-mathematically, and for this reason it is misleading to distinguish between mathematical and non-mathematical readers. But the distinction between mathematical and non-mathematical readings is useful, because any given individual engaging with any given passage at any given time either was or was not scrutinising the mathematics. The value of highlighting this distinction is that the existing secondary literature overlooks non-mathematical readings almost entirely. Locke's is the only example that is ever referred to in histories of the reception of the *Principia*, where he is habitually cited as an example of a mathematically ignorant reader who had to ask others whether the proofs could be trusted. As we shall see, the secondary literature also misrepresents the way in which Locke engaged with the text, in ways that have significant implications for analyses of his epistemology of mathematics and natural philosophy. But more importantly, the primary sources show that Halley, Fatio and Leibniz all read the greater portion of the *Principia* in exactly the same way. They too read the majority of the book without checking the mathematics, just like Locke. And this creates a problem for the existing secondary literature, because such readings cannot easily be categorised using the vocabulary of "mastery" and "understanding." Articulating the extent to which the *Principia* could meaningfully be understood without checking the proofs will not be simple: I will approach the problem by providing a careful description of the documentation that records how non-mathematical readers engaged with the text.

Notwithstanding this division, we will observe the unifying pattern that all readings of the *Principia* involved an active process of reconstruction. When reading non-mathematically, Locke and Leibniz picked up a pen, selected short excerpts from the text, and wrote out isolated quotations. And when reading mathematically, Gregory, Flamsteed, Fatio and Huygens picked up a pen, found a piece of paper, and wrote out reconstructed versions of Newton's arguments. The proofs in the *Principia* typically comprise long, prose compositions, and in order to verify them readers were obliged to recreate them in condensed, symbolic form. Importantly, they frequently did so using concepts and notation different from those Newton employed in the text. When confronted with the printed text on the page, that is, readers reconstructed Newton's arguments in their own terms, and according to their individual, pre-existing conceptual framework. Reading the text mathematically thus entailed reconstructing its demonstrations in a different form. Having done so, readers judged the validity of its arguments based on their own reconstructions rather than Newton's originals. Expressed in its strongest form, this will lead to the conclusion that readers were rarely persuaded by Newton's arguments in the versions that he presented them, and only granted assent if they were successfully able to reconstruct them within their pre-existing conceptual framework.

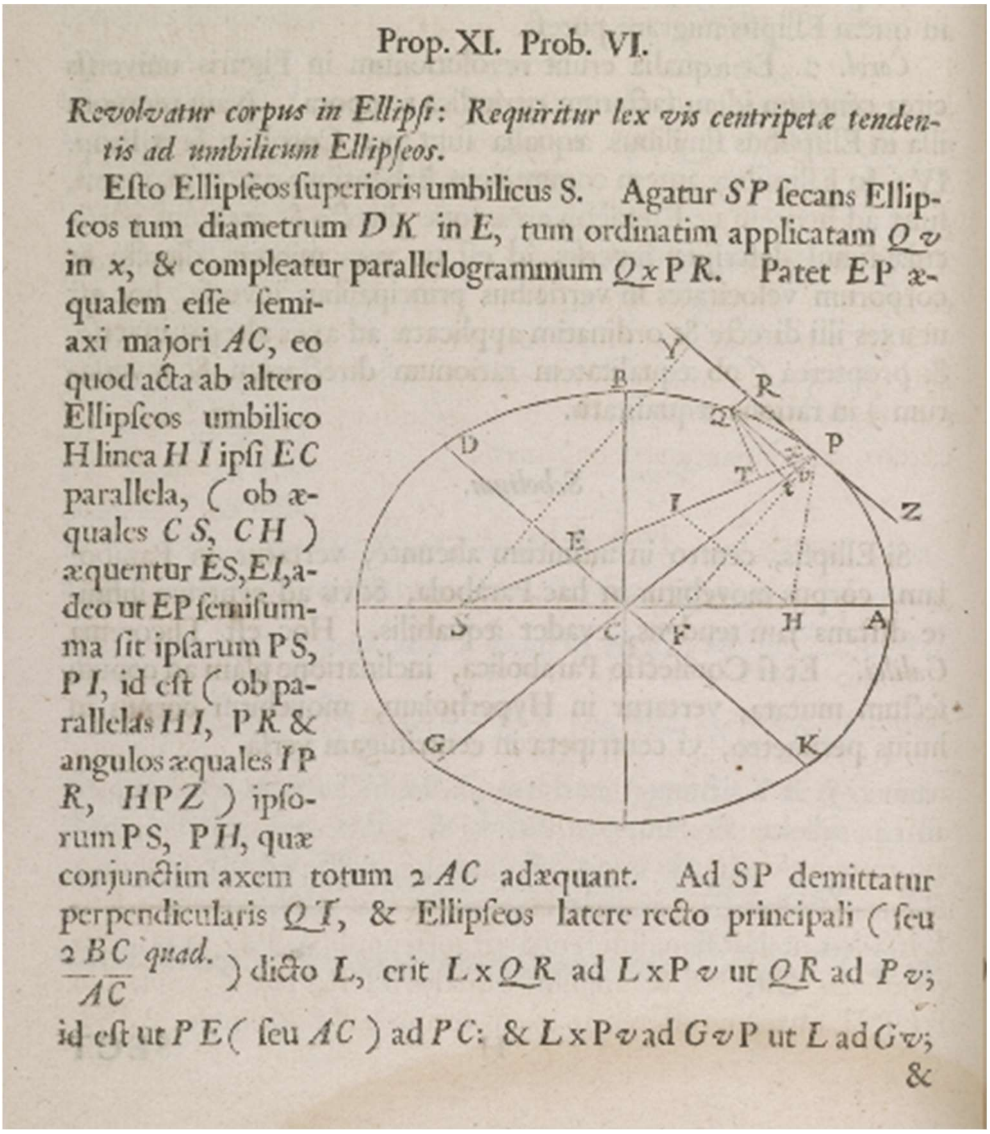
Furthermore, this act of reconstruction automatically led readers to produce what they thought were improved versions of Newton's proofs. Editing the *Principia* was integral to the act of reading it. Because reading the book involved picking up a pen and recreating its arguments, the printed text was merely a jumping-off point from which readers produced their own versions of Newton's demonstrations. As they did so they altered his terminology, reformed his conceptual foundations, and remodelled his arguments. In attempting to replicate them, readers often simplified Newton's original demonstrations, provided alternative derivations of his results, or extended them to produce new discoveries. What began as an act of obedient reproduction often extended organically into a process of creative generation. At the very least, everyone who read the book did so by producing alternative formulations of its arguments that they found more persuasive. In some instances, readers were able to communicate these alternatives back to the author. For example, Halley, Fatio and Gregory held face-to-face meetings with Newton in which they queried his proofs, haggled over his wording, and urged him to restructure his arguments. Indeed, the secondary literature acknowledges Halley's role as "midwife" to the first edition, as well as Fatio's and Gregory's attempts to establish themselves as editors of a second in the 1690s. But to establish a separate category of "editor" distinct from ordinary "readers" is to misrepresent the way in which the book was read. All of Newton's readers reconstructed his arguments in terms that they personally found more amenable to comprehension. Whether or not they then communicated these reconstructions back to the author so they could be considered for inclusion in subsequent editions is a matter of historical contingency. Halley, Fatio and Gregory prompted changes to the text after holding private meetings, but so too did Clerke after an exchange of letters, and we may plausibly speculate that other readers would have done the same if opportunity had allowed. In order to verify the arguments in the book, readers had to reconstruct them in alternative versions that they found more persuasive. It is in this gap between the printed text and his readers' reconstructions of it that we can identify Newton's contemporaries negotiating the validity of his proofs: editing the *Principia* was an automatic consequence of the act of reading it.

2.2 Reading the *Principia* mathematically

2.2.1 Reconstructing Newton's connected prose in symbolic form

The written style of the *Principia* determined the ways in which it was read. Like contemporary texts such as Huygens's *Horologium* and Grégoire de Saint Vincent's *Opus geometricum*, its demonstrations are generally expressed in long, verbose sentences structured in paragraphs of connected prose. This meant that any reader who wished to verify one of the proofs generally did so by recreating its argument in condensed, symbolic form. Checking the validity of Newton's mathematical arguments entailed picking up a pen and recreating his verbal explanations in the form of equations and proportions of ratios.

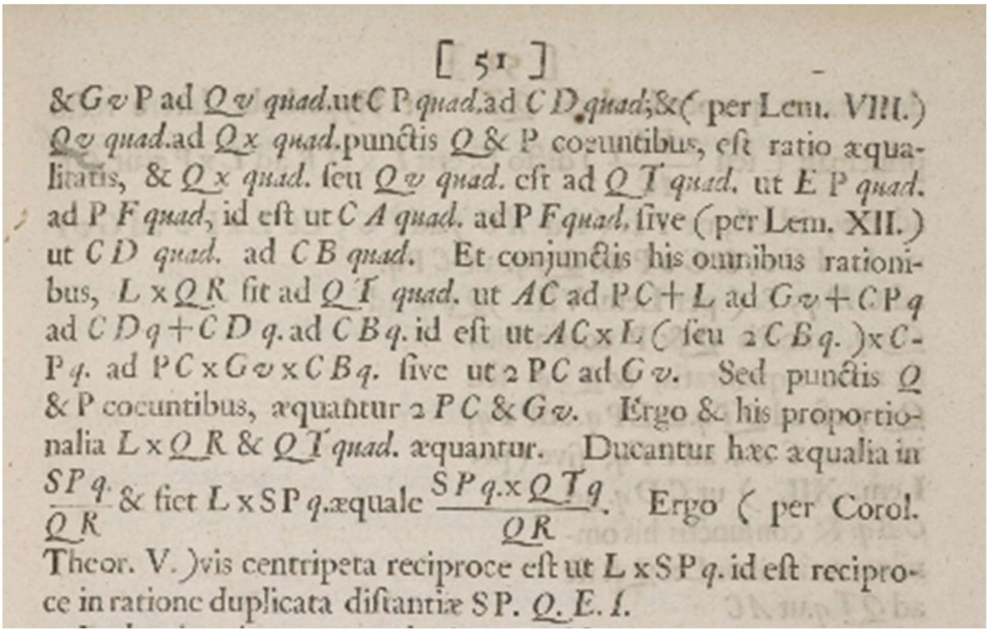
As an example of this process, I shall consider readers' responses to Proposition 11 of Book 1. This is Newton's proof of the inverse-square law for a body moving in an elliptical orbit, and its style is usefully representative of many other demonstrations. This image shows how the result appears in the printed text of the first edition:



Prop. XI. Prob. VI.

Revolvatur corpus in Ellipsi: Requiritur lex vis centripetæ tendentis ad umbilicum Ellipseos.

Esto Ellipseos superioris umbilicus S. Agatur *SP* secans Ellipseos tum diametrum *DK* in *E*, tum ordinatim applicatam *Qv* in *x*, & compleatur parallelogrammum *Qx*P*R*. Patet *E*P æqualem esse semiaxi majori *AC*, eo quod acta ab altero Ellipseos umbilico H linea *HI* ipsi *EC* parallela, (ob æquales *CS*, *CH*) æquentur *ES*, *EI*, adeo ut EP semisumma sit ipsarum PS, P*I*, id est (ob parallelas *HI*, P*R* & angulos æquales *I*P*R*, *H*P*Z*) ipsorum PS, P*H*, quæ conjunctim axem totum 2*AC* adæquant. Ad SP demittatur perpendicularis *QT*, & Ellipseos latere recto principali (seu $\frac{2BC\ quad.}{AC}$) dicto *L*, erit *L*×*QR* ad *L*×P*v* ut *QR* ad P*v*; id est ut *PE* (seu *AC*) ad *PC*: & *L*×P*v* ad *Gv*P ut *L* ad *Gv*; &


[51]
& *Gv* P ad *Qv quad.* ut C P *quad.* ad *C D quad*; & (per Lem. VIII.) *Qv quad.* ad *Qx quad.* punctis *Q* & P coeuntibus, est ratio æqualitatis, & *Qx quad.* seu *Qv quad.* est ad *QT quad.* ut *E* P *quad.* ad P *F quad*, id est ut *C A quad.* ad P *F quad.* sive (per Lem. XII.) ut *C D quad.* ad *C B quad.* Et conjunctis his omnibus rationibus, *L* x *QR* fit ad *QT quad.* ut *AC* ad P*C* + *L* ad *Gv* + *C* P*q* ad *C Dq* + *C D q.* ad *C Bq.* id est ut *AC* x *L* (seu 2 *C Bq.*) x *C*P*q.* ad P*C* x *Gv* x *C Bq.* sive ut 2 P*C* ad *Gv*. Sed punctis *Q* & P coeuntibus, æquantur 2 P *C* & *Gv*. Ergo & his proportionalia *L* x *QR* & *QT quad.* æquantur. Ducantur hæc æqualia in $\frac{SPq}{QR}$ & fiet *L* x SP *q.* æquale $\frac{SPq. \times QTq}{QR}$. Ergo (per Corol. Theor. V.) vis centripeta reciproce est ut L x SP *q.* id est reciproce in ratione duplicata distantiæ SP. *Q. E. I.*

There are many steps a modern reader encounters in verifying this proof, and they can usefully be grouped into three stages.[124] First, the reader is required to process the details of the geometrical construction. He is told that the curve being considered is an ellipse, and that its foci are at *S* and *H*. It has been previously stated in Proposition 10 that *DK* and *PG* are conjugate diameters (which means that *DK* is parallel to the tangent *ZPR*) and that *PF* is perpendicular to *DK*. *QxPR* is a parallelogram; *HI* is parallel to *EC*; and *QT* is perpendicular to *PS*. As the reader comes across each of these facts stated in the text, he finds himself looking at the diagram to see how it fits in with the overall picture. It may be said that the reader understands the construction by corresponding the words in the text with the visual information in the diagram. Notice that, in practical terms, the printed diagram is essential for understanding the construction. It would not be impossible to draw out the diagram from its description if had not been provided, but it would be exceptionally difficult.

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[124] The reader who finds it helpful to do so is encouraged to consult Cohen and Whitman's translation. This is based on the second edition, which appended an alternative proof; two of the cross-references were also slightly altered.

In the second stage, Newton establishes a string of proportions between the component parts of the diagram, based on their geometrical properties. Newton either explicitly states these properties in the text, or, more commonly, assumes that his reader possesses this prior knowledge. In the proof of Proposition 11, Newton assumes that his reader already knows that the corresponding sides of similar triangles are proportional to one another; that the sum of the distances from the foci to any point on an ellipse is constant (the focal distances property); that at any point on an ellipse, the angle between the tangent and the line joining the point to one focus is equal to the angle between the tangent and the line joining the point to the other focus (the reflective property); and the intersecting chord theorems in Propositions 35 and 36 of Book 3 of Euclid's *Elements*. He also reminds the reader that the latus rectum of an ellipse is defined to be the quantity $2BC^2/AC$. In addition, at various stages in the proof he explicitly invokes the earlier results of Lemma 8 (which says that, in the limit of a particular construction, two particular lengths are equal), Lemma 12 (the areas of parallelograms formed by conjugate diameter pairs are always equal) and Proposition 6 (which he calls Theorem 5: the centripetal force is inversely proportional to $SP^2 \times QT^2/QR$).

Based on these properties, Newton first demonstrates that $EP = AC$. The modern reader can most easily understand his argument by recreating it in more familiar notation, as follows:

$$ES = EI \qquad \text{(similar triangles)}$$

$$EP = \frac{PS + PI}{2} \qquad \text{(E is midpoint of S and I)}$$

$$= \frac{PS + PH}{2} \qquad \text{(reflective property)}$$

$$= \frac{2AC}{2} \qquad \text{(focal distances property)}$$

$$= AC \qquad (*)$$

Newton then establishes five further proportions:

$$\frac{L \times QR}{L \times Pv} = \frac{QR}{Pv}$$

$$= \frac{PE}{PC} \qquad \text{(similar triangles)}$$

$$= \frac{AC}{PC} \qquad \text{(by * above)}$$

$$\frac{L \times Pv}{Gv \times vP} = \frac{L}{Gv}$$

$$\frac{Gv \times vP}{Qv^2} = \frac{PC^2}{CD^2} \qquad \text{(intersecting chords)}$$

$$Qv^2 = Qx^2 \qquad \text{(in the limit, by Lemma 8)}$$

$$\frac{Qx^2}{QT^2} = \frac{EP^2}{PF^2} \qquad \text{(similar triangles)}$$

$$= \frac{CA^2}{PF^2} \qquad \text{(by * above)}$$

$$= \frac{CD^2}{CB^2} \qquad \text{(by Lemma 12)}$$

In order to understand where these proportions come from, close attention needs to be paid as Newton sets them out in turn. A modern reader might just about get away with not writing anything down yet, but he certainly needs to be alert to verify each step as it comes. He will frequently need to refer back to the diagram and to previous results in the book.

Having established each of these proportions from the geometrical properties of the construction, in the third stage Newton manipulates them. In the case of Proposition 11, this involves combining all five together. In modern terms, he multiplies together all five left-hand sides, multiplies together all five right-hand sides, and then simplifies:

$$\frac{L \times QR}{L \times Pv} \times \frac{L \times Pv}{Gv \times vP} \times \frac{Gv \times vP}{Qv^2} \times \frac{Qv^2}{Qx^2} \times \frac{Qx^2}{QT^2} = \frac{AC}{PC} \times \frac{L}{Gv} \times \frac{PC^2}{CD^2} \times \frac{CD^2}{CB^2}$$

$$\frac{L \times QR}{QT^2} = \frac{AC \times L \times PC}{Gv \times CB^2}$$

$$\frac{L \times QR}{QT^2} = \frac{2BC^2 \times PC}{Gv \times CB^2} \qquad (\text{since } L = \frac{2BC^2}{AC})$$

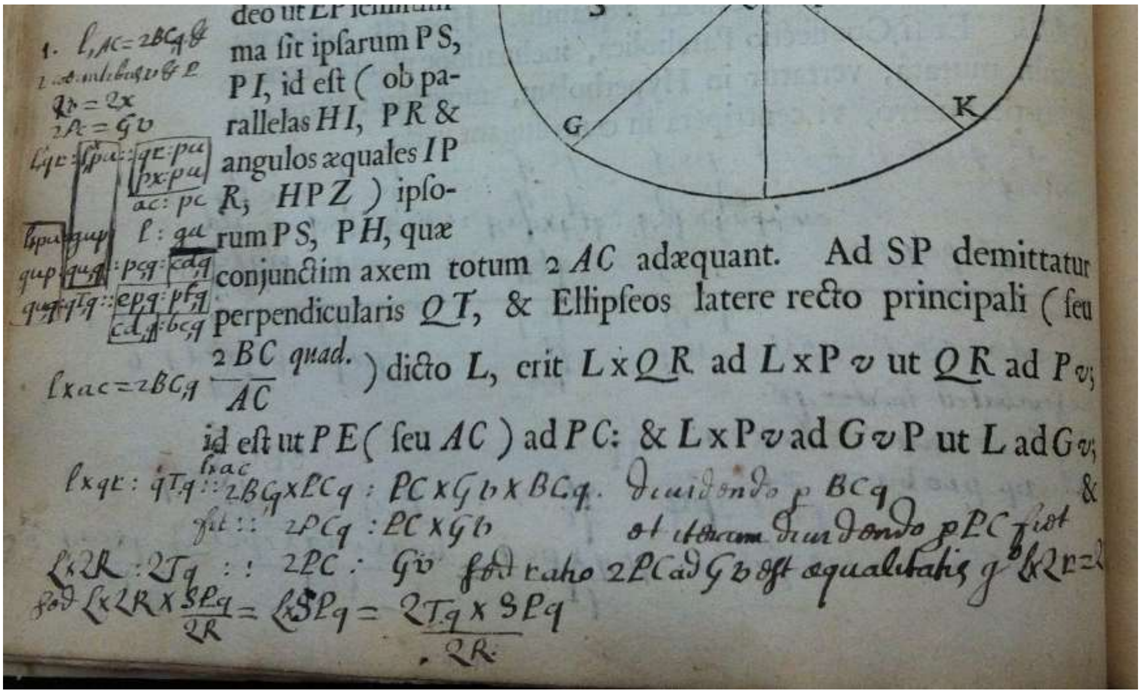
$$\frac{L \times QR}{QT^2} = \frac{2PC}{Gv}$$

Finally, Newton argues that in the limit as *Q* moves towards *P*, $Gv \to 2PC$ from the construction. This means that $L \times QR \to QT^2$, and so $L \times SP^2 \to QT^2 \times SP^2 / QR$. He then invokes Proposition 6 to infer that the centripetal force is inversely proportional to $L \times SP^2$. Since *L* is fixed for a given ellipse, the centripetal force must therefore be inversely proportional to SP^2, as required.

It is during this third stage, I suggest, that any modern reader wishing to check the validity of Newton's argument is obliged to pick up a pen. To understand the construction in the first stage without writing anything down is eminently possible; during the second stage, in which Newton establishes the basic proportions from their geometrical properties, an alert reader can get away with simply nodding along with each ratio as Newton sets it out. But looking back at the way the proof was presented on the page, it is difficult to imagine verifying the third stage without writing out the ratios and seeing the terms simplify. The way the terms are listed in sentences horizontally across the page makes it impossible to see that they reduce in the way that Newton says they do. Once the ratios are set out vertically underneath each other, as they have been above, it is much easier to be convinced of the validity of the proof without writing anything down. But this is not the case when they are set out in continuous prose as they are on the pages of the *Principia*. A modern reader who wishes to verify the third stage in the proof is required to rewrite Newton's verbal explanations in a format that allows the simplification of the ratios to be seen.

The primary evidence shows that this is exactly what Newton's contemporary readers also did. Here are the annotations on page 50 of Flamsteed's copy, directly alongside the proof:[125]

[125] RS, RCN 18577.



[1] 1. $L, AC = 2BC, q$ &

[2] 2 coeuntibus v & P

[3] $Qv = Qx$

[4] $2PC = Gv$

[5] $L \times qr : L \times pv$ :: $qr : pv$

[6] $px : pv$

[7] $ac : pc$

[8] $L \times pv : gvp$ [::] $L : gv$

[9] $gvp : qv, q$ :: $pc, q : cd, q$

[10] $qv, q : qT, q$ :: $ep, q : pf, q$

[11] $cd, q : bc, q$

[12] $L \times ac = 2BC, q$

[13] $L \times qr : qT, q \overset{L \times ac}{::} 2BCq \times PCq : PC \times Gv \times BCq$ dividendo p[er] BCq

[14] fit :: $2PCq : PC \times Gv$ et iterum dividendo p[er] PC fiet

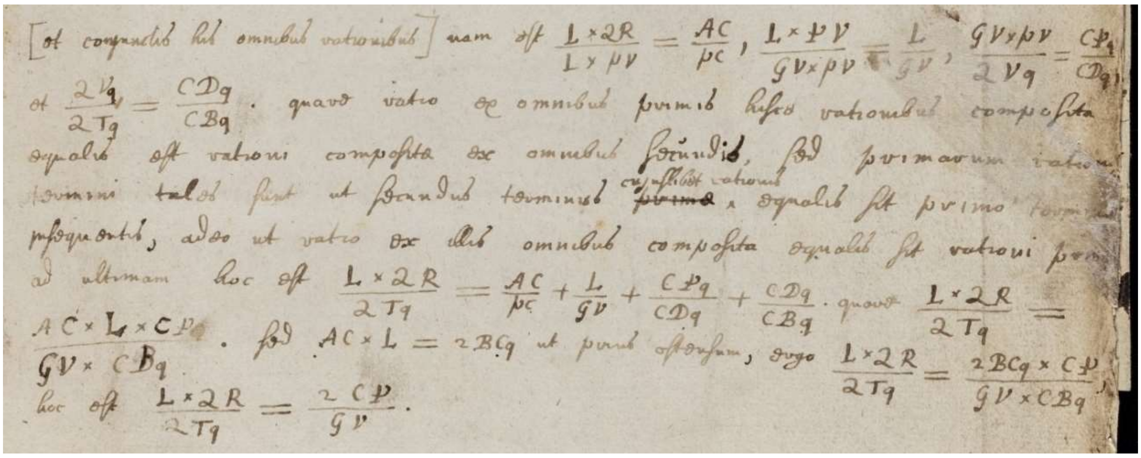
[15] $L \times QR : QTq$ :: $2PC : Gv$ sed ratio $2PC$ ad Gv est aequalitatis [∴] $L \times Qr = QTq$

[16] sed $L \times QR \times \frac{SPq}{QR} = L \times SPq = \frac{QTq \times SPq}{QR}$

Flamsteed has proceeded exactly as above above: in order to see how the terms combine, he has reconstructed the ratios listed in Newton's prose as symbols laid out vertically underneath each other. He begins by re-stating the definition of the latus rectum at line 1, and then records in lines 2–4 that he is considering the limit, at which point (as he can see from the diagram) $Qv = Qx$ and $2PC = Gv$. The key part of his verification comes in lines 5–11, where Flamsteed writes out the ratios in the order that Newton lists them in the text, but set out vertically underneath each other so he can more easily see how they combine. He has drawn boxes around the terms that will cancel. He writes out the result of this combination at line 13, where he also notices that $L \times AC$ can be replaced with $2BC^2$; and over the remaining couple of lines he completes the proof. Importantly, as he works through the ratios his line of thought is very slightly different from the path followed in the text: in line 6, he infers the equality of the ratios $QR : Pv$ and $AC : PC$ by going via the ratio $Px : Pv$, which is not explicitly referred to by Newton; and Flamsteed's line 13 is different from the equivalent line in the printed proof, which still has CD^2 in both the antecedent and the consequent. His reconstruction is therefore not quite the same as the argument in the text.

For comparison, here is Gregory's reconstruction of Proposition 11 in his *Notae*:[126]

[126] RS, MS210, 5r.



[et conjunctis his omnibus rationibus] nam est $\frac{L \times 2R}{L \times pv} = \frac{AC}{pc}$, $\frac{L \times pv}{Gv \times pv} = \frac{L}{Gv}$, $\frac{Gv \times pv}{2vq} = \frac{CPq}{CDq}$ et $\frac{2vq}{2Tq} = \frac{CDq}{CBq}$. quare ratio ex omnibus primis hisce rationibus composita aequalis est rationi compositae ex omnibus secundis, sed primarum rationum termini tales sunt ut secundus terminus cujuslibet rationis ~~primae~~ aequalis sit primo termino subsequentis, adeo ut ratio ex illis omnibus composita aequalis sit rationi primi ad ultimam hoc est $\frac{L \times 2R}{2Tq} = \frac{AC}{pc} + \frac{L}{Gv} + \frac{CPq}{CDq} + \frac{CDq}{CBq}$. quare $\frac{L \times 2R}{2Tq} = \frac{AC \times L \times CPq}{Gv \times CBq}$. sed $AC \times L = 2BCq$ ut prius ostensum, ergo $\frac{L \times 2R}{2Tq} = \frac{2BCq \times CP}{Gv \times CBq}$ hoc est $\frac{L \times 2R}{2Tq} = \frac{2CP}{Gv}$.

Gregory is using slightly different notation from Flamsteed (as will presently be discussed), but his need to verify the proof by writing out the ratios is evident. The reader who consults Fellmann's transcription of his *Marginalia* will see that Leibniz did the same.[127] In all the examples we possess, checking Newton's proofs entailed an active process of independent recreation. To verify his mathematics, Newton's readers had to pick up a pen and rewrite his prose descriptions of proportions in the form of symbolic, algebraic equations.

2.2.2 Using different notation and foundational concepts

The small changes Flamsteed made when writing out Proposition 11 exemplify another important feature of readers' recreations of Newton's proofs: they were often different from those they found printed in the book. As they reconstructed Newton's arguments, readers employed different notation, followed different logical paths, and reinterpreted them using different foundational concepts. They then based their judgements not on Newton's original text, but their own reconstruction of it. Readers were only persuaded by the arguments presented in the *Principia* when they could successfully recreate them on their own terms.

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[127] *Marginalia*, 57–58.

Good examples are provided by readers' responses to Proposition 6. This result is of fundamental importance to the opening sections of the book, and establishes that for a body moving along an orbit of any shape, the centripetal force is inversely proportional to $SP^2 \times QT^2 / QR$, where *SP*, *QT* and *QR* are particular lengths in the construction.[128] In his printed demonstration, having described the construction, Newton once again expresses the resulting ratios in long, prose sentences running across the page. Any reader wishing to verify the proof must therefore pick up a pen and reconstruct his argument symbolically, as may be seen in the written responses of Huygens and Gregory. However, both Huygens and Gregory did so in different terms from those Newton used in the text. Common to both is an adherence – lacking in the printed demonstration – to the Euclidean theory of proportions. Articulated in the fifth book of the *Elements*, this was the main conceptual tool employed in the mathematization of natural philosophy in the first half of the seventeenth century, as used by Galileo and his followers, and Huygens in his *Horologium*. It involved establishing ratios between two homogeneous magnitudes, which is to say, between two magnitudes of the same kind. The ratio between two areas could therefore be formed, or the ratio between two forces, but not the ratio between an area and a force, or between a weight and a volume. This meant that in their studies of kinematics, for example, the Galileans could not say that the velocity of a moving body was the ratio of its distance to its time, but instead had to consider two bodies moving with the same velocity, and observe that the ratio of the two distances is equal to the ratio of the two times. Over the course

[128] The reader is referred to the translation provided in the Notes of Cohen and Whitman's translation. The version in the main body of their text is based on the second edition, in which Proposition 6 was substantially rewritten.

of the seventeenth century, Euclidean proportions were gradually supplanted by Cartesian analytical algebra, which is not restricted by the need for homogeneity, and in which it is acceptable to combine heterogenous quantities. Newton himself cast much of his early work on orbital dynamics in Euclidean form, and the demonstration of the inverse square law he sent to Locke in March 1690 is presented in this way. But this is generally not the case for the proofs in the *Principia*. The clearest indication of this is that, in his orbital proof theorems, Newton considers one body on an orbital path and establishes an expression for that the force at that point, rather than considering two bodies on the same orbital path and establishing the ratio between them.[129]

A version of Proposition 6 that adheres to the theory of proportions would, then – as the printed text does not – derive an expression of the ratio of the forces acting on a body at two different points. This is exactly what Huygens and Gregory reconstructed in their notes. However, this forced them to recreate the proof in very different ways from Newton's original.

The notes Huygens made when working through this result fit onto one side of notes, split into two sections.[130] The first section reads as follows:

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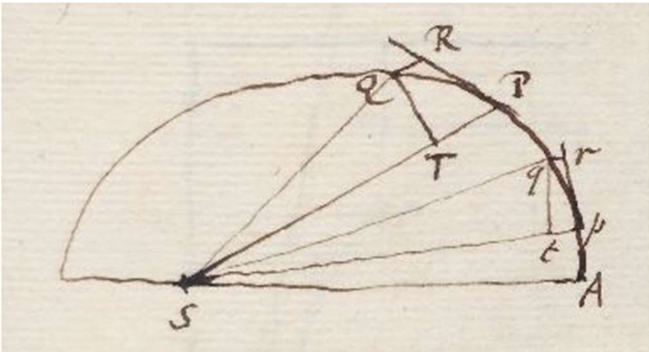
[129] This account of the Euclidean theory of proportions draws on Guicciardini, *Reading the* Principia, 133–5. Locke's proof is at Bodleian, MS Locke c. 31, 101–104.
[130] CH, HUG 7, 15r (=*OC*, 21:417–18). Huygens's commentary on Proposition 6 has also been analysed by Guicciardini, *Reading the* Principia, 130–35.



Ad propos. 6 lib. 1. Neutoni

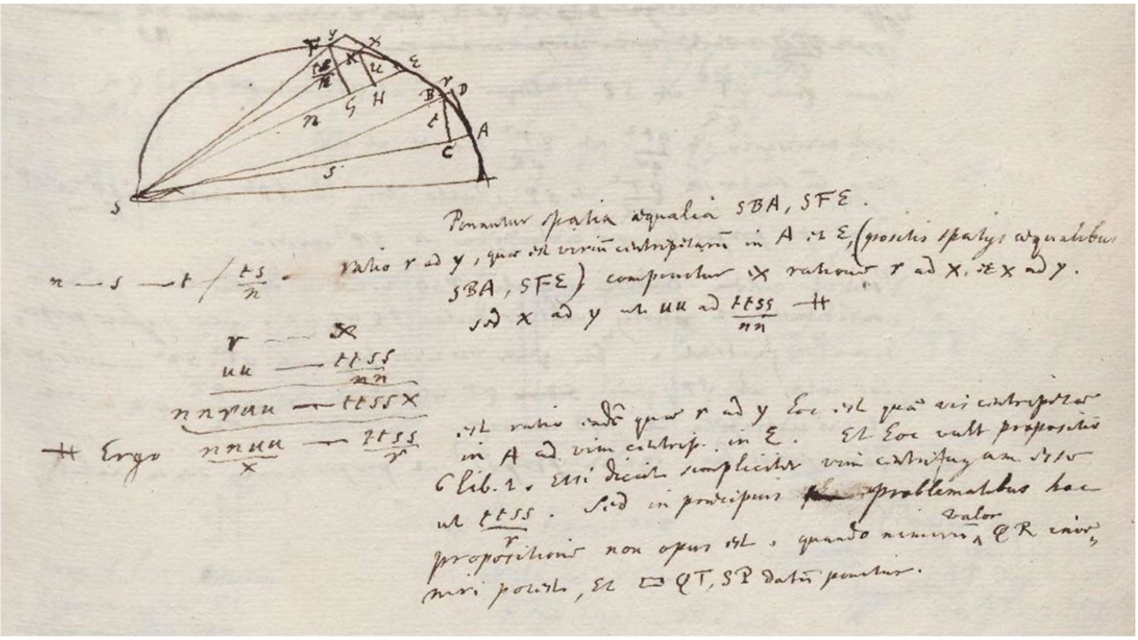
Dicit vim centripetam in P esse *reciproce* ut solidum SP^2 in QT^2/QR.

Commentarium. Ut possit dicere reciproce, necesse est alterum insuper punctum poni vel intelligi ut *p*, in quo vis centripeta comparetur ad vim centripetam quae in *P*. Ut autem hae vires inter se conferantur, oportet spatia *QSP*, *qSp* aequalia esse; hoc est □ *SP*, *QT* aequale □° *Sp*, *qt*. tumque erunt vires centripetae sicut rectae minimae *RQ* ad *rq*. Nec video quid aliud sibi velit haec propositio; nam si dicit esse vim centripetam in *P* ad vim centripetam in *p* sicut Sp^2 in qt^2/rq ad SP^2 in QT^2/RQ, hoc est sicut *RQ* in Sp^2 in qt^2 ad rq in SP^2 in QT^2, haec ratio manifestè eadem est quae *RQ* ad *rq*, quia *Sp*.*qt* aequale *SP*.*QT*, adeoque $sp^2.qt^2$ aequale $SP^2.QT^2$. Quidni igitur dixit vis centrifugas in *P* et *p* esse ut *RQ* ad *rq*. aut quare potius eas esse reciproce ut $SP^2.QT^2/QR$ ad $Sp^2.qt^2/qr$ quam ut reciproce $SP.QT/QR$ ad $Sp.qt/qr$ vel ut reciproce $SP^3.QT^3/QR$ ad $Sp^3.qt^3/qr$.

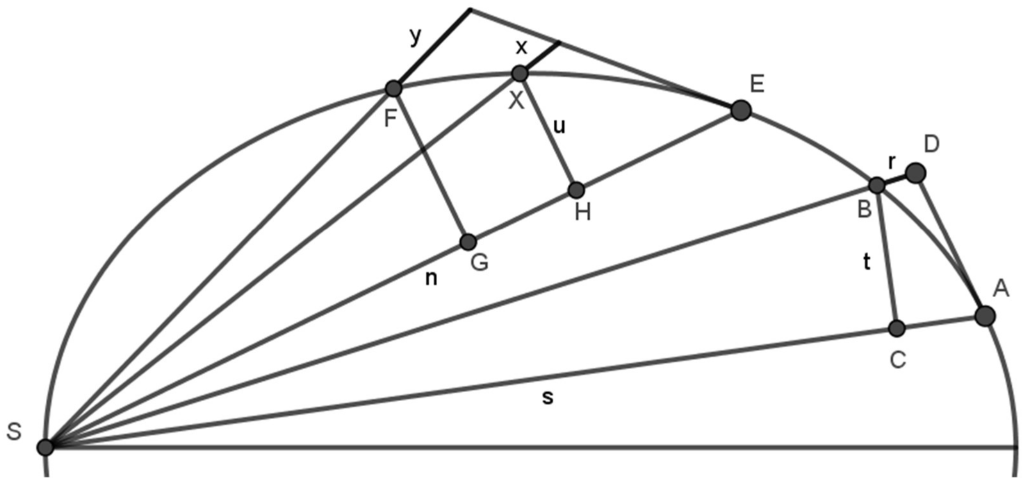
An voluit positis spatijs *QSP*, *qSp* inaequalibus, comparare tamen vires centripetas in *P* et *p*. Hoc erat.

Huygens states the need to consider a second point on the orbit and establish the ratio between the two forces in his opening sentence. He therefore draws a diagram showing two points *p* and *P*, with the required construction on each. For the remainder of his first paragraph he considers the case when the forces act for the same time interval, which means (by Kepler's area law) that the area *QSP* is equal to the area *qSp*, and so $SP \times QT$ is equal to $Sp \times qt$. If the forces at *P* and *p* are measured by the deflections QR and qr from the inertial paths, he therefore observes that it is trivially true that their ratio will be as $Sp^2 \times qt^2/qr$ to $SP^2 \times QT^2/QR$, or reciprocally as $SP^2 \times QT^2/QR$ to $Sp^2 \times qt^2/qr$. Indeed, as Huygens notes, for as long as $SP \times QT$ is equal to $Sp \times qt$, we could say that they will be reciprocally as $SP \times QT/QR$ to $Sp \times qt/qr$, or reciprocally as $SP^3 \times QT^3/QR$ to $Sp^3 \times qt^3/qr$, since all of these ratios are equal to *QR* to *qr*.

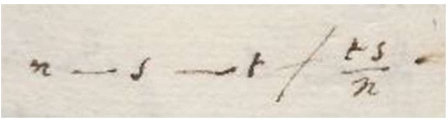
In order to prove Newton's assertion, he argues, he needs also to consider what happens when the time intervals are not equal. He does this in the second half of his note, which is harder to transcribe:



His diagram may be reproduced as follows:



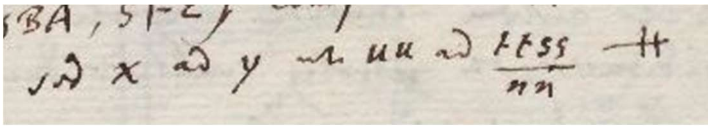
The two bodies are at points *E* and *A*, and he is considering their motion in unequal times to points *X* and *B* respectively. Huygens first considers the point *F* that the first body would reach in the same time that it takes the second body to move from *A* to *B*. That is, point *F* is such that the areas *SBA* and *SFE* are equal, and under the diagram on the right he has written “Ponantur spatia aequalia *SBA*, *SFE.*” Under the diagram on the left, he has written



This is notation is equivalent to

$$n : s = t : \frac{ts}{n}$$

This is trivially true: Huygens is writing it down to establish that, if *n*, *s* and *t* represent the lengths *ES*, *AS* and *BC*, then *FG* is represented by ts/n . In other words, this is the notation Huygens uses to establish what is called the "fourth proportional" to three given quantities. Since the areas *SBA* and *SFE* are equal, he knows that the ratio of the force at *A* to the force at *E* is equal to the ratio of the length *r* to the length *y*. This ratio is composed of the ratio of *r* to *x* and the ratio of *x* to *y*. However, Huygens has written,



In other words,

$$x : y = u^2 : \frac{t^2 s^2}{n^2} \qquad (*)$$

It may not be immediately obvious where this has come from: Guicciardini explains that, for an arc subtended by an infinitely small angle, "the versed sine, $1 - \cos\theta$, is proportional to the square of the sine," as the modern reader may easily verify from series expansions.[131] This gives the ratio of the deflections experienced by one body acted on by the same force over different periods of time. To establish the ratio of the deflections of two bodies acted on by different forces over different periods of time, (*) is composed with the ratio $r : x$ to produce

[131] Guicciardini, *Reading the* Principia, 133.

$$r : y = ru^2 : \frac{t^2 s^2 x}{n^2}$$

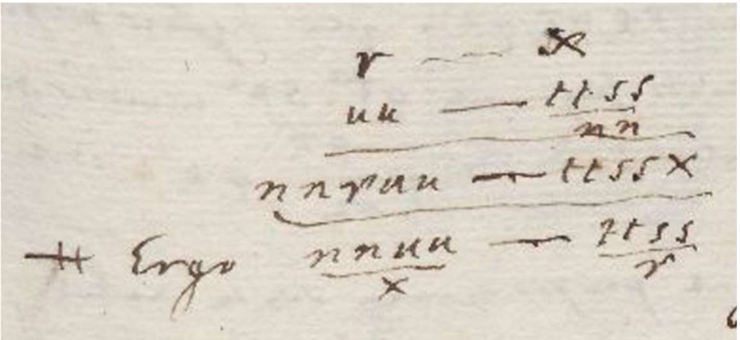
which is to say

$$r : y = n^2 r u^2 : t^2 s^2 x$$

or

$$r : y = \frac{n^2 u^2}{x} : \frac{t^2 s^2}{r}$$

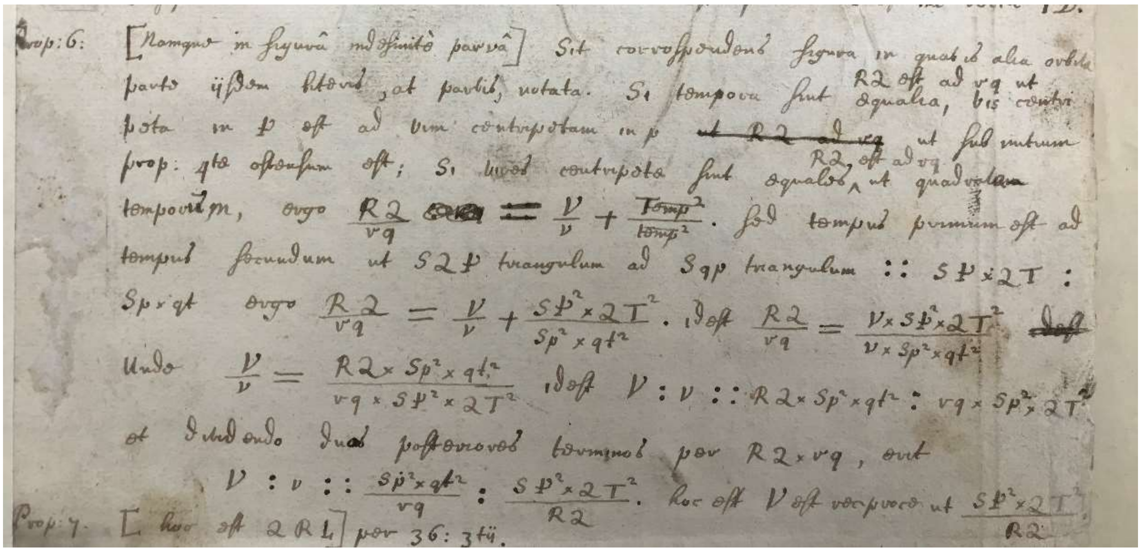
Huygens presents this as follows:



He has therefore established that the ratio of the force at *A* to the force at *E* is equal to the ratio of n^2u^2/x to t^2s^2/r, or reciprocally as t^2s^2/r to n^2u^2/x. Referring back to Huygens' diagram shows that these terms are indeed equivalent to Newton's expression $SP^2 \times QT^2/QR$ at the respective points, as required.

The details of Huygens' proof are less important than the fact that it is extremely different from Newton's. He uses different notation, and operates within a different conceptual basis. He deploys a result about the versed sine of an angle that is not referred to in the text. Because he wishes to adhere to Euclidean proportion theory, he is forced to construct an argument logically distinct from the one that appears in the book.

Gregory also recreates Newton's proof in terms of Euclidean proportions, but does so in a different way. Here is his entry on Proposition 6 in the *Notae*:[132]



Like Huygens, Gregory also imagines a "corresponding figure in some other section of the orbit," and considers the ratios between the forces at these two points. To represent the forces he introduces the symbols *V* and *v*, not used by Newton, and derives the required proportion. But it is noticeable that Gregory employs a variety of notation within this passage. He uses two distinct sets of symbols, and switches between them as he writes. On the one hand, he uses

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[132] RS, MS210, 4r.

proportions of ratios, like Huygens; and on the other, he employs what look like algebraic equations. He also writes

$$\frac{RQ}{rq} = \frac{V}{v} + \frac{Time^2}{time^2}$$

as a shorthand for “the ratio of *RQ* to *rq* is compounded of the ratio of *V* to *v* and the ratio of T^2 to t^2.” Indeed, in the fifth line of the note he begins to write “ $RQ : rq :: \ldots$”, but realises that he cannot easily compound ratios using this notation, so has to switch. Once he has done so, he can combine the terms to produce

$$\frac{RQ}{rq} = \frac{V \times SP^2 \times QT^2}{v \times Sp^2 \times qt^2}$$

which he can rearrange to

$$\frac{V}{v} = \frac{RQ \times Sp^2 \times qt^2}{rq \times SP^2 \times QT^2}$$

He is then able to retranslate this back into the proportion $V : v = RQ \times Sp^2 \times qt^2 : rq \times SP^2 \times QT^2$, from which the result quickly follows. Because the notation of proportions doesn't easily allow him to compound complicated ratios, he has to manipulate the equations first, until they can be re-expressed in Euclidean form. Indeed, this is typical of his workings in the *Notae*:

Gregory repeatedly forms Euclidean proportions from Newton's text, immediately rewrites them as equations, manipulates the equations, and then turns them back into proportions. The style of Newton's proofs forces him to do this. Newton's text is typically expressed in terms of the force at a single point rather than in terms of the ratio of the two forces at different points, and yet his ratios are more easily manipulated when expressed in algebraic form. Gregory is therefore obliged repeatedly to switch between the two.

In his analysis of Huygens' notes on Proposition 6, Guicciardini labels Huygens' comments as "criticisms," and concludes that "the use of proportion theory in the *Principia* was not acceptable to Huygens."[133] But seen alongside Gregory's notes, I suggest that "criticisms" is too strong a word. Rather, Newton's readers reformulated his demonstrations in their own terms, and for Huygens and Gregory an acceptable reformulation needed to adhere to the principles of Euclidean proportion theory. Verifying Newton's proofs involved a positive act of reconstruction, and each reader reconstructed the demonstration in a slightly different way. They used different notation, employed different foundational concepts, and reached the conclusion by different logical routes. Newton's readers rarely followed exactly the same path as the author to arrive at his conclusions.

2.2.3 Simplifying the proofs

Sometimes in the process of verifying the proofs Newton's readers spotted ways in which they could be simplified. A clear example occurs in Fatio's reading notes, where he recorded a straightforward adjustment that could be

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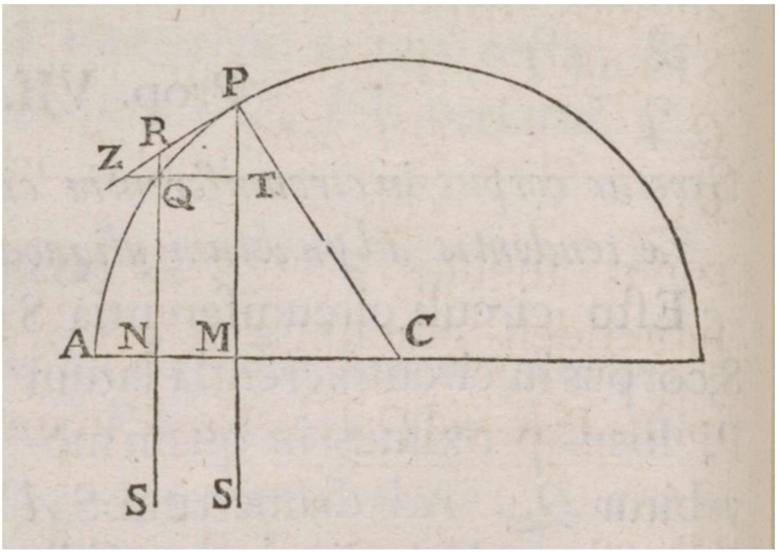
[133] Guicciardini, *Reading the* Principia, 119; he describes the notes as "criticisms" on pages 118, 125 and 128.

made to simplify the proof of Proposition 8 in Book 1. This result concerns a body orbiting along a semi-circular path under the action of a force always directed to an infinitely distant point:



The semi-circular path of the orbit is as marked, and the infinitely distant point towards which the force is directed, S, is imagined to be a long way below the diagram, such that the line of action of the force *PMS* is always perpendicular to the diameter *ANMC* as *P* moves around the orbit.

Proposition 8 proves that the magnitude of the force as *P* moves around the semi-circle must vary inversely as the cube of the distance *PM*. Newton's argument in the first edition is as follows. The triangles *CPM* and *PZT* are similar, and in the limit as *Q* approaches *P*, Lemma 8 tells us that *PZT* is similar to *PQT*. Therefore $CP^2 : PM^2 = PQ^2 : QT^2$. But Lemma 7 says that, in the limit as *Q* approaches *P*, $PQ = PR$. This gives $CP^2 : PM^2 = PR^2 : QT^2$. Furthermore, because the orbit is a semi-circle, we have from Proposition 36 of Book 3 of the *Elements* (the tangent-secant theorem) that $PR^2 = QR \times (RN + QN)$. In the limit as Q approaches R, this becomes $PR^2 = QR \times 2PM$. Combining these, we have $CP^2 : PM^2 = QR \times 2PM : QT^2$, and therefore

$$\frac{QT^2}{QR} = \frac{2PM^3}{CP^2}$$
$$\frac{QT^2 \times SP^2}{QR} = \frac{2PM^3 \times SP^2}{CP^2}$$

Proposition 6 says that the force is inversely proportional to $QT^2 \times SP^2 / QR$. Because CP is constant and SP can be considered unchanging because the force is directed towards an infinitely-distant point (the phrase in the text is "neglecta ratione determinata $2SP$ quad./CP quad."), this implies that the force is inversely proportional to PM^3, as required.

Fatio's improvement to this proof is extremely simple: he merely points out that in the opening steps, the invocations of Lemmas 8 and 7 are unnecessary. This is his entry in his reading notes:[134]

> l. 10 jungetur *CP*. Ob similia triangula *CPM*, *TPZ*, est *CP*q. ad *PM*q. ut *PR*q. ad *QT*q. et ex natura circuli &c. Nulla enim lemmatum VIII et VII citatione opus est.

CPM is similar to *PZT*, says Fatio, and so the ratio of *CP* to *PM* is equal to the ratio of *PR* to *QT*. This leads directly to $CP^2 : PM^2 = PR^2 : QT^2$, without having to invoke the limit result of either of the Lemmas.

As it happens this improvement was not transcribed by Newton into either his annotated or interleaved copies, although a very similar change was made

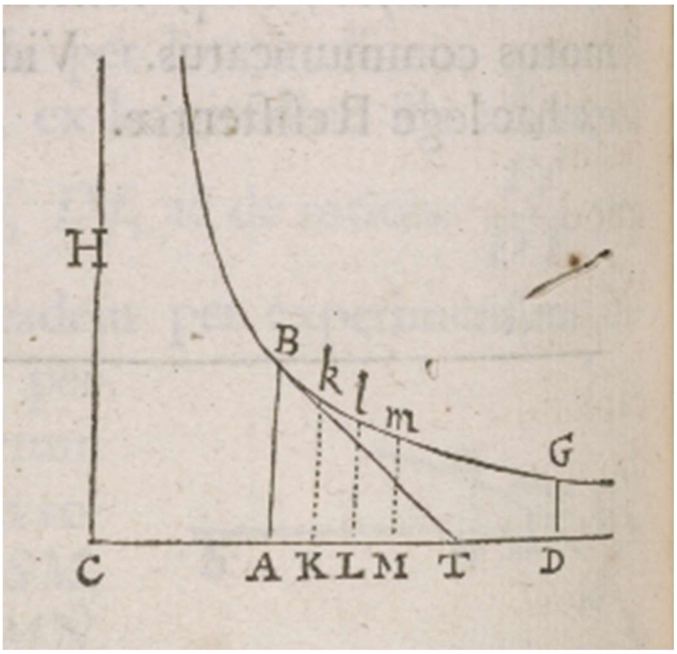
[134] *OC*, 10:149.

much later in the third edition. It is nevertheless clear that, as he was recreating the proof in order to verify it, Fatio noticed an improvement that could be made. And although superficially it only involves deleting a handful of words from the printed text, from a logical point of view the simplification is very substantial, since Fatio's version does not depend upon the proofs of Lemmas 7 and 8, which in turn depend on Lemmas 1, 5, and 6. Fatio generated a new, simpler, logically distinct proof as an automatic consequence of the act of reading it.

2.2.4 Proving the same result by different means

In addition to simplifying Newton's own proofs, readers sometimes generated their own demonstration of the conclusion in the text by substantively different means. A good example is Huygens' reading of Proposition 5 of Book 2, in which, unable to follow the derivation in the text, Huygens generated his own proof using series expansions. This example will also be relevant to the discussion in Chapter 5 of Newton's limit methods, but my purpose here is merely to demonstrate how Huygens verified this result by constructing a proof different from the one in the text.

Proposition 5 concerns a body moving in a resistive medium in which the resistance is proportional to the square of the velocity. The main result states that under such conditions, if a series of times is taken in decreasing geometric progression, then the velocities at these times will vary inversely as that progression, and the spaces described between the times will be equal. This is very easily proved using modern notation: omitting constants of proportionality, Newton's Second Law gives the differential equation $-v^2 = m dv/dt$, which yields $v = 1/t$; this leads to $s = \ln t$, from which the result about geometric progression follows. Newton's proof runs in the other direction. He starts by assuming that the velocity is inversely proportional to the time, and then derives that the decrements in the velocity must be proportional to the squares of those velocities; the result about geometric progression then follows. He therefore begins by drawing a hyperbola:



The horizontal axis represent time, and the vertical axis velocity. *AK*, *KL* and *LM* represent equal increments of time. Because the curve is a hyperbola, we have

$$AB \times CA = CK \times Kk$$
$$\frac{AB}{Kk} = \frac{CK}{CA}$$

Newton then manipulates these ratios as follows:

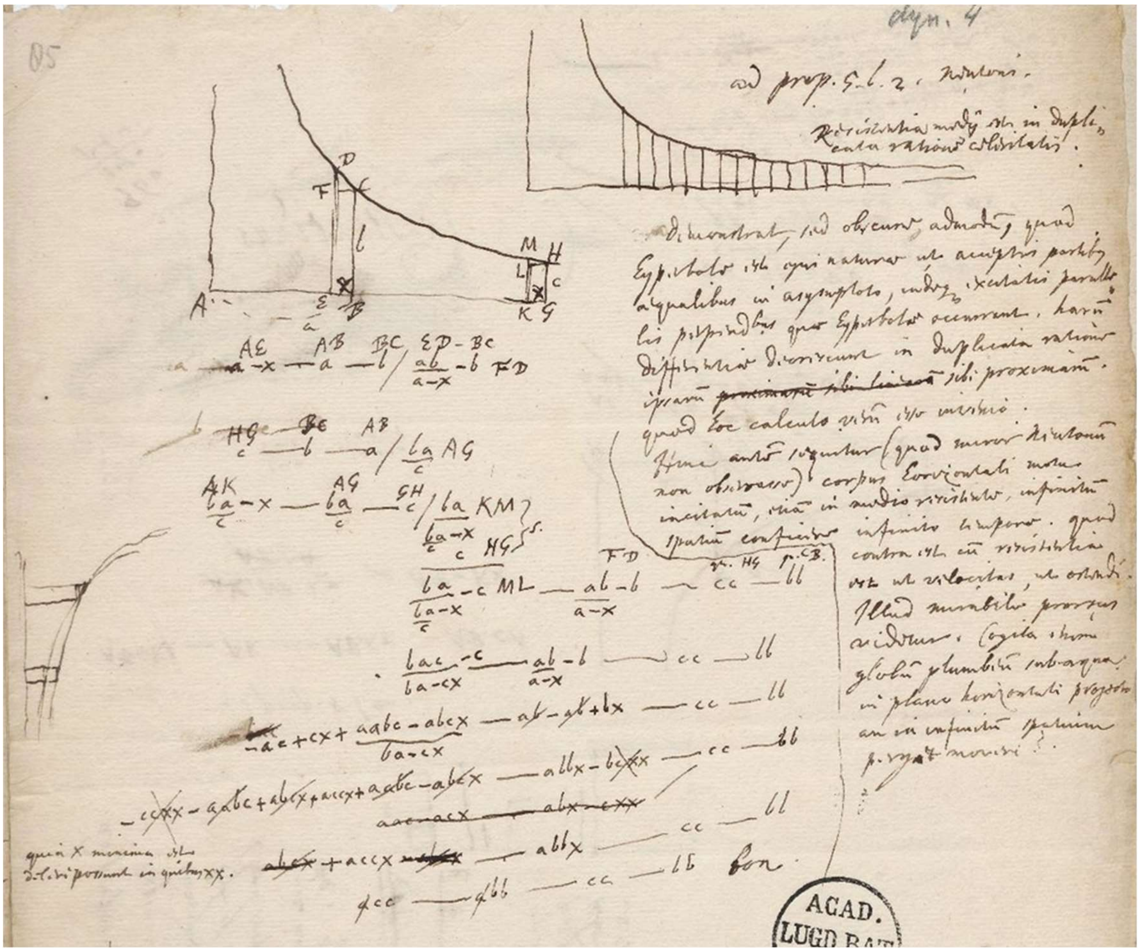
$$\frac{AB}{Kk} = \frac{CK}{CA}$$
$$\frac{AB - Kk}{Kk} = \frac{CK - CA}{CA}$$
$$\frac{AB - Kk}{Kk} = \frac{AK}{CA}$$
$$\frac{AB - Kk}{AK} = \frac{Kk}{CA}$$
$$\frac{AB - Kk}{AK} = \frac{AB \times Kk}{AB \times CA}$$

For a given hyperbola, Newton then notes that $AB \times CA$ is fixed. And he had previously set *AK*, *KL* and *LM* to be fixed, equal intervals. So as the point *A* varies in location along the horizontal axis, $AB - Kk$ will be proportional to $AB \times Kk$. If the distance *AK* is now decreased, then in the limit ("ultimo, ubi coeunt *AB* & *Kk*"), $AB - Kk$ will be proportional to AB^2. A similar argument gives

$$AB - Kk \propto AB^2$$
$$Kk - Ll \propto Kk^2$$
$$Ll - Mm \propto Ll^2$$

So, if the horizontal axis represents time and the vertical axis represents velocity, then successive decreases in velocity are proportional to the square of the starting velocity, which means that the resistive force is proportional to the square of the velocity. Newton has thus demonstrated that if the velocity-time graph is a hyperbola, then the resistive force is proportional to the square of the velocity. Reversing the direction of implication shows that if the resistive force is proportional to the square of the velocity, then the velocity is inversely proportional to the time, as required.

Huygens proves Newton's result by completely different means. Here are his notes on this proposition:[135]



The text on the right may be transcribed as follows:

Ad prop. 5 l. 2. Neutoni.

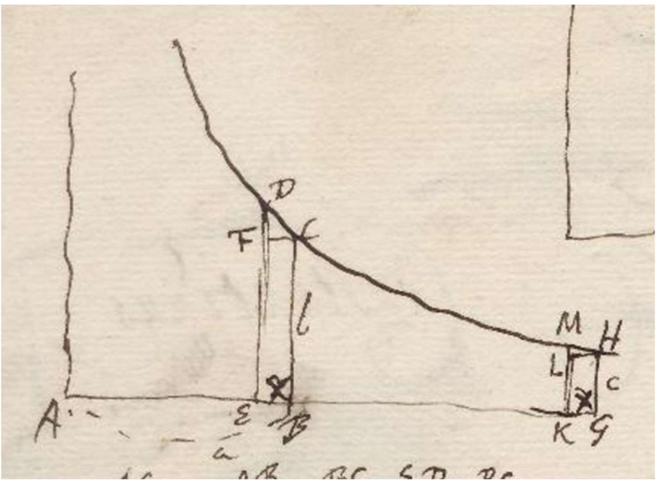
Resistentia medij est in duplicata ratione celeritatis.

[135] CH, HUG 26, 85r (=*OC*, 21:420–1).

Demonstrat, sed obscure admodum, quod hyperbole est ejus naturae ut acceptis partibus aequalibus in asymptoto, indeque excitatis parallelis perpendicularibus quae hyperbolae occurrant, harum differentiae decrescunt in duplicata ratione ipsarum sibi proximarum. quod hoc calculo verum esse invenio.

Hinc autem sequitur (quod miror Neutonum non observasse) corpus horizontali motu incitatum, etiam in medio resistente infinitum spatium conficere infinito tempore. quod contra est cum resistentia est ut velocitas, ut ostendi. Illud mirabile prorsus videtur. Cogita enim globum plumbeum sub aqua in plano horizontali projectum an in infinitum spatium perget moveri?

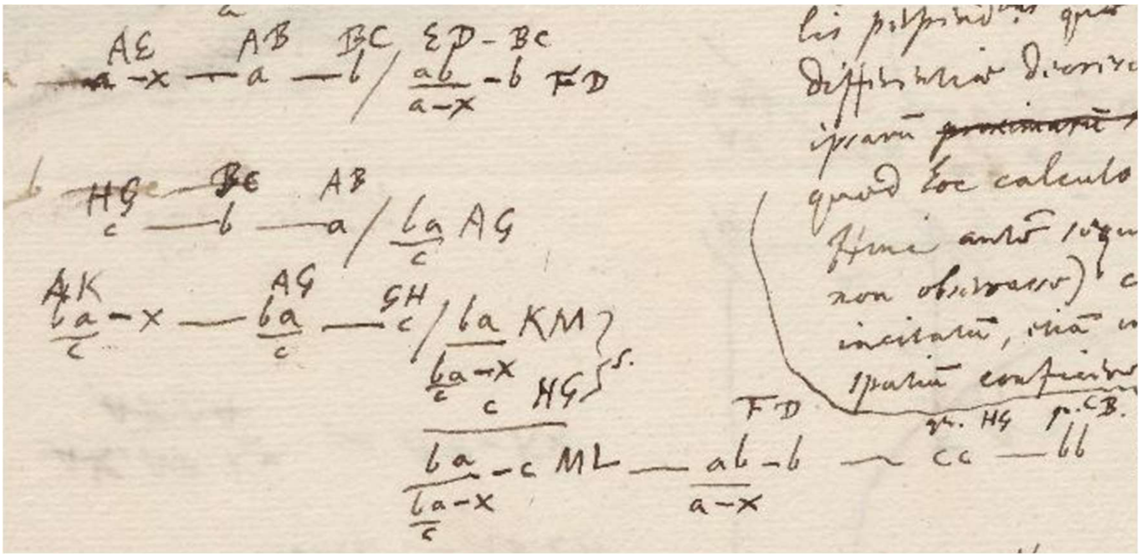
It is immediately obvious that Huygens’ diagram is very different from Newton’s:



In accordance with Euclidean proportion theory, he is going to establish the ratio of a certain quantity at two different points on the curve, rather than – as Newton does – an expression for that quantity calculated in the limit at one point on the curve. He has labelled the points differently, and has marked on two equal time intervals *EB* and *KG*, assumed small, which are equivalent to

Newton's interval *AK*. Huygens has also drawn horizontal lines *CF* and *HL* across the top of each section of the curve, which Newton has not.

The working underneath contains Huygens' notation for finding the fourth proportional, recognisable from the above discussion of Proposition 6:



Huygens has labelled each lower-case expression with the corresponding upper-case length in the construction, so $(a-x)$ is equal to *AE*, *a* is equal to *AB*, and so on. The last term written on each line is the quantity he is trying to evaluate, such that in the first line he derives an expression for *FD*, in the second an expression for *AG*, then *KM*, *HG* and *ML*.

Each of Huygens' lines needs to be worked through in turn to understand how his proof differs from Newton's. Huygens begins by finding an expression for *FD*. Because the curve is a hyperbola, $AE : AB = BC : ED$, and so *ED* is the fourth proportional of *AE*, *AB* and *BD*. On his diagram he has marked $AB = a$,

$BC = b$, and $EB = x$. So ED is the fourth proportional of $(a-x)$, a, and b. That is, $ED = ab/(a-x)$. And because $FD = ED - BC$, this gives $FD = ab/(a-x) - b$.

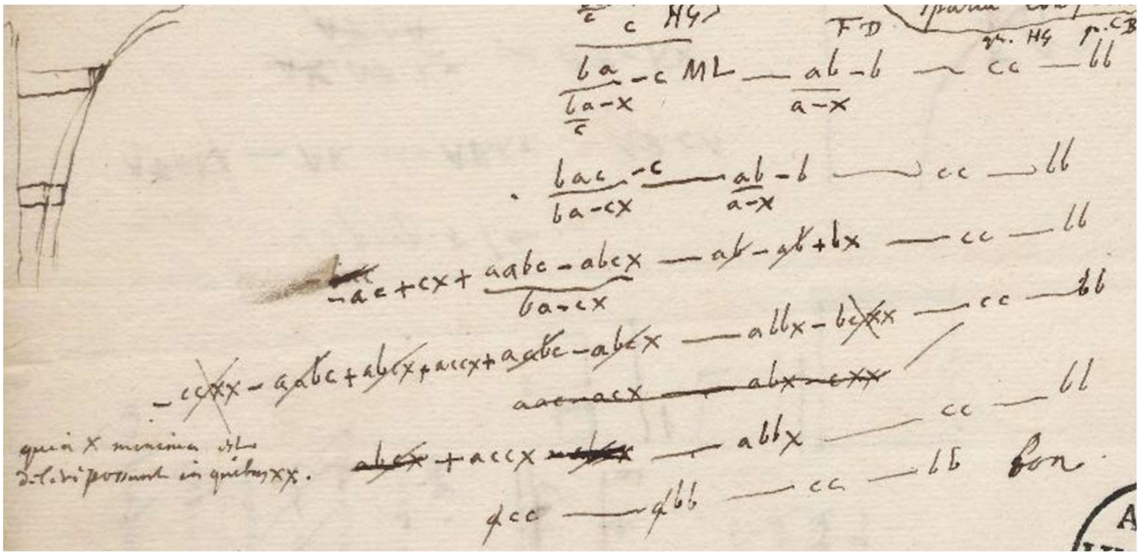
In an extremely similar way, in the second line he establishes that $AG = ab/c$, and in the third that

$$KM = \frac{ab}{\frac{ab}{c} - x}.$$

Directly underneath he records that *HG* is denoted on the diagram by *c*, and then he subtracts ("s.") to give:

$$ML = \frac{ab}{\frac{bc}{c} - x} - c$$

ML and *FD* represent the decreases in velocity during the respective time intervals *KG* and *EB*, and so are a measure of the resistive forces acting. Huygens is aiming to prove that the ratio of the resistive forces is equal to the ratio of the squares of the velocities. In the final lines of working he therefore verifies that $ML : FD = c^2 : b^2$:



It can be seen that he has done so by leaving the right-hand ratio $c^2 : b^2$ alone, and manipulating $ML : FD$ on the left. He has first simplified the expression for ML and then, in the third line, multiplied both terms by $(a-x)$. To get to the fourth line, he multiplies by $(ab-cx)$ to give

$$-ccxx - aabc + abcx + accx + aabc - abcx : abbx - bcxx$$

His instinct (in the deleted antepenultimate line) is to try dividing the terms towards the right by *b*, but he then realises that the left will simplify:

$$-ccxx + accx : abbx - bcxx$$

Crucially, he then sees that "quia *x* minima est deleri possunt in quibus *xx,*" and spots that he can make the result work by neglecting the x^2 terms, just as he is used to doing when dealing with infinite series. This leaves him with $accx : abbx$, which is indeed equal to $c^2 : b^2$. The resistive forces will thus be in the ratio of the squares of the velocities, and the stated results of Proposition 5 about times in geometric progression follow immediately.

It is obvious that Huygens' proof is completely independent of Newton's. He has used a different diagram, a different notation, a different conception of proportion, and the methodology of infinite series rather than geometric limits. He has produced a substantively different derivation of the result, proving Newton's conclusion by entirely different means. Moreover Huygens did not generate his proof in addition to working through Newton's, but because he was not able to follow it. The lines at the top of his note can be translated as follows:

> With regard to Newton's Proposition 5 of Book 2. The resistance of the medium is in the squared ratio of the speed. He demonstrates, but very obscurely, that the hyperbola is of its nature such that, if equal sections are taken along the asymptote, and from there parallel perpendiculars are constructed which go to meet the hyperbola, the differences of these decrease in the squared ratio of the next one. Which I find to be true by this calculation.

The phrasing here ("Demonstrat, sed obscure admodum", "quod hoc calculo verum esse invenio") suggests that Huygens was not able to verify the proof of Proposition 5 he found in the text, and was only able to believe its conclusion because of his own, independent working. If he hadn't generated his own derivation, he would not have been persuaded of the correctness of Newton's assertion: it was his own reconstruction, and not the words printed on the page, that convinced him the result was true.

2.2.5 Generating new results

As well as reconstructing, simplifying and producing alternatives to Newton's proofs, the act of working through his demonstrations sometimes led readers to generate novel conclusions. Not all readers did this, since their circumstances and motives varied, as was shown in Chapter 1. But Bertoloni Meli has demonstrated that reading the *Principia* directly stimulated Leibniz to develop his own ideas; and the other reader who routinely generated new results as he studied Newton's book was Fatio. His developments are far less substantial than Leibniz's, but as he read he often produced extensions of the results he found in the text.

Small instances are easy to list. His marginal note alongside Lemma 26 demonstrates that the three circles in the construction do indeed intersect, for example; this concurrence is not required for Newton's result and so Fatio's proof is not necessary, but he composes one nevertheless. He also correctly notes that the result of Proposition 38 about falling bodies can be applied to pendula describing very small arcs. And when working through the Scholium after Proposition 29, he correctly observes that if the quadrilateral *fghi* expands in such a way that *f*, *g* and *h* all move along straight lines, then the point *i* must also move along a straight line. This remark is not particularly relevant to the result, but it is true, and Fatio says he is able to prove it.[136]

If these extensions are relatively obscure, other suggestions are more prominent. For example, he made the following note alongside Proposition 4 in Book 1:[137]

> Si sint T, t; tempora period. R, r; radii circulorum; erunt $\frac{R}{T}$, $\frac{r}{t}$; ut velocites. $\frac{R}{T^2}$, $\frac{r}{t^2}$; ut vires centrip. Si sit praetera $T^n.t^n :: R^m.r^m$ erunt

[136] Bodleian, Arch. A d.37, 97, 121, 103; see also the loose sheet between pages 100 and 101.
[137] Bodleian, Arch. A d.37, 42. He made an almost identical note in his reading notes, *OC*, 10:149.

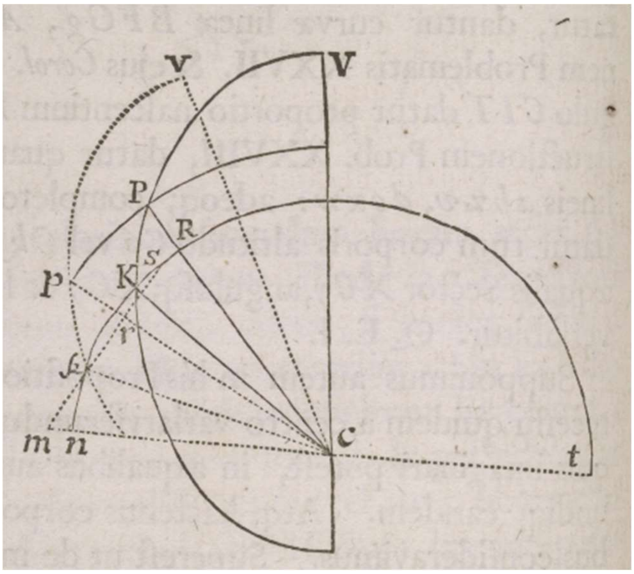
$R^{1-\frac{m}{n}}$; $r^{1-\frac{m}{n}}$ ut veloc. $R^{1-\frac{2m}{n}}$; $r^{1-\frac{2m}{n}}$ ut vires Centr. Itemque erunt $T^{\frac{n}{m}-1}$; $t^{\frac{n}{m}-1}$; ut veloc. $T^{\frac{n}{m}-2}$; $t^{\frac{n}{m}-2}$; ut vires centr.

If the time periods are T and t, and the radii of the circles R and r, then the velocities will be as $\frac{R}{T}$ and $\frac{r}{t}$, and the centripetal forces as $\frac{R}{T^2}$ and $\frac{r}{t^2}$. Moreover if $T^n \times t^n = R^m \times r^m$, then the velocities will be as $R^{1-\frac{m}{n}}$ and $r^{1-\frac{m}{n}}$, and the centripetal forces as $R^{1-\frac{2m}{n}}$ and $r^{1-\frac{2m}{n}}$; and furthermore the velocities will be as $T^{\frac{n}{m}-1}$ and $t^{\frac{n}{m}-1}$, and the centripetal forces as $T^{\frac{n}{m}-2}$ and $t^{\frac{n}{m}-2}$.

This important theorem concerns uniform circular motion, and in the first edition contained seven corollaries. Expressed in modern notation, Corollary 1 says that $F \propto v^2/r$; Corollary 2 that $F \propto r/T^2$; Corollary 3 that if T is fixed then $F \propto r$ and $v \propto r$; Corollary 4 that if $T^2 \propto r$ then F is fixed and $v \propto \sqrt{r}$; Corollary 5 that if $T^2 \propto r^2$ then $F \propto 1/r$ and v is fixed; Corollary 6 that if $T^2 \propto r^3$ then $F \propto 1/T^2$ and $v \propto 1/\sqrt{r}$; and Corollary 7 that these proportions hold when bodies describe similar parts of similar figures. In his marginal note, Fatio has generalised these results. He first recapitulates that $v \propto r/T$ and that $F \propto r/T^2$, and then correctly notes that, if $T^n \propto r^m$, then $v \propto r^{1-\frac{m}{n}}$, $F \propto r^{1-\frac{2m}{n}}$, $v \propto t^{\frac{n}{m}-1}$ and $F \propto t^{\frac{n}{m}-2}$. These expressions are correct, as the reader may easily verify; and Newton was sufficiently persuaded of their utility that as a result of their discussion in May 1690 he inserted them as an extra corollary, as the pages of his annotated copy show.

A much more substantial example lies in the long marginal note Fatio made alongside Proposition 43 of Book 1. I shall refer to this example again in

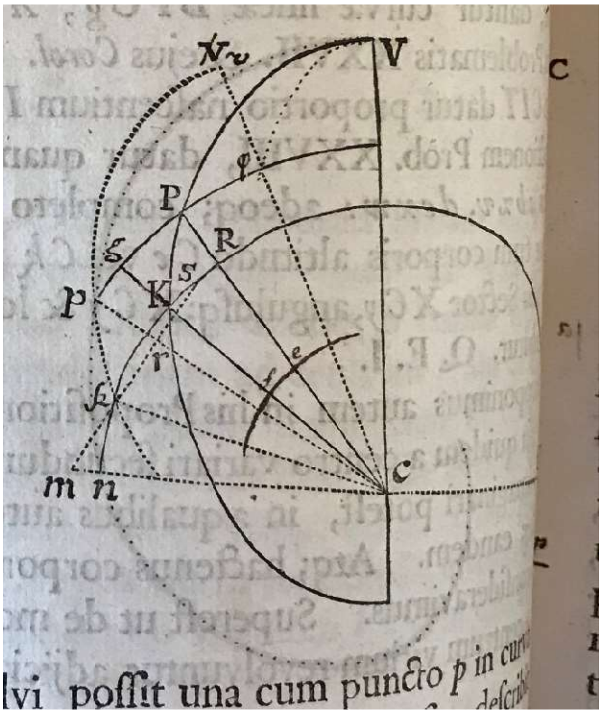
Chapter 4, since it is the only instance I have found of a reader applying Newton’s geometrical limit methods to create a new result. Proposition 43 appears at the start of Section 9, which concerns the precession of orbits. In his construction, Newton considers two bodies *P* and *p* following orbits of the same shape, but *p*’s orbit precesses anticlockwise:



The solid curve *VPK* represents the stationary orbit along which the body *P* travels, and the dotted curve *vpk* represents the similar precessing orbit of body *p*. Both orbit the fixed point *C* in such a way that at any given moment the distance *CP* is equal to the distance *Cp*. Newton restricts his analysis in Section 9 to the case of an orbit precessing such that both (i) angle *vCp* is always equal to angle *VCP*, and also (ii) the angle *VCp* increases in direct proportion to the angle *VCP* (i.e. *VCp* is always a fixed multiple of *VCP*). It is notable these two restrictions imply that the angle *VCv* is almost certainly not increasing at a steady rate, unless *VCP* is increasing in a particularly idiosyncratic way.

Newton's analysis does not therefore apply to what might naturally be described as a "steadily precessing" orbit.

Unhelpfully, the path of *p* as its orbit precesses is not indicated on Newton's diagram. Fatio has marked it in on his copy, as can just about be seen from this image:[138]



Both *P* and *p* start at *V*, at which point their orbits coincide. As they both begin to move and the orbit of *p* precessess anticlockwise, the body *p* moves through the point on the diagram marked *p*, then on through the point labelled *n*, and along a spiral back up the other side of *C*. In Proposition 43, Newton demonstrates that – so long as the orbit *vpk* precesses subject to the restrictions (i) and (ii) above – if the body *P* orbits about the point *C* in accordance with Kepler's area law, then so too does body *p*. The force acting

[138] Bodleian, Arch. A d.37, 132.

on *p* must therefore be centripetal, with no transverse component. In Proposition 44 Newton finds the size of this centripetal force, proving that the additional centripetal force required to make any given orbit precess is inversely proportional to the cube of the distance *Cp*.

In the margin of his copy, Fatio has composed a long note and additional Scholium, intended to be inserted between Proposition 43 and Proposition 44. In his proposed Scholium, Fatio first acknowledges explicitly – as Newton does not – that if restrictions (i) and (ii) do not hold and instead the angle *VCv* increases at a steady rate, then none of the conclusions in Section 9 will hold:

> Si positis caeteris, quae in Propositione praecedente posita sunt, linea apsidum *Cv* supponatur aequabiliter, vel alia quacunque lege praeter eam quae dicta est, revolvi corpus *p* non amplius describet circa centrum *C* areas temporibus proportionales; ac proinde non ea vi tantum urgebitur, quae in punctum *C* perpetuo tendat.[139]

> If with the other things set down, which in the preceding Proposition are set out, the line of apsides *Cv* is supposed to revolve uniformly, or by whatever other law except the one which has been stated the body *p* will no longer trace out areas proportional to the times about the centre *C*; and therefore it will not only be driven by this force which is continually directed to the point *C*.

This is not the only instance of a reader drawing attention to the limitations of the *Principia's* results in this way, acknowledging more explicitly than the text did the mathematical conditions upon which results depended, as I will discuss in the next chapter.

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[139] Bodleian, Arch. A d.37, 133.

After his proposed Scholium Fatio records a new result he has derived. In it, he proves that (in modern terminology) the angular velocity of any orbiting body acted on by a centripetal force is inversely proportional to the square of its distance from the centre. For the modern reader this result follows from the conservation of angular momentum, but Fatio derives it from Newton's construction. His note begins as follows:

> Lineae *CP* velocitas repraesentetur per arcum *ef*, quem in dato circulo, cujus centrum sit *C*, ‸et radius *Ce*, dato tempore quam minimo ipsa linea *CP* describit. Circulum *Pgp* centro *C* descriptum secet linea *CfK* in puncto *g*, et erit *Ce* ad *ef* ut *CP* ad *Pg*, quae proinde aequabitur $ef \times CP/Ce$. Jam $Pg \times CP$ est ut area dato illo tempora quam minimo descripta, et propterea aequatur semper datae magnitudini: et, substituendo ipsius *Pg* valorem jam inventum, $ef \times CPq/Ce$ erit semper ejusdem magnitudinis: Et cum *Ce* sit data erit *ef* reciproce ut *CPq*.[140]

> The velocity of the line *CP* is represented by the arc *ef*, which the line *CP* describes on a given circle whose centre is *C* and radius *Ce*, in a given very small time. The line *CfK* cuts the circle *Pgp* described about centre *C* at the point *g*, and *Ce* to *ef* will be as *CP* to *Pg*, which therefore will equal $ef \times CP/Ce$. Now $Pg \times CP$ is as the area described in that given very small time, and therefore is always equal to a given magnitude: and, substituting the value of *Pg* now found, $ef \times CP^2/Ce$ will always be of the same magnitude: And when *Ce* is given, *ef* will be inversely proportional to CP^2.

Fatio's argument refers to the annotated diagram above, where he has added a small circular arc *ef* with arbitrary radius *Ce*, and extended the line *cfK* to the point *g*. He considers the body *P* moving along a very short section of its orbit

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[140] Bodleian, Arch. A d.37, 132.

from the point *P* to the point *K*. Over this very short period of time ("dato tempore quam minimo"), this path *PK* may be approximated by the circular arc *Pg*, with radius *CP*. As the radius sweeps around from *CP* to *CK*, Fatio takes the length of the circular arc *ef* as a measure of "the velocity of the line *CP,*" or what the modern reader would call the angular velocity. Because the circular sectors *Cef* and *CPg* are similar, $Ce/ef = CP/Pg$ and so $Pg = ef \times CP/Ce$. But the area swept out by the radius over this short period of time – which Fatio is approximating by the sector of a circle – will be proportional to $Pg \times CP$. By Kepler's area law, this quantity will be fixed. Combining these, the quantity $ef \times CP^2/Ce$ will also be fixed. Since the distance *Ce* was chosen to take an arbitrary, fixed value, this means that *ef* will be inversely proportional to CP^2, as required.

This result is much more substantial than the simple algebraic generalisation of Proposition 4. Rather than merely extending the results in the text, he has here derived a novel conclusion, independent from Newton's, with an extended proof. It required a confident manipulation both of Newton's construction and his methodology. But like his smaller suggestions, we can see that it emerged organically from the act of reading the text. Fatio had a pen in his hand as he worked through Newton's proofs, and in the process of verifying them he extended their methods to produce new ideas. Just as Bertoloni Meli has shown to be the case for Leibniz, the process of reading the *Principia* automatically led Fatio to generate his own results.

2.3 Reading the *Principia* non-mathematically

The existing secondary literature struggles to account for non-mathematical readings of the *Principia*. Other than the story that Locke asked Huygens if the demonstrations in the book could be trusted, whereupon – as Westfall put it – "he applied himself to the prose and digested the physics without the mathematics,"[141] instances in which contemporaries read sections of the book without working through the mathematical proofs have been overlooked by Newton scholars. There are two reasons for this. The first is that such readings leave less of a trace in the historical record. If a reader picks up a pen and works through Newton's proofs to verify them, that process produces a material artefact that does at least have some chance of being preserved in an archive, but if he simply reads through the words without writing out the mathematics, no physical trace of that activity will remain. This evidential bias, however, does not mean that no analysis is possible at all. On the contrary – as we shall presently discover – the notes left by Newton's contemporaries demonstrate that passages in his book were commonly read without checking the demonstrations. Leibniz, Halley, Fatio and Locke all read the greater portion of the *Principia* in this way, without stopping to verify the mathematical proofs.

The second problem is the importance of "understanding" and "mastery" in much of the secondary literature, as discussed in the Introduction above. For users of this vocabulary, non-mathematical readings of the text pose a significant difficulty. If a reader has successfully worked through one of Newton's proofs, then he can confidently be said to have "understood" it; and if he has tried but failed – or not even tried – then it can reasonably be asserted that he has not. But classifying a reader who reads through the statement and

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141 Westfall *Never at Rest*, 470.

proof of a result but does not pick up a pen to verify it, is much more complicated. The extent to which such a reader possesses “understanding” or has “mastered” the demonstration is much less clear. Rather than articulate a response to this question, the secondary literature has in general chosen to ignore it, by choosing not to pay attention to the many occasions on which individuals read through passages in the text without stopping to verify the proofs. I hope in what follows to supply such a response, and by closely examining what the practice of reading the *Principia* non-mathematically entailed, to gain a fuller awareness of what it might have meant to “digest the physics without the mathematics.”

2.3.1 Halley and Fatio’s corrections

One source of evidence of non-mathematical readings of Newton’s text are the grammatical and typographical corrections given to him as part of their editorial duties by both Halley and Fatio. For the large part these are very simple to explain, and equally brief to analyse: both Halley’s feedback sheets and Fatio’s reading notes contain dozens of instances in which they alert Newton to errors of typography, syntax or morphology in the text. It is not particularly instructive to list examples, but, to take two representative instances, when reading a draft of what became Proposition 71, Halley corrected “Agantur de corpusculis lineae PHK, PIL, phk, pil auferentes a circulis maxmis AHB, ahb, aequales arcus” to “Agantur a corpusculis lineae…”; and in the middle of Proposition 51 Fatio noticed that the printed text “Pendulis igitur duabus APT, Apt de perpendiculo

AR inaequaliter deductis" should have read "Pendulis igitur duobus APT, Apt…". Similar examples abound in both documents.[142]

Mostly these corrections reveal nothing about Halley and Fatio's response to the meaning of Newton's words. However, this is not true of all the edits. In some instances, Halley and Fatio asked Newton to clarify his phrasing, while not altering the substance of his meaning. This may be taken as evidence that they found the original argument difficult to follow, perhaps because of the novelty of the methodology, or perhaps because of the author's misjudgement of the prior knowledge of his readers; I will provide examples in Chapter 3. And Halley made Newton tweak the definitions of some foundational physical and mathematical concepts such as mass, inertia, centripetal force, relative and absolute space, and infinitesimal moments. In these cases, even small changes are of considerable significance, and highly revealing of latent preconceptions: these will be discussed in Chapters 4 and 5. Furthermore, because the corrections can only have been detected by continuously reading the surrounding passages, they provide firm evidence that these sections were fully read, as detailed in Chapter 1. Finally, the superficial nature of many of the corrections do not mean that the mathematics was being ignored. I have already provided evidence that Fatio worked through many of Newton's proofs. But Halley's notes also show that he must have paid some attention to the mathematics. He correctly picked up an error in the draft of the Scholium after Proposition 21, for example; he spotted mistakes in the diagrams of Proposition 6 of Book 1 and Propositions 4 and 9 of Book 2; and he suggested small re-

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[142] Fatio's copy of the first edition also has hundreds of such corrections marked into the text, although these are harder to date, and – because they might have been transferred individually into his copy – do not reliably denote that the passage has been read in full.

phrasings of the proofs of Propositions 18, 33 and 65 of Book 1.[143] Halley could not have done so without following the details of at least some of the proofs with a high degree of care. Leibniz also combined mathematical and non-mathematical readings of the text, providing further evidence that the processing of Newton's demonstrations was often partial and incomplete: the verification of the arguments in the *Principia* was rarely absolute.

2.3.2 Locke's commonplace notes

The non-mathematical readings which have received most attention from historians are those undertaken by Locke in the late 1680s and early 1690s. Chapter 1 highlighted a notable asymmetry in the secondary literature regarding Locke's relationship with the *Principia*: Newton scholars dismiss his attempt to read the book and explain his failure to understand it on his lack of mathematical training, while Locke specialists invoke his knowledge of Newton's work as a central event in his intellectual development. I suggest that neither narrative is right, because all the secondary literature on Newton and Locke for the last half-century has been based on Axtell's analyses from the 1960s, which misrepresent the way in which Locke engaged with the text. The content of his reading notes can instead be accounted for by Richard Yeo's study of the humanist tradition of commonplacing. The primary evidence shows that Locke read the book in full three times, skipping over the mathematical sections on each occasion, but taking careful notes on everything else. He extracted short quotations verbatim from the text, usually involving definitions, numerical data or experimental results. Locke thus read the *Principia* in exactly the same way that he read Cicero and Tacitus.

[143] CUL, MS Add. 3965, 96r, 97r, 99r.

Locke consumed a vast number of books over the course of his life, and habitually took notes in a highly systematic and regular manner. As Yeo explains, this was based on his use of commonplace books, a type of notebook in which short quotations, epigrams and excerpts on the same theme were arranged together under standard headings. A reader with a new notebook would begin by inserting titles on each page ("honour," "virtue," "beauty," "friendship"), and whenever in his subsequent study he came across a proverb, adage or fact he wished to preserve, would make a note under the appropriate heading. Notes on a given topic were thus grouped together – in a "common place" – making it easy to retrieve and memorise information. Often each note was accompanied by a page reference to the book from which it came; some readers maintained more than one commonplace book, covering different subjects. The habit of commonplacing was promoted by Renaissance humanists, although its influence had waned by the mid-1600s, owing to its association with scholasticism and deference to textual authority. Nevertheless, Yeo argues that a small group of seventeenth-century English virtuosi connected with the Royal Society – Boyle, Hooke, Pepys, Aubrey, Evelyn, Hartlib, and Locke – used notebooks "in both traditional and novel ways" in their pursuit of empirically-derived knowledge.[144] In particular, commonplace books were well suited to meet the needs of Baconian natural history, because they could be used to compile observational data and record details of reported events and phenomena. By comparing a large mass of individual entries, inductive generalisations could be made; and because the books could be shared, they allowed for easy collaboration. For this group of scholars

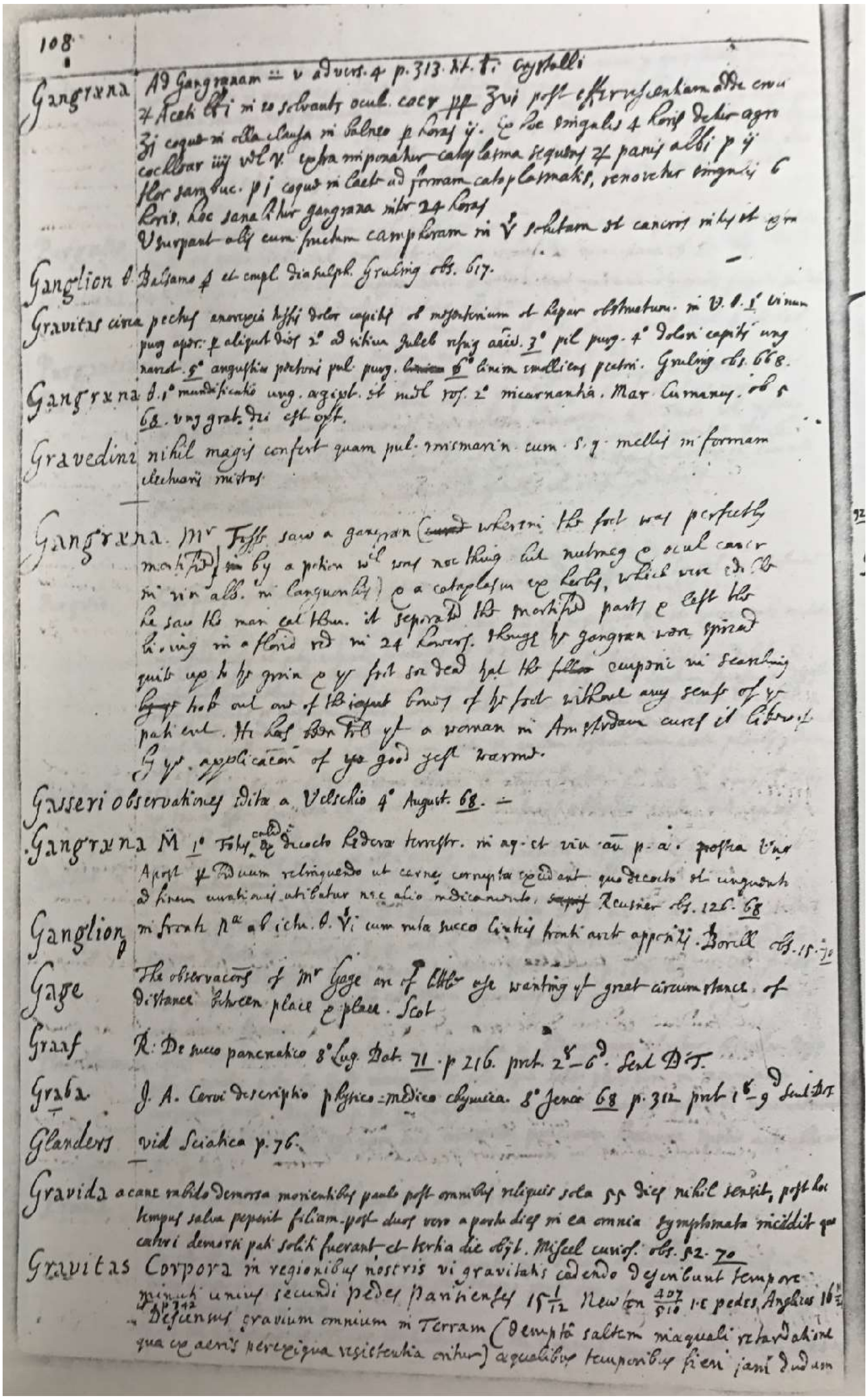
[144] Yeo, *Notebooks, English Virtuosi, and Early Modern Science*, 5.

notebooks were no longer repositories of ancient material, but a technology for collating and processing new information.

Yeo reports that Locke himself was a “fastidious, and perhaps obsessive” note-taker.[145] He kept dozens of notebooks and journals, containing tens of thousands of quotations from more than a thousand different texts. His larger books typically contain between one and two thousand separate entries. He sometimes logged personal observations and second-hand testimonies, but overwhelmingly recorded excerpts from his vast private reading, writing out short quotations as he read. He almost never marked the books themselves, rarely even adding marginal annotations, but sometimes made a list on the inside back cover to indicate from which pages he had taken extracts. From the 1660s, he kept different notebooks for different disciplines, sorting medical, chemical and natural philosophical notes into his “Physica” books, and religious, social and political matters into “Ethica.” Locke organised his commonplace books (which he called “adversaria,” a term originally used for books of accounts) according to a non-standard scheme of his own devising, publicised under the title “A New Method of Making Common-Place-Books” in 1706. According to this method, entries were sorted not by theme, but by the combination of the title’s first letter and leading vowel. For example, f. 108 of MS Locke d. 9 (the commonplace book in which Locke recorded his third set of notes on the *Principia* in 1691, shown below) contains notes on a series of unconnected topics, each of which starts with the letter “g” and has a first vowel “a” – “Gravitas,” “Ganglion,” “Gangraena” and so on. Each entry consists of a title, followed by a short quotation or fact, and a reference to the book from

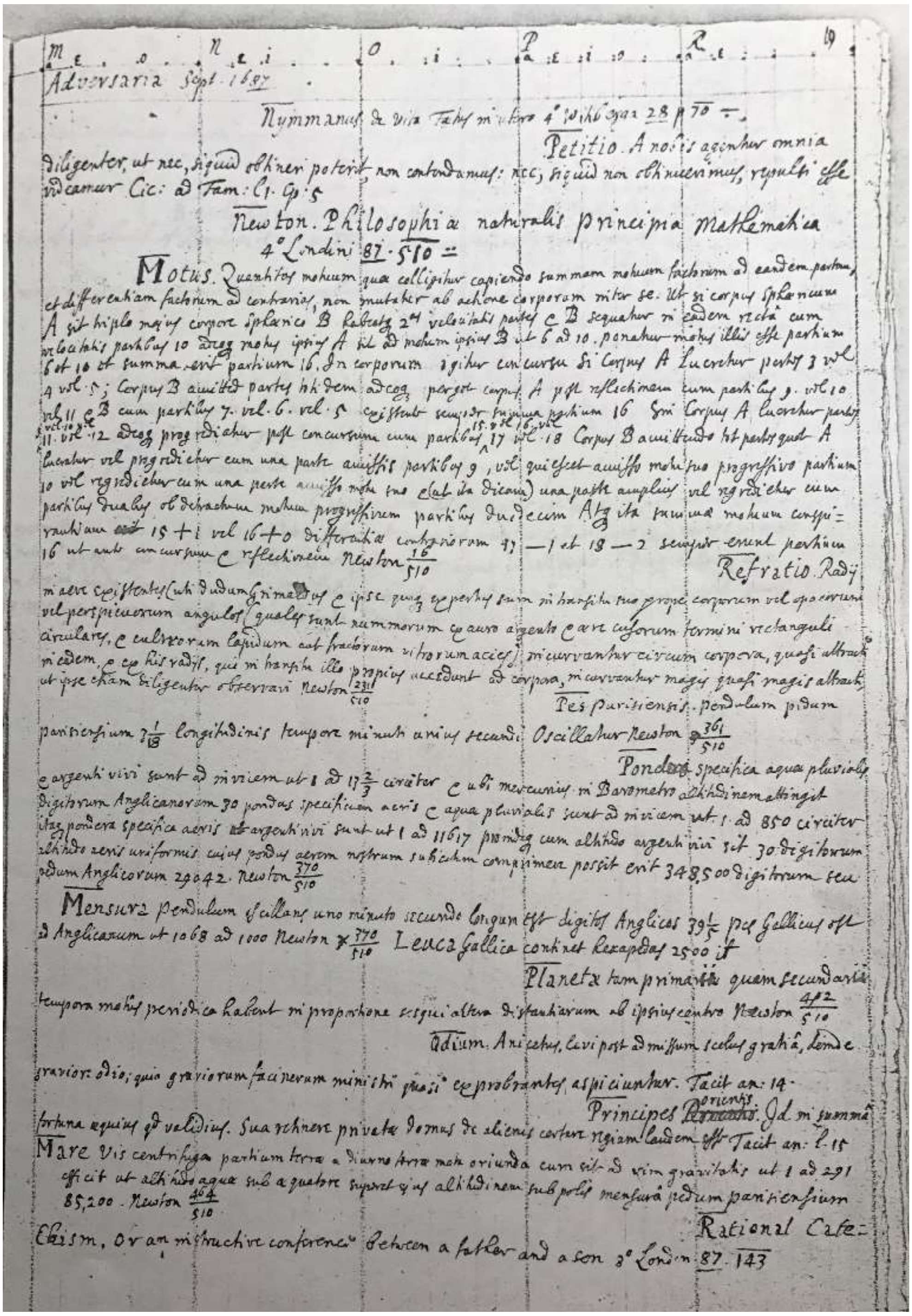
[145] Ibid., 184.

which it was taken. The page numbers of each letter-vowel pair were recorded in an index at the front, so that information could efficiently be retrieved.



As detailed in Chapter 1, Locke read the *Principia* in full on three separate occasions, and took a full set of notes on each. These notes are all now held in the Bodleian Library, Oxford. Those covering his reading in September 1687 comprise sheets from a reading journal now at MS Locke c. 33, 19–20; when he read the book for a second time in 1688 in order to compose his review in the *Bibliothèque Universelle*, he made notes on loose sheets of paper now in MS Locke c. 31, 99–100; and his third reading of 1691 is recorded in a commonplace book now at MS Locke d. 9. Although the overall structure of these notes varied, the style of individual entries within them was very consistent, and Locke's manner of extracting quotes, results and empirical data verbatim from the text is exactly accounted for by Yeo's description. The mode of reading Locke's commonplacing habits encouraged directly determined how he engaged with the *Principia*.

Because he was in Rotterdam at the time of his first reading, he did not have access to his usual commonplace books, so made reading notes on individual sheets. He adapted his New Method for use in a compressed space, as may be briefly explained with reference to f. 19r of MS Locke c. 33 below. Locke has drawn vertical lines down the page dividing it into five columns, each labelled at the top with a letter of the alphabet. Each column has five further subheadings, one for each vowel. As in his commonplace books, every entry starts with a keyword, or title ("Petitio," "Motus," "Refratio"), sorted according to its first letter and leading vowel. But rather than each letter-vowel combination occupying an entire page, in these sheets the titles are positioned horizontally in the appropriate columns. So the entry on "Planetae" towards the bottom of f. 19r starts in the “P–a” column, since the word starts with “P” and has “a” as its leading vowel. “Odium” immediately below is slightly to the left, under “O–i”; “Principes orientis” is further to the right, under “P–i.” The publication details of Newton’s book are listed towards the top of the page, under the “N–e” column:


Adversaria Sept. 1687
Petitio
Newton. Philosophiæ naturalis principia Mathematica
4° Londini 87. 510
Motus
Refratio Radij
Pondera specifica
Mensura
Leuca Gallica
Planetæ
Odium
Principes orientis
Mare
Rational Case-
Chism, Or an instructive conference between a father and a son 8° Londini 87. 143

Locke includes a page reference with every entry, which allows his notes to be compared with the text on which he based them. Most commonly, Locke copies out after his title a direct quotation from the text. Here, for example, is his note on “Mare” at the foot of f. 19r, which he marks “Newton 464/500”:

> Mare. Vis centrifuga partium terrae a diurno terrae motu oriunda cum sit ad vim gravitatis ut I ad 291 efficit ut altitudo aquae sub aequatore superet ejus altitudinem sub polis mensura pedum parisiensium 85,200. Newton $\frac{464}{510}$.

A comparison with page 464 of the first edition shows that Locke has copied out part of a sentence from the Corollary to Proposition 36 of Book 3 almost word for word. Similarly, this entry on the heat of boiling water on f. 20r is a verbatim transcription of a sentence from the long example after Proposition 41 of Book 3, on pp. 498–99:

> Calor aquae ebullientis est quasi triplo major quam calor quem terra arida concipit ad aestivum solem ut expertus sum: & calor ferri candentis (si recte conjector) quasi triplo vel quadruplo major quam calor aquae ebullientis. Adeoque calor quem terra arida apud cometam in perihelio versantem ex radiis solaribus concipere posset quasi 2000 vicibus major quam calor ferri candentis. Newton $\frac{498}{510}$.

And this note on gravity on f. 20v consists of a sentence lifted from Proposition 4 of Book 3, supplemented with some words Locke came across sixty pages later, in the Corollary to Proposition 36:

> Gravitas. Corpora in regionibus nostris vi gravitatis cadendo describunt tempore minuti unius secondi pedes parisienses $15\frac{1}{2}$. Newton $\frac{407}{510}$. vis centrifuga partium terrae a diurno motu terrae oriunda est ad vim gravitatis ut 1 ad 291

That Locke so often transcribed Newton's words directly should not be taken to imply that he did not understand what he read. He was not mindlessly copying out quotations or data for the sake of filling out his notes. This is clear from his entry on the density of water on f. 20r, which originally read "Aquae densitas ad densitatem Argenti vivi est ut 1 ad 13 vel 14 circiter," taken from the Scholium after Proposition 31 of Book 2 on p. 350. But Locke later edited this note, because twenty pages later, in the Scholium after Proposition 50, Newton gave a more precise measurement of "1 ad $13\frac{2}{3}$ circiter," causing Locke to update his record. A similar level of engagement is shown in this note on f. 19r on the length of a one-second pendulum:

> Mensura pendulum oscillans uno minute secondo longum est digitos Anglicos $39\frac{1}{5}$ pes Gallicus est ad Anglicarum ut 1068 ad 1000 Newton $\frac{370}{510}$.

This is not a direct quote, but a paraphrase of the second half of page 370, using Newton's vocabulary. Locke has composed a précis. He understood what he read, processed the details of the text, and made cross-references between different sections.

Just these few examples are enough to give a clear sense of the style of Locke's notes. He read with his pen in his hand, copying out sentences and short passages verbatim, just as was his habit for classical works. He transcribed a handful of definitions, but otherwise all his notes extracted numerical data from Newton's text – about the speed of sound, heat capacities, the relative weights of water, air, mercury and gold, the strength of gravity, the length of pendulums, the size of the earth, and the properties of the heavenly bodies. Very few of his entries don't contain measurements. Locke reported nothing of the system connecting these individual observations, which had been Newton's main achievement: Locke was less interested in the physical principles underlying the phenomena than the phenomena themselves. Moreover, tracking the location of his notes through the pages of the *Principia*, suggests an even stronger conclusion. Because earlier entries are higher up the page, the order in which Locke made his notes can be inferred. So having first recorded the publication details on f. 19r ("Newton"), it is probable that he next copied out some of the Definitions (f. 19v, and f. 20v under "Gravitas"), and then sections from Corollary 3 to Law 3 on p. 16 ("Motus," f. 19r). But Locke then made no more notes from Book 1 until shortly before the end, when he reached the Scholium after Proposition 96 ("Refratio," f. 19r; "Lux," f. 20v). It is notable that these are just about the only pages in Book 1 which do not look densely mathematical to a casual observer: it is plausible that Locke skipped over the geometrical proofs, stopping only to read the parts he thought he might understand. Book 2 is also full of forbidding geometrical demonstrations, but there are a few non-mathematical parts in Section 8, and these are exactly the pages from which Locke extracts notes ("Pes Parisiensis," f. 19r; "Pondera specifica," f. 19r; "Mensura pendulum," f. 19r; "Sonus," f. 19v; "Aquae densitas," f. 20r). And when he reaches the start of Book 3, which contains almost no mathematics, the frequency of his notes sharply increases. The location and order of his notes is consistent with Locke leafing through every page of the book from the beginning, passing over everything that looked like a mathematical proof, and reading everything else.

The same patterns can be seen in the notes from his second and third readings. Those from 1688, on which his review in the *Bibliothèque Universelle* were based, comprise two sheets at MS Locke c. 31, 99–100. The notes on Books 1 and 2 largely constitute a list of Newton's section headings copied out verbatim (with the exception of Sections 12 and 14 of Book 1, which are composed of transcribed portions of the text). Because it contains more non-mathematical content, Locke's notes for Book 3 are much fuller: he extracts a series of short, individual quotations and facts from the text, just as he did in his first reading. Most of it consists of astronomical data, such as the size of the earth, the distances between the planets, and the relative strength of the heat from the sun. However, this fragmentary style should again not be taken as an indication that Locke did not understand what he read. This note about comets on f. 100v appears at first glance to be a transcription, but Locke has made it by stitching together four separate quotations on the same theme, spread across the last thirty pages of the book:

> Cometae sunt Luna superiores & in regione planetarum versantur. Splendent luce solis a se reflexae. Genus planetarum sunt motu perpetuo in orbem redentes. corpora eorum sunt solida compacta fixa ac durabilia.
>
> Comets are higher than the moon and move in the planetary regions. They shine by the light of the sun reflected from them. They are a kind of planet, revolving in orbit with a continual motion. Their bodies are solid, compact, fixed and durable.

The first sentence is the heading of Lemma 4 on p. 474; the second is the statement of Corollary 1 to this lemma, on p. 479; the third is taken from the text of Corollary 3, on the following page; and the fourth is extracted from deep within the long example that follows Proposition 41, on p. 498. There is no way Locke could have constructed this note without reading all thirty pages, following Newton's overall argument, and selecting representative extracts. Locke understood what he read, processed its meaning, and was able to make cross-references within the text.

The review in the *Bibliothèque Universelle* that (as discussed in Chapter 1) so puzzled Axtell and Cohen is now easily explained: it is largely a direct translation of these Latin notes into French. In some instances Locke has switched their order, but almost every paragraph of the review is a direct translation of an entry in the notes. Locke must have made the notes for the express purpose of writing the review, because at the bottom of f. 99v he left a note reminding himself to insert two of Newton's tables about planetary distances and the satellites of Jupiter, which do indeed appear on pages 443, 444 and 445 of the *Bibliothèque*. However, although the Latin and French texts are very similar, they are not quite identical: Locke made a handful of small changes in the process of composing his review, and also inserted a long section about Cartesian vortices that is almost a direct translation of the Scholium at the end of Section 9.

The notes Locke made on the occasion of his 1691 reading follow the same routine. They are contained in MS Locke d. 9, which is a commonplace book of the type described by Yeo. The entries are structured according to his New Method outlined above, and so his notes on the *Principia* are scattered throughout the book. Locke records the publication details under the title "Newton," for example, next to an entry on "Nephritis": I have counted twenty pages which contain entries from the *Principia*. On each of the three occasions that he read the *Principia*, then, Locke did so exactly in the manner of a humanist scholar studying a classical text. He extracted short quotations and generally copied them out verbatim. He occasionally combined separate sentences in the text to produce what appears to be a precis, which shows that he must have understood what he read. He bypassed the mathematical sections entirely, but seems to have paid close attention to everything else: the location of his notes is consistent with him having leafed through every page of the book in order, reading everything that looked non-mathematical. He extracted short, self-contained quotations as he went, most of which contained numerical data and experimental results. He thus processed Newton's text in an identical manner to that he adopted for the thousands of other books he read in his life.

There are three final points to note. First, although on each of the three readings he tended to focus on the same non-mathematical sections, his notes were different each time. That is, although he read the same pages, he chose to extract different quotations and record different data. There is some overlap between the three sets of notes, but their contents are sufficiently different to conclude that he made each set from scratch. Locke made three entirely independent, complete readings of the whole work. Second, there is a causal link between how Locke read and what he read. The structure of his notes necessitated dividing knowledge into small, discrete chunks, and I suggest that this process of extracting a series of disconnected items of information was incompatible with processing a large, interconnected, coherent scheme such as Newton's. Locke's commonplacing technique forced him to sift through the text for individual items of data and read the *Principia* in the manner of a Baconian natural historian, and it would have been impossible for Locke to process any of the geometrical proofs within his system. The atomising tendency of his note-taking was incompatible with the deductive structure of extended mathematical proofs. Even if he had had the technical capability, if Locke had wanted to verify any of Newton's demonstrations, he would have had to do so outside the pages of his commonplaces. He would have had to work through the proofs on separate pieces of paper, and it would have made no sense at all to transcribe short excerpts from the derivations under summary headings. He would have been obliged temporarily to suspend his note-taking, and process the text in an entirely unfamiliar way, quite different from the routine he adopted for all the other thousands of books he read in his life. This means that if Yeo's broader thesis about the importance of notebooks to an influential group within the early Royal Society is right, and if it is true that "although notebooks were central to the bookish culture attacked by some apologists for that new institution, they played a part in the making of early modern science"[146] then Locke's notes could be revealing of the way in which the early Royal Society handled mathematical knowledge. As a technology, commonplace books were incapable of processing mathematical proofs. If Locke's inability to verify Newton's demonstrations is to be characterised as a deficiency – as it is in all of the existing secondary literature – then it was not just a personal weakness, but a failure of the mode of reading with which he engaged with the text.

And third, the foregoing analysis has significant implications for historians of Locke. The asymmetry within the secondary literature on the topic of Locke's reading of the *Principia* has already been acknowledged: Newton scholars characterise his attempt as a failure owing to his lack of mathematical expertise, while historians of Locke identify his contact with the book as a central event in

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[146] Yeo, Notebooks, English Virtuosi, and Early Modern Science, 36.

his intellectual development. However, the secondary literature examining the relationship between Locke and Newton is based on Axtell's papers from the late 1960s, in which he maintained that Locke's reading of the *Principia* was the proximate cause of his renunciation of Cartesianism. Axtell specifically identified the two pages about vortices in the review in the *Bibliothèque Universelle* as his moment of conversion: this argument may need reconsidering in the light of the knowledge that these lines are almost direct translation of the Scholium at the end of Section 9, and do not appear either in the Latin notes on which he based his review, or in the record of his first and third readings. If Newton's destruction of Cartesian vortices was so significant to Locke, its relative absence from all three sets of reading notes needs to be explained.[147] There are further consequences for more recent studies of Locke's view on natural philosophy such as that by Peter Anstey, which maintains that Locke altered his views in the 1690s in response to the *Principia*.[148] Following Axtell, Anstey places great significance on the passage in the review about Cartesian vortices. More importantly, Anstey's argument that Locke viewed the *Principia* as a paradigm of demonstrative reasoning is not easy to reconcile with the primary evidence that shows that Locke did not engage with Newton's proofs. Because he did not scrutinise the demonstrations, Locke does not acknowledge that the mathematical methodology of the *Principia* is more complicated than the imagined ideal of Euclidean geometry on which his scheme is based. Locke's epistemology therefore does not account for any of the novel methodologies on which Newton's achievement depended; and neither does Anstey's analysis,

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[147] Axtell, "Locke's Review of the *Principia*," 156.
[148] Anstey, *John Locke and Natural Philosophy*.

even as he asserts the importance of the *Principia* in his intellectual development.

2.3.3 Leibniz's *Excerpts*

Finally, the non-mathematical readings of the *Principia* recorded in Leibniz's *Excerpts* may be considered. I have purposefully left this section until the end of the chapter in order to draw attention to the important fact that Leibniz processed the majority of Newton's book in a very similar way to Locke: his two sets of *Excerpts* largely comprise transcriptions or paraphrases of passages in Newton's text, and are only very occasionally supplemented by his own commentary. The general style of these notes is clear even from a cursory examination. He begins the first set by copying out Newton's eight Definitions and three Laws, before recording Lemmas 9–11 and Proposition 6, along with a few of his own notes. He then transcribes a handful of items from the second half of Book 2, followed by a larger quantity of material from Book 3. Like Locke, he either transcribes Newton directly, or composes a précis using the vocabulary in the text. And like Locke, he often records numerical data, such as pendulum lengths, the speed of sound, and the dimensions of planetary orbits. Unlike Locke, however, he also copies out statements of mathematical results, generally without their proofs. The second set of *Excerpts* is very similar, and contains large numbers of verbatim transcriptions and paraphrases, along with occasional commentary. He again writes out the Definitions and Laws, and then, having skipped Section 1, reads through most of the rest of Book 1 and the start of Book 2 in sequential order. There is evidence of detailed engagement with the proofs in Section 11, but otherwise he just copies out extracts from passages of interest.

In his *Marginalia* and *Notes* – as will be discussed in Chapters 4 and 5 – Leibniz worked through the detail of some of the orbit proofs in Sections 2 and 3, and the method of first and last ratios in Section 1. In other words, Leibniz undertook both mathematical and non-mathematical readings of the text, just as Fatio and Halley did. Naturally, it was his close study of the theorems in the opening passages that had the greatest impact on his own ideas about planetary motion, as Bertoloni Meli has demonstrated. Nevertheless, this should not disguise the fact that Leibniz read through almost everything else in the book – so far as can be inferred from the extant primary sources – without verifying the proofs. His critical engagement with the demonstrations of the *Principia* was mostly limited to the opening fifty pages. While it is too strong to claim that Leibniz also "digested the physics without the mathematics," it does seem that he didn't check most of it. That is, Leibniz read the majority of the *Principia* in a very similar way to Locke: the narrative in the secondary literature that their readings were diametrically opposite in character owing to the disparity of their mathematical expertise is not sustained by the primary evidence.

3. Deconstructing the difficulty of the *Principia*

3.1 Introduction

The material presented in the previous chapter will now permit a critical examination of the pervasive trope that the *Principia* was self-evidently difficult to understand. As stated in my Introduction, I do not deny that the book was indeed difficult: my aim here is merely to deconstruct what this label means. Rather than impugning the competence of Newton's peers, I start from the assumption that the difficulties contemporaries had understanding the *Principia* were a function of the characteristics of the text. The book was hard to understand for reasons of which its author was the cause. Newton did not have to write a book that was difficult to understand, because not all books of mathematics are difficult to understand, but as a matter of historical contingency the *Principia* was. My aim here is to provide a detailed taxonomy of the obstacles Newton's readers encountered when processing his proofs, and to establish those qualities of the book that contributed to its impenetrability. In other words, I wish to identify the characteristics of the *Principia* that made it so ineffective at persuading its readers of the validity of the arguments it contained.

The evidence presented in the previous chapter showed that to verify the proofs in the text readers were required to pick up a pen and actively reconstruct them. Any difficulties in understanding therefore arose from obstacles to this process: Newton's book became impenetrable as and when his arguments became impossible to reconstruct independently from the information printed on the page. The reading notes of Newton's peers contain many specific examples of such occurrences, as will be demonstrated in this chapter. I will show that Newton's verbose prose was often not easy to reconstruct in symbolic form; that his text used idiosyncratic terminology with which its readers were unfamiliar; that its explanations were frequently unclear; and that it was not uncommon for readers to perceive large gaps in the proofs. There is also some evidence that the intimidating length of the book actively discouraged readers from scrutinising individual proofs, as did its unclear logical structure. The chapter will close with detailed analyses of two case studies exemplifying many of these tendencies: Flamsteed's failure to understand Corollary 2 to Law 3, which led him to give up on his initial aim of reading the opening sections all the way through, and the proof of Proposition 9 of Book 1, which both Huygens and Gregory thought they understood, when they did not. I also wish to emphasise that in this chapter, so far as possible, I have only drawn examples from proofs based on established geometrical practices. I have postponed discussion of Newton's innovative limit methods until the next chapter, where I will examine readers' responses to his method of first and last ratios. My present aim is to show that, separately from the unfamiliarity of its radical mathematical methodology, Newton wrote the book in such a way that it contained many obstacles to its comprehension.

In categorising these obstacles, Gilbert Clerke is a useful figure to consider. I suggested in Chapter 1 that, to the extent that he is mentioned at all in the secondary literature at all, Clerke is held in low regard. He is viewed by historians as an insignificant hobbyist who had the audacity to try to read Newton's book. But it will become apparent over the course of this chapter that Clerke's reading experience was more representative of his illustrious peers' than has been recognised. The difficulties he had with the text were shared by all its better-known readers. Clerke summarised his complaints as follows:

> so contracting the ratios by changing the Quad's, & getting them in rank & file one under another, I easily saw how I was to multiply & divide… and indeed you should have sett them so, for your booke is hard enough, make it as easie as you can: so there should have been two prickd lines from the center to the tangents in prop. 5. p. 44 & you should have had marginal references to Eucl. & Apoll: & been prodigal or per this & per that, of your nempe's & quoniam's & enlargd your scholium's; but you masters doe not consider the infirmities of your readers, except you intended to write only to professours or intended to have your books lie, moulding in libraries or other men to gett the credit of your inventions[149]

In these words of November 1687 Clerke was accurately prophesying the obstacles many readers would encounter with Newton's book over the coming years. Flamsteed was also confused by the ratios not being presented "in rank and file one under another." Fatio, like Clerke, found himself frustrated by incomplete diagrams. Gregory, too, sought missing references to Euclid and Apollonius. Huygens and Leibniz would likewise have benefited from enlarged scholiums to explain the proofs. And the fact that, as was demonstrated in Chapter 1, so much of the book was so regularly left unread vindicates his warning that the book was destined to "lie moulding in libraries." Clerke unknowingly provided a helpful summary of the ways in which Newton's text would not be understood by his contemporaries. His evidence shows that the difficulties readers experienced with the *Principia* owed as much to the characteristics of Newton's book as they did to their own competence.

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[149] Clerke to Newton, 7 Nov. 1687, *NC*, 2:492.

3.2 Misprints and mistakes

I begin by briefly acknowledging one potential source of difficulty which in general did not obstruct understanding of the text: there were dozens of misprints and typographical slips in the first edition, but these overwhelmingly did not impede readers' attempts to verify the arguments. Newton's peers generally recognised misprints for what they were, and corrected accordingly. Hall referred in his famous article to "the vast number of minor slips, some due to the author's, some to the printer's carelessness,"[150] and many of these are recorded on the pages of Newton's annotated and interleaved copies, as well as in Fatio's reading notes. In Lemma 2, for example, "curva *AcE*" should have read "curva *acE*"; the Scholium after Proposition 4 refers to "arcum *BC*" when it should say "arcum *BD*"; and the text of Proposition 37 mislabels "*GA*" as "*CA*". The most conspicuous example is the frequent omission of the word "reciproce" from many of the orbital force theorems in Sections 2 and 3, including Propositions 4, 7, 9, 10 and 12. Yet none of these instances led to misunderstandings for Gregory, Huygens, Fatio, Flamsteed or Leibniz, all of whom easily corrected the text when as they were working through the proofs, as the primary sources show. Although there were many misprints in the book, as a rule they did not cause difficulties for its readers.

That said, not every example is straightforward. Hall was not quite right to say that these slips "were such as would cause little difficulty to readers skilled in both Latin and mathematics."[151] In clear-cut instances when there was an obvious correction to be made, Newton's readers usually did so. But when an

[150] Hall, "Correcting the *Principia*," 293.
[151] Ibid., 293.

unconfident reader working through a poorly explained proof had cause to doubt whether the printed text was perfectly accurate, the situation was more complicated. When examining Flamsteed's reading of Corollary 2 to Law 3 at the end of this chapter, I will show that, soon after making a legitimate correction to the text, he mistakenly imagined a second error that derailed his attempt to verify the proof. He was not certain that the printed text was correct, and that added to his difficulty in understanding the passage. So while it is generally the case that readers successfully navigated errors, there were exceptions: in some instances the quantity of misprints throughout the book added to the burden of readers trying to understand it.

3.3 Length and structure

The intimidating length of Newton's book is taken for granted by the secondary literature, but the impact this had on its reception, and the extent to which the length of the text is distinct from its difficulty, is never directly addressed. I demonstrated in Chapter 1 that the length of the book altered the way in which readers engaged with it, because it was so time-consuming to carry out a complete, cover-to-cover examination of all of the arguments it contained. So far as we can tell from the documentary evidence, only Gregory could meaningfully claim to have done this, and it took him almost seven years. Fatio appears to have set out to do the same, but did not finish; so too did Leibniz, although he read much of it without verifying the proofs, and also stopped before the end. Locke, of course, read the book in full three times, but did not examine the mathematics. It is likely that Halley cast his eyes over every page, although the extent to which he scrutinised the proofs is unclear. Readers such as Flamsteed and Clerke appear to have loosely followed the advice Newton later gave to Bentley that he read the first sixty pages and then move on to Book 3, but it is important to observe that even those individuals who carried out this instruction carefully and completely (which Flamsteed and Clerke appear not to have done) would still not have come across any of Newton's arguments concerning universal gravitation, for example.[152]

Another practical effect of the *Principia*'s oppressive length was that it made it more difficult to verify its mathematical arguments. Suppose, for instance, that a reader wished to subject to critical examination Newton's proof of the inverse square law for elliptical orbits. One thing he could do is start at page 1 and keep reading carefully until he came across the result. But Newton doesn't announce at the beginning on which page the proof of the inverse square law concludes, so unless the reader has received external advice he will embark on this study not knowing whether he will reach his destination in five pages or five hundred. A more pragmatic approach – the one which his marginalia suggest determined the initial investigations of Leibniz, for example – is to start by flicking through the pages to find the conclusion of the proof in Proposition 11 on page 50, before working backwards to check the supporting results. Proposition 11 cites Proposition 6, Lemma 8 and Lemma 12; these in turn invoke Law 2 and Lemma 10; and these lead back further to the preceding Lemmas and Laws. So the reader can either begin working backwards in order, or alternatively stop at some point and choose to take the remaining results on trust. This latter appears to have been the approach of Flamsteed, who, having been directed

[152] *NC*, 3:155–6. This advice is repeated at the start of Book 3.

back to Lemma 11 from Newton's proofs about centripetal forces, examined this result only cursorily and then took the remaining steps for granted. Huygens provides a yet more instructive example. As was shown in Chapter 1, he appears to have made no attempt to work back along the chain of deduction, and when he examined the orbital force theorems in Section 2, there is no evidence that he went back to verify any of the results in Section 1 on which they depended. The note he left when reading Proposition 10 is particularly revealing. In Corollary 2 to this result, Newton makes a claim about the velocities of bodies at the end points of elliptical orbits, but does not justify the claim in the text, and does not supply a proof anywhere in the preceding pages. Both Gregory and Fatio, when they came across this unsubstantiated assertion, satisfied themselves of its truth by generating their own short demonstrations. Yet Huygens merely noted "Il devoit avoir montré auparavant quelle raison il y doit avoir entre les celeritez du corps à l'endroit ou aboutissent les grands diametres des Ellipses," revealing both his inclination to accept a proof from Newton without examining it, and his reluctance to check that the author had indeed produced it. This short note is betraying a remarkable fact about the reception of the mathematical arguments of *Principia*: it shows that Huygens was willing to take on trust the validity of a proof that was not even there.[153]

These difficulties were compounded by the fact that Newton provided the reader with no extra material to help him navigate the logical structure of the text. It might just about have been possible to find Proposition 11 and then work backwards through all the supporting proofs, but without starting at the beginning and working through every page sequentially, it would have been

[153] RS, MS210, 4v; *OC*, 10:150; CH, HUG 7, 12r (=*OC*, 21:417).

impracticable to locate and then verify an individual result hidden in the middle of the book. Fatio suggested some improvements to facilitate exactly this:

> In summa quaque pagina adscribi deberet tum liber, tum numerus propositionum et rerum de quibus agitur summum caput. Variae praeterea construendae essent tabulae, una quae seriem propositionum contineret, altera quae esset materiarum, tertia quae etiam eadem esse posset cum prima, ostenderet ex quibus propositionibus quaevis pendeat propositio.[154]
>
> At the top of each page there should be written first the book, then the number of the propositions, and a heading above of the things about which it is concerned. Moreover, various tables should be constructed, one of which should comprise the order of the propositions, another of which is of the subject matters, and a third of which yet could be the same as the first, and would show on which propositions any proposition depends.

In his own copy Fatio did indeed label each page in this way, and I showed in Chapter 1 that both Huygens and Leibniz sought out results on particular topics, so would also have benefited from such headings. And the final table Fatio suggests would have made it more viable to locate and scrutinise the argument in support of any individual result. His suggestion that the reader be told "on which propositions any proposition depends" would have enabled readers to navigate towards isolated results without reading the whole book. As it stood this was prohibitively difficult, and I have found no evidence to suggest that it was ever even attempted.

[154] *OC*, 10:151. The editors of the *OC* have the second sentence as "… una quae seriem proportionum contineret": I have not seen the original manuscript, but I suggest this is a mistranscription.

In other words, irrespective of its contents and style, the length and structure of Newton's book was in and of itself an obstacle for its readers. If it had been shorter or more clearly organised – if Halley had published the *De Motu* rather than the *Principia*, say – then more readers would have been able to examine its arguments more closely. But the weight of the book in his hands announced to almost every potential reader that a complete study was going to be impossible, and the opaque structure meant that a careful scrutiny of any individual result was going to be very difficult. Most readers had no choice but to take many of Newton's arguments on trust. The length of the *Principia* made it harder to challenge its contents: the large number of pages it contained had the practical effect of increasing the authority of the book.

3.4 Unfamiliar terminology

A much smaller obstacle to Newton's readers was some of the mathematical terminology he used in his proofs. One of the themes of Clerke's letters, for example, was Newton's vocabulary when manipulating ratios. In the first edition, many of the proofs used the term "sesquiplicata", a neologism coined by Newton and intended to be equivalent to the word "sesquialtera", which in modern terms is equivalent to raising to the power of 3/2.[155] Clerke erroneously inferred from his study of Lemma 11 that it must represent raising to the power of 3, but this interpretation failed him in Proposition 15, so he sought clarification from the author. Clerke also argued that instead of the word "dimidiata" to represent the square root of a quantity (Clerke thought it more commonly denoted the process of halving), Newton should instead have used

[155] See Sylla, "Compounding ratios," 14.

"subduplicata," which is "a more proper, more usual & better knowne word". Newton must have been sympathetic to Clerke's misgivings, because he changed the text in his annotated and interleaved copies, appending the sentence "Rationem vero sesquiplicatam voco quae ex triplicata subduplicata componitur, quamque alias sesquialteram dicunt" to the proof of Lemma 11, and replacing every occurrence of the word "dimidiata" in the book with "subduplicata." And Clerke was not the only reader unfamiliar with the terminology Newton used for exponents, as can be seen from responses to Proposition 7 of Book 1, in which Newton deployed "quadrato-cubus" to stand for what the modern reader would call the fifth power: Fatio proposed that this be replaced with "quinta potestas", and Gregory wrote a note explaining that "per *SP* q[uadatro-]c[ubus] intelligetur quinta potestas ipsius *SP* sive SP^5 ut ex ejus generatione patet."[156]

Two further aspects of Newton's terminology caused Fatio disquiet. One concerned the vocabulary used to describe angles. Fatio struggled greatly with the proof of Lemma 26 of Book 1, as can be seen from his uncharacteristically messy marginal notes, and it cannot have helped that in the first edition Newton denoted by the "complementum" of an angle *A* both $\left(360^\circ - A\right)$ and $\left(180^\circ - A\right)$, depending on the context. Fatio suggested that to avoid confusion he drop this term and instead use "supplementum" for $\left(180^\circ - A\right)$, as a modern reader would.[157] Newton instead re-wrote the proof using the phrase "complementum ad duos rectos" for this quantity ("complementary to two right angles"), in the same way that he had in Proposition 17. And the other clarification Fatio sought

[156] Clerke to Newton, 3 Oct. and 7 Nov. 1687, *NC*, 3:488–96; OC, 10:149; RS, MS210, 4r.
[157] Bodleian, Arch. A d.37, 97.

was to the vocabulary used to describe conic sections. For what the modern reader would describe as the "major axis" of an ellipse, the first edition used either "axis transversus" (when specifically describing an ellipse) or "latus transversum" (the general term used by Apollonius for the line joining the vertices of any conic section). By March 1690 Newton had already relabelled the "axis transversus" of the ellipse in Proposition 15 as its "axis major" in his interleaved copy, and as its "axis principalis" in his annotated copy (where he had also added the sentence "Axes vero sectionum conicarum principales appello in quibus earum umbilici jacent").[158] Fatio subsequently proposed replacing the phrase "latus transversum" in each of the three places he found it in the text: with "diameter sive latus transversum" in Lemma 19 (which result concerns conic sections generally), "latus transversum sive axis major" in Proposition 44 of Book 1 (which concerns an ellipse), and to "axis major" in Proposition 42 of Book 3 (which concerns a parabola, wherefore Newton did not make the edit).[159] In other words, both Newton and Fatio agreed that the original vocabulary of "axis transversus" and "latus transversum" was sufficiently unfamiliar that it could usefully be clarified.

In her analysis of Newton's exchange with Clerke, Sylla argues that their disagreement over terminology signifies the *Principia*'s status as "a work belonging to the transitional period between the medieval and modern treatment of ratios." According to this reading, Newton's old-fashioned manner of compounding ratios was shared with late-mediaeval texts by Bradwardine and Oresme, and the letters between Newton and Clerke "echo noisy struggles

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[158] *OC*, 10:153.
[159] Bodleian, Arch. A d.37, 97. Bodleian Arch. A d.37, 75, 136, 510. Newton altered the first two as Fatio suggested, but not the third.

within seventeenth-century mathematics, in the process of which much of the distinctively medieval science of ratios was, along with some inferior seventeenth-century conceptions of ratio, left behind."[160] Seen in the context of other readers' responses to the *Principia*'s terminology, I think this claim is overstated. Newton's unfamiliar vocabulary wasn't restricted to ratios, and there isn't a clear pattern its provenance. The simplest explanation is probably the correct one, which is that the experience of his readers was not at the front of Newton's mind when he composed the text.

3.5 Newton's verbal style

I demonstrated in Chapter 2 that a necessary component of the act of processing the mathematical sections of the *Principia* was translating Newton's prose expressions into equations involving ratios. A large proportion of the primary evidence of readings of the *Principia* – most of Gregory's *Notae*, for example, or Flamsteed's marginalia – consists of strings of equations that are symbolic reformulations of sentences in the text. This task was often successfully completed, but on other occasions it constituted a significant hurdle. Here, for example, is how the most famous proof in the book, the inverse square law of Proposition 11, is set out on the page:

[160] Sylla, "Compounding ratios," 12.

[51]

& GvP ad Qv *quad.* ut CP *quad.* ad CD *quad*; & (per Lem. VIII.) Qv *quad.* ad Qx *quad.* punctis Q & P coeuntibus, eſt ratio æqualitatis, & Qx *quad.* ſeu Qv *quad.* eſt ad QT *quad.* ut EP *quad.* ad PF *quad*, id eſt ut CA *quad.* ad PF *quad.* ſive (per Lem. XII.) ut CD *quad.* ad CB *quad.* Et conjunctis his omnibus rationibus, L x QR fit ad QT *quad.* ut AC ad PC + L ad Gv + CP*q* ad CD*q* + CD*q*. ad CB*q*. id eſt ut AC x L (ſeu 2CB*q*.) x CP*q*. ad PC x Gv x CB*q*. ſive ut 2PC ad Gv. Sed punctis Q & P coeuntibus, æquantur 2PC & Gv. Ergo & his proportionalia L x QR & QT *quad.* æquantur. Ducantur hæc æqualia in $\frac{SPq.}{QR}$ & fiet L x SP*q*. æquale $\frac{SPq. \times QTq}{QR}$. Ergo (per Corol. Theor. V.) vis centripeta reciproce eſt ut L x SP*q*. id eſt reciproce in ratione duplicata diſtantiæ SP. *Q.E.I.*

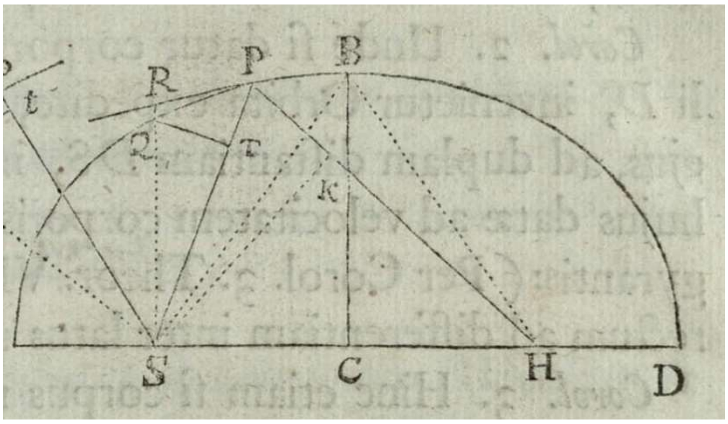
I demonstrated in Chapter 2 how Flamsteed and Gregory successfully translated these sentences into equations, neatly set out underneath each other to make their manipulation easier. In his letters to Newton, Clerke bemoaned this process, and the extra difficulty he found in “contracting the ratios by changing the Quad’s, & getting them in rank & file one under another”. But by telling Newton that “you should have sett them so, for your book is hard enough, make it as easie as you can” Clerke only expressed the frustration that Flamsteed and Gregory may well also have felt.[161]

Clerke also had difficulty with these lines, from Proposition 17 of Book 1:

[161] Clerke to Newton, 7 Nov. 1687, *NC*, 2:492.

PH, in qua umbilicus alter H locatur. Demiſſo ad PH perpendiculo SK, & erecto ſemiaxe conjugato BC, eſt SPq. − 2KPH + PHq. (per Prop. 13. Lib. II. Elem.) = SHq. = 4CHq. = 4BHq. − 4BCq. = $\overline{SP+PH}$ quad. − L x $\overline{SP+PH}$ = SPq. + 2SPH + PHq. − L x $\overline{SP+PH}$. Addantur utrobiq; 2KPH + L x $\overline{SP+PH}$ − SPq. − PHq. & fiet L x $\overline{SP+PH}$ = 2SPH + 2KPH, ſeu SP + PH ad PH ut 2SP + 2KP ad L. Unde datur PH tam longitudine quam poſitione. Nimirum ſi ea ſit corpo-

The proof relates to this construction on an ellipse, although a detailed understanding will not be needed to get the gist of Clerke's complaint:



If the terms are set out "in rank and file one under another", this passage is easy to follow. Newton begins by applying Proposition 13 of Book 2 of the *Elements* to triangle *PHS* (equivalent to the modern cosine rule), and then proceeds to use Pythagoras' Theorem and the basic properties of ellipses (where *L* is the latus rectum, equal to $2BC^2/CD$):

$$
\begin{aligned}
SP^2 - 2KP \times PH + PH^2 &= SH^2 \\
&= 4CH^2 \\
&= 4BH^2 - 4BC^2 \\
&= (SP + PH)^2 - L \times (SP + PH) \\
&= SP^2 + 2SP \times PH + PH^2 - L \times (SP + PH)
\end{aligned}
$$

Adding $2KP \times PH + L \times (SP + PH) - SP^2 - PH^2$ to each side gives

$$L \times (SP + PH) = 2SP \times PH + 2KP \times PH$$

which produces the ratio

$$(SP + PH) : PH = (2SP + 2KP) : L$$

In this form, it is simple enough to follow. But set out on the page as it is in the first edition, Clerke had to ask Newton for help:

> in propos: 17. p. 59. lin: 15. where you say, addantur utrobique you adde 2*KPH*. but on one side, & $L \times \overline{SP + PH}$, but once on the one side & twice on the other; upon wch your concludeing analogism doth depend. & if the equation should be rightly reduced it would be $2SPH + 2KPH = 2SPH + 2KPH$.[162]

It is clear to see what Clerke's misunderstanding here is: when Newton instructs the reader to make the addition half-way through, Clerke adds it not to the first

[162] Clerke to Newton, 26 Sep. 1687, *NC*, 2:485–6.

and last expressions in the chain as intended, but to the preceding two. That is, he adds $2KP \times PH + L \times (SP + PH) - SP^2 - PH^2$ to both sides of

$$(SP + PH)^2 - L \times (SP + PH) = SP^2 + 2SP \times PH + PH^2 - L \times (SP + PH)$$

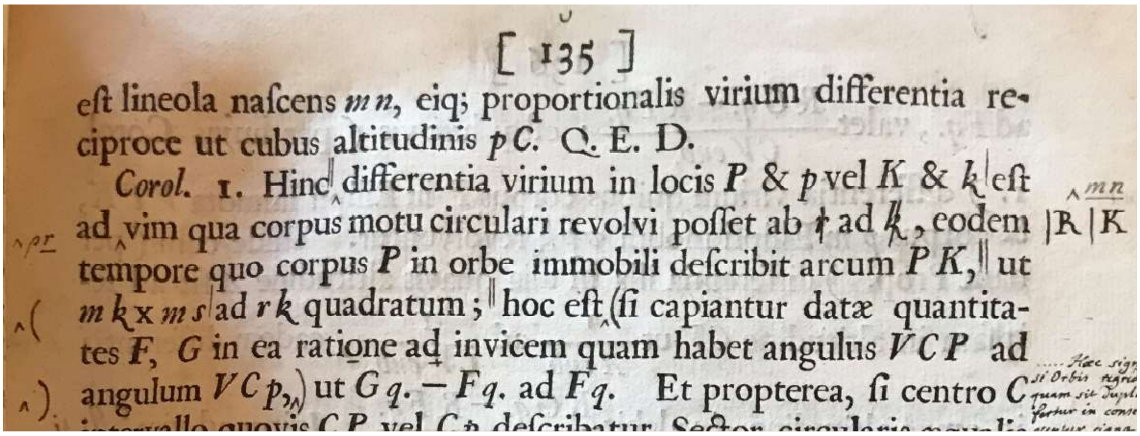
Since the quantities on either side here are identically equal, it is not surprising that he ends up with another identical equality

$$2SP \times PH + 2KP \times PH = 2SP \times PH + 2KP \times PH$$

as he states in his letter. Newton explained the misunderstanding in his reply, and Clerke retorted politely that it would have helped if Newton had "hooked in the intermediate equations with Lunula's" and had "written better to be understood."[163]

However, Clerke was not the only reader who had this difficulty. Here, for example, are the annotations Fatio made in his copy to the statement of Corollary 1 to Proposition 44 of Book 1:[164]

[163] Clerke to Newton, 3 Oct. 1687, *NC* 2:488–90.
[164] Bodleian, Arch. A d.37, 135.


[135]

eſt lineola naſcens *mn*, eiq; proportionalis virium differentia reciproce ut cubus altitudinis *pC*. Q. E. D.

Corol. 1. Hinc differentia virium in locis *P* & *p* vel *K* & *k* eſt ad vim qua corpus motu circulari revolvi poſſet ab *r* ad *k*, eodem tempore quo corpus *P* in orbe immobili deſcribit arcum *PK*, ut *mk* x *ms* ad *rk* quadratum; hoc eſt (ſi capiantur datæ quantitates *F*, *G* in ea ratione ad invicem quam habet angulus *VCP* ad angulum *VCp*,) ut *Gq.* − *Fq.* ad *Fq.* Et propterea, ſi centro *C*

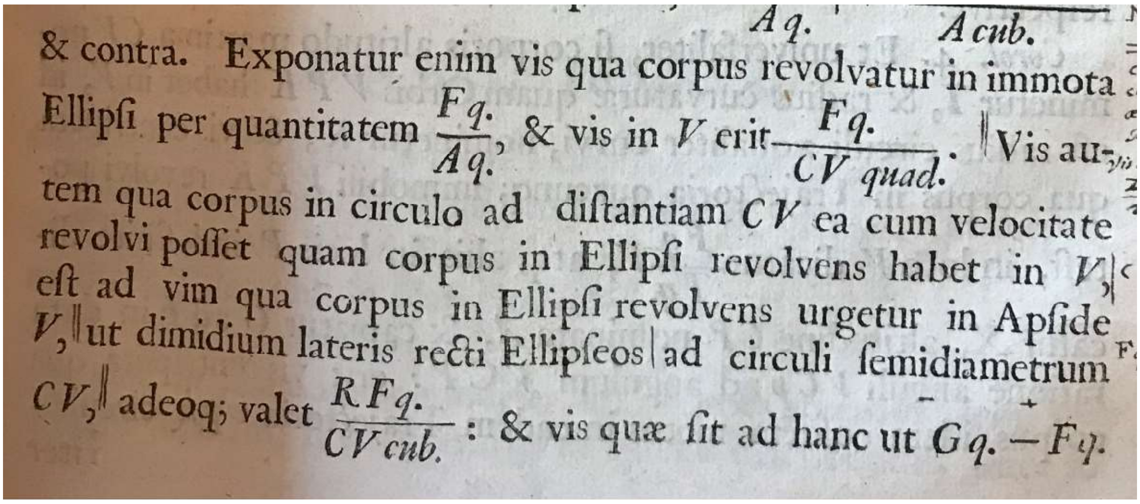
In their translation of this sentence, Cohen and Whitman accidentally did a very good job of conveying the intractability of the original:

> Corollary 1. Hence the difference of the forces in the places *P* and *p* or *K* and *k* is to the force by which a body would be able to revolve with circular motion from *R* to *K* in the same time in which body P in an immobile orbit describes the arc *PK*… as $mk \times ms$ to rk^2, that is, if the given quantities *F* and *G* are taken in the ratio to each other that the angle *VCP* has to the angle *VCp*, as $G^2 - F^2$ to F^2.[165]

An understanding of the construction to which this passage refers is not needed to recognise that, even in English, this is an extremely difficult sentence to parse. It takes a great deal of effort to understand which ratios are being claimed by Newton to equal to which. Fatio only manages it by annotating the text in a number of ways. He uses vertical bars to break the sentence down into its component phrases, with double (‖) and single (|) bars showing which terms correspond. He also introduces algebraic notation to represent some of the verbal expressions, inserting "*mn*" for "differentia virium in locis *P* & *p* vel *K* & *k*" and "*pr*" for "vim qua corpus motu circulari revolvi posset." And he

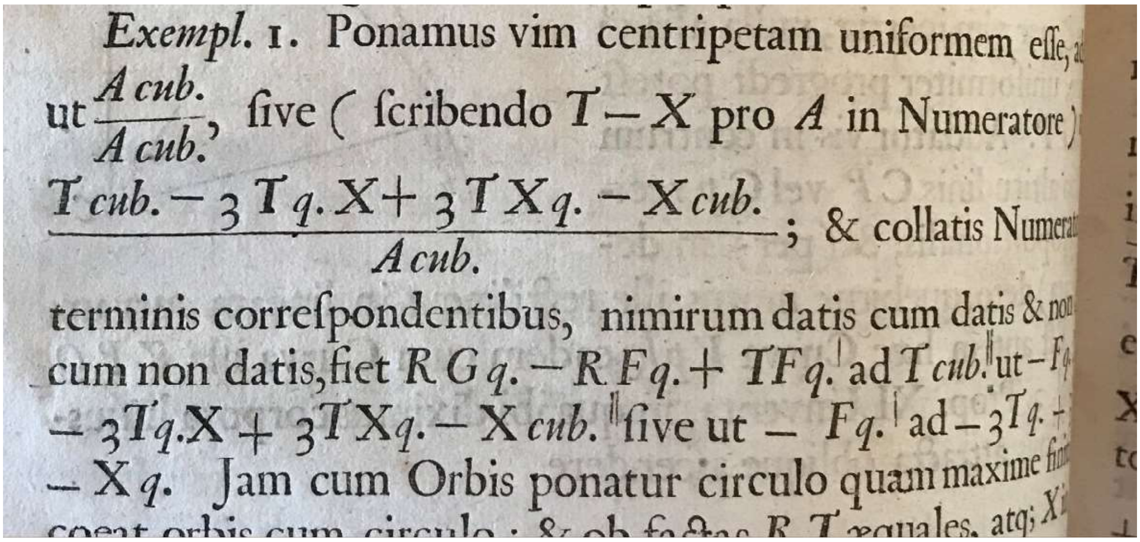
[165] The ellipsis in the middle of the sentence stands for a phrase added to the second and third edition, on which Cohen and Whitman's translation is based.

separates off “si capiantur… ad angulum *VCp*” from the main sentence with parentheses – that is, exactly as Clerke suggested, he “hooks in the intermediate equations with lunulas.” Fatio must have discussed these annotations with Newton, since in his annotated copy Newton made some small clarifications to this passage. But the vertical bars cannot have been an editorial suggestion: Fatio needed to use them to understand the text. And neither are these markings a one-off. They appear again in Corollary 2: [166]


Aq. Acub.
& contra. Exponatur enim vis qua corpus revolvatur in immota Ellipſi per quantitatem $\frac{Fq.}{Aq.}$, & vis in V erit $\frac{Fq.}{CV\ quad.}$. ‖Vis autem qua corpus in circulo ad diſtantiam CV ea cum velocitate revolvi poſſet quam corpus in Ellipſi revolvens habet in V,| eſt ad vim qua corpus in Ellipſi revolvens urgetur in Apſide V,‖ut dimidium lateris recti Ellipſeos|ad circuli ſemidiametrum CV,‖ adeoq; valet $\frac{RFq.}{CV\ cub.}$: & vis quæ ſit ad hanc ut $Gq. - Fq.$

And here is the same thing three pages later, in Example 1 after Proposition 45:[167]

[166] Bodleian, Arch. A d.37, 135.
[167] Bodleian, Arch. A d.37, 138.



Exempl. 1. Ponamus vim centripetam uniformem eſſe,

ut $\frac{A\,cub.}{A\,cub.}$, ſive (ſcribendo $T - X$ pro A in Numeratore)

$\frac{T\,cub. - 3\,Tq.\,X + 3\,T\,Xq. - X\,cub.}{A\,cub.}$; & collatis Numera

terminis correſpondentibus, nimirum datis cum datis & non

cum non datis, fiet $RGq. - RFq. + TFq.$ ad $T\,cub.$ ut $-Fq$

$- 3Tq.X + 3TXq. - X\,cub.$ ſive ut $- Fq.$ ad $- 3Tq. +$

$- Xq.$ Jam cum Orbis ponatur circulo quam maxime fin

In both cases Fatio is again using the bars to separate long sentences into their component phrases, so that he can see how to form the ratios. The single and double bars enable him to see which terms correspond: in the above example, he is clarifying the statement that the ratio of $\left(RG^2 - RF^2 + TF^2\right)$ to $\left(T^3\right)$ will become the same as the ratio of $\left(-F^2X\right)$ to $\left(-3T^2X + 3TX^2 - X^3\right)$, or as the ratio of $\left(-F^2\right)$ to $\left(-3T^2 + 3TX - X^2\right)$.

None of these examples involves mathematical techniques that were at the time innovative, and in each case the ratios being formed are extremely simple. Yet it is clear that the verbal style in which Newton expressed his mathematics formed a significant obstacle to his readers' understanding. The form in which the arguments in the *Principia* were presented hindered their comprehension. Clerke was not able to overcome this difficulty without guidance from the author. In Fatio's case, he could only understand Newton's proofs by making additional annotations to render the text comprehensible. In both cases the printed text was ineffective at persuading its readers.

3.6 Unclear explanations

A large number of passages in the *Principia* were difficult to understand owing to the lack of clarity of Newton's explanations. Strong evidence for this lies in the editorial notes from Halley and Fatio, which demonstrate the ways in which Newton's peers wanted his proofs to be more clearly worded.

Some of these editorial suggestions comprise only very minor adjustments to Newton's phrasing. In the discussion of the rotating bucket in the Scholium after the Definitions, for example, Halley suggested altering "defluet ipsa paulatim de medio" to "recedet ipsa paulatim e medio," and "motus autem relativi… effectibus veris omnino vacant" to "motus autem relativi… effectibus veris omnino destituuntur." When in the very next paragraph Fatio read the sentence "Igitur quantitates relativae non sunt eae ipsae quantitates, quarum nomina prae se ferunt, sed earum mensurae illae sensibiles… quibus vulgus loco mensuratarum utitur" he noted "pro *mensuratarum* legi velim *quantitatum mensuratarum.*" Newton made the change in all three cases, and Halley's feedback sheets and Fatio's notes and marginalia contain dozens of similar examples. However, it is difficult to argue that such instances increase the clarity of the text. Halley and Fatio in these cases are not making Newton's text easier to understand, so much as rewriting the passage as they themselves would have phrased it. In this sense, these edits are perhaps most helpfully seen in the context of the themes of Chapter 2, as further examples of readers recreating Newton's proofs in their own terms during the act of understanding them.[168]

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[168] CUL, MS Add. 3965, 94r; *OC*, 10:147.

Many of Fatio and Halley's other suggestions, however, are much more obviously motivated by a desire to clarify a proof, or at least to sharpen its articulation. In the Corollary to Lemma 27, for example, Fatio adds the sentence "Nam ex constructione patet lineam *BL* eadem ratione secari in *D* et *R* qua *FI* secatur in *G* et *H*" to help explain how the ratios correspond to each other from the diagram in the demonstration. Halley likewise adjusts Proposition 22 to make the construction easier to understand, changing the word order to "Namque ob proportionales *CD*, *CP*, linea *AB* communis est utriusque figurae *RPB*, *DEB* diamter. Bisecetur eadem in *O*…", in order to clarify that the line *AB* is the object that Newton means to be bisected. And in the discussion of what the modern reader would call the conservation of momentum in the Scholium after the Laws, Fatio sharpened "in partes contrarias mutatio motus erat corpori utriq; illata" ("a change of motion was brought about in opposite directions for each body") by inserting the crucial word "aequalis" ("an equal change of motion"). Dozens of similar examples could be adduced. In some cases these changes were prompted by Fatio or Halley not understanding Newton's proof until they made a crucial realisation that they then felt obliged to articulate; in others, they merely felt that any reader looking to follow in their footsteps could usefully be given a little assistance.[169]

Elsewhere, Fatio and Halley clarified the logic of Newton's arguments by making explicit the restrictions on which they depend. For example, Fatio inserts the qualification that Proposition 44 only holds in the limit ("punctorum *P*, *K* distantia intelligatur esse quam minima"); that Proposition 72 requires the density of the sphere to be given ("ac detur tum sphaerae"); and that the

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[169] Bodleian, Arch. A d.37, 101; CUL, MS Add. 3965, 97r; *OC*, 10:152.

argument of Lemma 27 is only valid if the straight line within the oval revolves at a uniform rate ("uniformi cum motu"). Halley likewise insists that Corollary 2 of Proposition 4 in Book 2 only holds "cum velocitate data" ("when the velocity has been given"). Again, these amendments can either be viewed as precision fine-tuning of the rigour of proofs, or as indications that Newton's original demonstrations could only be understood when their conditions were made sufficiently clear.[170]

Halley and Fatio also improved a large number of diagrams that they felt were substandard or misleading. They urged clearer labelling for the diagrams in Proposition 7, Lemma 20, Proposition 41, Proposition 43, and Proposition 96 of Book 1, as well as Proposition 9 of Book 2; and Fatio had to redraw the construction because the original did not match the description in Lemma 26, Lemma 27, Proposition 50 and Proposition 53 of Book 1, and Proposition 41 of Book 3.[171]

A few final examples will illustrate the further variety of ways in which Newton's text was perceived to be unclear. While Halley's editorial role seems to have been limited to tweaking individual sentences, Fatio appears to have been allowed wider scope, and was able to propose more substantial changes. For example, this is how Proposition 3 of Book 1 read in the first edition:

> Corpus omne quod, radio ad centrum corporis alterius utcunq; moti ducto, describit areas circa centrum illud temporibus proportionales, urgetur vi composita ex vi centripeta tendente ad corpus alterum & ex vi omni acceleratrice, qua corpus alterum urgetur.

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[170] Bodleian, Arch. A d.37, 133, 105; *OC*, 10:150; CUL, MS Add. 3965, 96r
[171] Bodleian, Arch. A d.37, 76, 96, 99, 100, 128, 132, 151, 157; *OC*, 10:150, 151; CUL, MS Add. 3965, 96r, 97r.

Nam (per Legum Corol. 6.) si vi nova, quae aequalis & contraria sit illi qua corpus alterum urgetur, urgeatur corpus utrumq; secundum lineas parallelas, perget corpus primum describere circa corpus alterum areas easdem ac prius: vis autem qua corpus alterum urgebatur, jam destruetur per vim sibi aequalem & contrariam, & propterea (per Leg. 1.) corpus illud alterum vel quiescet vel movebitur uniformiter in directum, & corpus primum, urgente differentia virium, perget areas temporibus proportionales circa corpus alterum describere. Tendit igitur (per Theor. 2.) differentia virium ad corpus illud alterum ut centrum. Q.E.D.

Every body that, by a radius drawn to the centre of a second body moving in any way whatever, describes about that centre areas that are proportional to the times is urged by a force compounded of the centripetal force tending toward the second body and of the whole accelerative force by which the second body is urged.

For (by Corollary 6 of the Laws) if each body is urged along parallel lines by a new force that is equal and opposite to the force by which the second body is urged, the first body will continue to describe about the second body the same areas as before; but the force by which the second body was urged will now be annulled by an equal and opposite force, and therefore (by Law 1) that second body either will be at rest or will move uniformly straight forward; and the first body, urged by the difference of the forces, will continue to describe areas proportional to the times about the second body. Therefore, the difference of the forces tends (by Theorem 2) towards that second body as its centre. Q.E.D.

A modern reader may well sympathise with Fatio's view that this passage "obscurior esset propter frequentiorem usum vocis *alterum*" ("is rather obscure because of the very frequent use of the word *alterum*"). To remedy this, he suggested using the letters *L* and *T* for the two bodies, and inserting the clarifying phrases "sibimet ipsi jam relictum" and "id est urgente vi reliqua" in the penultimate sentence, which is exactly what Newton did in the second edition.[172]

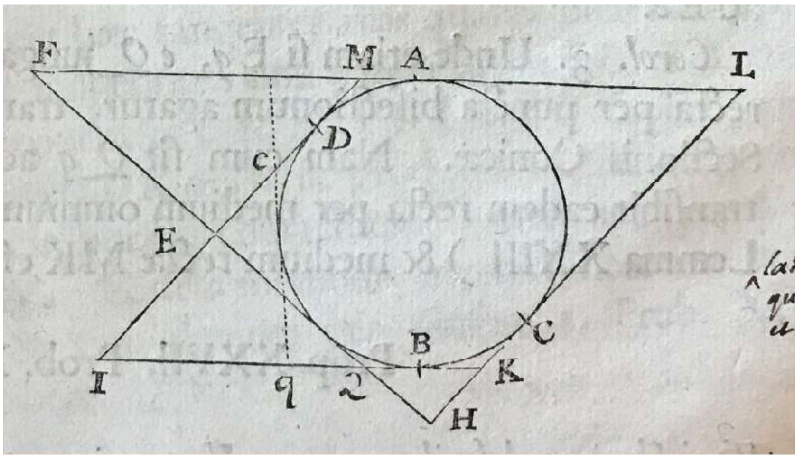
Two pages later, Fatio came across Corollary 7 of Proposition 4:

> Eadem omnia de temporibus, velocitatibus & viribus, quibus corpora similes figurarum quarumcunq; similium, centraq; similiter posita habentium, partes describunt, consequuntur ex Demonstratione praecedentium ad hosce casus applicata.
>
> In cases in which bodies describe similar parts of any figures that are similar and have centres similarly placed, all the same proportions with respect to the times, velocities, and forces follow from applying the foregoing demonstrations to these cases.

His comment on this passage was simply "Coroll. pag. 42 obscurum est," whereupon Newton appended further explanation.[173]

Fatio also advocated substantial rewrites to many of the results in Section 5 and Section 9. Lemma 25, for example, derives a result that is easy to state symbolically, but very difficult to articulate verbally. With reference to the diagram below, it proves that the ratio of *ME* to *MI* is equal to the ratio of *BK* to *KQ*:

[172] *OC*, 10:148–9.
[173] *OC*, 10:149.



Newton’s wording of this result in the first edition was:

> Si parallelogrammi latera quatuor infinite producta tangant sectionem quamcunq; Conicam, & absincdantur ad tangentem quamvisquintam; sumantur autem abscissae terminatae ad angulos oppositos parallelogrammi: dico quod abscissa unius lateris sit ad latus illud, ut pars lateris contermini inter ounctum contactus & latus tertium, ad abscissam lateris hujus contermini.
>
> If the four sides of a parallelogram, infinitely produced, touch any conic section and are cut off at any fifth tangent; and if the cut-offs are assumed ended at opposite angles of the parallelogram: I say that the cut-off of one side [*ME*] is to that side [*MI*] as the part of the adjoining side between the point of contact and the third side [*BK*] to the cut-off of this adjoining side [*KQ*].

Unsurprisingly, Fatio felt the need to attempt to phrase this more clearly:

> … sumantur autem abscissae laterum duorum conterminorum, quae latera dicantur primum et secundum, terminatae ad angulos oppositos parallelogrammi: dico quod abscissa primi lateris sit ad latus illud, ut pars

lateris secundi inter punctum contactus & latus tertium, id est latus primo lateri oppositum, ad absciccam lateris hujus secondi.[174]

… and if the cut-offs of two adjacent sides, which sides will be designated the 'first' and 'second', are assumed ended at opposite angles of the parallelogram: I say that the cut-off of the first side [*ME*] is to that side [*MI*] as the part of the second side between the point of contact and the third side, that is the side opposite to the first side [*BK*], to the cut-off of this second side [*KQ*].

Although his amended version was slightly different, Newton was indeed persuaded of the need of a rewrite. And as a final example Fatio's adjustments to Lemma 21 may be cited, which were even more involved. There is not space to discuss them in detail here, but his marginalia show that although he did not challenge the validity of the result, he encouraged Newton to clarify its statement, tweak the labelling on the diagram, and rephrase many of its sentences to make it easier to follow.[175]

3.7 Incomplete proofs

It might be argued that the stylistic faults highlighted in the previous sections are fundamentally cosmetic, and that although they make the book harder to read, they do not undermine the integrity of Newton's arguments. That cannot be said of the examples considered in this final section, which highlight instances of readers being confronted by what they perceived to be logical gaps in Newton's demonstrations. In such circumstances, they were obliged either to attempt to fill in the missing steps themselves, seek help from the author, or resign themselves to incomprehension. Naturally, there are few absolute distinctions to

[174] Bodleian, Arch. A d.37, 91.
[175] Bodleian, Arch. A d.37, 77–9.

be had here. What a reader might perceive as an unexplained step in the proof, the author might consider to be prior knowledge he could reasonably expect. There is no clear-cut boundary between a reader lacking expertise and the author overestimating the skills of his readership. Nevertheless, I hope in this section to present a convincing case that Newton's readers sometimes had good cause to feel frustrated by the inadequacy of the arguments on the page.

First, it is noteworthy that it was common for readers to buttress the proofs by supplying references that were missing from Newton's text – references either to standard propositions from Euclid and Apollonius, or to other results within the *Principia*. I have already shown Clerke express his desire for more such marginal citations, to ease his understanding of the demonstrations. Gregory's *Notae* are full of such references to standard texts, and Fatio is meticulously careful to ensure that the cross-references within the *Principia* are correct. What is indicated by readers' desire to supply these references is harder to infer. Fatio (and possibly Gregory), may merely have been keen to be seen to be thorough in his editorial duties. However, references to external texts may also indicate that prior knowledge Newton took for granted was not shared by his peers. For example, a number of the proofs in Section 2 of Book 1 rely on Proposition 36 from Book 3 of Euclid's *Elements*, but Newton nowhere makes this reference explicit. He appears to expect his reader to be confident applying the result. Gregory, however, marks the reference in his *Notae* every time it occurs. This doesn't prove that Gregory didn't know the result and had to refer to his library for assistance, but it does suggest that it was sufficiently unfamiliar to be noteworthy, or that he thought that others might find it to be so.[176]

In other instances, Newton left gaps that standard works were unable to fill in. The primary sources present clear evidence of a handful of such cases. In the Scholium at the end of Section 6 of Book 1, for example, the text refers in passing to “the method of Dr Seth Ward” for calculating the position of planets, which the marginalia in his copy show that Fatio did not know about until he asked Newton.[177] Likewise, Halley could not see why two triangles in Proposition 8 of Book 1 were similar, until the author explained that the construction should be considered in the limit and Lemma 7 invoked.[178] I showed above that both Gregory and Fatio were forced in their notes to reconstruct the missing steps in the proof of Corollary 2 to Proposition 10 in Book 1, which Newton justified only cursorily.[179] Huygens similarly filled in gaps in the demonstrations of Proposition 8 and Lemma 2 of Book 2, as did Gregory for the Scholium after Proposition 4 in Book 1. The details of these last examples will be examined in subsequent chapters. I will therefore restrict myself in this section to one particularly egregious instance of a gap in the proofs, which is revealed by the difficulties Fatio encountered when working through Corollary 2 of Proposition 30 and Corollary 2 of Proposition 44.

Of the two results, Corollary 2 of Proposition 30 is simpler to follow. Proposition 30 is the first result in Section 6, whose theme is “To find motions in given orbits.” The main proof provides a very elegant construction whereby a

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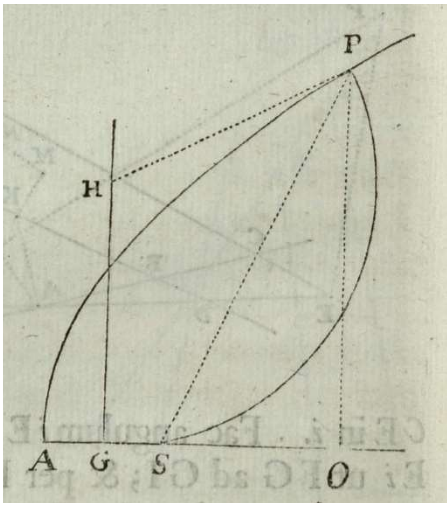
176 See, for examples, RS, MS210, 4r; *OC*, 10:151, and Bodleian Arch. A d.37, 82, 102, 411, 486, 487, 488, 489.
177 Bodleian Arch. A d.37, 114.
178 CUL, MS Add. 3965, 97r.
179 RS, MS210, 4v; *OC*, 10:150; CH, HUG 7, 12r (=*OC*, 21:417).

given area can be cut off from a parabola by a line radiating from the focus. Since under the action of a centripetal force equal areas are swept out in equal times, this is equivalent to being able to find the position of a body (such as a comet) moving along a parabolic trajectory at a given time. Here is the diagram Newton supplies:



S is the focus and *A* is the principal vertex; the point *O*, which is used in the finer details of the proof, can be safely ignored in everything that follows. Given any parabola (that is, with *S* and *A* fixed) Newton's aim is to construct a line *SP* such that the curvilinear area *APS* is equal to a given quantity. He achieves this as follows. Call the area to be cut off $4AS \times M$, where *AS* is the given length between the focus and the principal vertex (and so 4*AS* is the latus rectum) and *M* is some constant. Label the midpoint of *AS* as *G*, and construct the perpendicular bisector. Mark the point *H* such that *GH* is equal to 3*M*, and finally construct a circle with centre *H* and radius *HS*. This circle will intersect the parabola at the required point *P*, as shown. Newton proves that this construction does indeed generate an area *APS* equal to $4AS \times M$ using half a dozen lines of simple algebra. The demonstration is based on Pythagoras' Theorem, the basic properties of the parabola, and a standard result about its quadrature from Archimedes.

This construction provides a dynamic geometrical model in which the point *P* represents a body moving along a parabolic trajectory under the action of a centripetal force directed towards *S*, with the distance *GH* representing a measure of time. To understand why this is so, the construction needs to be imagined moving. For a given parabola *A* and *S* are fixed, which means that *G* (the midpoint) is also fixed. The body *P* is to be imagined moving along the curve, away from *A*. As it does so, *H* moves up the perpendicular, away from *G*. It does this in such a way that the distance *HS* is always equal to the distance *HP*, which means that the circular arc joining *S* to *P* moves at the same time. As *P* moves out along the parabola, *H* moves up the straight line. Moreover, Newton has shown that at any given moment the area *APS* is given by $4AS \times M$, where *AS* is fixed and *M* is variable. But in the construction the length *GH* was set equal to 3*M*. This means that the area must be directly proportional to the length *GH*; and so the length *GH* can be used as a measure of time. It is perhaps more useful to think of the motion of *H* driving the motion of *P*, rather than the other way around: as *H* moves up *GH* at constant speed, *P* moves out along the parabola with decreasing speed, in accordance with Kepler's area law. It is an ingenious and very pleasing construction.

Fatio appears to have had no problems following this proof. However, the three corollaries that follow the main result are a different matter. Here is my translation of these results as they appear in the first edition:[180]

> Corollary 1. Hence *GH* is to *AS* as the time in which the body described the arc *AP* is to the time in which it described the arc between the vertex *A* and the perpendicular to the axis erected from the focus *S*.
>
> Corollary 2. And if a circle *ASP* continually passes through the moving body, the velocity of point *H* is to the velocity which the body had at the vertex *A* as 3 to 8, and thus the line *GH* is also in this ratio to the straight line which the body could describe in the time of its motion from *A* to *P* with the velocity which it had at the vertex *A*.
>
> Corollary 3. Hence also, conversely, the time can be found in which the body described any assigned arc *AP*. Join *AP* and at its midpoint erect a perpendicular meeting the straight line *GH* in *H*.

The reader who consults the original text will find that Newton has supplied no additional diagram to accompany these corollaries, nor any explanatory material. Anyone who wishes to sympathise more directly with Fatio's experience may at this point try to verify the results themself: if they do so, they will not find any help in the translation by Cohen and Whitman, which contains no guidance and two errors, one of which reveals the authors have not understood the statement of this result, never mind the proof.[181]

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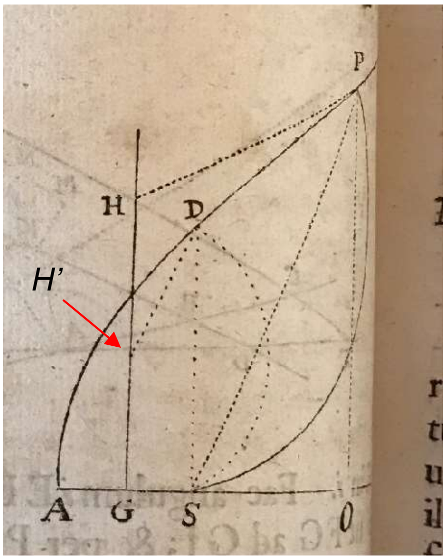
[180] The printed text of Corollary 2 erroneously had "velocitas puncti *G*", but the *Errata* instructed it to be changed to "velocitas puncti *H*", as shown here. In later editions "per corpus movens" was altered to "per corpus motum *P*", but that change is not germane to the discussion here.
[181] Towards the end of the main proof, AO^2 is misprinted as AQ^2, even though there is no point *Q* in the proof; and in the first corollary "perpendiculum ad axem ab umbilico *S* erectum" is mistranslated as "a perpendicular erected from the focus *S* to the axis", which makes no sense in terms of the construction.

The first corollary submits easily enough. The perpendicular erected from the focus is not marked on the printed diagram, so Fatio wisely decides to mark it onto his copy, and label the point where it meets the parabola "*D*":



Corollary 1 says that, as the body P moves along the parabolic trajectory *ADP*, the ratio of *GH* to *AS* is always equal to the ratio of the time it took to move from *A* to *P* to the time it took to move from *A* to *D*. It has already been established that *A*, *G*, *S* and *D* are fixed, and that as *P* moves along the parabola, *H* moves up the perpendicular from *G*. It has also been shown that for any position *P* on the trajectory the area *APS* is given by $4/3 \times GH \times AS$, which means that the time elapsed since leaving the point *A* is proportional to $4/3 \times GH \times AS$. To establish the time taken to move from *A* to *D*, the location of *H* when the body is at *D* needs to be considered. Fatio has drawn in dotted lines the construction lines at this point, but not explicitly marked the position where *H* will lie when the body is at *D*: I have labelled it H'. The distance GH' is required, and there are a few ways to find it. One is to realise that, since $H'D$ and $H'S$ are equal,

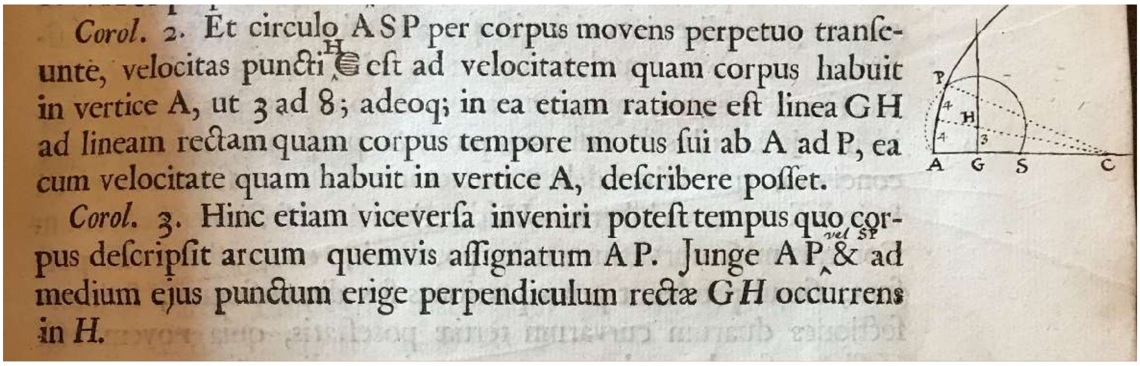
$H'DS$ is an isosceles triangle; and since both *SD* and *GH* are perpendicular to the axis *AGSO*, GH' must be equal to $1/2 \times SD$. But *S* is the focus, and so from the defining properties of a parabola, *SD* is the semi-latus rectum and *AS* is half of the semi-latus rectum. This means that GH' must be equal to *AS*. Therefore the area *ADS* must be $4/3 \times GH' \times AS = 4/3 \times AS \times AS$. Since the area *APS* was $4/3 \times GH \times AS$, the ratio of the area *APS* to the area *ADS* is equal to the ratio of *GH* to *AS*; and so the ratio of the time taken to move from *A* to *P* to the time taken to move from *A* to *D* must also be *GH* to *AS*. This is the required result.

The third corollary is even simpler. It proposes that, wherever the point *P* lies on the trajectory, the time it has taken to get there from the vertex *A* can be found. Since *GH* is the measure of time, this amounts to finding the point *H* given the point *P*. This is easy: construct the perpendicular bisector of *PS*, and it will intersect the upright through *G* at the required point.

Corollary 2, however, is baffling. The reader is invited at this point to read it again. The set-up is comprehensible, and now it has been intuited that *H* moves with constant speed up the perpendicular as *P* moves with decreasing speed along the curve, it is makes sense to consider the ratio of the speed of *H* to the speed of the body at the vertex. But the sudden appearance of the ratio of 3 to 8 is puzzling.

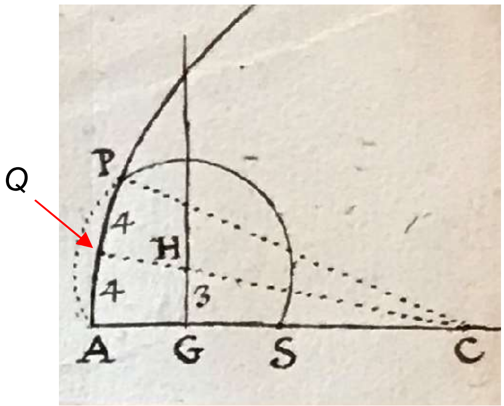
The margin of Fatio's first edition alongside this result contains the following diagram:[182]

[182] Bodleian Arch. A d.37, 105.



Corol. 2. Et circulo ASP per corpus movens perpetuo tranſeunte, velocitas puncti H eſt ad velocitatem quam corpus habuit in vertice A, ut 3 ad 8; adeoq; in ea etiam ratione eſt linea GH ad lineam rectam quam corpus tempore motus ſui ab A ad P, ea cum velocitate quam habuit in vertice A, deſcribere poſſet.

Corol. 3. Hinc etiam viceverſa inveniri poteſt tempus quo corpus deſcripſit arcum quemvis aſſignatum AP. Junge AP vel SP & ad medium ejus punctum erige perpendiculum rectæ G*H* occurrens in *H*.



It is not at all clear where this mysteriously crisp, impeccably drawn diagram has come from, nor how it relates to Corollary 2. There are some familiar features – *A*, *G* and *S* are where they should be, and there is a trajectory emerging from *A* and a circle passing through *A*, *P* and *S*. A ratio of 3 to 8 is ready to emerge somehow, although where the 3 and the two 4s have come from is unclear. But *P* has moved much lower down the curve beyond the perpendicular *GH*, and a new point, *C*, has appeared, which is not referred to in the text. Fatio has provided no further notes to help: there is no explanatory text anywhere else on the page.

It takes considerable effort to decode this diagram. *P* has moved down the trajectory because the motion close to the vertex *A* is being considered; *H* is constructed on the perpendicular as before, with a circle through *A*, *P* and *S*.

But the trajectory emerging from *A* in this diagram is not in fact a parabola: rather, it is the osculating circle of the parabola at *A*, which is being used to approximate the parabola near *A*. *C* is the centre of this osculating circle, and therefore the distance *AC* (and the distance *PC*) is equal to the radius of curvature of the parabola at the vertex. It so happens that the radius of curvature of a parabola at its vertex is equal to half of the latus rectum. The latus rectum of the parabolic trajectory the osculating circle is approximating was equal to 4*AS*, which means that the distance *AC* must be equal to 2*AS*. Since *G* is the midpoint of *AS*, the points *A*, *G*, *S* and *C* must therefore divide the horizontal axis in the ratio $AG:GS:SC = 1:1:2$. Specifically, the ratio of *GC* to *AC* is 3 to 4.

Fatio's diagram can now be fully understood. I have labelled the point on the curve between *A* and *P* as *Q*, as indicated. Because the small circle passing through *A*, *P* and *S* has its centre at *H* (from the original construction), the distance *AH* must be equal to the distance *PH*. But because *C* is the centre of the osculating circle, *AC*, *QC* and *PC* must all be equal. So *QHC* is a line of symmetry of the sector *APC*; this means that the length *AQ* must be equal to the length *PQ* (measured either curvilinearly or rectilinearly). Finally, consider the limit when *P* (and therefore *Q*) are very close to *A*. In the limit, the shape *ACQ* is similar to the shape *GCH*. It does not matter whether in this limit the sector *ACQ* is viewed as becoming a triangle with a right-angle at *A*, or the triangle *GCH* is viewed as becoming a sector with *HC* equal to *CG* – either way, the two shapes become similar. But it was previously established that the ratio of *GC* to *AC* is 3 to 4. This means that, in the limit, the ratio of *GH* to *AQ* is also 3 to 4. And since $AQ = PQ$, the ratio of *GH* to *AP* is 3 to 8. This has finally established the result in Corollary 2: in the limit as *P* approaches *A* the ratio of the distance moved by *H* to the distance moved by *P* in the same time is 3 to 8, which is another way of saying that the ratio of the velocity of the point *H* to the velocity the body has at *A* is 3 to 8.[183]

Now that the proof has been understood, it can be considered how a reader such as Fatio might have been expected to reproduce it himself. If he is to successfully recreate the proof of Corollary 2 from the bare statement Newton supplies, the reader needs to have two flashes of insight, and one specialised piece of knowledge. The first insight is to use the osculating circle to approximate the parabola. The piece of knowledge is that the radius of curvature of a parabola at its vertex is equal to half of the latus rectum. Then, having constructed the diagram above and calculated the ratios of all its component parts, the reader needs to realise that they should consider what happens in the limit as *P* is close to *A*. All three are required to establish the ratio of 3 to 8. And yet Newton's text does not give the slightest hint of any of them: there is no mention of the osculating circle in this or any of the preceding proofs, no statement about the radius of curvature of a parabola anywhere in the book, and for any reader working through the propositions in order there is no expectation that he needs to make a return to limit methods, which were last used in Section 3.

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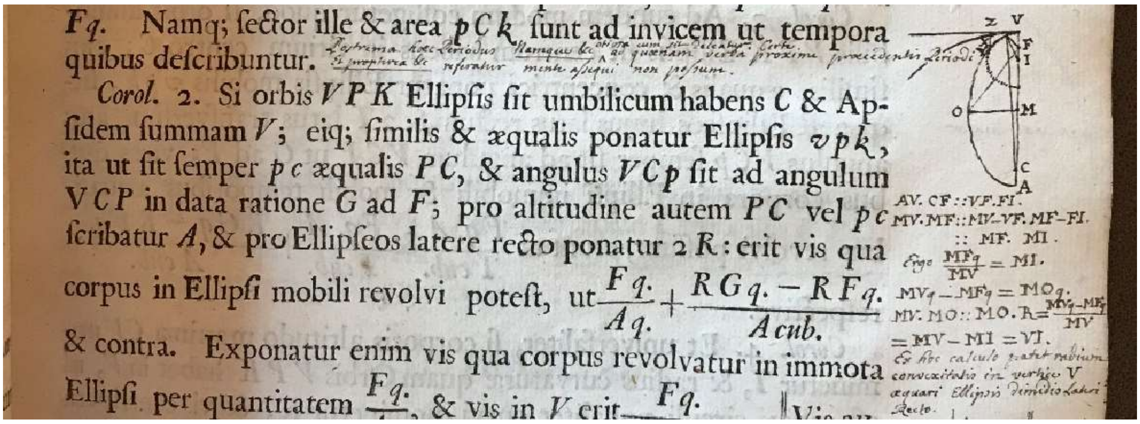
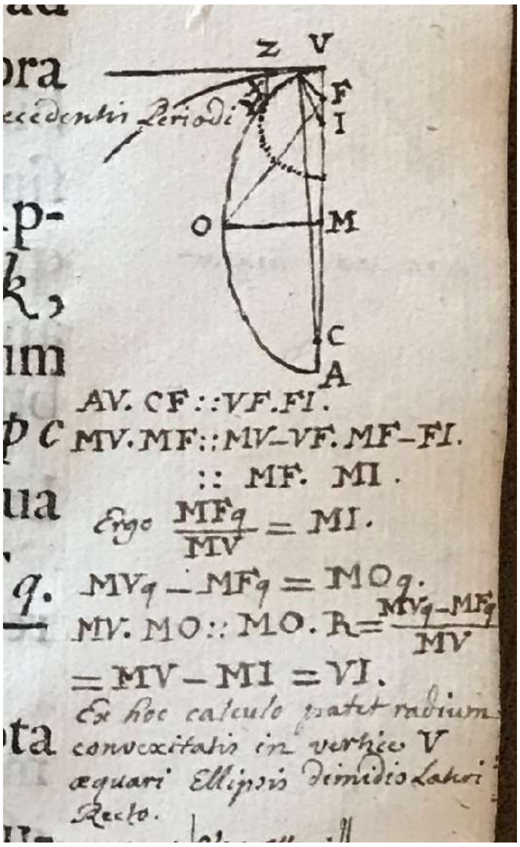
[183] The second claim of the corollary, that at any point in the trajectory 3 to 8 is also the ratio of the distance *GH* to the distance the body would have covered if it had spent the intervening time travelling in a straight line with the speed it had at *A*, follows immediately.

There is a connection between these annotations and those in Fatio's margin thirty pages later in Section 9, next to the start of Corollary 2 of Proposition 44:[184]


Fq. Namq; ſector ille & area pCk ſunt ad invicem ut tempora quibus deſcribuntur.

Corol. 2. Si orbis VPK Ellipſis ſit umbilicum habens C & Apſidem ſummam V; eiq; ſimilis & æqualis ponatur Ellipſis vpk, ita ut ſit ſemper pc æqualis PC, & angulus VCp ſit ad angulum VCP in data ratione G ad F; pro altitudine autem PC vel pc ſcribatur A, & pro Ellipſeos latere recto ponatur 2R: erit vis qua corpus in Ellipſi mobili revolvi poteſt, ut $\frac{Fq.}{Aq.} + \frac{RGq. - RFq.}{A\,cub.}$ & contra. Exponatur enim vis qua corpus revolvatur in immota Ellipſi per quantitatem $\frac{Fq.}{\ }$, & vis in V erit $\frac{Fq.}{\ }$



[184] Bodleian Arch. A d.37, 135.

Section 9 concerns orbits that precess; that is, orbits in which the body follows a path of fixed shape which itself gradually rotates. Proposition 44 demonstrates that the extra force required to make a given orbit precess at a particular rate is inversely proportional to the cube of the distance of the orbiting body from the centre of attraction. Corollary 2 applies result this to the case of a precessing elliptical orbit. Fatio expended considerably energy working through Section 9 and left many marginal notes whose details need not concern us here. For present purposes our attention can be confined to the annotations above, which lie alongside this assertion in the proof:

> But the force by which a body could revolve in a circle at the distance *CV* with the velocity that a body revolving in an ellipse has at *V* is to the force by which a body revolving in an ellipse is urged at the apsis *V* as half of the latus rectum of the ellipse to the semidiameter *CV* of the circle

V is the vertex (or apsis) of the ellipse, as shown in Fatio's diagram, and Newton's claim follows immediately from the fact that the radius of curvature of an ellipse at its vertex is equal to half the latus rectum. Of course, this is the same fact that was needed in the proof of Proposition 30, where it was applied to a parabola rather than an ellipse. But once again, Newton supplies no justification of this assertion in the text. The purpose of Fatio's marginal note here is to derive this relationship, as can be seen from the sentence at the bottom "Ex hoc calculo patet radium convexitatis in vertice *V* aequari Ellipsis dimidio Lateri Recto."

Even knowing that this is what Fatio's note sets out to prove, it is still difficult to parse. He has drawn another meticulously precise diagram, this time showing an ellipse *VOA*, which has its foci at *C* and *F*, and its centre at *M*. Part of a large circle with centre C and radius CV has been drawn in, as has the osculating circle at the vertex V, which has its centre at I. The radius of curvature at V is therefore equal to the distance VI. Fatio begins by stating that

$$AV : CF :: VF : FI$$

The question of where this proportion comes from will be considered presently; for the moment let it simply be accepted that it is true.[185] Proceeding from here, it is clear from the construction that the ratio $AV : CF$ is the same as the ratio $MV : MF$, and so

$$MV : MF :: VF : FI$$

Fatio does not write this down. Rather, he immediately manipulates these ratios to give

$$MV : MF :: (MV - VF) : (MF - FI)$$

From the construction, the right-hand side is obviously equal to $MF : MI$, whence Fatio concludes that

$$MI = \frac{MF^2}{MV} \qquad (+)$$

[185] The reader is invited to derive it for themselves; I have been able to do so only by using infinite series.

He then considers triangle *MOF*, as indicated on the diagram. By Pythagoras' Theorem $OF^2 = MO^2 + MF^2$, but by the basic properties of an ellipse $OF = MV$. This gives

$$MV^2 - MF^2 = MO^2 \quad (*)$$

Fatio next considers the latus rectum of the ellipse, which is defined as the quantity $2MO^2/MV$. He denotes this as 2*R* (meaning that *R* is half of the latus rectum). Therefore

$$MV : MO :: MO : R$$

and so $R = MO^2/MV$. Using (*), this gives

$$R = \frac{MV^2 - MF^2}{MV}$$

which can be decomposed further as

$$\begin{aligned} R &= \frac{MV^2}{MV} - \frac{MF^2}{MV} \\ &= MV - \frac{MF^2}{MV} \\ &= MV - MI && \text{by (+)} \\ &= VI && \text{by construction} \end{aligned}$$

From the definition of *R*, this means that the radius of curvature at the vertex *V* of the ellipse is equal to half of the latus rectum, as required.

The proofs of both Propositions 30 and 44 therefore depend upon the fact that the radius of curvature of a conic section at its vertex is equal to half the latus rectum. Yet in neither proof is this fact stated, nor implied to be relevant. Radius of curvature is not mentioned. Moreover, a reader who wishes to prove Corollary 2 to Proposition 30 – even one who knows about the radius of curvature of conic sections – is required to realise that the osculating circle needs to be constructed, and then consider what happens to the ratios of its components in the limit as *P* moves towards *V*.

These notes therefore imply important information about Fatio's reading of the book, and the wider characteristics of Newton's text. The only reasonable conclusion to draw from his annotations is that Fatio did not know this fact about radius of curvature, and so was not able to reproduce either proof: he was only able to do so after Newton explained the derivations to him in a face-to-face meeting. His notes are the post-hoc record of a conversation with Newton. The meticulous clarity of the diagrams, the absence of any surrounding explanatory material in Proposition 30, and the sudden appearance of the proportion $AV : CF :: VF : FI$ at the start of the note for Proposition 44 are most plausibly explained by the inference that Fatio was unable to make sense of the printed proofs, sought help from the author, and wrote up the contents of their conversation afterwards. And I have not seen any evidence that any of Newton's contemporary readers were able to understand these passages on their own: the printed explanations were so deficient that they were only rendered comprehensible by a direct, face-to-face conversation with their author. The text of the first edition did not – and it this case, it is tempting to say, could not – convince its readers of the validity of the arguments it contained.

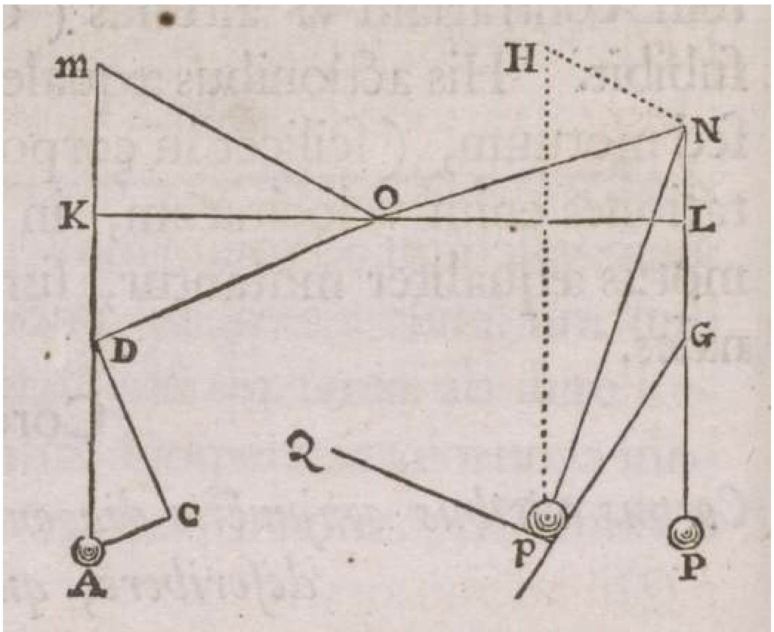
3.8 Two case studies

I end this chapter with two detailed case studies of readers not understanding proofs, each of which displays a combination of the textual characteristics discussed so far. The first is Flamsteed's failure to successfully process the proof of Corollary 2 to Law 3, and the second concerns the derivation of Proposition 9 of Book 1, which was misunderstood by both Huygens and Gregory. The distinction between the two cases is that Flamsteed was aware of his misunderstanding while Huygens and Gregory were not; what unites them – as I hope by now does not need to be emphasised – is that the failure of understanding was as much due to the qualities of the text as the competence of its readers.

3.8.1 Flamsteed gives up on Corollary 2 to Law 3

As was shown in Chapter 1, the marginalia in Flamsteed's first edition are far less copious than those in Fatio's, but they nevertheless enable a reasonably precise reconstruction of his reading of the *Principia*. I have suggested that Flamsteed initially set out to read the book in order from the beginning, but soon gave up and instead skipped ahead to the results that interested him about orbital forces in Section 2: I propose that Corollary 2 to Law 3 marks the exact point at which he capitulated. It is certainly the first result in the book that is sufficiently mathematically involved that a reader needs to pick up a pen to verify its claims, because the preceding Definitions and Laws contain no complicated geometry and no ratios to manipulate. In examining Flamsteed's difficulties, therefore, we should consider that Corollary 2 bears extra significance for any reader who reasonably imagines that this opening proof sets the tone for what lies ahead. We may speculate that Flamsteed intuited from his failure to understand this result that he was going to struggle with the rest of the book, and so chose not to continue.

Law 3 is Newton's assertion that to any action there is an equal and opposite reaction, and Corollary 1 is what the modern reader recognises as the parallelogram rule for the vector addition of forces. Corollary 2 concerns the decomposition of forces into components, and it is this with which Flamsteed had difficulty.[186] The physical set-up Newton considers is two weights hanging from cords, the other ends of which are attached to rigid spokes radiating from the centre of a wheel. He wishes to consider the conditions required for the hanging weights to balance, such that the spokes do not rotate about the centre of the wheel. Here is the diagram that accompanied the text in the first edition:



[186] The reader is invited in what follows to consult Cohen and Whitman's translation.

O is the centre of the wheel, and *OM* and *ON* are the rigid spokes. *MA* and *NP* are cords, to the ends of which are attached weights *A* and *P*. It is immediately noticeable that there are three ways in which this diagram is not as clear as it could be. First, *M* (upper case) is labelled *m* (lower case). This inconsequential slip was corrected in subsequent editions. Second, the lines *OK*, *OD*, *DC*, *AC* and *OL* are marked as solid lines, even though they do not represent physical objects. They are merely construction lines that will be used in the proof, yet they are not distinguished from the spokes *OM* and *ON* and the cords *MA* and *NP*. And third, the weight on the right appears in two separate positions (to be explained presently). The position on the far right is labelled with an upper case *P*, while the position to its left is marked with a lower case *p*; this distinction between upper case *P* and lower case *p* is exceptionally difficult to make out from the diagram. It will become clear that both features of the picture added to Flamsteed's confusion.

When considering the circumstances under which the hanging weights balance and the wheel does not rotate, Newton considers two cases. In the first case, the weight *P* hangs freely straight down, as shown on the far right of the diagram. The modern reader acquainted with the basic principles of moments will recognise that the weights balance if the weight of *A* multiplied by the distance *OK* is equal to the weight of *P* multiplied by the distance *OL*. Newton acknowledges that this is "a very well-known property of the balance, the lever, and the wheel and axle." Newton derives this result by constructing the point *D* on *MA* such that $OD = OL$, and then representing the weight of *A* by the length *AD*. He constructs the triangle *ACD* such that *ODC* and *ACD* are both right angles, and thence resolves the weight represented by *AD* into (what the modern reader would call) two components, represented by *AC* and *CD* respectively. *AC* has no turning effect about the centre since it acts parallel to *OD*, and so the overall turning effect is merely the turning effect of *CD*. But since $OD = OL$, the perpendicular distance of *CD* from *O* is the same as the perpendicular distance of *PN* from *O*. So *A* and *P* will balance if the weight of *P* is equal to the force represented by the length *CD*; that is (since the weight of *A* is represented by the length *AD*), if the ratio of the weight *P* to the weight *A* is equal to the ratio of the length *CD* to the length *AD*. By similar triangles, this is equal to the ratio of the length *OK* to the length *OD*; and since $OD = OL$ "the weights *A* and *P* are inversely as the spokes *OK* and *OL*," as required. Newton knows that this is not a new result: his purpose in deriving it here is to demonstrate the utility of decomposing forces into components (as he does when he resolves *AD* into *AC* and *CD*).

Flamsteed successfully followed the argument for this first case, as the annotations he left in his copy demonstrate:[187]

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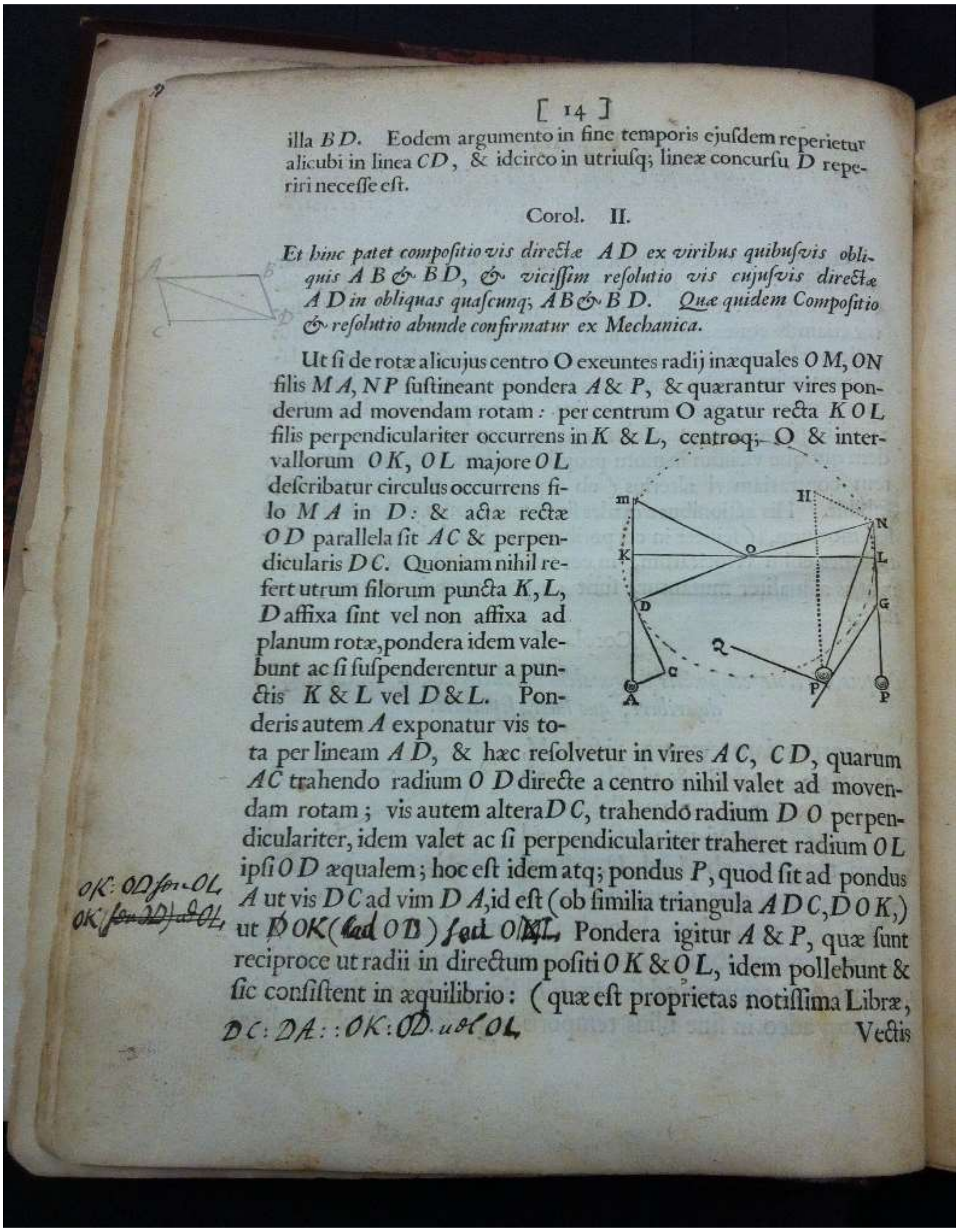
[187] RS, RCN 18577, 14.


[14]

illa *BD*. Eodem argumento in fine temporis ejusdem reperietur alicubi in linea *CD*, & idcirco in utriusq; lineæ concursu *D* reperiri necesse est.

Corol. II.

Et hinc patet compositio vis directæ AD ex viribus quibusvis obliquis AB & BD, & vicissim resolutio vis cujusvis directæ AD in obliquas quascunq; AB & BD. Quæ quidem Compositio & resolutio abunde confirmatur ex Mechanica.

Ut si de rotæ alicujus centro O exeuntes radij inæquales *OM*, *ON* filis *MA*, *NP* sustineant pondera *A* & *P*, & quærantur vires ponderum ad movendam rotam: per centrum O agatur recta *KOL* filis perpendiculariter occurrens in *K* & *L*, centroq; O & intervallorum *OK*, *OL* majore *OL* describatur circulus occurrens filo *MA* in *D*: & actæ rectæ *OD* parallela sit *AC* & perpendicularis *DC*. Quoniam nihil refert utrum filorum puncta *K*, *L*, *D* affixa sint vel non affixa ad planum rotæ, pondera idem valebunt ac si suspenderentur a punctis *K* & *L* vel *D* & *L*. Ponderis autem *A* exponatur vis tota per lineam *AD*, & hæc resolvetur in vires *AC*, *CD*, quarum *AC* trahendo radium *OD* directe a centro nihil valet ad movendam rotam; vis autem altera *DC*, trahendo radium *DO* perpendiculariter, idem valet ac si perpendiculariter traheret radium *OL* ipsi *OD* æqualem; hoc est idem atq; pondus *P*, quod sit ad pondus *A* ut vis *DC* ad vim *DA*, id est (ob similia triangula *ADC*, *DOK*,) ut OK (ad OD) sed OL. Pondera igitur *A* & *P*, quæ sunt reciproce ut radii in directum positi *OK* & *OL*, idem pollebunt & sic consistent in æquilibrio: (quæ est proprietas notissima Libræ, Vectis

OK : OD for OL

DC : DA :: OK : OD. vel OL

In understanding the construction, he has followed Newton’s instruction “describatur circulus occurrens filo *MA* in *D*” literally, and drawn the correct circle on to the diagram using a compass (the hole is visible in the paper at *O*). He has also left some notes regarding the manipulation of the ratios at the foot of the page: Flamsteed has transferred a correction listed on the *Errata* page into the text, and his understanding is demonstrated by his confirmatory statement “$DC : DA :: OK : OD$ vel OL” at the bottom.

The second case is more complicated. The reader is encouraged to consult the original text and read Cohen and Whitman's translation; in the process of explaining the proof as clearly as I can, it will become obvious where the possible causes of confusion lie. In the second case, Newton takes the weight *P* and – with the other end of the cord still attached to the point *N* – rests it on an oblique plane, which is labelled G in the diagram. The new position of the weight is labelled *p* (lower case), while the original position was labelled *P* (upper case). The weight has not changed, so while the position *p* is different from the position *P*, the weight *p* is the same as the weight *P* ("pondus *p* ponderi *P* aequale"). Newton wishes to consider – just as he did in the first case – the conditions required for this weight resting on the plane *G* to balance the weight *A*, which is still hanging from the point *M* as before. He is going to prove that they will balance if "the weight *p* is to the weight *A* in a ratio that is compounded of the inverse ratio of the least distances of their respective cords *AM* and *pN* from the centre of the wheel and the direct ratio of *pH* to *pN*." Even in translation, this is not a straightforward sentence to parse: Newton's verbal style is already causing difficulties.

Newton proves this as follows. Construct a line passing vertically through *p*. Then construct a line passing through *N* that is perpendicular to the plane *G*. Call the point where these two lines intersect *H*. This is shown on the diagram. But it is noticeable that at first glance *HN* looks perpendicular to *pN*, rather than *G*; notice also that the cord joins the weight to the point *N* and not the point *H*, and so the line *pH* does not represent any physical object. Newton's diagram is not very easy to use. Now let the downwards force of the weight be represented by the vertical line *pH*. By the parallelogram rule from Corollary 1, this can be resolved into two components *pN* and *HN* (which, the reader must remind themself, are not perpendicular).

Newton now establishes that these components *pN* and *HN* represent, respectively, the tension in the cord and the reaction force from the plane *G*. Newton does this by temporarily imagining into existence a second plane *Q* perpendicular to *pN*, abutting the plane *G* at the point *p*. (This is marked on the diagram: notice that at a casual glance *Q* looks parallel to *HN*, which it is not.) If the cord *pN* were then severed, the plane *Q* would help support the weight instead. Indeed, because *Q* is perpendicular to the cord, the support it provides (a modern reader would call it the normal reaction force) would act in the direction *pN* and be equal in size to the tension that had previously been in the cord. The normal reaction force provided by the other plane *G* would remain unchanged, acting in the direction *HN*. With the cord severed there would thus be three forces holding the body in equilibrium: the downward weight, the reaction force from *Q* acting parallel to *pN*, and the reaction force from *G* acting parallel to *HN*. Newton has already chosen to represent the downward weight by the length of the line *pH*, so the reaction forces must therefore be represented by the lengths of the lines *pN* and *HN* respectively. This means that the size of tension in the cord must also be represented by the length of the line *pN*.

This result seems intuitive enough. However, the reader must be careful to observe that the tension in the cord is represented by the oblique line *pN* only if the size of the downwards weight is represented by the line *pH*. In the first case discussed above, the size of the downwards weight had been represented by the line vertical *PN*. But these two weights are equal, because the two cases correspond to the same object in different positions. Therefore Newton is correct to write that “the tension of this oblique cord will be to the tension of the other, perpendicular cord *PN* as *pN* to *pH*” (“tensio fili huius obliqui erit ad tensionem fili alterius perpendicularis *PN*, ut *pN* ad *pH*”).

Newton is now able to derive the conditions under which the weight lying on the plane *G* (with the temporary plane *Q* removed) will balance the weight hanging vertically at *A*. In modern terms, the anticlockwise turning effect on the rigid spoke *OM* is equal to the weight of *A* multiplied by the distance *OK*. The clockwise turning effect on the rigid spoke *ON* is equal to the tension in the cord *pN* multiplied by the perpendicular distance from *O* to the line *pN* (which distance is not marked on Newton’s diagram). But it has just been established that the ratio of the tension in the oblique cord *pN* to the tension in the vertical cord *PN* is equal to the ratio of the length *pN* to the length *pH*. And the tension in the cord *PN* when hanging freely is (of course) equal to the weight of the body. So the two weights will balance if:

$$\begin{matrix}\text{tension in}\\ \text{oblique cord } pN\end{matrix} \times \begin{matrix}\text{perp. distance}\\ \text{from } O \text{ to } pN\end{matrix} = \text{weight of } A \times OK$$

$$\begin{matrix}\text{tension in}\\ \text{vertical cord } PN\end{matrix} \times \frac{pN}{pH} \times \begin{matrix}\text{perp. distance}\\ \text{from } O \text{ to } pN\end{matrix} = \text{weight of } A \times OK$$

$$\text{weight of } p \times \frac{pN}{pH} \times \begin{matrix}\text{perp. distance}\\ \text{from } O \text{ to } pN\end{matrix} = \text{weight of } A \times OK$$

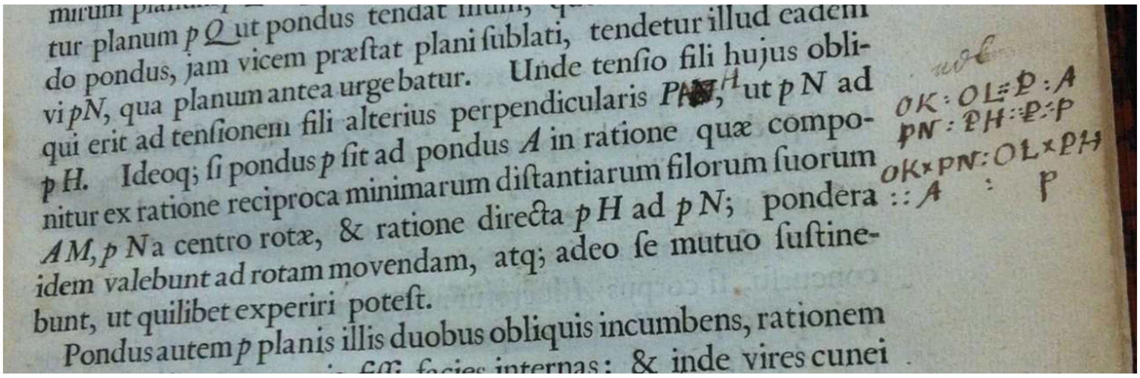
$$\frac{\text{weight of } p}{\text{weight of } A} = \frac{OK}{\text{perp. dist. from } O \text{ to } pN} \times \frac{pH}{pN}$$

When compared to the long sentence referred to above expressing Newton's statement of the result

> Therefore, if the weight *p* is to the weight *A* in a ratio that is compounded of the inverse ratio of the least distances of their respective cords *AM* and *pN* from the centre of the wheel and the direct ratio of *pH* to *pN*, the weights will have the same power to move the wheel and so will sustain each other, as anyone can test.

it will be seen that the required conclusion has been reached.

With this proof understood, Flamsteed's response can now be evaluated. He has left the following annotation in the margin next to the lines just quoted: [188]


mirum pla[...] ut pondus tendat illum, q[...]
tur planum *pQ* [...] tendetur illud eadem
do pondus, jam vicem præstat plani sublati,
vi *pN*, qua planum antea urgebatur. Unde tensio fili hujus obli-
qui erit ad tensionem fili alterius perpendicularis *PH*, ut *pN* ad
pH. Ideoq; si pondus *p* sit ad pondus *A* in ratione quæ compo-
nitur ex ratione reciproca minimarum distantiarum filorum suorum
AM, *pN* a centro rotæ, & ratione directa *pH* ad *pN*; pondera
idem valebunt ad rotam movendam, atq; adeo se mutuo sustine-
bunt, ut quilibet experiri potest.
Pondus autem *p* planis illis duobus obliquis incumbens, rationem
[...] facies internas: & inde vires cunei

OK : OL = P : A
pN : PH :: P : p
OK×PN : OL×PH
:: A : p

It is difficult to recreate Flamsteed's line of thought in its entirety, but this note suggests that he misunderstood two separate steps of Newton's proof.

First, it is notable that he has made an alteration in the main body of the text: Flamsteed has crossed out *PN*, and replaced it – after much uncertain dithering, it appears – with *PH*. Comparing this to the statement of the result quoted above, it is clear that Flamsteed is wrong to make this change. The ratio in question is that of the tension in the oblique cord *pN* to the tension in the

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[188] RS, RCN 18577, 15.

perpendicular cord *PN*, not to the cord *PH*. Indeed, there is no cord *PH*; the line *PH* is not even perpendicular. I think that Flamsteed means to change it to *pH*, not *PH*. The line *pH* is at least vertical, and the text is not helpful at distinguishing between (lower case) *p* and (upper case) *P*: the symbols look almost identical on the diagram, and throughout the proof *p* and *P* represent the same weight. And at first glance the sentence "the ratio of the tension of this oblique cord [*pN*] will be to the tension of the other perpendicular cord *pH* as *pN* to *pH*" looks plausible. But it is definitely incorrect, because it misunderstands which forces are represented by which lengths in the construction, and because there is no cord running along the vertical line *pH*.

Flamsteed's second mistake is revealed in the note in the margin, which can be cautiously transcribed as follows:

$$
\begin{aligned}
OK : OL &:: P : A \\
pN : PH &:: P : p \\
OK \times pN : OL \times PH &:: A : p
\end{aligned}
$$

My caution lies in the transcription of the *p*'s and *P*'s the second line, as will become apparent. In this note Flamsteed is trying to make sense of Newton's lines quoted above, and the basic structure is that he is trying to compound the first two ratios to produce the third. He needs to write something down because it is not immediately obvious what the "minimarum distantiarum filorum suorum *AM*, *pN* a centro rotae" refers to. Flamsteed understands that the first of these distances is *OK*, but he incorrectly thinks that the second is *OL*. That must surely be because, as discussed above, he is not alert to the difference between *pN* and *PN*. *OL* is the minimum distance between the centre and the cord *PN*, but the proof instead requires the minimum distance between the centre and the cord *pN* (and this distance, as we have previously observed, is not marked on the diagram). He therefore erroneously forms the ratio $OK : OL$. He immediately realises that he earlier proved this to be equal to the ratio of the weight *P* to the weight *A*, and so writes down that $OK : OL :: P : A$. This statement is true, but, because of his error with the perpendicular distance, is irrelevant to the matter in hand.

It may be imagined that it is only now that he realises that he has missed the word "reciproca," which means that he has written this ratio the wrong way round. To maintain consistency, he therefore writes "ratione directa *pH* ad *pN*" backwards as well. But this is exactly the ratio he misunderstood previously, and it is at this point that he realises that something has gone awry. I cannot make sense of what he has written in the second line, and his handwriting is difficult to read, but the first and third terms appear to have been altered. One possibility is that he began to write out the first three terms of $PN : PH : P : ...$ before he noticed the distinction between *p* and *P*, whereupon he changed the line to $pN : PH : P : p$. It is not clear why he did this, since there is no consistent reading within which $pN : PH : P : p$ makes any sense: the best I can do is suggest that Flamsteed thought that the line *PN* (which he erroneously writes as *pN*) represents the weight *P*, and the line *pH* (which he erroneously writes as *PH*, and which is not a physical object anyway) represents the weight *p*.

Having formed these two erroneous ratios from the text, he finally compounds them. The left-hand sides combine to produce $OK \times pN : OL \times PH$. According to Newton's text the composition should produce the ratio of *p* to *A*, but because he forgot the "reciproca", Flamsteed writes *A* underneath the first and p under the second. The right-hand sides, however, do not combine correctly. There is no way that $P : A$ can compound with either $P : p$ or $p : p$ to give $A : p$. Flamsteed knows that he has not understood the proof. He is aware that he has got lost somewhere along the way. He knows that he has failed, and he takes this as a sign that the rest of the book is beyond him too.

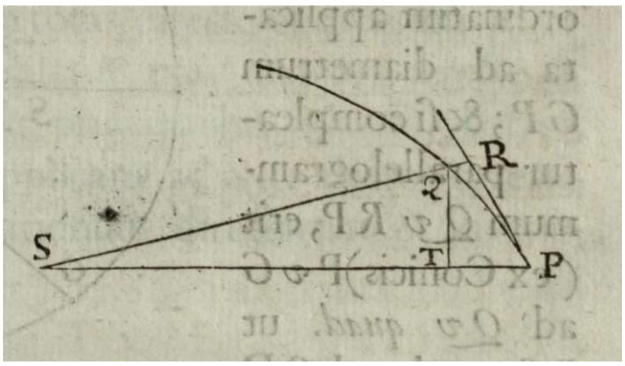
This failure is just as much the author's failure of persuasion as it is the reader's failure of understanding. On the one hand, the proof contains no novel mathematical techniques, and there are no conceptual difficulties to prevent the modern reader getting his head around it. Newton might reasonably have expected the then Astronomer Royal to have been able to do the same. On the other, I have already highlighted the many ways in which the printed demonstration does not make things simple for the reader. The diagram is unhelpful: the difference between *p* and *P* is very difficult to make out; the construction of *HN* perpendicular to *G* and *Q* perpendicular to *pN* is unclear; the perpendicular distance between *O* and *pN* is not marked; and there is no distinction between lines that represent physical cords, spokes and planes (*AM*, *PN*, *pN*, *MO*, *ON*, *Q*, *G*) and those that are merely construction lines (*pH*, *HN*, *OK*, *OL* etc.). Newton's verbal style, too, hindered Flamsteed's understanding: his errors in the final lines directly arose from the process of translating the words of the text into sets of ratios. If Newton had supplied the ratios directly, Flamsteed would have successfully made it to the end of the proof. This marginal note provides a concrete example of a case when Clerke's suggestion that the ratios be set out on the page underneath each other would have facilitated understanding. And we can also consider the effect of making corrections to the text. Recall that in the simpler first case of the Corollary, the printed text contained a slip that the *Errata* page instructed Flamsteed to rectify. This means that when he incorrectly – and fatally – changed *PN* to *PH* on page 15, he had only moments before correctly changed *DO* to *OK* on page 14. He had been primed by the *Errata* to be alert to the possibility that there were mistakes in the proofs, and this caused him to lack confidence in the printed text when he had no cause to do so. It is too strong to say that published corrections undermined readers' confidence in the overall validity of the proofs, but the existence of the *Errata* seems to have made Flamsteed sensitive to the possibility that there might be small mistakes in the text along the way. There was doubt in his mind as he read.

I have argued above that these marginalia document the moment Flamsteed gave up on reading all the way through the *Principia*. That is why they deserve the close attention I have given them: they are a record of the moment when a plausibly competent reader decided to give up working his way through the text. When we say that Newton's contemporaries found his book difficult to read, this primary source reveals very precisely what that difficulty entailed, and it is clear that the characteristics of the text were just as responsible for its impenetrability as the competence of its readers.

3.8.2 Huygens and Gregory both misunderstand Proposition 9

I end with an unusual example, in which the difficulty of the *Principia* is manifested not in readers' failure to understand the text, but in their misunderstanding of it. Both Gregory and Huygens mis-read Proposition 9 of Book 1, and although each thought they understood the proof and had successfully arrived at the desired conclusion, their notes clearly reveal that they had not correctly followed Newton's arguments. This example is significant because, first, Gregory and Huygens cannot be dismissed as lacking expertise, and so it further emphasises the extent to which Newton was responsible for the difficulties his readers encountered. And second, this example encourages critical examination of the vocabulary of “understanding” and “mastery” in the context of mathematical proofs. It will become clear that both Huygens and Gregory thought they had understood the text, when they had not; they were persuaded of the validity of Newton’s conclusions, but not for the reasons he gave. This significantly complicates the wider discussion of the means by which Newton’s book was approved. Even when his most competent peers carefully studied the demonstrations, they sometimes gave their assent to the results of the *Principia* for what Newton would have said were the wrong reasons.

Proposition 9 demonstrates that for a body orbiting in a logarithmic spiral under the action of a centripetal force acting towards the centre of the spiral, that force must be inversely proportional to the cube of the distance of the body from the centre. Because Newton’s proof is short, and because its brevity contributes to Huygens and Gregory’s misunderstanding, it is worth providing its diagram and translation in full:[189]



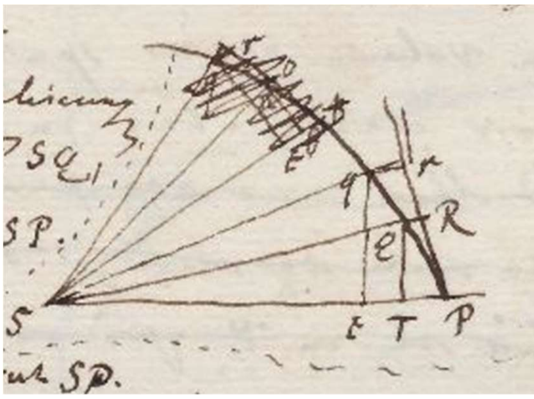
[189] This translation is based on Cohen and Whitman’s, with some changes to reflect the fact that they were translating the third edition rather than the first.

Let the indefinitely small angle *PSQ* be given, and because all the angles are given, the species of the figure *SPRQT* will be given. Therefore, the ratio QT/QR is given, and QT^2/QR is as *QT*, that is as *SP*. Now change the angle *PSQ* in any way, and the straight line *QR* subtending the angle of contact *QPR* will be changed (by Lemma 11) as the square of *PR* or *QT*. Therefore QT^2/QR will remain the same as before, that is as *SP*. And therefore $QT^2 \times SP^2/QR$ is as SP^3, that is (by the Corollary to Theorem 5) the centripetal force is inversely as the cube of the distance *SP*. Q.E.I.

Newton's argument is intended to be understood as follows. A logarithmic spiral is given around its centre *S*. A defining property of logarithmic spirals is that they are self-similar, which is to say that as the spiral is traced out from the centre, each successive section of the curve is an enlargement of the previous section. As a consequence, for any given logarithmic spiral the angle *RPS* between the tangent *RP* and the radial line *SP* is fixed, and is independent of the location of *P* on the spiral. This is reflected in the opening statement of the result (not quoted above), "Gyretur corpus in spirali *PQS* secante radios omnes *SP*, *SQ*, &c. in angulo dato."

It is critically important to Newton's argument that there are two distinct steps in his proof. In the first step, the figure *SPQRT* is constructed for a fixed, very small angle *PSQ* ("Detur angulus indefinite parvus *PSQ*"). Because of the self-similarity property, this construction will (for a given angle *PSQ*) always be the same shape irrespective of where *P* lies on the curve. That is, if the angle *PSQ* is kept constant but the point *P* is free to slide along the spiral, the figure *SPQRT* will retain the same shape but contract and expand as *P* moves in and out along the curve. The ratio QT/QR will therefore be a fixed value, independent of the location of P. The ratio QT^2/QR will therefore be proportional to *QT*; and *QT* is in turn proportional to *SP*. Therefore, for a given angle *PSQ*, QT^2/QR will always be proportional to *SP*, irrespective of where *P* lies on the spiral. In the second step, Newton considers what will happen when *P* is held fixed but the angle *PSQ* is changed ("Mutetur jam utcunq; angulus *PSQ*"). Specifically, Newton considers what will happen in the limit as *Q* slides along the curve towards *P*. Lemma 11 shows that, as the angle *PSQ* decreases, *QR* will be proportional to PR^2. Because the angle *RPS* is fixed, *QT* is proportional to *PR*, which means that *QR* will be proportional to QT^2. As *Q* slides along the curve towards *P* the ratio QT^2/QR will therefore not change. That is, QT^2/QR will remain proportional to *SP*. Therefore, in the limit as *Q* approaches *P*, $QT^2 \times SP^2/QR$ will be proportional to SP^3; and so by Proposition 6 the centripetal force is inversely proportional to the cube of the distance, as required. To repeat the important idea: in the first step of the proof Newton fixes the angle *PSQ* and shows that, no matter where *P* lies on the curve, QT^2/QR is proportional to *SP*; and in the second step Newton fixes the position of *P* but allows angle *PSQ* to diminish, and shows that in the limit as *Q* moves towards *P* the ratio QT^2/QR does not change. The result then follows easily.

Both Huygens and Gregory misunderstand the proof in very similar ways, because neither of them acknowledges the importance of these two distinct steps. Here are Huygens' notes on this passage, in full, followed by my translation:[190]



Ad Prop. 9. I. 1. Spiralis haec semper appropinquat puncto *S*, circumvolutionibus infinitis numero, nec unquam ad ipsum pervenit. ac tamen longitudinem certam non excedit.

Demonstratio perobscura est, in qua cum dicit, mutetur jam utcunque angulus *PSQ* &c, hoc tantum propositum habet ut ostendat qualiscunque et ubicunque accipiatur angulus *PSQ*, semper QT^2/QR esse ut *SP*. velut si accipiatur angulus major *qSP*, erit hic quoque qt^2/qr ut *SP*. Nam quia QT^2/QR ut *SP*, estque qt^2 ad QT^2 ut *qr* ad *QR*, erit necessario et qt^2/qr ut QT^2/QR hoc est ut *SP*. Itaque cum semper sit QT^2/QR ut *SP*, ducto utroque in SP^2 erit $QT^2.SP^2/QR$ ut SP^3. ideoque per propos. 6, vis centripeta ut SP^3 inversè.

Poterat autem eundem angulum *PSQ* velut in duobus locis adsumtum considerasse, et utrobique similiter ductas *PR*, *QT*, *QR*. quae figurae proportionales fuissent. Et quia vis centrifuga ut $QT^2.SP^2/QR$ inversè per 6. hoc autem ut SP^3, quia *QT* ut *SP*, et ita quoque *QR*: erit et vis centripeta ut SP^3 inverse.

[190] Codices Hugeniani, HUG 7, 15v (=*OC*, 21:418–19).

Demonstravi hanc eandem propositionem, ut propositione sexta nihil opus esset.

Proposition 9, line 1. This spiral always approaches the point *S*, with turns infinite in number, and does not ever reach itself. And nevertheless it does not exceed a certain length.

The proof is very obscure, in the part where he says “mutetur jam utcunque angulus *PSQ* &c”; the entire proposition aims to show that whatever and wherever the angle *PSQ* is taken, QT^2/QR is always as *SP*. Just as if a bigger angle *qSP* were to be taken, this qt^2/qr will also be as *SP*. For because QT^2/QR is as *SP*, and qt^2 to QT^2 is as *qr* to *QR*, qt^2/qr will also necessarily be as QT^2/QR, that is as *SP*. And so since QT^2/QR is always as *SP*, with both multiplied by SP^2, $QT^2 \times SP^2/QR$ will be as SP^3. Therefore by Proposition 6, the centripetal force will be inversely as SP^3.

But it was possible to have considered the same angle *PSQ* taken as if in two places, and *PR*, *QT* and *QR* similarly drawn on both. These figures would have been similar. And because the centripetal force is inversely as $QT^2 \times SP^2/QR$ by [Proposition] 6, this is as SP^3, because *QT* is as *SP*, and so also is *QR*; and the centripetal force will be inversely as SP^3.

I have demonstrated this same proposition, such that there is no need for the sixth proposition.

It is noticable that Huygens – not for the only time, as was shown in Chapter 1 – openly declares his difficulty with the proof (“Demonstratio perobscura est”). I also argued in Chapter 1 that there is no evidence that Huygens ever made a detailed study of the method of first and last ratios in Section 1 of Book 1. This will be developed further in Chapter 4, where it will become clear that Huygens’ habit when reading the *Principia* was always to re-interpret Newton's geometrical limit arguments in terms of infinitesimals. I also demonstrated in Chapter 2 that he generally reconstructed Newton's proofs in terms of Euclidean proportions, and so preferred to consider the ratios of quantities evaluated at two different points, rather than (as Newton did) an expression for the quantity at one point. These considerations should be borne in mind when examining his notes: Huygens ends up deriving the required quantity SP^3, but he does so fallaciously, because he does not appreciate the importance of the order of the two steps in Newton's argument.

His opening line confirms that he knows the geometrical features of a logarithmic spiral, and he does not challenge the assertion in the second sentence of Newton's proof that QT^2/QR is proportional to *SP*. Newton, of course, makes this claim in the first step of his argument, when the angle is fixed and *P* is imagined to be sliding up and down the line. But Huygens does not acknowledge this distinction and immediately, without appearing to realise it, slips into step two and applies Lemma 11 as instructed. He does so, as we might expect, by considering a second point q on the curve further away from *Q* than *P*, and noting that the ratio of qt^2 to QT^2 is as *qr* to *QR*. He thus concludes that QT^2/QR remains proportional to *SP* as angle *PSQ* changes, and so $QT^2 \times SP^2/QR$ will be proportional to SP^3, as required. But it is not at all clear that Huygens is comfortable with this. There is no evidence that he ever closely examined Lemma 11, and no acknowledgement that this result only holds in the limit when the angle is small. Indeed, if we take "qualiscunque et ubicunque accipiatur angulus *PSQ*" at face value, what Huygens writes here is false: the ratio does not hold for large values of the angle *PSQ*.

If his second paragraph hints at his uncertainty, his misunderstanding is fully revealed by his third, in which he returns to consider the first step of Newton's proof. He correctly understands the self-similarity of the construction as *P* moves around the curve, but the incriminating line is that in which he writes that $QT^2 \times SP^2 / QR$ is proportional to SP^3, "quia *QT* ut *SP*, et ita quoque *QR*". Replacing both *QT* and *QR* with *SP* in the ratio for the centripetal force does indeed generate SP^3. It is also true that both *QT* and *QR* are proportional to *SP* as *P* moves but with the angle *PSQ* held constant (step one). But *QT* and *QR* are definitely not proportional to *SP* as the angle *PSQ* diminishes with *P* held fixed (step two). In that circumstance, it is only the ratio QT^2 / QR that remains proportional to *SP*; the individual lengths *QT* and *QR* do not. It is therefore wrong merely to substitute *SP* for both *QT* and *QR* into $QT^2 \times SP^2 / QR$ to produce SP^3, tempting though that short-cut is. Huygens' willingness to make this substitution reveals that he has incorrectly understood the logical structure of the proof. He has not acknowledged that it must first be shown that (as *P* moves along the curve, with angle *PSQ* fixed) QT^2 / QR is proportional to *SP*, and then that (in the limit as *Q* moves towards *P*, with *P* held fixed) this ratio QT^2 / QR remains constant.

It would be easy to dismiss Huygens' error as a small and unremarkable slip, were it not for the fact that Gregory misunderstands Newton's proof in a very similar way. Unlike Huygens, Gregory gives no indication that he finds the proof difficult to follow, and thinks he has correctly followed Newton's argument.

There are five entries on Proposition 9 in the *Notae*, the first four of which are straightforward.[191] Gregory first acknowledges the properties of the logarithmic spiral; then establishes that all the angles in the construction are determined once angle *PSQ* is given; then confirms that this fixes the ratio QT/QR; and then infers that as the position of *P* and *Q* varies with angle *PSQ* held fixed, this ratio QT/QR is proportional to *SP*. In terms of the vocabulary we have been using, this takes us to the end of step one. However, he then adds a fifth and final comment:

> Facilius hoc ita invenietur, vis centripeta est ut $QTq \times SPq/QR$ reciproce, quare est etiam ut quodvis huic proportionale reciproce. Sed QTq/RQ est ut *SP* quare $QTq \times SPq/QR$ est ut *SP* cub: quare vis centripeta est ut *SP* cub reciproce. Videtur vocem reciproce hic omitti; et a Mutetur ad Quare, demonstrationi non condit.
>
> It will be found more easily in this way: the centripetal force is reciprocally as $QT^2 \times SP^2/QR$, hence it is also as anything that is reciprocal to this proportion. But QT^2/RQ is as *SP*, hence $QT^2 \times SP^2/QR$ is as SP^3; hence the centripetal force is inversely as SP^3. It seems that the word "reciprocally" is here omitted; and from "Mutetur" to "Quare" does not add to the demonstration.

This note clearly reveals that Gregory has misunderstood how Newton intends his proof to work: he has not realised the importance of the second step to the integrity of the demonstration. Since the centripetal force is inversely proportional to $QT^2 \times SP^2/QR$, Gregory argues, and because we have just deduced that QT^2/RQ will be proportional to *SP*, we can make a simple

[191] RS, MS210, 4r.

substitution to conclude that the centripetal force is inversely proportional to SP^3. According to this reading, the two sentences "from 'Mutetur' to 'Quare'" (which is to say, exactly the lines that correspond to the second step) are redundant, and there is no need to invoke Lemma 11. But this is wrong. Gregory has, like Huygens, not distinguished between the two steps of the proof. When in his third note he recorded that the ratio QT/QR is constant, Gregory failed to acknowledge the essential qualification that this is only true during step one, while the angle *PSQ* is held fixed and *P* moves. In step two, as *P* is held fixed but the angle *PSQ* is reduced, the ratio QT/QR does not remain constant. And since the expression for the centripetal force from Proposition 6 only applies in the limit as Q moves towards *P*, the angle *PSQ* must be made indefinitely small. In this circumstance the ratio QT/QR changes; yet Newton correctly points out that (by Lemma 11) the ratio QT^2/QR does not. That is why he can conclude that the centripetal force is inversely proportional to SP^3. Far from the sentences from 'Mutetur' to 'Quare' adding nothing, they are essential to the logic of his demonstration.

Both Huygens and Gregory therefore misunderstood Newton's proof of Proposition 9, and did so for very similar reasons. Their notes very clearly show that neither of them correctly followed the argument printed on the page. And yet they both agreed that its outcome was correct. They agreed with the result, despite having misunderstood the argument presented in its support. They assented to the validity of the conclusion, but Newton would say that they did so for the wrong reasons. This significantly complicates our attempt to establish the means by which the claims of the *Principia* were verified: how we should categorise two readers, who cannot reasonably be said to lack expertise, and who both determined that this result was correct, but who did so fallaciously, is not at all obvious. But let it finally be noted that Huygens has left one further clue that deserves to be pursued. Although he acknowledges that he found Newton's proof difficult, he explains in the final sentence of his note that "Demonstravi hanc eandem propositionem, ut propositione sexta nihil opus esset." In other words, while Huygens did successfully manipulate Newton's ratios to attain the desired expression, it was not this that persuaded him to agree that the result was correct. He agreed that the result was correct because he had produced his own proof, separate from Newton's. I have not been able to locate this demonstration, but we may reasonably assume that it adopted Euclidean proportions, neglected the method of first and last ratios, and quite possibly employed the methodology of infinite series, exactly as his other notes do. Huygens, it appears, had previously proved the result of Proposition 9 using his own notation, terminology and conceptual framework. It was his own independent reconstruction of the proof that persuaded him of this result's validity: these readers' assent to the claims in the *Principia* was not founded on their acceptance of the arguments they found printed in the text.

4. Responses to the new mathematics in the *Principia*

4.1 Introduction

Over the previous chapters I have demonstrated that, on the occasions that his peers engaged with his mathematical proofs, reading comprised an active process of recreation. Readers verified Newton's arguments by reconstructing them using different notation, commonly within a different conceptual framework, and often via a different logical path. They granted assent to his claims according to whether they were successfully able to reconstruct his arguments in their own terms. In addition, I argued in Chapter 3 that the *Principia* was difficult to understand because the format, style and composition of the text often obstructed readers' ability to successfully reconstruct its arguments, which is another way of saying that Newton's book was very ineffective at persuading his readers. This was the case even for those proofs based on uncontentious, established mathematical techniques. My aim in this chapter is to show that this also applied to the demonstrations involving the novel mathematical methods contained within the *Principia* – which is to say, the geometrical limit and proto-calculus techniques that Newton deployed in many of his proofs.

Newton employed a variety of infinitesimal and limit methods in the *Principia*. He laid out the foundations for his method of first and last ratios explicitly in Section 1 of Book 1, yet geometrical limit and proto-calculus techniques appear throughout the book, in a range of formulations and a variety of styles of proof. In this chapter I will first consider readers' responses to the limit methods they encountered in the main body of the work, before then examining how they engaged with the proofs in Section 1. It will quickly become clear that it is not helpful to attempt to establish a binary distinction between peers who “understood” and “mastered” Newton’s proofs and those who did not. It was often the case that both understanding and mastery were partial. Flamsteed engaged with Newton’s novel methodology only very superficially, and Halley very uneasily. Gregory and Fatio were generally successful in assimilating his techniques, but both Huygens and Leibniz reinterpreted his geometrical proofs in terms of infinitesimals and infinite series. In Huygens’ case, this was because – like Flamsteed – he didn’t make a meaningful attempt to engage with the details of the proofs in Section 1. Leibniz – like Clerke – appears to have tried, but was obstructed by the poor wording and opaque logical structure of Newton’s limit definition in Lemma 1. Halley, Fatio and Gregory all appear to have intuited the general meaning of the foundational proofs, but sought clarification of key details. Importantly, there is no evidence that any of Newton’s readers recognised the need for the unusual style of proof Newton deployed at the heart of his method of first and last ratios in Lemmas 6, 7, 8 and 9. Fatio and Gregory in particular could not see the value of the “microscope” argument Newton employed in these four results, and urged that they all be re-written. Gregory – either during his initial reading or after consultation with Newton – also perceived a flaw in the application of Section 1 to Proposition 6, critically undermining the logical validity of all the orbital force theorems at the start of the book, including the proof of the inverse-square law.

In other words, even when Newton’s readers understood the foundational basis of his methodology, and even when they agreed with his conclusions, they thought he had justified his claims in the wrong way. Their responses show that there were no established criteria for what constituted an acceptable limit-based argument at the time of publication. They objected to both the articulation of his underlying concepts and the formulation of his demonstrations, encouraging him to re-phrase, re-write, and re-structure his proofs. In short, contemporary readers were not persuaded by the geometrical limit arguments they found printed in the text: Newton had to negotiate the validity of the mathematical methodology of the *Principia* with his readers on a case-by-case basis.

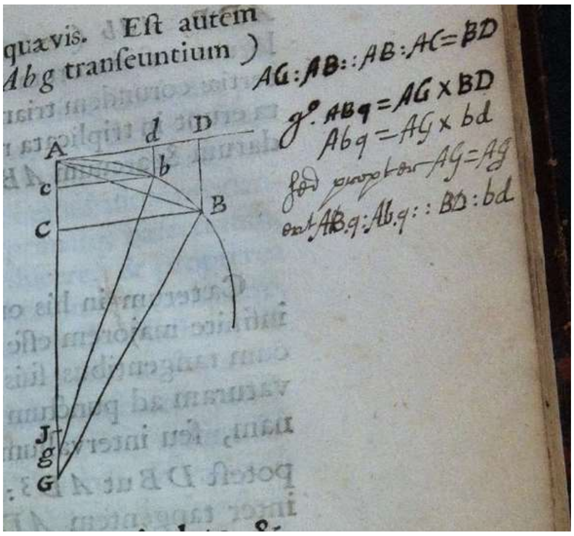
4.2 General responses to Newton's geometrical limit methods

As outlined above, the primary evidence reveals a variety of responses to the limit methods Newton used in the main body of the book. These ranged from a cursory glance at the details of his arguments, all the way up to a full assimilation of Newton's methodology. We will consider this spectrum of responses in turn, starting at one end and moving towards the other.

4.2.1 Flamsteed's superficial engagement

Among the range of possible responses to the *Principia*'s limit methods, one was not to engage with them at all. I demonstrated in Chapter 1 that most readers engaged with the book in discontinuous, piecemeal chunks, and argued in Chapter 2 that its intimidating length encouraged readers to leave large sections of the text unexamined. Locke avoided all the mathematical content of the book, as is well known. I will show below that Huygens also made no sustained attempt to assimilate Newton's geometrical limit methods. However, the clearest example of a reader who engaged with Newton's limit arguments only to a superficial degree is Flamsteed. I have argued previously that

Flamsteed set out to work through the opening sections in order, but got stuck on Corollary 2 of Law 3, whereupon he skipped ahead to the orbital force theorems in Sections 2 and 3. His disengagement with Newton's limit arguments is consistent with this scheme. He left only one marginal note in Section 1, against Lemma 11:[192]



Lemma 11 proves that, given a fixed curve, "the vanishing subtense of the angle of contact is ultimately in the squared ratio of the subtense of the conterminous arc." That is, with reference to the diagram above, in the limit as the point *B* moves along the curve towards the point *A*, *BD* is proportional to AB^2. It is the only result in Section 1 whose proof requires any manipulation of ratios, and so it is possible that Flamsteed cursorily read through the whole section but only needed to make written notes on this passage. I suggest, however, that he was instead sent back to Lemma 11 when he found it invoked by Proposition 4, which his marginalia show he also studied.

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[192] RS, RCN 18577, 33.

Either way, this annotation reveals that Flamsteed engaged with Newton's limit arguments only superficially. As the reader will confirm if they consult the original text, in these five lines Flamsteed manipulates the ratios in the proof to verify the final proportion. He checks that the terms combine to produce the required result that *BD* is proportional AB^2. In one sense, therefore, it could be argued that Flamsteed has confirmed the validity of the conclusion of Lemma 11. But it cannot reasonably be claimed that he gave his considered assent to the broader methodology of first and last ratios. There is no evidence that he engaged with Newton's definition of a limit in Lemma 1, for example, nor has he commented on the discussion that follows in the Scholium. He has not asked any of the questions his peers raised – as we shall see below – when examining the other proofs in this section. He has computed the ratios in Lemma 11 but not interrogated their foundational basis. Logically his reading of this proof may have been complete, but conceptually it was not. Indeed, I have previously noted that Flamsteed appears not to have studied Proposition 6, which is an essential component of the centripetal force proofs in Sections 2 and 3. This is consistent with the general pattern that he verified some aspects of some of the proofs in the logical chain of deduction. He superficially verified a selection of Newton's ratios without engaging with the underlying methodology: Flamsteed's assent to the geometrical limit methods in the *Principia* was partial.

4.2.2 Halley's latent unease

Flamsteed did at least satisfy himself that the result of Lemma 11 was correct, and proceeded to the orbit theorems convinced in his own mind of its validity. It is not at all clear that that is also true of Halley, whose ambiguous response to Newton's limit methods is revealed in his editorial notes. Neither the quantity nor the content of the documentary evidence is conclusive, but I suggest that the changes he proposed to passages involving limit methods reveal a latent unease in their rigour. It is probably too strong to claim that Halley felt that the method of first and last ratios was unreliable, but at the very least he felt the need to negotiate with Newton over what constituted an acceptable limit-based argument.

As outlined in Chapter 2, the majority of Halley's editorial comments comprised either grammatical or typographic corrections, or small tweaks to Newton's phrasing. Not many of his suggested alterations substantively changed the meaning of the text. However, it is noticeable that Halley re-worded and reformulated Newton's limit arguments disproportionately frequently. Some of his adjustments are very small. When reading a now-lost draft of Proposition 3 of Book 2, for example, which concerns a body falling against a resistance proportional to its velocity, he appears to have advised changing the words "descensus initio" ("at the beginning of the fall") to "ipso descensus initio" ("at the very beginning of the fall"), an alteration which helps focus the reader's attention on the moment at the start of the limit process, and the first ratio at the instantaneous beginning of the descent. Perhaps for similar reasons, in a geometrical argument about a converging arc and line in Proposition 34 of Book 1, he suggested adding either "donec tandem" ("until eventually") or "usque" ("all the way", "continuously"). In Proposition 1 of Book 1, he suggested replacing "temporis momentis" ("moment of time") with "temporis particulis" ("particle of time"). In Proposition 2 of Book 2, in a passage about infinitesimal subdivisions, he suggested improving "Minuantur jam aequales illae temporum particulae, & augeatur earum numerus infinitum" ("Now let those equal particles of time be diminished, and let their number increase indefinitely") by inserting either "augeatur terminorum numerus" ("let the number of boundaries increase") or "augeatur spatiorum numerus" ("let the number of spaces increase"). In the discussion of first and last ratios in the Scholium at the end of Section 1, he suggested adding the word "revera" ("in fact", "actually") to the sentence "Rationes illae ultimae si rigide loqamur non sunt rationes quantitatum ultimarum sed limites" ("Those ultimate ratios, if we speak strictly, are not ratios of ultimate quantities but limits"), and Newton was encouraged to change these lines yet further before publication. And he did not realise that the proof of Proposition 8 in Book 1 only holds in the limit, prompting Newton to insert a note to this effect. From one point of view, these are all insubstantial changes. None of them amounts to a dispute about the validity of Newton's arguments. But taken together they indicate that the vocabulary and syntax of limit proofs was yet to be codified. They are evidence that the standards by which limit arguments were to be judged, the terminology they should employ, and the assumptions that an author could expect in his reader, were not fixed. At the very least, Halley in all of these instances tried to help Newton's readers follow proofs with whose novel methodology he suspected they would be unfamiliar.[193]

In two instances, however, Halley's alterations were much more significant. One concerns an adjustment to Lemma 1, and is discussed below. The other relates to a passage in Lemma 2 of Book 2, and both Halley's proposed alteration and Newton's response reveal a latent unease regarding the logical status of the limit arguments. Lemma 2 contains an important discussion of the

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[193] CUL, MS Add. 3965, 96–97.

nature of infinitesimal moments, and is one of a handful of sections of the *Principia* which explicitly address the foundations of Newton's limit methods. The draft manuscript Halley saw is no longer extnat, but a translation of the corresponding passage in the published first edition reads as follows:

> I here consider these quantities as indeterminate and variable, and increasing or decreasing as if by a continual motion of flux; and it is their momentary increments or decrements that I mean by the name 'moments', in such a way that increments are considered as added or positive moments, and decrements as subtracted or negative moments. But take care that you do not understand them to be finite particles. Moments, as soon as they are of finite magnitude, cease to be moments. They must be understood to be the just-now nascent beginnings of finite magnitudes. For in this lemma the magnitude of moments is not regarded, but only their first proportion when nascent. It comes to the same thing if in place of moments there are used either the velocities of increments and decrements (which it is also possible to call motions, mutations, and fluxions of quantities) or any finite quantities proportional to these velocities.

When Halley read the draft version of this passage (which, to repeat, may not have been identical to this text), he advised inserting the following, immediately after "But take care that you do not understand them to be finite particles" ("Cave tamen intellexeris particulas finitas"):

> Finiri enim repugnat aliqua tenus perpetuo earum incremento, aut decremento[194]

> For being finite is somewhat incompatible with their continual increment and decrement

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[194] Ibid., 96r.

These words do not reflect well on Halley's perception of the persuasiveness of the Newton's proofs. If the modern reader judges the original phrasing of Lemma 2 to be less than fully rigorous, then Halley's proposed addition strongly indicates misgivings with the underlying methodology. Even more disconcertingly, when he received this suggested edit from Halley, Newton agreed to include it. He inserted the words in a slightly different place (after "desinunt esse momenta" rather than "finitas"), and because the manuscript Halley read has been lost, it is not clear how much redrafting this passage underwent, but they appeared in full in the final publication. It is difficult to imagine how any reader would find the vague wording of this phrase would do anything other than weaken trust in the robustness of the concepts to which it refers. Furthermore, this addition calls into question the confidence Halley placed in the Scholium before he proposed it. If he had not had doubts about the validity of the passage as it stood, he would not have suggested inserting this sentence; and if Newton had been more confident, he would never have accepted. As it happens, Newton did replace the sentence in the second edition of 1713, along with "Momenta… desinunt esse momenta." But their later removal only serves to emphasise the point, which is that their inclusion in 1687 is admission of unease. Halley's suggested change to Lemma 2 provides a clear indication of his uncertainty about the logical characteristics of Newton's concept of a geometrical limit.

The importance of these alterations must not be over-stated. Taken together, Halley's comments on Newton's limit results count fewer than a dozen in a five-hundred-page book. His notes on f.97 of MS Add. 3965 show that Halley carefully studied the Lemmas that subsequently became Section 1, and (on paper, at least) did not register any objections either to individual results or the validity of its foundational techniques. Halley inspected Newton's arguments, and authorised their publication. The few comments he made advised small changes of phrasing. Nevertheless, it is legitimate to examine the circumstances of Halley's acceptance very closely. Sharing Newton's intuition that his conclusions are correct is not the same as being confident that his arguments are sound, and in this context fine adjustments of terminology are significant. Any tinkering with Newton's articulation of his foundational assumptions is of considerable importance. The distinction between a "moment" of time and a "particle" of time is significant if there is any doubt that either might be ill-defined. At the very least, Halley's view that a limit argument could be improved by the words "donec tandem", and that "ipso descensus initio" was more persuasive than "descensus initio," shows that the criteria by which such demonstrations were to be judged were, at the time of his reading, not agreed upon. In other words, these edits provide concrete evidence of a negotiation between Halley and Newton over what constituted an acceptable proof. And no editor confidently secure in the logical status of the method of first and last ratios would write the sentence "Finiri enim repugnat aliqua tenus perpetuo earum incremento, aut decrement," any more than an author capable of robustly defending his methods would accept this insertion. So while Halley shared Newton's intuitive grasp of his limit-based techniques and did not seek to challenge the correctness of his conclusions, his editorial notes give us good cause to question the extent of his approval: the primary evidence suggests that Halley's assent to the limit methods in the *Principia* was qualified.

4.2.3 Leibniz and Huygens' adherence to infinitesimals

I drew attention in the Introduction to the theme in the secondary literature that the decades following publication saw a programme of “translation” of the *Principia*’s geometrical limit-based demonstrations into proofs founded on infinitesimals. This programme both drove and was driven by the rivalry between Newton and Leibniz, and so was simultaneously ideological and sociological in origin.[195] But in this section I shall argue that it was also a function of how the book was read. The primary evidence shows that some of Newton’s early readers processed his arguments not in the geometrical limit format in which they were presented, but in terms of the infinitesimal framework to which they were accustomed. This was not a retrospective exercise readers embarked upon after they had already approved the text; they did not first read the book and then translate its proofs into infinitesimals. Rather, they processed Newton’s arguments by reconstructing them in their own terms. They judged the validity of the *Principia*’s conclusions by whether they could prove them using infinitesimals. They read the book from within a different conceptual framework in which it was written.

Two particularly important such readers were Leibniz and Huygens. Leibniz’s response to the mathematical methodology of the *Principia* is one of the themes of Bertoloni Meli’s studies, and he has shown that Leibniz paid detailed attention Newton’s geometrical limit proofs. In the *Notes* and *Marginalia* he made in 1688, Leibniz commented on almost all of the proofs in Section 1, and in the first set of *Excerpts* from 1689 he revisited Lemmas 9, 10 and 11. Leibniz disagreed with much of what he read, and his criticisms of individual

[195] See, for example, Hall, *Philosophers at War*; Bertoloni Meli, *Equivalence and Priority*; Guicciardini, *Reading the Principia*.

proofs will be considered in detail below. Their overall pattern, however, is very clear: Leibniz processed all Newton's proofs using the vocabulary and methodology of infinitesimals. He responded to Newton's geometrical limit definition in Lemma 1 by writing "Dubitari potest an sit aliqua ultima differentia"; in his *Excerpt* from Lemma 11, he described a geometrical quantity that is vanishing in the limit as a "magnitudinem initialem pervenisse primi gradus," and a short section of curve as an "arcum infinite parvum primi gradus"; and when he came across results he thought he had already proved, he wrote "hoc ita meo more effero" (in Lemma 2) and "Haec breviter meo more demonstrandi" (after Lemma 11). This attention on whether Newton's results could also be proved "meo more" shows that Leibniz's aim was not so much to assimilate Newton's proofs within the conceptual framework in which they were presented, but to ascertain whether they corresponded to results he could establish within his own. It was this conceptual mismatch, and not their internal validity, that led him to conclude that some of Newton's proofs were wrong. I will argue below that Leibniz was not persuaded by the mathematical demonstrations of the *Principia* in large part because he did not engage with them in the terms in which they were written.[196]

Unlike Leibniz, Huygens engaged in detail with only a handful of passages in the *Principia*. As shown in Chapter 1, the historical record suggests that he cherry-picked passages that aligned with his specific research interests, and at no point attempted a wider reading of the text. In line with this, there is no evidence that Huygens studied the foundational propositions of the method of first and last ratios in Section 1 of Book 1. He left no reading notes relating to

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[196] *Notes*, 226, 230; *Excerpts*, 480.

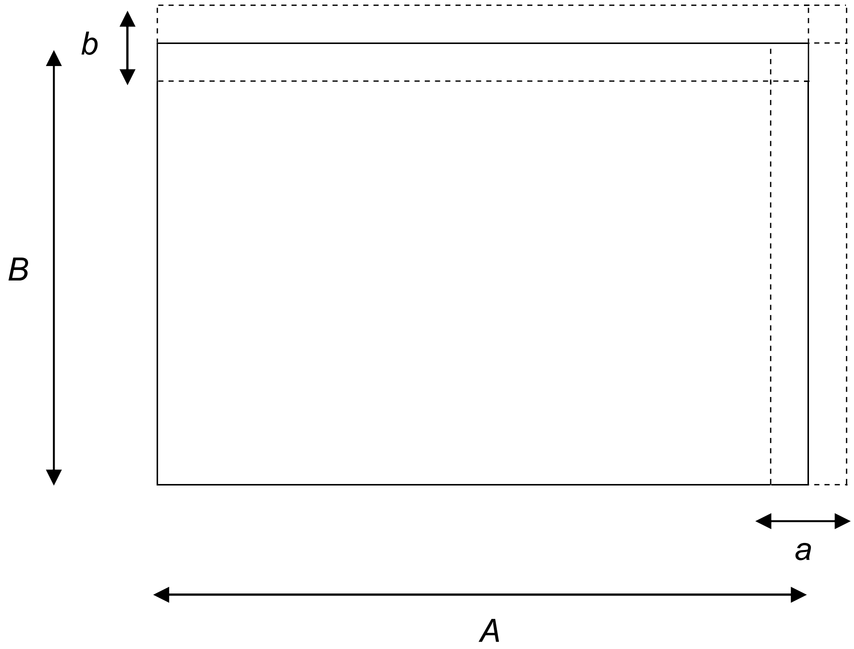
these results, and does not refer to them in detail in his correspondence. He did, however, engage with proofs involving limit arguments elsewhere in the book. The most reasonable way to summarise the manner in which he did so is that he reinterpreted Newton's geometrical limit arguments in terms of his pre-existing conceptual framework of infinitesimals and infinite series. Before presenting examples, however, let me emphasise that Huygens did not embark on these reformulations having first tried to assimilate the method of first and last ratios in Section 1. He reconstructed Newton's geometrical limit arguments in terms of infinitesimals because he had chosen not to study the conceptual foundations set out at the start of the book. In other words, when Newton provided what he thought was a careful and precise justification for his new methodology, Huygens decided to ignore it. Huygens made no attempt to engage with the conceptual framework in which the *Principia* was written.

Much of the relevant primary evidence in support of this claim has already been presented. In Chapter 2, I showed how Huygens verified Proposition 5 of Book 2 not by working through the argument in the text, but instead by generating his own independent proof based on the methodology of series. Because he chose not to engage with Newton's geometrical limits, he had to justify it by alternative means using his own conceptual framework. I also discused Huygens' recreation of Newton's proof of Proposition 6 of Book 1 in terms of Euclidean proportions, which on the face of it need not be incompatible with geometrical limit proofs. But Huygens' proof depended upon the fact that for an infinitely small angle θ, the versed sine, $1-\cos\theta$, is proportional to the square of the sine, and this result comes from series expansions. Just as in Proposition 5, then, Huygens needed to appeal to the methodology of infinitesimals to verify a proof Newton had constructed in terms of geometrical limits. Furthermore, I demonstrated in Chapter 4 that when reading Proposition 9 of Book 1 Huygens misunderstood the logical structure of Newton's argument because he did not see the importance of the limit argument in the second step. These three examples all therefore show Huygens attempting to process Newton's dynamic, geometrical limit proofs using the methodology of static, infinitesimal increments. In Propositions 5 and 6 this caused him to generate alternative derivations based on infinite series, and in Proposition 9 it led him to misunderstand the argument. Huygens did not in any of the extant evidence engage with Newton's arguments using the conceptual framework in which they were presented.

Huygens' reading notes on Lemma 2 and Proposition 8 of Book 2 provide further examples. Lemma 2 comprises a general statement about the heuristics for differentiation and differentials, containing what the modern reader recognises as the product rule and quotient rule, as well as rules for differentiating powers and roots. Newton expresses his results using the vocabulary of moments ("momenta"), which he describes as the "instantaneous increments or decrements" of variable quantities. Lemma 2 considers six separate cases, and Huygens's note relates to the first, which concerns the moment of the quantity *AB*. Reformulated in modern terminology, this is:

$$d\left(AB\right) = B.dA + A.dB$$

This is equivalent to the product rule for differentiation, and there are a few different ways in which a modern textbook might justify this result, depending on its readership. Newton's explanation can be reconstructed as follows.

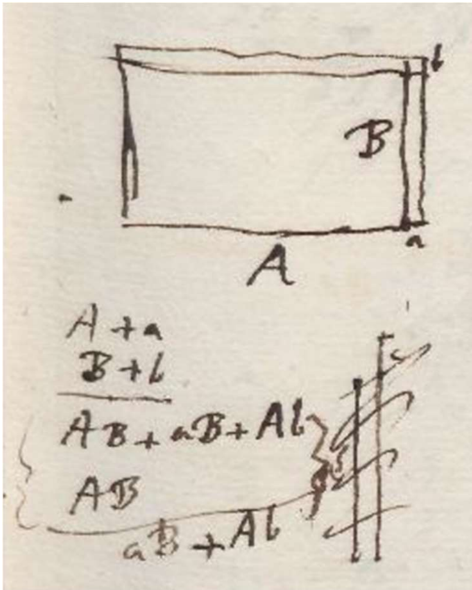


The reader who consults the original text will see that Newton does not supply this diagram, which the reader is expected to infer. It shows an expanding rectangle of width *A* and height *B*. The width and height are increasing at different rates, with the instantaneous increment of *A* (its "moment", *dA*) labelled as *a*, and that of *B* labelled *b*. Importantly, Newton directs his reader to consider the rectangle half a moment in time before it has area *AB*, and half a moment in time after it has area *AB*. It is easy to compute the area of the rectangle at these two instants:

$$\text{area half a moment before} = \left(A - \tfrac{1}{2}a\right)\left(B - \tfrac{1}{2}b\right) = AB - \tfrac{1}{2}aB - \tfrac{1}{2}bA + \tfrac{1}{4}ab$$
$$\text{area half a moment after} = \left(A + \tfrac{1}{2}a\right)\left(B + \tfrac{1}{2}b\right) = AB + \tfrac{1}{2}aB + \tfrac{1}{2}bA + \tfrac{1}{4}ab$$

Subtracting the first from the second gives that the increment in the area of the rectangle over the whole moment is $aB + bA$, as was to be proved.

In his short note on this result, Huygens supplies an alternative proof:[197]



It is clear from his diagram that, unlike Newton, Huygens wishes to consider not the half-moment before and the half-moment after the rectangle has area *AB*, but one full moment afterwards. At this instant the area is

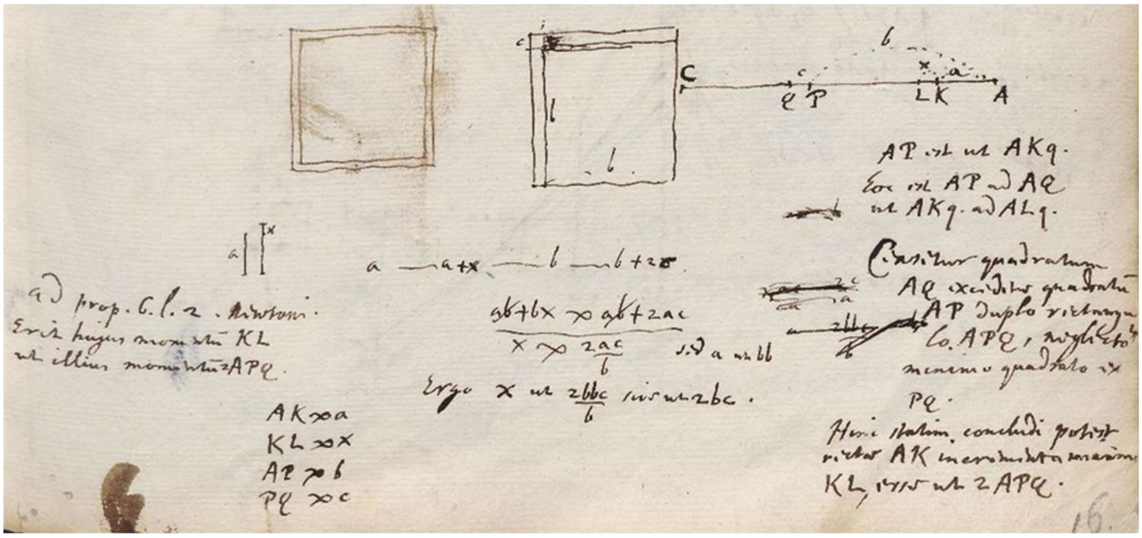
$$\text{area one moment after} = (A + a)(B + b) = AB + aB + bA + ab$$

The increase in area is therefore $aB + bA + ab$. Neglecting the infinitesimal term *ab*, this leaves $aB + bA$, as required. Huygens' proof arrives at the result almost immediately, using only one line of working to Newton's two.

[197] CH, HUG 7, 16r (=*OC*, 21:421).

Both proofs of this simple result are easy to understand. Nevertheless, Huygens' is simpler. It is analogous to the intuitive justification a modern reader might give of the product rule today. Indeed, Newton's insistence on considering the half-moment before and the half-moment after seems unnecessarily fiddly. His reason for doing so is in the step in Huygens' proof that was carefully elided above: the neglecting of the infinitesimal term *ab*. To Huygens this step was sufficiently automatic that he did not write "*ab*" anywhere on his note. Ignoring infinitesimal terms was such an established part of his conceptual framework that he didn't make it explicit. For Newton, on the other hand, avoiding infinitesimals was of the utmost importance. The contortion of his proof to consider not the moment after but the half-moment before and the half-moment after ingeniously achieves this aim. But his text was not read in the same way that it was written: Huygens processed its proofs from within a conceptual framework different from Newton's.

Huygens' note on Proposition 8 appears as follows:[198]



[198] CH, HUG 7, 16r (=*OC*, 21:422).

Proposition 8 concerns a body falling vertically under gravity through a medium which provides resistance proportional to the square of the velocity, and Huygens' figure at the top right represents the horizontal axis of the diagram on page 255 of the *Principia*. The details of Proposition 8 are not important here, because Huygens' note refers to a single line in Newton's proof that can be understood in isolation from the rest, and which Huygens has written out on the far left: "Erit hujus momentum *KL* ut illius momentum 2*APQ*".[199] In Proposition 8 *KL* is defined as the moment of *AK*, which has been shown to be proportional to AP^2; and *PQ* is the moment of *AP*. Huygens therefore wishes to verify that the moment of AP^2 will be $2AP \times PQ$, where *PQ* is the moment of *AP*. The modern reader will recognise this as a simple example of the chain rule. Newton does not justify this assertion explicitly in Proposition 8, relying on the reader to realise that it follows from the general statement of Lemma 2. Huygens on the other hand – who, so far as can be inferred from the evidence, only worked through the first of the six cases considered in Lemma 2 – wishes to verify it directly.

At the bottom left of his note, he introduces the symbols *a*, *b*, *c* and *x* to represent *AK*, *AP*, *PQ* and *KL* respectively. He then considers an expanding square of side *b*, as he has drawn at the top. Since $AK \propto AP^2$, the area of the square is *a*. The moment of *b* is *c*, and the moment of *a* is *x*, as he has shown on the diagram. Huygens explains his geometrical reasoning in the text on the right:

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[199] Huygens has mislabelled this note "ad prop. 6. l. 2. Newtoni", probably because the alternative title for Proposition 8 is Theorem 6. This slip has not been corrected by either the editors of the *OC* or Guicciardini.

Censetur quadratum *AQ* excedere quadratum *AP* duplo rectangulo *APQ*, neglecto minimo quadrato ex *PQ*. Hinc statim concludi potest rectae *AK* incrementa minima esse ut 2*APQ*.

In symbolic terms, the area of the larger square $(b+c)^2$ exceeds the area of the smaller square b^2 by double the rectangle *bc*, so long as the minimally small square c^2 is neglected. Thus the infinitesimal increment *x* is as 2*bc* as required.[200]

Huygens has therefore established to his own satisfaction the assertion in Proposition 8 he wished to verify. However, he is again explicitly using the methodology of infinitesimals that Newton so studiously avoids ("neglecto minimo quadrato ex *PQ*"), as is consistent with the pattern established by the other examples examined in this section. Huygens did not merely translate the geometrical limit results into the language of infinitesimals; rather, the only way he had of verifying Newton's claims was by attempting to re-prove them using the analytical techniques with which he was already familiar. In most cases this resulted in alternative derivations which involved neglecting higher-order terms in series expansions. Viewed alongside his neglect of the method of first and last ratios in Section 1 of Book 1, the only fair conclusion to draw is therefore that he made no attempt to assimilate Newton's methodology. He did not understand the proofs of the *Principia* on their own terms, because he did not try to do so: at best, he produced his own derivations of the conclusions at

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200 He has supplied an algebraic derivation of this result in the centre of the page, although I confess that I do not fully understand his train of thought. He appears to begin with the ratio $a:(a+x)=b:(b+2c)$, from which the subsequent lines easily follow. But why he feels able to begin with this line, I am not sure: the only way I can make sense of it is by referring back to the geometrical argument he has provided in his note.

which they arrived. Huygens, like Leibniz, read Newton's proofs within a different conceptual framework from that in which they were written.

4.2.4 Gregory and Fatio's successful assimilation

If Flamsteed was casual, Halley uneasy, and Leibniz and Huygens resistant in their attitudes towards the mathematical methodology of the *Principia*, other readers were much more comfortable engaging with Newton's proofs in the terms in which they were written. Gregory and Fatio are two such individuals, and I have already given a number of instances in which they carefully processed the limit arguments in the geometrical terms in which they were presented in the text, rather than judging them according to the criteria of an alternative infinitesimal methodology. In Gregory's case, this was facilitated by his familiarity with his uncle James' work, whose similarity to Newton's is acknowledged at the end of his comments on Section 1 in the *Notae*. He here describes the method of first and last ratios as "a middle way between the method of exhaustion and infinitesimals" ("… de methodo exhaustionum et indivisibilibus ac inter illas mediam maluit insistere viam"). Guicciardini reports that in sections of the *Notae* I have been unable to examine, Gregory produces analytical proofs of results omitted by the text, employing algebraic methods Newton generally avoided in the *Principia*.[201] Gregory therefore appears to have been able to adapt his methods to suit the circumstances. This willingness to engage with a variety of styles of proof is not displayed by any of Newton's other readers, and is consistent with the conceptual flexibility he adopted when working through his proofs on orbital motion, as we shall see in Chapter 5.

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[201] RS, MS210, 1v; Guicciardini, *Reading the* Principia, 179–84.

Although less obviously alert to alternative methodologies, Fatio also engaged with Newton's limit proofs in the geometric terms in which they were presented. He studied the arguments in Section 1 closely, just as he did the orbital force theorems that employed them in Sections 2 and 3. The long marginal note Fatio made alongside Proposition 43 of Book 1 was examined in Chapter 2, where my emphasis was on the novelty of this result, and the way in which the act of verifying Newton's original proof organically led Fatio to generate this new piece of mathematics. Here I make the additional observation that this new proof is based on a geometrical limit argument. Fatio sufficiently trusted the validity of Newton's methodology, and was sufficiently adept at manipulating it, to use it to produce a new piece of mathematics: in fact, it is the only example I have come across of a reader extending Newton's results to generate a new conclusion using his method of first and last ratios. Just as with Gregory, however, Fatio's successful assimilation of Newton's methodology did not mean that he uncritically assented to his foundational proofs in Section 1, as I shall now address.

4.3 Critiques of the method of first and last ratios in Section 1, Book 1

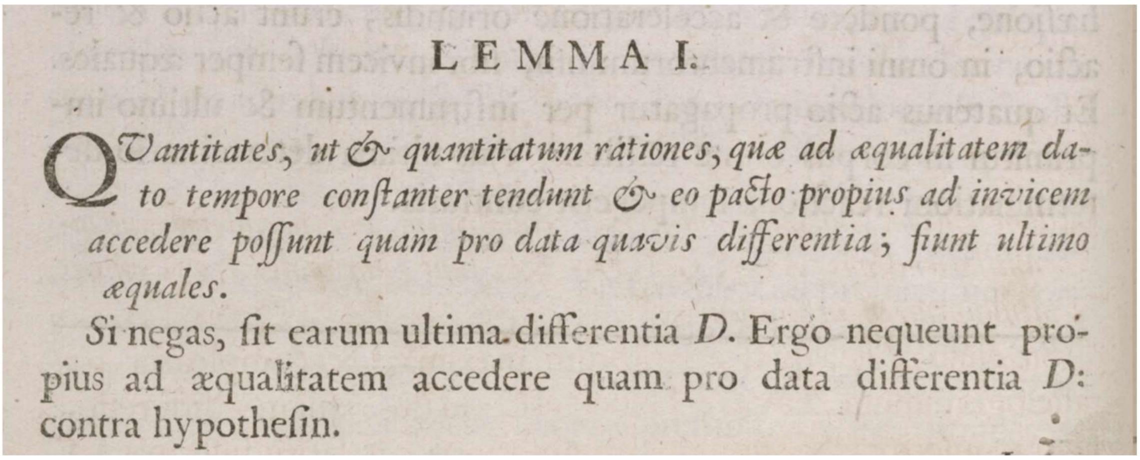
Having considered the general responses to the geometrical limit methods deployed by Newton over the course of the *Principia*, I will now examine readers' responses to the foundational exposition of the method of first and last ratios set out in the eleven lemmas and concluding scholium of Section 1 of Book 1. Although reactions to these passages varied, some clear patterns emerge. No objections were raised in the extant evidence against Lemmas 2, 3 and 4, which concern what the modern reader recognises as definite integrals, nor to the simple statement regarding similar shapes in Lemma 5. Surprisingly, this is also true of the circle of curvature proof in Lemma 11, possibly owing to its relative unimportance in the first edition. Readers of Lemma 10, on the other hand, were keen to clarify exactly how Newton intended the preceding mathematical results to apply to the concepts of physical mechanics.

There are two particularly significant observations to make, however. First, many readers either misunderstood or contested the limit definition in Lemma 1, and it is important to note that they did so because Newton's poor wording rendered its logical structure obscure. Assent was thus denied to the foundational mathematical proposition of the book not on the grounds of an ideological objection to Newton's methodology, but because the logic of his demonstration was unclear: Newton wrote the very first proof in the *Principia*, on which all the others depended, badly. And second, his readers did not see the need for the unusual "microscope" style of proof he deployed in Lemmas 6, 7, 8 and 9. Both Fatio and Gregory thought the microscope was redundant, and urged its removal. This shows that, even when they were sympathetic to his mathematical methodology and agreed with its conclusions, Newton's peers sometimes judged the arguments he provided in its support to be faulty.

4.3.1 The limit definition in Lemma 1

Lemma 1 comprises Newton's geometrical definition of a limit, and is of extreme importance both to the internal logic of the *Principia*, and the broader history of mathematical analysis. Its precise articulation therefore merits careful scrutiny. On a local level, an individual's reaction to Lemma 1 tended to determine how he subsequently engaged with all the other limit proofs in the book, and the response to this opening result generally provides a good indication of how a given reader engaged with Newton's methodology in later sections. Responses to Lemma 1 therefore varied widely, and cannot easily be summarised: some readers ignored it completely, some approved it without comment, some misunderstood its argument, some demanded it be rewritten, and some objected to the fundamental concepts with which it was articulated. The only definitive conclusion that can be drawn is that it did not receive universal assent.

Here is how the result appears in the printed first edition:



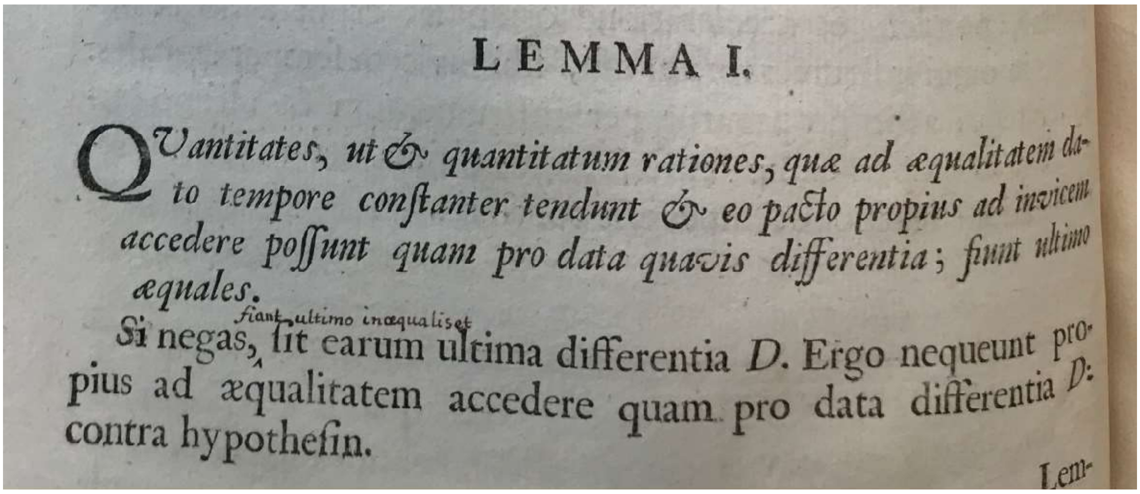
LEMMA I.

Quantitates, ut & quantitatum rationes, quæ ad æqualitatem dato tempore conſtanter tendunt & eo pacto propius ad invicem accedere poſſunt quam pro data quavis differentia; ſiunt ultimo æquales.

Si negas, ſit earum ultima differentia *D*. Ergo nequeunt propius ad æqualitatem accedere quam pro data differentia *D*: contra hypotheſin.

> Quantities, and also ratios of quantities, which in a given time constantly tend to equality, and which in this way are able to approach closer to each other than any given difference, become ultimately equal.
>
> If you deny this, let their ultimate difference be *D*. Therefore they cannot approach closer to equality than the given difference *D*, contrary to the hypothesis.

The modern reader will immediately perceive affinities between Newton's phrasing and the nineteenth-century, epsilon-delta formulation of a limit. Newton subsequently made significant alterations to both the statement and its proof, and it is an important aim of this section to establish the extent to which these changes were made in response to his readers. Here is the updated version as it appears in the presentation copy he gave to Locke:


LEMMA I.

Quantitates, ut & quantitatum rationes, quæ ad æqualitatem dato tempore conſtanter tendunt & eo pacto propius ad invicem accedere poſſunt quam pro data quavis differentia; fiunt ultimo æquales.

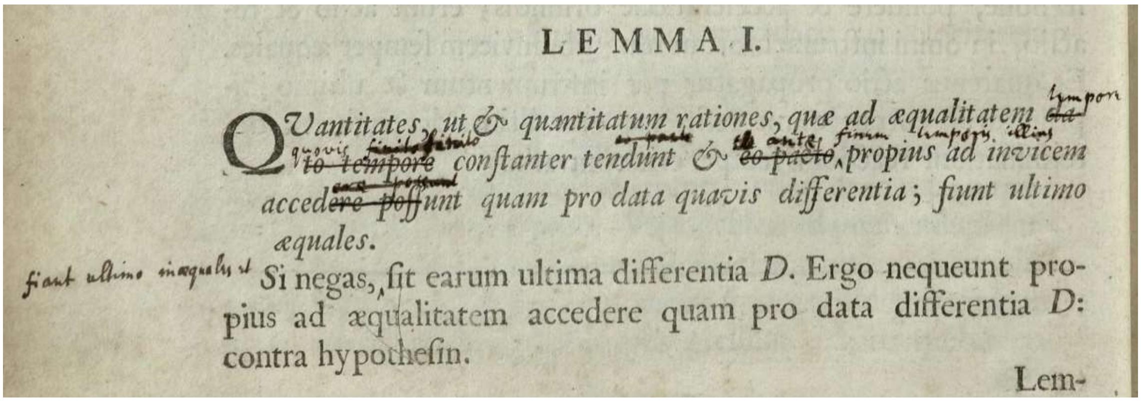
fiant ultimo inæqualis et

Si negas, ſit earum ultima differentia *D*. Ergo nequeunt propius ad æqualitatem accedere quam pro data differentia *D*: contra hypotheſin.

Lem-

The words “fiant ultimo inaequalis et” have been inserted at the start of the second sentence, which now translates as “If you deny this, let them become ultimately unequal, and let their ultimate difference be *D.*” This edit also appears in the “Alia Errata” section of Fatio’s reading notes, which means that it pre-dates his meeting with Newton in March 1690.[202] However, Newton made further changes to the first sentence. Here is the final version as it appears in his annotated copy:

[202] *OC*, 10:153.


LEMMA I.

Quantitates, ut & quantitatum rationes, quæ ad æqualitatem da-to tempore conſtanter tendunt & eo pacto propius ad invicem accedere poſſunt quam pro data quavis differentia; fiunt ultimo æquales.

Si negas, ſit earum ultima differentia *D*. Ergo nequeunt propius ad æqualitatem accedere quam pro data differentia *D*: contra hypotheſin.

Lem-

There have clearly been a number of alterations, but “Dato tempore” has ultimately been replaced with “tempore quovis finito”; “eo pacto” has become “ante finem temporis illius”; and “accedere possunt” has become “accedunt”. This is the version that appears in the third edition. However, it is noteworthy that Cohen and Whitman unilaterally made even more changes when producing their translation:

> Quantities, and also ratios of quantities, which in any finite time constantly tend to equality, and which before the end of that time approach so close to one other that their difference is less than any given quantity, become ultimately equal.
>
> If you deny this, let them become ultimately unequal, and let their ultimate difference be *D*. Then they cannot approach so close to equality that their difference is less than the given difference *D*, contrary to the hypothesis.

It can be seen that they chose to translate “propius… quam pro data quavis differentia” as “so close... that their difference is less than any given quantity” and “propius… quam pro data differentia *D*” as “so close… that their difference is less than the given difference *D*.” Cohen and Whitman did not in their apparatus acknowledge that they had altered Newton’s wording in this way, but clearly considered their changes to be of benefit to any modern reader who wished to understand the meaning of these lines. Three hundred years after they were originally composed, Newton's words are still not easy to follow.

The logical structure of Lemma 1 also merits close consideration. In particular, it is important to note that Newton is using a proof by contradiction. He claims that, given two quantities that approach each other in such a way that they can come closer than any given difference, they must become ultimately equal. He proves this by arguing that, if they are not ultimately equal, then they cannot approach each other closer than any given difference. He therefore proves his original statement indirectly, by deriving a contradiction from its negation. The logical structure of this proof by contradiction will be very important in what follows.[203]

Some of Newton's readers did not remark on Lemma 1. It does not appear in Fatio's commentary, for example, nor in Gregory's *Notae*. For these two readers, it is fair to infer from this absence that they assented to the validity of the original wording. This is consistent with their successful engagement with the limit methods later in the *Principia*. On the other hand, the absence of any record of Huygens commenting on Lemma 1 is consistent with the assumption that he never studied the opening section, and that he processed the later proofs in the book in terms of infinitesimals precisely because he never made any attempt to assimilate Newton's methodology.

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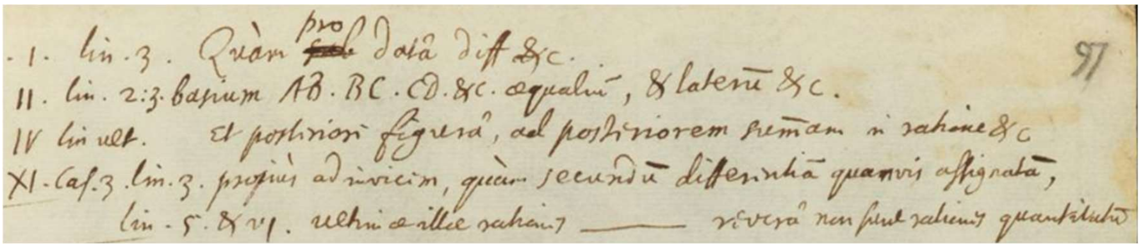
[203] The proof may alternatively be viewed as a proof by contraposition: *if* two quantities approach each other in such a way that they can come closer than any given difference, *then* they must ultimately become equal. According to this reading, Newton proves the statement by demonstrating the contrapositive: if they don't become ultimately equal, then they cannot come closer than any given difference. *Mutatis mutandis*, my analysis holds either way.

However, Halley's notes – which reflect his reading of draft manuscripts before publication – do make a handful of references to the method of first and last ratios:[204]



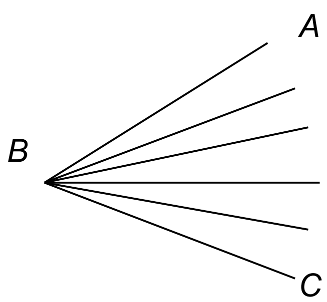
Two of these lines relate to the phrasing of Lemma 1, and neither can be conclusively decoded. The first is the top line. Halley originally wrote "quam sub data diff &c," and either he or Newton – I cannot tell from the handwriting – subsequently changed "sub" to "pro." The draft version on which this note is based no longer exists, so it is not clear what change Halley is proposing, nor whether it relates to the "quam pro data differentia" in the statement or the proof. The absence of "quavis" suggests the latter, but "lin. 3" may indicate the former. He might intend it to apply to both. However, the fourth line in the image provides further information. It relates to a very similar form of words in Case 3 of Lemma 11, which considers two angles tending to equality in the limit. For this edit the manuscript draft on which Halley's edit was based does still exist, and it reads "propius accedent ad invicem quam‸pro differentia quavis assignata." The text is a fair copy transcribed by Newton's amanuensis, to which Newton has subsequently added "pro" in exactly the position Halley suggested "secundum."

[204] CUL, MS Add. 3965, 97r.

Putting this together, I suggest that Newton's original phrasing in both Lemma 1 and Lemma 11 had been "propius… quam differentia." Halley suggested "quam sub" in Lemma 1 and "quam secundum" in Lemma 11, but Newton preferred "quam pro" in both cases, as the final version reflects. If this reconstruction is correct, these edits demonstrate Halley altering the phrasing of one of the most significant lines in the book: he is negotiating with Newton his definition of a geometrical limit. It might be argued that he is only tinkering with unimportant prepositions, but the example of Cohen and Whitman's unauthorised rewording shows that even small changes of phrasing make a difference to how easily a passage may be understood, and thus how effectively it persuades its readers. And it is clear that Newton agreed with Halley that an improvement could be made, since he changed his text in response. Moreover, a second, more tantalising, possibility presents itself. There is no way of telling from Halley's manuscript, but it is possible that the word "data" was also absent from the draft he read, and that he was suggesting adding "pro data" to the phrasing of Lemma 1. This is not impossible, since the lines still make sense with "data" removed – and if this were the case, then Halley would have made an extremely significant contribution to the formulation of the concept of a limit. It is only the inclusion of "given" which makes it clear that the difference D should be conceived as a fixed, finite quantity that a second variable is then challenged to fall beneath, as conveyed by the symbols "$\forall \varepsilon > 0$" in the modern epsilon-delta definition. But this suggestion is speculative, and there is no firm evidence in its support: what is clear, however, is that Halley thought he could improve on the phrasing of Lemma 1, and successfully lobbied Newton to change it.

The most direct criticism of Lemma 1 in the historical record appears in Clerke's letter to Newton of 21 November 1687:[205]

> but I must needs crave your pardon for one more touch at parting; viz: concerning your lemmata. If your first lemma had been thus Quantitates ut et Quantitatum rationes quae ad aequalitatem dato tempore constanter tendunt, fiunt ultimo aequales, who could have denied it? But then the words dato tempore, seem to make the proposition identical; as if it were said quantities wch will be equal will be equal: the Asymptots will indefinitely draw nearer & nearer though never to touch dato tempore.
>
> But your adding et eo pacto – pro data quavis differentia; if upon this it had been said – nullam habent ultimam differentiam & proved – si negas, sit earum ultima differentia *D*, ergo nequeunt propius accedere quam, per datam differentiam *D*. contra hypothesin: who could denie it? If not! then your Lemma cannot be admitted; for whereas you say, sit earum ultima Differentia *D*, it is already proved that there is no such thing, & indeed all mathematicians have hitherto held that there is no such thing as quantitas indvisibilis [sic] or ultima ratio but you say ultima ratio quacum evanescent p.35. wch determines nothing for with wch of all these lines or angles doth the Angles *ABC* evanescere determine wch of these or any other you will, a lesse may be given.



[205] Clerke to Newton, *NC*, 2:496–500.

His sense in his first opening sentences is very clear. If Newton had just written "quantities which tend towards equality in a given time become ultimately equal," then that proposition would have been undeniable. Newton's inclusion of the words "in a given time" make it tautologically true, Clerke says, because if they tend to equality within a given time, then of course they will ultimately be equal. This is a misunderstanding on Clerke's part. He is erroneously understands "tend to equality" to mean "attain equality," which is not Newton's intention. But his more significant confusion is in the second half of the passage. When Newton writes "sit earum ultima differentia *D*," Clerke objects that this is not possible, because there is no such thing as an "ultimate difference." However small the difference is, there can always be a smaller one. It has been proved that there isn't such a thing as an "ultima differentia" or a "quantitas indivisibilis" or an "ultima ratio." As his final diagram shows, given any small angle, a smaller one can always be found. This invalidates the lemma, Clerke argues, and having denied Newton's definition of a geometrical limit, he therefore denies all the other results in Section 1. In Lemmas 6, 7, 8 and 9 it is not true to say that the respective angles, curves, tangents and triangles coincide in the given ratios, because when the points meet the constructions on which they are based disappear. As he writes later in his letter, "after coition of the points… there will be no lines left for coincidence." Clerke concedes that he might have granted these results if Newton had only claimed that they were approximately equal rather than perfectly identical. He writes that "you might wth the consent of all have taken *propemodum aequales* per *aequalibus*." But as it stands, "such things cannot passe for strict Geometrie." The entire method of first and last ratios is undermined as soon as Newton considers the "ultima differentia" between two approaching quantities, he objects, because there is no such thing as a "last difference."

Of course, the modern reader can see that this is not what Newton intends. Clerke has misunderstood. And the core of his misunderstanding is that he has not recognised that Newton is using a proof by contradiction. Newton writes "sit earum ultima differentia *D*" not because he thinks there is an ultimate difference between two approaching quantities, but because that is the negation of the statement he is trying to prove. To someone who intuits what Newton intends, it is obvious that in this context he means "ultima differentia" to stand for a fixed, finite limit beyond which the two approaching quantities do not converge. But Clerke does not intuit that. He thinks by "ultima differentia" Newton means the final difference between two approaching quantities. But that isn't what he means, and Newton would agree that such a final difference does not exist. Clerke's confusion is driven by his failure to acknowledge the structure of the demonstration, since he does not recognise that this is a proof by contradiction. His misunderstanding is logical, not conceptual.

A problem for historians who dismiss Clerke's criticisms as naïve is that Leibniz misunderstood Lemma 1 in just the same way. He also did not recognise that Newton was using a proof by contradiction. In his *Notes* on Newton's proof he wrote "Dubitari potest an sit aliqua ultima differentia,"[206] almost exactly as Clerke did: Leibniz's immediate response to reading the words of Lemma 1 was to object that such an "ultimate difference" does not exist. His *Marginalia* alongside Lemma 1 read "Si asymptotae sint potest esse

[206] *Notes*, 226.

nulla differentia ultima, et tamen nunquam fient aequales,"[207] which is identical to Clerke's retort that asymptotes approach closer and closer with no "ultimate difference" but never become equal. Like Clerke, Leibniz understood from "Si negas, sit earum ultima differentia *D*" that Newton thought there existed an ultimate difference between two approaching quantities, and objected that this was not true. He also did not grasp that Newton made this assumption as part of a proof by contradiction.

And like Clerke, this denial of Lemma 1 lead him to reject the methodology in the remainder of the section. Having been unpersuaded by Newton's articulation of a limit in his opening result, Leibniz proceeded to assimilate the proofs in terms of his pre-existing conceptual framework of infinitesimals. This is particularly clear in his *Excerpt* from Lemma 11. This is mostly a transcription lifted directly from the text, but one of the few occasions when he deviates from Newton's wording is when he replaces Newton's sentence "Est ergo, per Lemma I, ratio ultima *AB* quad. ad *Ab* quad. aequalis rationi ultimae *BD* ad *bd*" with "Ergo postremo coincident."[208] He also objects that "Si accedent usque ad A ergo coincident… dicendum est non coincidere, sed ad magnitudinem initialem pervenisse primi gradus."[209] Furthermore, he says,

> Mirum quod haec dicantur ultimo rationem habere cum coincidant, sed respondeo revera non coincidere, cum formant arcum infinite parvum primi gradus, qui a recta non differet nisi incomparabiliter.[210]

In other words, when Newton in Lemma 11 asserted that two quantities approach each other in the limit (terminology which has been defined by

[207] *Marginalia*, 52.
[208] *Excerpts*, 480.
[209] Ibid., 480.
[210] Ibid., 480.

Lemma 1), Leibniz raised the objection that they are in fact separated by infinitesimals of the first order (concepts which the text studiously avoids). He judged Newton's method of first and last ratios by whether or not it conformed to the expectations of his pre-existing conceptual framework. He did not engage with Newton's proofs in the terms that they were written. Lemma 1 did not persuade him to adopt Newton's concept of a limit, because the argument it presented did not convince him – and the cause of this was his failure to realise that it was a proof by contradiction. Notwithstanding the manifest differences in their responses to Newton's book, the very first mathematical argument in the *Principia* failed to persuade Leibniz in exactly the same way that it failed to persuade Clerke.

If, in the light of Clerke and Leibniz's comments, the edits Newton made to Lemma 1 after publication are re-examined, they reveal something very interesting. I have argued that both Clerke and Leibniz rejected the method of first and last ratios because they did not understand the logical structure of Lemma 1. However, this logical structure is extremely unclear in the original phrasing in the first edition. Furthermore, Newton's later insertion of "fiant ultimo inaequales et" significantly clarifies his meaning. The effect of these words is to make it clear that Newton does not in that sentence consider the two quantities to be approaching ever closer towards an ultimate difference, but that they are in the end separated by a fixed difference. That is, these words make it much clearer that the assumption "sit earum ultima differentia *D*" is part of a proof by contradiction. The alterations to the first sentence likewise emphasise that a finite time period is being considered in the proof, rather than an infinitely extending example in which the quantities approach asymptotically. The timeline for these edits is uncertain, because although Fatio's notes show that "fiant ultimo inaequales" must have been inserted before March 1690, the other changes cannot reliably be dated. But it is clear that they address difficulties readers had understanding the proof. These edits don't fix conceptual difficulties so much as logical ones: they are the author's attempts to clarify his argument for correspondents who he knows have misunderstood it. And there is a strong possibility that at least some of the edits were made in direct response to Clerke's complaints, because "fiant ultimo inaequales" resolves his logical misunderstanding, and the changes to "dato tempore" and "eo pacto" correspond exactly to the phrases he picks out in his letter. If this is indeed the case, then far from being dismissed by the secondary literature, Clerke's name deserves to be known for prompting Newton to re-write some of the most important lines in the *Principia*, and thus to refine the articulation of one of the foundational concepts in the history of mathematics.

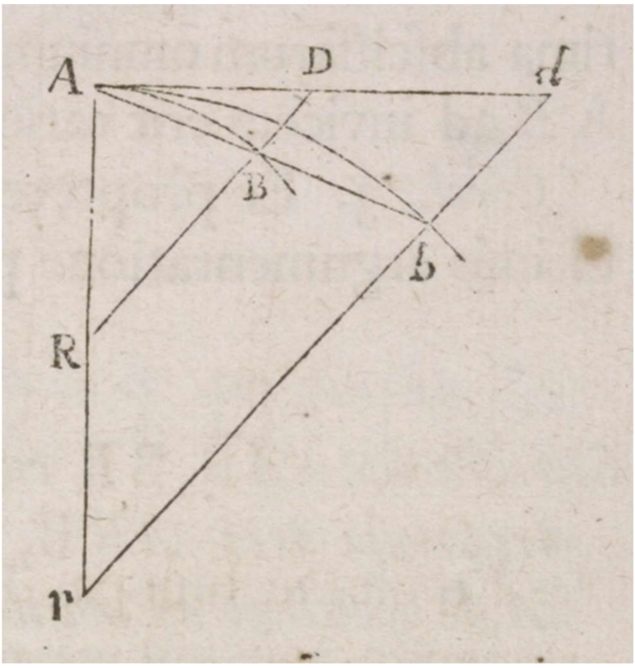
4.3.2 Newton's microscope in Lemmas 7 – 9

A second feature of Section 1 that drew attention from Newton's readers was the style of proof he used in Lemmas 6, 7, 8 and 9. This style was deployed very similarly in all four results, and when it is discussed in the secondary literature is sometimes labelled "Newton's microscope."[211] But it will become apparent that, although this microscope argument was of critical importance to Newton's justification of his method of first and last ratios, his readers either struggled to understand it or thought that it was irrelevant. Lemma 6 merits separate consideration, and will be discussed in the next section; of the others, Lemma 7 is the most convenient to analyse, and it will be a simple matter to see

[211] Densmore, *Newton's* Principia*: The Central Argument*, 84.

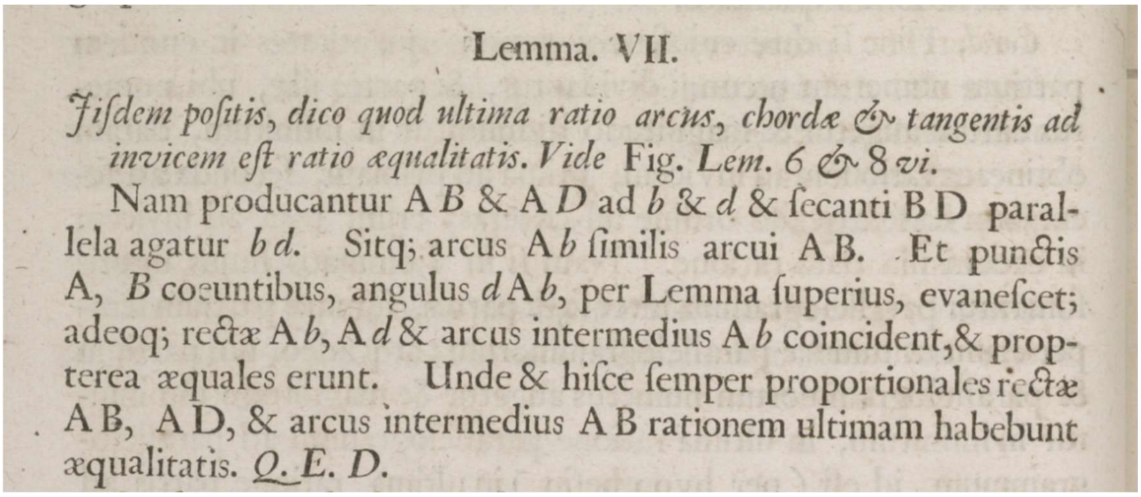
how this style of proof, as well as readers' responses to it, apply directly to Lemmas 8 and 9.

Lemma 7 is based on the following construction (which is identical to that used in Lemmas 6 and 8, and very similar to that in Lemma 9):



It will be helpful to gain an intuitive understanding of Lemma 7 before examining Newton's statement and proof. This is best done by focussing on the points *A*, *B*, *D* and *R* in the diagram (and ignoring, for the moment, points *b*, *d* and *r*). Some fixed curve passes through point *A* and point *B*, and – even though many other points in the construction are about to move – this curve remains fixed throughout. The line *AD* is the tangent to this curve at *A*. The point *R* lies on some straight line meeting the curve and the tangent at *A*: in the diagram angle *RAD* looks like a right angle, but this is not a requirement. The straight line *AB* is the chord of the arc *AB*, and Newton is going to describe *BD* as the "secant." In intuitive terms, Lemma 7 says that when the curve and the point *A* are held fixed, as the point *B* slides along the curve towards the point *A*, bringing points *D* and *R* with it, then in the limit the arc *AB*, the chord *AB* and the tangent *AD* are all equal in length.

With an intuitive understanding of the result, the difficulties in proving it satisfactorily are immediately apparent. Clerke succinctly articulated the problem in the letter quoted above, when he wrote that "after coition of the points *A*, *B*. there will be no lines left for coincidence": as soon as the point *B* arrives at *A*, the arc, chord and tangent all disappear, and so the result becomes meaningless. Newton attempted to overcome this problem using the points *b*, *d* and *r*. Here is how the statement and its proof appear in the first edition:



Lemma. VII.

Jiſdem poſitis, dico quod ultima ratio arcus, chordæ & tangentis ad invicem eſt ratio æqualitatis. Vide Fig. *Lem. 6 & 8 vi.*

Nam producantur A *B* & A *D* ad *b* & *d* & ſecanti B D parallela agatur *b d*. Sitq; arcus A *b* ſimilis arcui A B. Et punctis A, *B* coeuntibus, angulus *d* A *b*, per Lemma ſuperius, evaneſcet; adeoq; rectæ A *b*, A *d* & arcus intermedius A *b* coincident, & propterea æquales erunt. Unde & hiſce ſemper proportionales rectæ A B, A D, & arcus intermedius A B rationem ultimam habebunt æqualitatis. *Q. E. D.*

The construction of the points *r*, *b* and *d* is reasonably clear, with triangle *dAr* an enlargement of triangle *DAR*, and with arc *Ab* an enlargement of arc *AB*. Now Newton lets *B* move along the curve towards *AB*. As it does so, the angle *dAb*, which is equal to angle *DAB*, tends towards zero. But the last three lines of the proof are less obvious. As *B* moves towards *A* along the arc *AB* and the triangle

DAR shrinks, it is not immediately clear how the point *b* is moving. If it is also moving towards *A* along the arc *Ab*, such that triangle *dAr* is shrinking in proportion, it makes some intuitive sense that the straight line *Ab*, the straight line *Ad* and the arc *Ab* will coincide. But then little has been gained by considering *d*, *b* and *r* rather than *D*, *B* and *R*, since if all six points are vanishing towards the point *A*, then both constructions cease to exist when they ultimately meet.

The reader may already have gathered that this is not what Newton intends. Rather, he means it to be understood that, as *B* slides along the arc towards *A* and triangle *DAR* shrinks, triangle *dAr* remains the same size. That is, as points *D* and *R* move in towards *A*, points *d* and *r* stay fixed. At all times the arc *Ab* is understood to be constructed similar to the arc *AB*, which means that as the point *B* gradually approaches the (moving) point *D*, the point *b* gradually approaches the (fixed) point *d*. The line *Ad* therefore does not change in either position or length, as the line *Ab* gradually comes up to meet it. So while throughout the process the construction *dArb* is an enlargement of the construction *DARB*, the scale factor of that enlargement increases as *B* approaches *A*, in such a way that the size of triangle *dAr* remains fixed. The construction “microscopes” in. And since the construction *dArb* is always finite, this allows Newton unproblematically to infer that in the limit the line *Ab*, the line *Ad* and the arc *Ab* are in the ratio of equality; and because it is at all times an enlargement of the construction *DARB*, this allows him to conclude that in the limit the line *AB*, the line *AD* and the arc *AB* are also in the ratio of equality, as required.

Any reader who did not immediately grasp that this was what Newton meant is in good company. Here are the notes Fatio made on this result:[212]

> p. 30. l. 7. lege. Nam dum punctum ad punctum *A* accedit, intelligantur semper produci *AB* et *AD* ad *b* et *d*, ut sint *Ab*, *Ad* magnitudines finitae hoc est magnitudines non infinitae parvae, et secanti *BD* parallela agi *bd* quae proinde lineam *ADd* secabit in *d*, cum ea constituens angulum *Adb* aequalem ang^o *ADB* tangentis cum secante atque adeo non infinite parvum. Sitque arcus *Ab* semper similis arcui *AB*. Et punctis &c.
>
> p. 30. l. 9. evanescet; coincident autem puncta *b* et *d*, adeoque &c.
>
> p. 30, l. 7 read: For during the time in which the point [*B*] approaches the point *A*, let *AB* and *AD* be understood always to be produced to *b* and *d*, so that *Ab* and *Ad* are finite magnitudes, that is, not infinitely small magnitudes; and let *bd* be understood always to be drawn parallel to the secant *BD*; and so *bd* will therefore cut the line *ADd* at *d*, forming with it the angle *Adb* equal to the angle *ADB* of the tangent with the secant, and so therefore not infinitely small. And let the arc *Ab* always be similar to the arc *AB*. And with the points etc.
>
> p. 30, l. 9 [And as the points *A* and *B* come together, the angle *dAb*] will vanish; but the points *b* and *d* will coincide, and therefore [the straight lines *Ab* and *Ad* and the intermediate arc *Ab* will coincide]

It should be clear how these comments correspond to the original text replicated above, and it is easy to see that Fatio's proposed edits directly address the ambiguities just discussed. He wishes it to be understood that *AB* and *AD* are "semper produci" to the points *b* and *d* "dum punctum [*B*] ad punctum *A* accredit." He wants to make it clear that *Ab* and *Ad* are "magnitudines finitae

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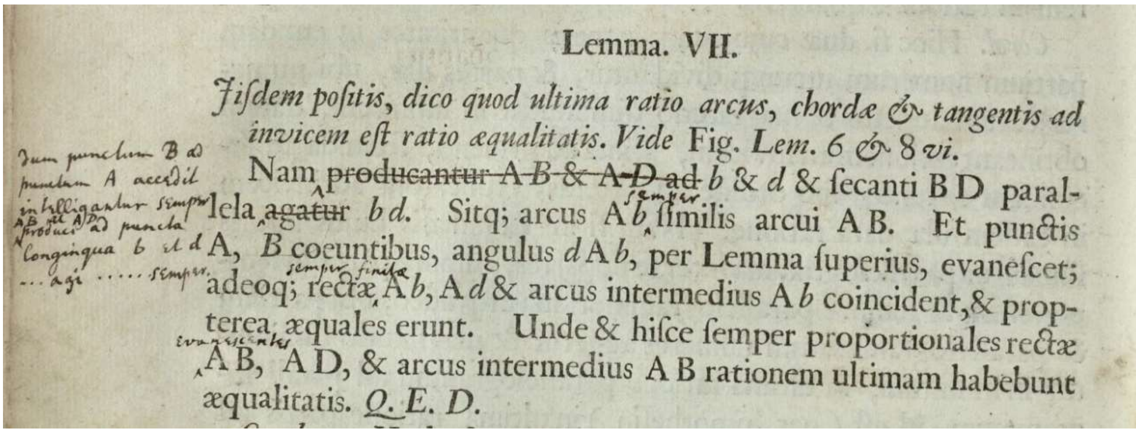
[212] *OC*, 10:148.

hoc est magnitudines non infinitae parvae," that the arc *Ab* is "semper" similar to the arc *AB*, and that in the limit points *b* and *d* "coincident." In other words, he wishes to clarify how Newton's microscope method works. It is important to note that Fatio composed these notes during his own independent study of the text, before he sat down to discuss his difficulties with Newton in March 1690. That is, he was not so baffled that he needed Newton to explain the proof to him. But although had managed to work out what Newton meant, he wanted it to be re-written much more clearly. He didn't think that the original text constituted a persuasive mathematical argument, and so negotiated with Newton over what constituted an acceptable proof. And Fatio was successful in that negotiation, because Newton decided to change the text, as this image from his annotated copy shows:


Lemma. VII.

Jisdem positis, dico quod ultima ratio arcus, chordæ & tangentis ad invicem est ratio æqualitatis. Vide Fig. *Lem. 6 & 8 vi.*

Nam ~~producantur AB & AD ad~~ *b* & *d* & secanti BD parallela ~~agatur~~ *bd*. Sitq; arcus A*b* similis arcui AB. Et punctis A, *B* coeuntibus, angulus *d*A*b*, per Lemma superius, evanescet; adeoq; rectæ A*b*, A*d* & arcus intermedius A*b* coincident, & propterea æquales erunt. Unde & hisce semper proportionales rectæ AB, AD, & arcus intermedius AB rationem ultimam habebunt æqualitatis. *Q. E. D.*

In translation, the edited text reads:

> For during the time in which the point *B* approaches the point *A*, let it be understood that the lines *AB* and *AD* are always produced to the far-off points *b* and *d*, and that *bd* is drawn parallel to the secant *BD*. And let the arc *Ab* always be similar to the arc *AB*. Then with the points *A* and *B* coming together, the angle *dAb* (by the preceding Lemma) will vanish,

> and thus the always-finite straight lines *Ab* and *Ad* and the intermediate arc *Ab* will coincide, and therefore will be equal. Whence the disappearing straight lines *AB* and *AD* and the intermediate arc *AB*, always proportional with these, will have an ultimate ratio of equality. Q.E.D.

Newton's edits do not follow Fatio's proposals exactly, but his influence is very clearly identifiable. *AB* and *AD* are "semper" extended to the "longinqua" points *b* and *d* "dum punctum *B* ad punctum A accredit". The arc *Ab* is "semper" similar to the arc *AB*, and the distinction is emphasised between the lines *Ab* and *Ad*, which are "semper finitae", and the lines *AB* and *AD*, which are "evanescentes". Newton also made extremely similar changes to the proofs of Lemmas 8 and 9 in response to Fatio's notes, as the reader may easily attest.

Gregory and Leibniz were also struck by the unusual style of the microscope proofs, but responded very differently. Here is Gregory's comment in his *Notae*:

> Non facile est conficere quare in lemmatis hujus sectionis presertim in 8vo et 9no lemmate adhibeat Auctor arcum *Ab* ejusque tangentem *Ad* et secantem rd proportionales similes et similiter positos arcum *AB* ejusque tengenti *AD* et secant *AR*, ut demonstrandum primo concludat in arcu *Ab* ejusque adjunctis, illud postea transferendo ad arcum *AB* cum pari jure et ob eandem rationem, illud primo asseruisset de arcu *AB* primario quo de arcu simili et similiter posito *Ab*.[213]

> It is not easy to establish why in the lemmas of this section, especially in the eighth and ninth lemma, the author adds the curve *Ab*, its tangent *Ad* and secant *rd* proportional, similar and similarly placed to the curve *AB* its tangent *AD* and secant *AR* [sic – he means *RD*], in such a way that he might first conclude the thing to be demonstrated in the curve *Ab* and its additions, and then transfer it afterwards to the curve *AB*; with equal

[213] RS, MS210, 1r.

justice and by the same reasoning, he might have first asserted about the primary curve *AB* that which he asserted about the similar and similarly placed curve *Ab*.

Gregory references Lemmas 8 and 9 in these lines, but his claim equally applies to Lemma 7: his view is very clearly that the microscope is not required. The enlarged construction *dArb* is redundant and adds nothing, Gregory says, because the proofs are just as valid for the original construction *DARB*. This assertion could mean one of two things: either he has misunderstood Newton's microscope argument (perhaps thinking that as *DARB* shrinks *dArb* shrinks too), or he has correctly understood the argument but thinks it unnecessary. Whichever it is he records no objection to the result, so he must have been comfortable concluding directly that ratios of the line *AB*, the tangent *AD* and the arc *AB* to each other tend to equality, even as *DARB* vanishes. But he has definitely not recognised why Newton felt the need to include the microscoped enlargement. Gregory has not acknowledged that Newton is uncomfortable establishing the ratios in the vanishing construction, and he has not explained why he thinks Newton's discomfort is misplaced.

Leibniz, too, thought that the microscope was superfluous. In his *Marginalia* alongside Lemma 9, he wrote the following:[214]

hoc idem statim dici poterat de *B*, *F*, *D*, *C*, *G*, *E* quod hic dicitur de assumtis prius *b*, *f*, *d*, *c*, *g*, *e*. itaque frustra assumuntur. Quandoque bonus dormitat Homerus.

Whether Leibniz ultimately accepted the result of Lemma 9 is moot. In his later *Notes* he attempted to generate an alternative proof using infinitesimals, and

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[214] *Marginalia*, 52.

when he was not able to do so erroneously concluded that Newton was wrong; Bertoloni Meli asserts that its inclusion in the *Excerpts* is evidence that he subsequently changed his mind, but I am not persuaded of this.[215] Either way, Leibniz's comment here clearly shows that, like Gregory, he considered the microcoping to be unnecessary. Again, it is possible that he perceived Newton's concerns over the vanishing construction but could see good reasons why they were unwarranted. If so – like Gregory – he did not articulate those reasons on paper. He did, however, record in his *Notes* that he had not had enough time to fully examine what he described as the very confusing proof ("demonstrationem perplexiorem examinare non vacat"[216]). Combined with his dismissive aside in his *Marginalia* ("Sometimes even the worthy Homer nods"), this strongly suggests that Leibniz missed Newton's point entirely.

The microscope argument is used in four of the proofs in Section 1, and most of mathematics of the rest of the book ultimately depend on these results. And yet Newton's readers did not approve the justification he gave for it in the first edition. Their reasons for doing so varied: Fatio got the gist but thought it should be substantially re-written, while Gregory and Leibniz both thought that it was redundant, at least one of them because he didn't understand it. But this variety should not obscure a very significant fact about the reception of the *Principia*: none of the three most important readers of the book was persuaded by the justification of its foundational methods in the form that it appeared in the text.

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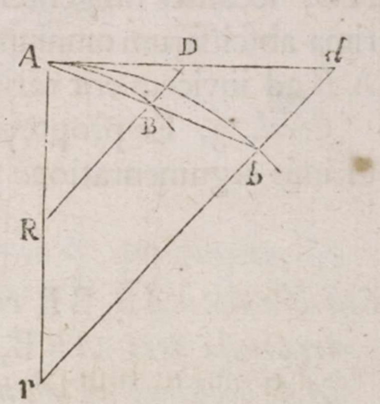
[215] *Notes*, 242, 101n13. Because the *Excerpts* overwhelmingly consist of transcriptions, the inclusion of Lemma 9 does not, as far as I can see, automatically denote assent. But I defer to Bertoloni Meli's judgement on this matter.
[216] Ibid., 229.

4.3.3 The tangent construction in Lemma 6

Newton also employed this microscope method in Lemma 6, although readers' responses were very different. Here is how Lemma 6 appears in the first edition:

Lemma VI.

Si arcus quilibet poſitione datus A B *ſubtendatur chorda* A B, *& in punčto aliquo* A, *in medio curvaturæ continuæ, tangatur a recta utrinq; producta* A D; *dein puncta* A, B *ad invicem accedant & coeant; dico quod angulus* B A D *ſub chorda & tangente contentus minuetur in infinitum & ultimo evaneſcet.*



Nam producatur *AB* ad *b* & *AD* ad *d*, & punctis *A*, *B* coeuntibus, nullaq; adeo ipſius *Ab* parte *AB* jacente amplius intra curvam, manifeſtum eſt quod hæc recta A*b* vel coincidet cum tangente A*d*, vel ducetur inter tangentem & curvam. Sed caſus poſterior eſt contra naturam Curvaturæ, ergo prior obtinet. *Q. E. D.*

If any arc *AB* (given in position) is subtended by the chord *AB*, and at some point *A* (in an interval of continuous curvature) is touched by the straight line *AD*, produced in both directions; and if then points *A* and *B* approach each other and come together; I say that the angle *BAD* contained by the chord and the tangent will be indefinitely diminished and will ultimately vanish.

For let the line *AB* be produced to *b* and the line *AD* to *d*, and with the points *A* and *B* coming together, and indeed with no part *AB* of that line *Ab* lying any more within the curve, it is evident that this straight line *Ab* will either coincide with the tangent *Ad* or be drawn between the tangent and the curve. But the latter case is contrary to the nature of curvature, and therefore the former obtains. Q.E.D.

Newton's argument is simple to grasp intuitively: with the arc *AB* held fixed, as the point *B* slides towards the point *A*, the angle *BAD* between the chord *AB* and the tangent *AD* tends towards zero, and so the chord moves up to meet the tangent. Lemma 6 thus defines the tangent to a curve as the limiting position of its chords. Newton's justification uses the enlargement *dArb*, constructed as before. He argues that, as *B* moves towards *A* along the arc *AB*, so too does *b* move up towards *d*. As it does so, and with the points *A* and *B* coming together, no part of the line *Ab* can lie below the arc *AB*: this is because the only part of *Ab* that could possibly lie below the arc *AB* is the chord *AB*, and that cannot lie below the arc if the points *A* and *B* have come together. So there are only two options: either the line *Ab* coincides with the tangent *Ad*, or the line *Ab* lies somewhere between the arc and the tangent. But the second case is "contra naturam Curvaturae," and so the line *Ab* must coincide with the tangent *Ad*. Transferring this to the smaller construction *DARB*, he can conclude that the line *AB* coincides with the tangent *AD*, as required.

Leibniz, as by now should come as no surprise, did not engage with the proof as Newton wrote it, but instead attempted to convince himself of its conclusion using infinitesimals. He did so very easily. In the diagrams in his *Notes* he dispensed entirely with the enlarged construction dArb, and wrote a handful of sentences to convince himself that "Hinc ob angulum BAC infinite parvum, differentia inter BAC evanescet." He followed this with a few lines of commentary on the discussion of the angle of contact in the Scholium at the end of the section, accepting Newton's claims but again reinterpreting them in terms of infinitesimals.[217]

Fatio, on the other hand, responded to Lemma 6 in the terms that it was written:

> p. 29. in ima pag. lege vel. Lemma 6 probatur per suppositionem ipsius lemmatis. Tota ejus demonstratio ita legi poterit expunctis inutilibus quibusdam. Nam punctis *AB* coeuntibus, nullaque adeo ipsius *AB* parte jacente intra curvam, manifestum est quod haec recta *AB* vel coincidet cum tangente *AD* cujus nulla etiam pars jacet intra curvam, vel ducitur inter tangentem et curvam. Sed casus posterior est contra naturam curvarum quae unicam in puncto *A* tangentem admittit ergo &c.[218]

> p. 29 at the foot of the page read "or Lemma 6 is proved by the assumption of the lemma itself." The entire proof of this will thus be able to be read with certain redundant bits removed. "For with the points *A* and *B* coming together, and indeed with no part of that line *AB* lying within the curve, it is evident that this straight line *AB* will either coincide with the tangent *AD* (of which indeed no part lies within the curve), or it is drawn between the tangent and the curve. But the latter case is contrary to the nature of curvature, which allows only a single tangent at the point *A*, therefore etc."

As discussed above, Fatio recognised the need for Newton's microscope argument in Lemmas 7 to 9. But he is here proposing that it is redundant in Lemma 6, and that the proof holds without the enlarged construction *dArb*. His argument is that Newton's proof depends upon the observation that in the limit no part *AB* of the line *Ab* lies within the curve ("nullaque adeo ipsius *Ab* parte *AB* jacente amplius intra curvam" in the original), whence *Ab* must coincide with

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[217] *Notes*, 227, 241–42.
[218] *OC*, 10:147–48.

Ad, and so *AB* must coincide with *AD*. But if Newton is going to use the fact that "no part *AB*" lies within the curve ("nullaque adeo ipsius *AB* parte jacente intra curvam" in his note), says Fatio, he may as well bypass the enlarged construction and infer directly that *AB* coincides with *AD*. Once he has established that in the limit there are no points on the chord *AB* that lie within the curve, he can immediately stop. There is no need to employ the larger construction.

Newton's response is extremely interesting. Immediately underneath Fatio's note, on the same document, Newton himself wrote out the following words:

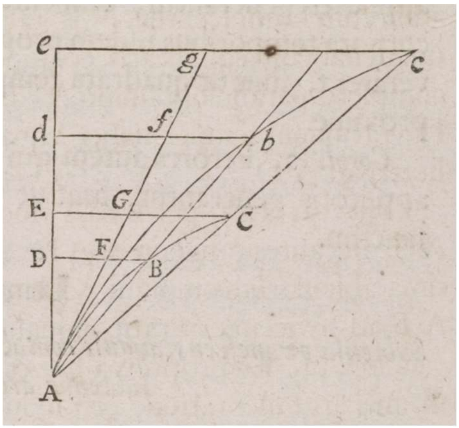
> deleatur demonstratio lemm. VI, vel legatur Nam si angulus ille non evanescit, continebit arcus *AB* cum tang. *AB* [sic: read *AD*] angulum rectilinio aequalem, et propterea curvatura ad punctum *A* non erit continua contra hypothesin.
>
> Let the proof of Lemma 6 be deleted, or let it read: For if that angle does not disappear, the arc *AB* contains with the tangent *AB* [sic: read *AD*] an angle equal to a rectilinear angle, and therefore the curvature at the point A will not be continuous, contrary to the hypothesis.

This new form of words was then transferred verbatim into his annotated copy, and thence to the second and third editions. So Newton did indeed remove the enlarged construction as Fatio advised. But he replaced his original with a completely different argument. He hasn't followed Fatio's suggestion that he stop once he has established that no part of *AB* lies within the curve: instead, he comes up with a new proof by contradiction, removing all reference to the straight line *AB*, and no longer based on the question of whether in the limit any portion of the chord lies within the curve. Maybe he rejected Fatio's argument because it is open to the objection that once *A* and *B* coincide, even though there is no portion of the chord within the curve, there is also no chord left to coincide with the tangent. It is also noticeable that both Newton and Fatio considered deleting the proof entirely. Newton's note begins "deleatur demonstratio lemm. VI, vel legatur…," and Fatio writes that "Lemma 6 probatur per suppositionem ipsius lemmatis"; presumably Fatio means that the result is implied tautologically by the restriction that point *A* is "in medio curvaturae continuae." From that point of view, Lemma 6 doesn't prove anything that isn't already contained within the meaning of the term "continuous curvature," but just provides an interpretation of the tangent as the limiting position of its chords. Either way, the final presentation of Lemma 6 was the result of a negotiation with Fatio, who did not approve of the words that were printed in the book: when challenged by one of his readers, Newton completely changed the argument in support of his conclusion.

4.3.4 Gregory's criticism of Lemma 9

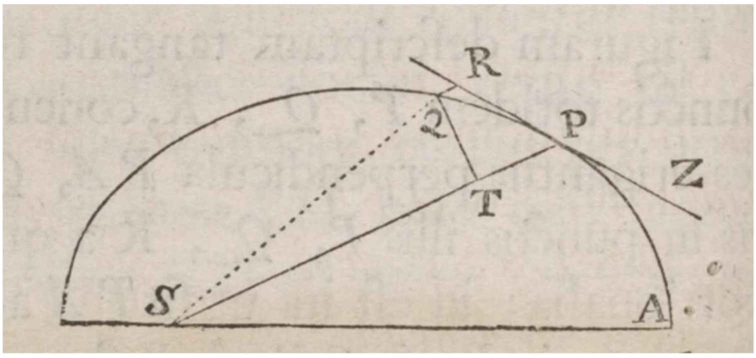
Gregory recorded in his *Notae* the startling claim that all of the orbit proofs in the opening sections of the *Principia* are compromised by a logical error that arises from Newton's application of Lemma 9. This result has a critically important role in the first sixty pages of the book, because Newton applies the mathematics of Lemma 9 to mechanical concepts in Lemma 10, which he then invokes when establishing his means of quantifying centripetal force in Proposition 6. All the remaining orbit proofs – including the inverse-square law for elliptical orbits – then depend upon Proposition 6. Gregory's bold allegation was that the result of Lemma 9 cannot be applied to Proposition 6, and thus all the orbit proofs in the *Principia* are logically invalid.

Lemma 9 concerns a line intersecting non-tangentially with a curve. Here is the diagram Newton supplies:



A fixed curve *ABC* is assumed to intersect a fixed straight line *ADEde* non-tangentially at the point *A*. The tangent *AFGfg* to the curve at *A* is also shown. Lemma 9 proves that, in the limit as *B* moves towards *A* along the fixed curve *ABC*, the area of the triangle *ABD* is proportional to the square of *AD*. Newton's proof requires a second construction *ACGE* so that he can express the result not in terms of a single triangle *ABD* in the limit as *B* approaches *A*, but as the ratio of two triangles *ABD* and *ACE* as both *B* and *C* approach *A*. He has also added the two constructions *Abfd* and *Acge*, which are enlargements of *ABFD* and *ACGE* as explained above.

The *Notae* show that Gregory did not understand the proof of Lemma 9 when he initially read it, and in the main body of his notes there is no entry between Lemmas 8 and 10. Instead, he pasted in a loose sheet after his meeting with Newton in May 1694, reporting that Newton had to explain the result to him ("Demonstratio Lem. IX prout nunc impressa, omnino intelligi nequit. Hanc tamen ab Auctore restitutam habeo."[219]) It is possible that his incomprehension was caused by the microscope structure of the proof, but he is not explicit on the matter. Whether or not this was the case, his conversation with Newton failed to convince him that the invocation of Lemma 9 is valid in the proof of Proposition 6. Here is the construction in Proposition 6:



Gregory makes the very simple objection that Lemma 9 cannot be invoked in Proposition 6 because Proposition 6 considers the tangent to a curve, but Lemma 9 does not. In Lemma 9, it is shown that the area between a curve and a non-tangential straight line is shown to be proportional to the square of the abscissa, and this is applied in Lemma 10 to show that the distance is proportional to the square of the time. In the proof of Proposition 6 this is then used to infer that the distance *QR* is proportional to the square of the time elapsed, and Newton explicitly invokes Lemma 10 in his proof. But in the construction of Proposition 6, *QR* is the displacement from the tangent *ZPR*, while the construction of Lemma 9 requires that *ADEde* is not a tangent. The result of Lemma 9, says Gregory, therefore cannot be applied to the construction in Proposition 6. The proofs of all the orbit proofs in Sections 1 and

[219] The sheet is pasted in at RS, MS210, 2.

2 – including the inverse-square law – are therefore logically invalid. Of Lemma 9, he writes the following:

> Supponit vero Demonstratio (quod et in ipsa Prop expresse supponitur ~~Rectam *AD*~~ hisce verbis se mutuo secent in angulo dato) rectam *ADE* non tangere curvam *ABC*; Et Lem. X lemmati huic IX superstructum idem supponit: Et tamen in Prop VI pag. 44 (primaria quidem cuique totus quantus, quantus est liber, inaedificatur) Lem: hocce X citatur tanquam verum, etiam tum recta tangit Curvam.
>
> The demonstration indeed supposes (which also in that proposition is expressly supposed ~~the line *AD*~~ by these words "intersect each other at a given angle") that the line *ADE* is not a tangent to the curve *ABC*; and Lemma 10 supposes the same construction as Lemma 9: however in Proposition 6 p. 44 (the principal result, indeed, and on which the entire bulk of the book is constructed) this Lemma 10 is cited as if true, yet in that case the line is tangent to the curve.

Honesty obliges me to admit that I am not certain that Gregory is correct.[220] But irrespective of its correctness, he follows through on the allegation by suggesting that Lemmas 9 and 10 are not required to prove Proposition 6. He says that it can validly be derived by alternative means:

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[220] He is right to point out that Proposition 6 concerns a tangent, while Lemmas 9 and 10 do not. But I do not think this invalidates the citation, because the two constructions are otherwise too dissimilar for this difference to be significant. In Lemma 10, Newton identifies the abscissa *AD* with time and the evanescent area *ABD* with distance, to show that, in the limit at the start of the motion, the distance is proportional to the square of the time. When he applies this result to Proposition 6, it is to infer that the distance *QR* is proportional to the square of the time. But the time in the construction for Proposition 6 is not given by the abscissa *PR*, but by the area *PSQ*. And while the displacement in Proposition 6 is given by the ordinate *QR*, the displacement in Lemma 9 is given by the area *ABD*. The two constructions are different. It therefore does not matter to Newton's logic that *ZPR* in Proposition 6 is a tangent and *ADEde* in Lemmas 9 and 10 is not, because the two lines represent different physical quantities. They are not supposed to correspond. I think Gregory is wrong to allege a logical flaw; Newton is correct to assert in his rewrite for the second edition that Proposition 6 may be proved from either Lemma 10 or Lemma 11.

> Verum quidem est posse demonstrari aliter quam per Lem. X. lineolam *RQ* (fig. Prop VI) sive spatium pervenissum esse (data vi) ut quadratum temporis. Sed tum Lemma X prout suo‸[loco] demonstratum, non debuit in Prop hac VI cum demonstratio illa supponat contrarium ei quod in Prop VI supponitur, nempe *ZPR* curvam *APQ* contingere. Verum Prop illa VI potest per Lem XI solum demonstrari, sicut in charta pag. 7 harum notarum annexa factum est.

> Certainly in truth it is possible to prove other than from Lemma 10 that the little line *QR* (fig. Proposition 6) or the space attained is (for a given force) as the square of the time. But then Lemma 10 having been proved in its place, it should not obtain in this Proposition 6, since that proof supposes the contrary to that which is supposed in Proposition 6, since without doubt *ZPR* is a tangent to the curve. In truth Proposition 6 is able to be proved by Lemma 11 alone, just as has been done in the attached sheet p.7 of these notes.

Gregory has indeed added a loose sheet to the main body of his notes concerning Proposition 6.[221] This note is headed "Prop: VI. Theor V poterit sit commode et dilucide demonstrari, deleta ea omni quae est in libro," reflecting Gregory's explicit desire to replace the proof in the first edition. The alternative he supplies is not very different from Newton's original. He expands and clarifies Newton's explanation, and then removes the reliance on Lemma 10 (and therefore Lemma 9) as follows:

> Et lineola *QR* spatium est quod corpus cadendo ab *R* eodem tempore describeret. ideoque (per Lem: X vel XI) est ut quadratum temporis si modo detur vis centripeta.

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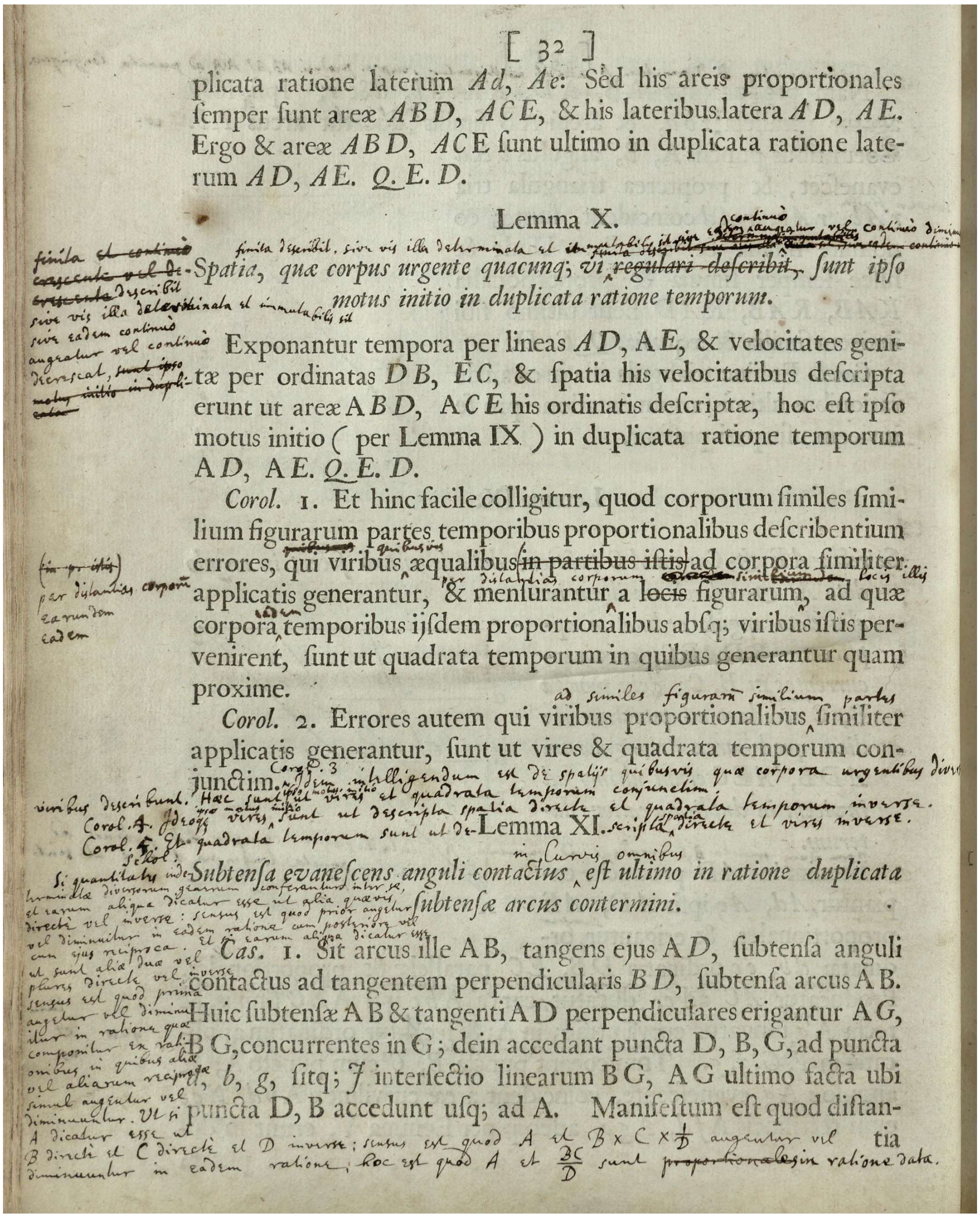
[221] This loose sheet is no longer attached to page 7, but has instead been re-bound as f.3 between Gregory's page 4 (f.2v) and page 5 (f.4r). I assume this is the note to which Gregory is here referring, although I have been unable to examine the entire document in person to verify this: at the time of writing, the library of the Royal Society is closed owing to COVID restrictions, and my analysis is based on the electronic images of the opening pages that the librarians have very kindly supplied.

That is, he simply asserts that *QR* is proportional to the square of the time "by Lemma 10 or 11." Gregory's reasons for thinking that Lemma 11 could legitimately be invoked instead are discussed. However, Newton's response to Gregory's suggestion also deserves close consideration. When Gregory visited Cambridge in May 1694, Newton showed him a radically restructured version he had planned of the opening sections of the book, including Proposition 6.[222] However, this restructured version was never published. Instead, the second and third editions contained a rewrite of the 1687 proof of Proposition 6, based on a consideration of what Newton labelled the "sagitta" of the arc (that is, the distance from the point on the arc to the chord joining two points either side, corresponding to the versine of a circle). This rewrite does not match the alternative Gregory provided, and there is no evidence that Gregory suggested the rewrite be based on the sagitta. However, it is noticeable that the rewrite that appears in later editions invokes Lemma 11 rather than Lemma 10. The only remaining reference to Lemma 10 is the sentence "Idem facile demonstratur etiam per corol. 4. lem. x." appended after the proof. So while it cannot be claimed that Gregory supplied the proof that appears in later editions, I suggest that when composing his rewrite Newton was at least in part responding to Gregory's encouragement to avoid reliance on Lemma 10. It is certainly the case that Gregory thought Proposition 6 was wrong and needed to be replaced. Newton's most attentive reader thought there was a logical flaw that undermined all of the orbital force theorems at the start of the *Principia*.

4.3.5 The application to mechanics in Lemma 10

[222] See Brackenridge, *The Key to Newton's Dynamics*, Chapters 8 and 9.

Between publication in 1687 and the release of the second edition in 1713, Lemma 10 underwent more changes than any other result in Section 1. This can be seen from the following image of Newton's annotated copy:[223]



[32]

plicata ratione laterum *Ad*, *Ae*: Sed his areis proportionales semper sunt areæ *ABD*, *ACE*, & his lateribus latera *AD*, *AE*. Ergo & areæ *ABD*, *ACE* sunt ultimo in duplicata ratione laterum *AD*, *AE*. *Q. E. D.*

Lemma X.

Spatia, quæ corpus urgente quacunq; vi ~~regulari describit~~, sunt ipso motus initio in duplicata ratione temporum.

Exponantur tempora per lineas *AD*, *AE*, & velocitates genitæ per ordinatas *DB*, *EC*, & spatia his velocitatibus descripta erunt ut areæ *ABD*, *ACE* his ordinatis descriptæ, hoc est ipso motus initio (per Lemma IX) in duplicata ratione temporum *AD*, *AE*. *Q. E. D.*

Corol. 1. Et hinc facile colligitur, quod corporum similes similium figurarum partes temporibus proportionalibus describentium errores, qui viribus æqualibus ~~in partibus istis~~ ad corpora similiter applicatis generantur, & mensurantur a ~~locis~~ figurarum, ad quæ corpora temporibus iisdem proportionalibus absq; viribus istis pervenirent, sunt ut quadrata temporum in quibus generantur quam proxime.

Corol. 2. Errores autem qui viribus proportionalibus similiter applicatis generantur, sunt ut vires & quadrata temporum conjunctim.

Lemma XI.

Subtensa evanescens anguli contactus est ultimo in ratione duplicata subtensæ arcus contermini.

Cas. 1. Sit arcus ille *AB*, tangens ejus *AD*, subtensa anguli contactus ad tangentem perpendicularis *BD*, subtensa arcus *AB*. Huic subtensæ *AB* & tangenti *AD* perpendiculares erigantur *AG*, *BG*, concurrentes in *G*; dein accedant puncta *D*, *B*, *G*, ad puncta *b*, *g*, sitq; *J* intersectio linearum *BG*, *AG* ultimo facta ubi puncta *D*, *B* accedunt usq; ad *A*. Manifestum est quod distantia

In translation, the original proof and corollaries read as follows:

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[223] Cambridge, Trinity College, NQ.16.200, 32.

> The spaces which a body describes when urged by any regular force, are at the very beginning of the motion in the squared ratio of the times.
>
> Let the times be represented by the lines *AD* and *AE*, and the velocities generated by the ordinates *DB* and *EC*; and the spaces described by these velocities will be as the areas *ABD* and *ACE* described by these ordinates, that is, at the very beginning of the motion (by Lemma 9) in the squared ratio of the times *AD* and *AE*. Q.E.D.
>
> Corollary 1. And hence it is easily concluded that the deviations of bodies describing similar parts of similar figures in proportional times, that are generated by equal forces in these parts similarly applied to the bodies, and that are measured from the places of the figures at which the bodies would arrive in the same proportional times without these forces, are very nearly as the squares of the times in which they are generated.
>
> Corollary 2. But the deviations that are generated by proportional forces similarly applied, are as the forces and the squares of the times jointly.

The modern reader can intuit this result by conceiving the diagram in Lemma 9 as a velocity-time graph on its side, with *AD* representing time, *DB* velocity, and the area *ABD* the displacement. Newton's contemporaries, however, found it less straightforward. Leibniz understood it well enough but denied that it was true because it depended on Lemma 9, whose validity he disputed. Clerke requested a fuller explanation, objecting that this result "should have been usherd in with more words." Gregory also had to work hard to fill in the gaps in Newton's justification, explaining to himself in a long note that "spatia omnia percursa sunt sicut omnia rectangula sub punctis rectae *AE* et ordinatim applicatis respectivis comprehensa haec est per Lemma 2m ut figura curvilinea

AEC". He then followed this with an example to help him clarify the meaning of Corollary 1.[224]

Gregory also reflected on whether the force under consideration was allowed to change or not, agreeing with Newton that "hoc verum est urgente quavis vi regulari an irregulari." Fatio, however, felt that "definiendum est quid per vim regularem oporteat intelligi,"[225] and his annotated copy shows that Newton did indeed do exactly that. Or, at least, he tried to: the image above records Newton's struggle to articulate what he meant by the phrase "vi regulari." He did so in two phases. First, as can be seen crossed out in the top-left of the image, he replaced "vi regualri" with "vi finita et continuo crescente vel decrescente." This edit was probably composed in London in March 1690, possibly with Fatio sitting in the room alongside him, and also appears in Locke's copy. At an unknown later date Newton further amended it to "vi finita… sive vis illa determinata & immutabilis sit, sive eadem continuo augeatur vel continuo diminuatur," which version appears in subsequent editions.

Fatio also prompted Newton to re-write the two corollaries, in which Newton lays the groundwork for the orbital force theorems in Sections 2 and 3 by deducing that the centripetal force acting on a body is measured by the deflection from its inertial path divided by the square of the time elapsed. As the modern reader may well sympathise, Fatio found the original wording difficult to parse. His editorial notes contain the following suggested alterations, relating to the final clause of each sentence:

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[224] *Notes*, 229–30, 242–43; *Excerpts*, 481, 485; Clerke to Newton, 21 Nov. 1687, *NC*, 2:496–500; RS, MS210, 1v.

[225] *OC*, 10:148.

> p. 32. l. 18. venirent sunt tum in eadem figura tum praecipue in duabus figuris inter se comparatis ut quadrata &c.
>
> Ibid. l. 21. generantur sunt tum in eadem figura, tum praecipue in duabus figuris inter se comparatis ut vires & c.[226]

That is, he suggests inserting the phrase "whether both in the same figure, or especially in two figures compared to each other" to make the meaning clearer. Newton, however, deleted these two notes from Fatio's manuscript, and chose to alter the text differently. The only change in Locke's copy is to the middle of Corollary 1, which reads "… qui viribus quibus vis aequalibus (in partibus istis) ad corpora similiter applicatis generantur, & mensurantur a locis figarum earundem…," and the image above shows that Newton made many further changes in his annotated copy. After some deliberation, Newton ended up with the following (I have marked in the changes from the original text):

> Corollary 1. And hence it is easily concluded that the deviations of bodies describing similar parts of similar figures in proportional times, that are generated by whatever equal forces ~~in these parts~~ similarly applied to the bodies, and that are measured ~~from the places~~ by the distances of the bodies from those points on the similar figures at which the bodies would arrive in the same proportional times without these forces, are very nearly as the squares of the times in which they are generated.
>
> Corollary 2. But the deviations that are generated by proportional forces similarly applied to similar parts of similar figures, are as the forces and the squares of the times jointly.

This is the wording that appears in subsequent editions. None of these changes substantively alters the meaning, and considered individually they are all minor

[226] *OC*, 10:148.

rewordings of unimportant phrases. But the difficulties Clerke, Gregory and Fatio had with this passage show that Newton's readers did not unequivocally assent to the text they found in his book: in order to approve Lemma 10, they needed to reconstruct its arguments in a clearer, more persuasive form than that the author provided.

Newton also added three new corollaries and a Scholium at the end of this result. The reader who consults the text of the third edition, or Cohen and Whitman's translation, will see that the three new corollaries are very straightforward reformulations of the first two, and the Scholium comprises a suspiciously basic explanation of his use of ratios, which on the face of it is unconnected to the rest of the result. I have not been able to establish when or why Newton chose to add them, although he may have done so as part of his reformulation of Proposition 6 in the early 1690s. Along with the later changes to "vi regulari" and Corollaries 1 and 2, they are absent from Locke's copy. It is possible that this second batch of changes was composed towards the end of Newton's stay with Fatio in March 1690, after Locke's copy had been prepared; or they might have been made during his meeting in May 1694 with Gregory, who also referred to the constancy of the force and the obscurity of the original corollaries in his *Notae*. They could also have been suggested by Cotes in preparation for the second edition of 1713; or possibly they were composed on three different occasions. But this is speculation. After Fatio's intervention, who or what instigated the second set of changes to Lemma 10 I have been unable to discover.

4.3.5 Curvature in Lemma 11 and the Scholium

Finally, readers' responses to Newton's discussion of curvature in Lemma 11 and the first paragraph of the subsequent Scholium deserve brief attention. Lemma 11 provides an argument that, in the limit at a particular point, a certain class of curves can be replaced by a circle (the "circle of curvature," or "osculating circle"). My discussion of this result will be short, since it provoked relatively little commentary from contemporary readers, partly because they found the explanation in the text comparatively easy to follow, and partly because this result came at the end of the section. None of Newton's peers raised objections to Lemma 11 that they had not already registered over the previous ten pages. Flamsteed – as discussed at the very beginning of this chapter – only engaged with Lemma 11 very superficially. Clerke objected in much the same way that he did to the other results in Section 1, arguing that replacing a curve with a circle is fundamentally unacceptable because "then the lines *evanescentes*, with wch you conclude will not be same with the *nascentes*, with wch you beganne." Perhaps surprisingly, Fatio – who showed in response to Propositions 30 and 44 that he was not confident with the techniques of radius of curvature – recorded no significant objections to Lemma 11. His only reference to this result in his editorial notes was to suggest clarifying the phrasing of Case 3, as indeed Newton did.[227]

Leibniz, according to Bertoloni Meli, "read the text superficially" and only made what he describes as a "cursory reading" of Lemma 11. I have examined Leibniz's commentary on this passage closely but still do not fully understand his views, so willingly defer to Bertoloni Meli's judgement. In the *Marginalia* Leibniz recorded brief statements of disagreement, writing "non videtur sequi" in

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[227] Clerke to Newton, 21 Nov. 1687, *NC*, 2:496–500; *OC*, 10:148.

the margin of the proof (next to Case 2), "suspectum hoc Lemma generale" next to a reference to it on page 41, and "Lemma XI generale nondum admitto" alongside another citation on page 42. He provided further commentary in his *Notes*, in which he alleged that it contradicts Lemma 9, but I cannot infer clearly the reasons for this objection. Lemma 11 is represented at length in the first set of *Excerpts,* where it is supplemented by remarks on Newton's geometrical limits, but this is no indication of assent. Bertoloni Meli asserts that on his first reading Leibniz misunderstood Lemma 11 only to apply to circles, but I am not sure I agree that the evidence supports this: I suggest that the large quantity of text Leibniz transcribed in the *Excerpts* reflects his uncertainty, and may indicate that he did not fully articulate an opinion as to the validity of the result.[228]

Gregory's *Notae* record that the last two lines of the proof of Lemma 11 in his copy were illegible, presumably owing to a printing error. Nevertheless, he correctly recreated the missing steps in Newton's argument, demonstrating that – unlike the microscope proofs earlier in the section – he could reconstruct the logic of the demonstration very easily, and did not wish to challenge it. However, he was eager to assert that among algebraic curves the result only applies to those of the second degree (which is to say, conic sections), making this observation in three successive entries. Gregory conceded that when studying astronomical orbits this limitation will not be significant, and also that this restriction is acknowledged in the Scholium, but it may have been him who encouraged Newton to make this qualification explicit by inserting the phrase "in curvis omnibus curvaturam finitam ad punctum contactus habentium" in the

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[228] *Notes*, 102, 230, 243–4; *Marginalia*, 52–54, *Excerpts*, 480–1, 483–4.

statement of the result, as appears in the second edition. Gregory also noted an apparent contradiction between Corollary 2 of Lemma 11 and Lemma 9, in which the areas of the evanescent triangles were shown to be proportional to the squares of the abscissas rather than the cubes. This may also have been the contradiction to which Leibniz was referring. This disagreement can be resolved by observing that (in modern terms) in Lemma 9 the curve is approximated by a straight line, whereas in Lemma 11 it is approximated by a quadratic, whence the areas contained will be proportional to the squares and cubes respectively. Seen in this way, the two constructions consider two different approximations. As discussed above, Gregory thought that this distinction rendered the invocation of Lemma 9 in Proposition 6 invalid, although in the second edition Newton recorded his view that it could be derived from either.[229]

One final reason for the lack of critical engagement with Lemma 11 is its relative unimportance within the logical structure of the first edition. The only significant result in the opening sections that invoked Lemma 11 was Proposition 4, but since Proposition 4 concerned motion in a circle, the claim that a non-circular curve could in the limit be approximated by a circle was in that instance surplus to requirements. This allowed Leibniz, for example, to deny the general validity of Lemma 11 but still accept the conclusion of Proposition 4, as will be shown in Chapter 6. However, Lemma 11 was of critical significance in the second edition of 1713, since Newton re-wrote Proposition 6 so that it invoked Lemma 11, to which he added two extra corollaries. All the orbit proofs in the opening sections were therefore logically

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[229] RS, MS210, 1v.

dependent on Lemma 11 in the second edition, but not in the first. Although it may have been encouraged by his conversations with Gregory, Newton's reasons for this rewrite have not been fully investigated: the role of Lemma 11 in the overall argument of his orbit theorems appears to have been more important to Newton during the years following publication than to his readers.[230]

[230] See Brackenridge, *The Key to Newton's Dynamics*, Chapters 8 and 9.

5. Responses to the new physics in the *Principia*

5.1 Introduction

Having considered readers' reactions to the new mathematical methods in the *Principia* in the previous chapter, I will now examine responses to the novel physical ideas contained in Newton's book. Taken as a whole, this is a significantly larger task, and the implications of Newton's ideas for the study of natural philosophy in the years, decades and centuries after publication is far too big a topic to be usefully engaged with here. But since my remit in this thesis is restricted to early readers of the *Principia*, my focus will only be on their immediate responses to the physical ideas expressed by Newton in his text. The *Principia* contained a number of claims about the real world and how it should be mathematised: my aim in this chapter is to assess the extent to which early readers of the book were persuaded by these claims.

The primary evidence shows very clearly that they were not. Over the course of the following sections I shall show, first, that readers challenged the Definitions and Laws as they found them articulated in the opening pages of the book; second, that they in some instances disputed the physical conclusions that Newton claimed to have proved mathematically; and third, that they were not persuaded by the way in which Newton mathematised force.

It is the discussion in this third section that will be the most significant. Hindsight judges that one of the most transformative contributions of the *Principia* was Newton's mathematisation of orbital motion in terms of centripetal rather than centrifugal forces – that is, in terms of an inward pull rather than an outward push. However, I shall demonstrate below that Newton's contemporary readers were explicitly ambivalent about the way in which he mathematised force. His peers knew that there was more than one way in which orbital motion could be mathematised, and recognised that the conclusions in the *Principia* could be reformulated using centrifugal forces (or a mixture of the two) without undermining their logical validity. Newton's revolutionary conception of a centripetal rather than a centrifugal force was not met with acceptance by his early readers, who were thus not persuaded by the fundamental physical assumptions on which Newton based his arguments. Furthermore, a close examination of the primary sources will reveal the need to reframe the established narrative of Newtonian scholarship that readers did not accept the reality of universal gravitation because it lacked a physical mechanism. The reader who is most commonly cited as objecting to universal gravitation owing to an absence of a mechanistic cause was Huygens, and it is true that he often communicated this view in his correspondence. However, this objection needs to be contextualised by the fact that there is no evidence that Huygens read any of the final four sections of Book 1 that invoked the existence of universal gravitation. Indeed, almost none of the early readers of the *Principia* read these sections: in the five years following publication, the only reader for whom there is concrete evidence confirming deliberate study of the passages in the book concerning universal gravitation was Leibniz, and even he does not appear to have attempted to verify the relevant proofs. The story that Newton's peers rejected universal gravitation owing to its lack of mechanism needs to be told in this context.

These, then, are the two important conclusions to be drawn from this chapter: Newton's readers did not give their assent to his conceptualisation of centripetal force, and they overwhelmingly did not engage with his proofs involving universal gravitation. In the last chapter we saw that the novel mathematical ideas contained in the *Principia* did not meet with the assent of its readers; as we will presently discover, neither were they persuaded by the new physical claims it made.

5.2 Challenges to the Definitions and Laws

I begin by considering readers' responses to Newton's articulation of the foundational physical concepts of mechanics, as set out in his Definitions and Laws. In doing so, a loose analogy may be drawn between the physical assumptions Newton establishes in these opening pages, and the mathematical assumptions he sets out in the method of first and last ratios in Section 1. This correspondence is by no means exact, but in both cases he articulates the conceptual basis on which the deductive structure of the proofs of the *Principia* will be founded.

From this point of view, it is noteworthy that – despite the two passages being read with apparently equal frequency – after publication Newton made markedly fewer changes to the text of the Definitions and Laws than he did to Section 1. His annotated copy shows a far smaller number of edits over these pages than in his method of first and last ratios. Moreover, those changes that he did make are generally small rephrasings of the text, rather than the substantive alterations to Newton's arguments that were considered in the last chapter. It is also the case that fewer of them can be directly identified as responses to specific feedback from early readers. Locke's copy, for example, carries very few edits in the opening pages, and those that are present generally comprise small grammatical corrections. The largest changes made for the second edition (the alterations to Definition 1 and Definition 5) are certainly missing. Fatio, whom I demonstrated in Chapter 1 to have conducted a close reading of the opening sections of Book 1 when preparing his aborted second edition, also proposed few alterations. His reading notes show that he alerted Newton to a number of typographical errors, and suggested a few changes of phrasing to isolated sentences, but he did not generally dispute the substance of Newton's assertions. The only clear exception I can find is an incorrect proposed amendment of Corollary 4 of the Laws, which Newton rightly deleted. Gregory likewise did not seek to dispute anything in the opening pages, and in the handful of entries on these passages in his *Notae* he merely explains Newton's reasoning to himself.[231]

Leibniz maintained different conceptions of mass, force, space and time from those articulated in Newton's book, and this is reflected in his readings of the opening passages. His *Notes* on Definition 1, for example, focus on Newton's neglect of an aethereal fluid, which is also addressed in his brief *Marginalia* and *Excerpts* from the Scholium after Proposition 40 of Book 2; he made a short marginal note next to Definitions 6 and 7 recognising lunar gravity as an example of a centripetal force; in his *Notes* on the subsequent Scholium, he questioned Newton's account of the rotating bucket experiment; he added a short note to his transcription of Law 2 in the *Notes* which Bertoloni Meli describes as reflecting his "difficulties in getting accustomed to Newton's

[231] Fatio's note at *OC*, 10:147 reads, "dubitari potest utrum in actionibus corporum inter se, si generentur motus circulares pergat centrum grav. in linea recta moveri". Gregory's notes on the opening pages are all on RS, MS210, 1r.

terminology"; and in the second set of *Excerpts* he added a short note to Law 3 about implications of actions inducing equal and opposite reactions throughout the universe. The many differences between Leibniz's metaphysics and that proposed by the *Principia* is a central theme of Bertoloni Meli's studies, to which the reader is directed for further analysis. That said, it strikes me that these foundational disagreements do not occupy a particularly significant proportion of the written records Leibniz made when reading the *Principia*. There are no *Marginalia* in the opening twenty-fives pages other than the single example just cited; in the first set of *Excerpts* Leibniz transcribed or paraphrased all eight Definitions and all three Laws without any note of criticism; and with the two exceptions just referenced, the same is true of both the second set of *Excerpts* and the *Notes.* On the face of it, his responses to the method of first and last ratios were much more substantial, and, as demonstrated in Chapter 4, he did at least examine closely many of the proofs in Section 1. But throughout the opening pages he engaged with Newton's text from within his own pre-existing conceptual framework. He read the Definitions and Laws to establish whether they agreed with his own metaphysics, and processed the proofs in Section 1 in terms of infinitesimals. Bertoloni Meli has shown how Leibniz's foundational physical assumptions were integral to the development of his own ideas, but he does not make the case that reading the *Principia* encouraged him to reconsider those foundational assumptions, merely that it reinforced his adherence to them. Just as with Section 1, there is no sign that the act of reading the Definitions and Laws provoked Leibniz to modify his views. At best, he recorded where he disagreed. Newton's text failed to persuade Leibniz of its own correctness, then; but neither are there many signs in his notes that Leibniz was open to the possibility of being persuaded by it.[232]

The only one of Newton's readers who made a sustained, detailed, critical engagement with the Definitions and Laws was Halley. In the editorial notes he made for draft sections of the *Principia* before publication, he made what can fairly be described as a very large number of textual changes to these passages: I have counted 28 proposed edits to the Definitions, plus a further thirteen relating to the Laws. There are, however, considerable practical difficulties in understanding the nature of these amendments, partly because the draft version of the Laws on which the notes are based no longer exists, and partly because the edits Newton subsequently made to the draft version of the Definitions are in some instances so heavy as to be illegible. It is clear that in many cases Halley was correcting Newton's grammar, or suggesting minor alterations to the phrasing, but in others his contribution appears to have been more substantive. He sought a clarification of Newton's definition of momentum in Definition 2, for example, and of centripetal force in Definition 5. He successfully petitioned to alter a number of phrases in the Scholium immediately following the Definitions, in passages concerning the nature of relative and absolute space, absolute motion, and the motion of composite bodies. Halley was also responsible for some of the final wording in the explanation of Law 1, although because the draft he read has since been lost

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[232] See especially Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz*, 97–101, 236–40, and Bertoloni Meli, "Leibniz's Excerpts from the *Principia Mathematica*," 502.

there is no way of judging the content of his contribution. The same is true of Corollary 4 to Law 3.[233]

He also prompted Newton to re-phrase his definition of the absolute quantity of centripetal force in Definition 6. The draft version that Halley saw read as follows:

> Vis centripetae quantitas absoluta est mensura ejusdem major vel minor pro potentia causae propagandi vires a centro per regiones in circuitu: uti virtus magnetica major in uno magnete minor in alio.[234]
>
> The absolute quantity of a centripetal force is the measure of it that is greater or less according to the power of the cause spreading the forces from the centre through the surrounding regions: just as magnetic force is greater in one lodestone and less in another.

Halley's response was to suggest two alternative rephrasings, either

> minor pro ratione‸[causae] fortius aut debilius eam propagantis a centro[235]
>
> [greater or] less according to whether the reckoning‸[of the cause] spreading it from the centre is stronger or weaker

or

[233] Halley's edits to the Definitions are on 94r of CUL, MS Add. 3965, and those to the Laws are on 95r. His notes on the Definitions are based on the document now at CUL, MS Dd.9.46,4–23, which Cohen labelled *LL*$_{\beta}$(1). Those on the Laws were based on an earlier version of this document, now lost.

[234] CUL, MS Dd.9.46, 5, published at Newton, *The Preliminary Manuscripts for Isaac Newton's 1687* Principia, 39.

[235] CUL, MS Add. 3965, 94r.

> vis centripetae quantitas absoluta est mensura ejusdem proportionalis causae suae eam per regiones in circuitu a centro propagantis sic virtus magnetis major est in
>
> The absolute quantity of a centripetal force is the measure of it proportional to its cause spreading it from the centre through the surrounding regions, as the force of a magnet is greater in [one lodestone and less in another.]

The draft manuscript MS Dd.9.46 shows that Newton initially implemented the first of these changes, before crossing it out and replacing it with “major vel minor pro efficacia causae eam propagantis a centro” (“greater or less according to the efficacy of the cause propagating it from the centre”), which is the wording used in the first edition. The net effect of Halley’s intervention was therefore to reduce the reliance on the exemplary phenomenon of magnetism. Newton had originally phrased the definition of the absolute quantity of a centripetal force in terms of the central “potentia” that was responsible for propagating an unspecified variety of “forces” throughout the surrounding regions; Halley encouraged him to adjust it to focus on the “efficacia” of the cause of the centripetal force, which was responsible for propagating “it”. The final wording is more abstract than the initial draft, and less dependent on the example of magnetism. The change in emphasis is away from the occult power of a centripetal force and towards its visible effect.

He also tried to change Newton’s characterisation of inertia in Definition 3. The middle sentence of the draft Halley examined, in which Newton attempted to articulate the meaning of the words “inherent force” (“vis insita”), ran as follows:

> Exercet vero corpus hanc vim solummodo in mutatione status sui per vim aliam in se impressam facta, estq; exercitium ejus sub diverso respectu et Resistentia et Impetus: respectu solo ab invicem distincta Resistentia quatenus corpus ad conservandum statum suum reluctatur vi impressae, Impetus quatenus corpus idem vi resitentis obstaculi difficulter cedendo conatur statum ejus mutare.[236]
>
> Indeed, a body exerts this force only in a change of its state brought about by another force impressed upon it, and its exercise is, in different aspects, both resistance and impetus: to distinguish each aspect from each other, resistance to the extent that the body opposes the impressed force in order to preserve its own state, and impetus to the extent that the same body, yielding with difficulty to the force of a resisting obstacle, attempts to change that obstacle's state.

Halley's suggested alteration reads as follows:

> exercitium autem ejusdem et Resistentia & impetus respectu solo ab invicem distinguuntur. Resistentia dicitur qua corpus statum suum ~~conservans~~ a vi aliqua impressa non ~~superatur~~ amittit. Impetus, quo idem corpus causam in se continet a qua mutare posset status alterius.[237]
>
> But its exercise [is] both resistance and impetus, distinguished from each other in one respect. Resistance is said to be that thing by which a body ~~conserving its state is not overcome by any impressed force~~ does not lose its state on account of any impressed force. Impetus, that thing by which the same body contains within itself the cause by which it would be able to change the state of another.

While the distinction between Halley's proposed text and Newton's original is very fine, it is not trivial. Halley is doing more than merely correcting spelling or re-ordering words: his definitions emphasise a body's active ability to either

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[236] CUL, MS Dd.9.46, 5, published at Newton, *The Preliminary Manuscripts for Isaac Newton's 1687* Principia, 39.
[237] CUL, MS Add. 3965, 94r.

resist change or impose it on another body, rather than its passive attempt to do so. As it happens, Newton did not adopt Halley's suggestion. The final text in the first edition is identical to the original draft, although the words "respectu solo ab invicem distincta" have been deleted. But Halley nevertheless thought he could improve on Newton's articulation of these foundational concepts of mechanics: he was not convinced by the suitability of the words he read.

Halley proposed other changes too. It was at his suggestion that Newton added the sentence "Whatever presses or draws something else is pressed or drawn just as much by it" to his statement of Law 3.[238] And he also left a note relating to the draft he read of Definition 1, Newton's definition of mass:

> Quantitas materiae [cujuscunq] est mensura ejusdem orta ex ~~illius~~ densitate‸$^{\text{partium}}$ et magnitudine‸$^{\text{totius}}$ conjunctim. ~~Hinc~~‸$^{\text{Quapropter}}$ in eodem vase major est‸$^{\text{mihi}}$ quantitas aeris aut pulveris, prout pulvis magis aut minus ~~compressione~~ comprimendo condensatur. ~~Hinc etiam~~ et corpus duplo densius in duplo spatio quadruplum erit, sub nomine corporis aut massae quantitatem istam ubiq. intelligo, neglecto ad medium respectu, si quod fuerit, interstitia partium libere pervadens.[239]

> The quantity of [any] matter [whatsoever] is the measure of it arisen from ~~its~~ the density‸$^{\text{of the parts}}$ and the bulk‸$^{\text{of the whole}}$ jointly. ~~Hence~~‸$^{\text{Wherefore}}$ the quantity of air or powder in the same container is bigger‸$^{\text{to me}}$, according as the powder is more or less condensed ~~by compression~~ by pressing. ~~Hence also~~ And a body twice as dense in twice the space will be quadruple; I understand this quantity by the term "body" or "mass" everywhere, with consideration to a medium freely pervading the interstices of the parts, if such a thing exists, disregarded.

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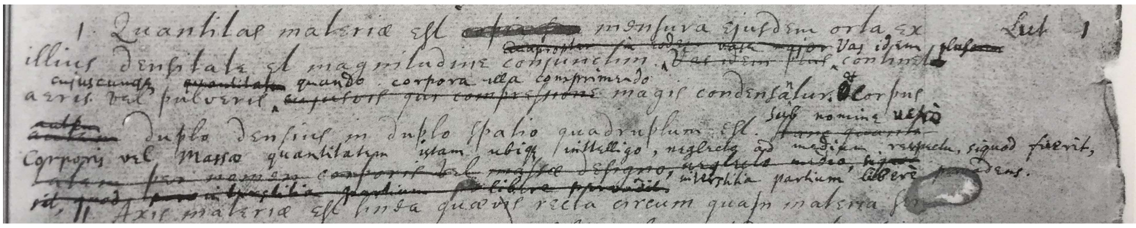
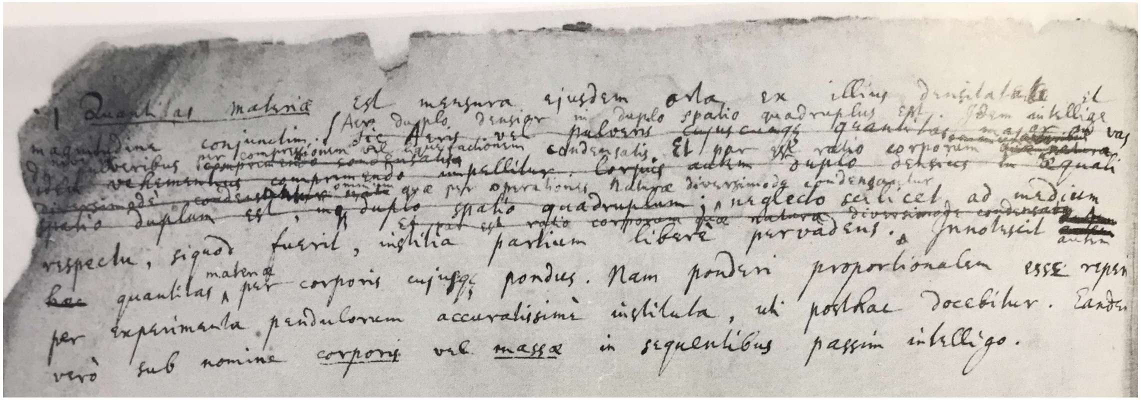
[238] See CUL, MS Add. 3965, 95r. where Halley's note reads "Quicquid premit vel trahit alterum, tantundem ab eo premitur." This sentence appears verbatim immediately after the statement of Law 3 in the printed first edition.

[239] CUL, MS Add. 3965, 94r.

It is difficult to work out exactly what the nature of Halley's proposed edit was, because the manuscript document on which it was based now looks like this:[240]





The top image represents Newton's early draft of Definition 1 on the recto side of the page, and the lower image his subsequent re-write on the blank verso opposite. Many of the edits here are illegible, and it is impossible to tell which of them were present when Halley made his reading. The reader who consults the final text will see that Newton made yet further changes before settling on the wording in the first edition. I think that the best interpretation of this evidence is that Halley's note is based on a reading of the first image above (showing the recto side of the page), on which Newton recorded the adjustments he made to his text in response Halley's comments. Newton then made further changes as

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240 CUL, MS Dd.9.46, 3–4, published at Newton, *The Preliminary Manuscripts for Isaac Newton's 1687* Principia, 37–8.

he produced the draft in the second image (the verso side), which he subsequently edited into the printed version. According to this reading, Halley made significant contributions to the final form of the definition. In the first sentence of his note, he appears to have suggested inserting the words “partium” and “totius,” to which Newton did not accede. He did, though, alter the phrasing of his second sentence in response to Halley’s note (“Quaepropter in eodem vase major…”), although we can see from the image that he subsequently deleted the phrase before adjusting it further. And Newton re-ordered his third sentence following Halley’s suggested “sub nomine corporis aut massae quantitatem istam ubiq. intelligo neglecto ad medium respectu, si quod fuerit, interstitia partium libere pervadens,” copying his suggestion directly onto his manuscript, from where these phrases made their way into the final version. Halley therefore altered sentences originally written by Newton, and by the time it reached the printed text his contribution was divided in two, but the final construction of these phrases is recognisably Halley’s.

It hardly needs to be said that these are all critically important passages in the history of the exact sciences, and the primary evidence clearly shows that Halley played an active role in their composition. He was partly responsible for the final definitions of mass, momentum, inertia, centripetal force, and relative and absolute space that appeared in the printed text of the *Principia*. He suggested these changes because he thought he could improve on the versions in the drafts that he read, which is another way of saying that he disagreed with the way in which his author had articulated his physical assumptions. He felt the need to negotiate the phrasing of their statement: Halley had a significant role in composing what are now called Newton’s Laws.

5.3 Challenges to the empirical predictions of Newton's mathematics

A second type of challenge to the physics of the *Principia* is disagreement with the empirical predictions contained within the text: instances in which readers found cause to object to physical predictions made by Newton's mathematics. In its totality, the project of first determining and then testing the measurable predictions of Newtonian theory lasted tens if not hundreds of years. The standard narrative identifies the French geodesic expedition in the 1730s as a critical turning point in the wider acceptance of Newtonianism, and Eddington's eclipse observations in 1919 as the moment of its overthrow. But my concern here is much more narrow. I wish to investigate instances in which early readers of the *Principia* immediately recognised – as they were in the act of reading it – that the text was making empirical predictions which they had reason to doubt.

Remarkably, I have only been able to find two such cases. The evidence presented in Chapter 5 showed that the criticisms Newton's readers made of his mathematical arguments were many and varied: yet the archive records only two occasions on which readers had cause to object to empirical predictions contained within the text. The first is Huygens' response to Proposition 5 of Book 2. In this result, Newton proves mathematically that (in modern terms) if a body moves against a resistance proportional to the square of its velocity, then its velocity will be inversely proportional to the time elapsed. This is very easy to prove using modern notation, and follows almost immediately from a simple differential equation. I demonstrated in Chapter 2 that Huygens convinced himself of this result not by working through Newton's geometrical argument, but by instead generating a separate proof based on infinite series. However, there was one detail in Huygens' reading notes that was not addressed in our analysis. It can be seen from the image provided in Chapter 2 that Huygens added two short paragraphs at the end of his analysis:

> Hinc autem sequitur (quod miror Neutonum non observasse) corpus horizontali motu incitatum, etiam in medio resistente infinitum spatium conficere infinito tempore. quod contra est cum resistentia est ut velocitas, ut ostendi.
>
> Illud mirabile prorsus videtur. Cogita enim globum plumbeum sub aqua in plano horizontali projectum an in infinitum spatium perget moveri?[241]
>
> But it follows from this (which I am amazed that Newton has not observed) that a body propelled with a horizontal motion, even in a resisting medium, completes an infinite distance in infinite time. Which is contrary to when the resistance is as the velocity, as I have shown.
>
> That seems utterly amazing. For consider whether a lead sphere projected in a horizontal plane underwater will continue to be moved for an infinite distance?

Huygens is, of course, correct. Newton's mathematics does indeed predict that a body moving horizontally against a resistance proportional to the square of the velocity travels a distance proportional to the logarithm of the time elapsed, which means that as the time increases without limit, so too does the distance. The body will continue to traverse an infinite distance in infinite time. Huygens is also right to point out that this does not happen when the resistance is proportional to the velocity, in which conditions the distance travelled in infinite time turns out to be finite. When the resistance is as the velocity, the distance travelled in infinite time is finite; yet when the resistance is as the square of the velocity (which would intuitively appear to make it larger), the distance travelled

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241 CH, HUG 26, 85r (=*OC*, 21:420–1).

is infinite. Huygens might well write that “Illud mirabile prorsus videtur,” because Newton’s mathematics seems to be predicting something that is physically absurd. Huygens isn’t missing anything. He is correctly pointing out why the mathematical ideal embodied in Newton’s model (to use anachronistic vocabulary) cannot obtain in the real world. A lead sphere projected horizontally underwater cannot possibly continue to move an infinite distance. The modern reader might say that Newton’s model requires further refinement; but Huygens’ more blunt observation is that the physical implication of Newton’s mathematics is obviously incorrect. Huygens here denies his assent to the *Principia*’s claims not because of his perceived invalidity of Newton’s mathematical argument, but owing to the manifest wrongness of its physical implications.

The only other such example I have found is Fatio’s reading of Proposition 37 of Book 2. This result is part of Section 7, which concerns the modern topic of fluid mechanics, and was almost completely rewritten by Newton between the first and second editions. In the version published in 1687, Proposition 37 concerns the speed with which water flows out of a container with a hole in the bottom, a problem now known as Torricelli’s Theorem. Newton argues that the speed of efflux is such that, were the emergent jet to be somehow redirected vertically upwards, it would reach a height equal to exactly half of the depth of the water in the container. When he read this claim, Fatio disagreed with it. He thought that the vertically directed jet would reach a height equal to the entire depth of the water, not half-way. As Torricelli’s Theorem confirms, Fatio was right.

To understand the manner of Fatio's objection, it will be helpful to have an overview of Newton's argument.[242] Newton imagines a vessel of water, filled to some depth *A*. There is a hole in the bottom of area *F*, out of which water is about to flow downwards. Newton draws attention to the vertical column of water within the vessel sitting directly above the hole. He considers what would happen if, rather than flowing out of the hole, this column of water were to be removed from the vessel and simply dropped to plummet in free fall in a vacuum. The central assumption on which Newton bases his argument is that the momentum that would be acquired if this column of water were to plummet in free fall for a fixed time *T* is equal to the total momentum that is in fact acquired by all the water that spurts out of the hole during the same fixed time *T*.

Newton first considers the momentum that would be acquired if the column were to descend in free-fall for a fixed time *T*. The column has a height of *A* and a cross-sectional area *F*, and so its mass is *AF*. If it were to fall from rest for a given time *T*, it would acquire some speed *V* that can be easily calculated from the Galilean laws of kinematics, and the momentum gained would therefore be *AFV*. He then considers the total momentum of all the water that spurts out of the hole during the fixed time *T*. Let the speed with which it emerges be labelled *w*. If the water spurts out of a hole of area *F* for time *T* with speed *w*, then the total volume of water that emerges will be *wFT* . Since it all has speed w, its

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[242] What follows is a sanitised summary of the argument that appears in the 1687 version of Proposition 37. While I have generally used the same symbols as Newton, I have reformulated his argument in modern terminology and notation for ease of comprehension. The reader who wishes to examine the original is directed towards the translation in Buchwald and Cohen, eds., *Isaac Newton's Natural Philosophy*, 302–04. They are warned against consulting Proposition 37 in any modern translation based on the third edition, in which Section 7 bears little relation to the first.

total momentum will be w^2FT. Invoking Newton's central assumption that the momentum that would be acquired if the column of water were to plummet in free fall for a fixed time *T* is equal to the total momentum that is acquired by all the water that spurts out of the hole during the same fixed time *T*, this gives

$$AFV = w^2FT$$

The *F*'s cancel, and the Galilean laws of kinematics will easily generate an expression for *V* in terms of *T*. In modern notation, everything simplifies to

$$w^2 = gA$$

where *g* is the gravitational acceleration constant. Finally, Newton considers what would happen if the emergent jet were somehow to be directed vertically upwards. The greatest height reached by a projectile is proportional to the square of its velocity: the modern reader will intuit this from energy considerations, whereas Newton simply writes "uti notum est". Both Newton and the modern reader are therefore able to reach the conclusion that a jet with velocity $w = \sqrt{gA}$ will reach a height equal to $A/2$. Thus the speed of efflux from the hole is such that, were the emergent jet to be somehow redirected vertically upwards, it would reach a height equal to exactly half of the depth of the water in the container, as required.

Modern fluid dynamics shows that Newton's conclusion is wrong. According to Torricelli's Theorem, the speed of efflux from the hole will be $\sqrt{2gA}$, and an upturned jet will reach the entire height *A* of the water. Torricelli's Theorem is properly proved from the Bernoulli equation, but its result can be derived algebraically by equating the gravitational potential energy lost by a particle of water as it falls from the top surface of the water down to the hole with the kinetic energy it gains in rising back up. Either demonstration is ultimately founded on the conservation of energy. Newton's analysis, on the other hand, is based on the assumption that the momentum gained by the emerging jet of water in a given time is equal to the momentum that would be gained by the column of water directly above the hole if it were to descend in free fall for the same amount of time. This assumption turns out not to be true, but I have not seen any indications in the secondary literature that this should have been obvious to him in 1687.[243] The important point is that the erroneous factor of $1/2$ in Newton's answer is not the result of a computational mistake, but arises from a completely different (and, we would say in hindsight, wrong) conceptual approach to the problem.

Fatio also disagreed with Newton's conclusion. He correctly thought that the height reached by such an upturned jet would be equal to the entire depth of the water, not half. He recorded his objections in three places in his first edition. In the margin next to the final paragraph of Newton's explanation on page 331, he wrote the following note:

> Newtonum ab erroribus in hac propositione contentis nullatenus liberare potui, quam per Experimentum; constructo scilicet Vase ad hunc ipsum usum destinato.[244]

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[243] See the analysis by George E. Smith in Buchwald and Cohen, eds., *Isaac Newton's Natural Philosophy*, 249–313.
[244] Bodleian, Arch. A d.37, 331.

> I have not by any means been able to free Newton from the errors contained in this proposition, other than through an experiment; obviously with a vessel having been constructed, designed for this very purpose.

He also made the following note on a loose sheet of paper, now inserted between the pages of his copy:

> dimidiam aquae altitudinum. Corrige et lege totam aquae altitudinum. Newtonum nostrum ab hoc errore vix liberare potui, idque facto demum experimento opere vasis quod conficiendum curavi.[245]

> "half the water's height." Correct and read "the total height of the water." I have been able to free our Newton from this error with difficulty, and in the end by an experiment made with the effect of a vessel which I took care should be performed.

And immediately below this, on the same sheet of loose paper, he wrote the following commentary on Proposition 37:

> Tota haec demonstratio fallax est ejusque conclusio falsa. Aqua enim effluens, ut notissimum est, si ejus motus sursum vertatur ascendet, in spatiis non resitentibus, ad ipsam superficiei altitudinem non autem ad altitudinem‸[illam] dimidiam. ‸[Cujus Propositionis] Demonstratio ex eo pendet quod si motus in aqua a gravitate genitus sursum vertatur donec aquae particulae altius ascendere non possint debeat totius aquae centrum gravitatis in eadem altitudine ac ante motum reperiri Vide quae de simili motus conversione‸[sursum] [Cl.] Hugenius in Horologio oscillatorio demonstravit Quae‸[Hugenii] demonstratio ad fluida non minus quam ad solida corpora extenditur Fluida enim ex exiguis solidis particulis

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245 See the loose sheet of paper inserted at Bodleian, Arch. A d.37, 332. This note relates to a line in Proposition 38 which invokes the result of Proposition 37.

componi versimile est, ac proinde nihil aliud esse quam plurium solidorum corporum congerium.[246]

This entire demonstration is fallacious and its conclusion is false. For the water flowing out, as is very notable, if its motion were to be turned back up, will ascend (in non-resisting spaces) to the very height of the top, not to half that height. The demonstration‸of which proposition depends upon this, that if the generated motion in the water were to be turned back up by gravity, as long as the particles of water are not able to ascend higher, the centre of gravity of all the water should be found again at the same height as before the motion. See those things that the famous Huygens has demonstrated about a similar upwards change of motion in the *Horologium Oscillatorium*. Which demonstration of Huygens is extended to fluids no less than to solid bodies. For it is apparently true that a fluid is composed of small solid particles, and therefore is nothing other than rather many solid bodies joined together.

It is clear that Fatio is forcefully recording his disagreement with Newton's conclusion. It is equally clear that he was right to do so. But it is the manner of his objection that is of most interest. First, Fatio and Newton must have had a free and frank exchange of views about Proposition 37, presumably face-to-face. Fatio did not tentatively suggest that Newton reconsider the precise wording of a nuanced passage. He boldly asserted that "Tota haec demonstratio fallax est ejusque conclusion falsa." He was able to convince Newton of his error, but only after a struggle ("vix"), which appears to have been protracted ("nullatenus"). Fatio strongly disagreed with what he read, and Newton's initial response was to defend it with equal vigour. And second, the grounds of Fatio's disagreement, and the type of evidence he supplies in his defence, are instructive. Newton's original argument in Proposition 37 may be

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[246] See the loose sheet of paper inserted at Bodleian, Arch. A d.37, 332.

described as mathematical: he establishes a mathematical model of the physical set-up (to use anachronistic language), and then supplies a mathematical argument to deduce the conclusion that if the emergent jet were somehow to be redirected vertically upwards, it would reach a height equal to exactly half of the depth of the water in the container. Fatio rebuts this mathematical argument with empirical evidence. The water does not reach half the depth of the water in the container, says Fatio, but to the very top – and he knows this because he has performed the experiment. Indeed, he has conducted the test with a specially constructed vessel, built expressly to verify this fact. It is reasonable to infer from the second note above that he performed the experiment for Newton personally, in front of his very eyes, probably in 1690. It is certainly the case that Newton admitted defeat, because he deleted the word "dimidiam" from "dimidiam aquae altitudinem" in his annotated copy. In other words, Fatio disproved Newton's mathematical argument by directly presenting him with empirical evidence to the contrary. In the previous example Huygens intuited that Newton's mathematical argument must be wrong because it led to an empirical prediction that is absurd; in this instance Fatio demonstrates that Newton's mathematical argument must be wrong because it leads to an empirical prediction that, as a matter of experimental fact, he can show to be untrue.

Fatio, like Huygens, did not attempt to identify the flaw in Newton's mathematics. Newton went to great lengths to do that himself, and after the publication of the first edition completely rewrote not only Proposition 37 but almost all of Section 7. This overhaul is the subject of George Smith's study

"Fluid Resistance: Why Did Newton Change His Mind?"[247] But because he does not know of the existence of Fatio's commentary, Smith does not acknowledge the answer to his rhetorical question: Newton changed his mind because Fatio performed an experiment to demonstrate that he was wrong. This demonstration caused Newton not just to rephrase the passage in question, but to re-examine and re-write the entire section. Fatio's intervention may alternatively be viewed in the context of the 250-year-long project to subject Newton's system of mathematical physics to experimental confirmation. Fatio's test of Proposition 37 in 1690, using a vessel that he constructed specially for that purpose, deserves to be acknowledged as the first entry in the long story of empirical verification of Newtonianism – a story which passes through La Condamine in 1749 and Cavendish in 1798 and finally ends with Eddington in 1919. The Swiss mathematician performed the first ever experimental test of the predictions Newton made in the *Principia*, and he showed them to be false.

5.4 The diversity of responses to Newton's analysis of force

My final consideration is of readers' responses to the ways in which Newton mathematised force in the *Principia*. The standard narrative in the secondary literature is that Newton's peers accepted his mathematical analysis of planetary forces, but lamented his failure to provide a mechanistic cause for universal gravitation. The primary sources demonstrate that, while grounded in some evidential truth, the emphasis of this narrative is misplaced. On the contrary, there was a plurality of responses to Newton's analysis of force in the *Principia*, just as there was to his mathematical methods. Rather than present

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[247] Smith, "Fluid Resistance: Why Did Newton Change His Mind?". See also Chapter 9 in Buchwald and Cohen, eds., *Isaac Newton's Natural Philosophy*.

this variety thematically, I have chosen to consider the responses of individual readers in turn. However, I wish to highlight four particularly important variables, which can be tracked through each of the examples that follow. These four factors are interdependent and inextricably interlinked, and in each individual case they are all present to a greater or lesser degree.

The first variable is the importance a given reader attached to the need for a mechanism which could account for the physical cause of gravity, which is already heavily emphasised in the traditional narrative. Huygens is commonly cited as an important reader who could not assent to the existence of universal gravitation owing to its lack of physical mechanism. Fatio also gave considerable thought to a causal mechanism for gravity. But it is very important to note that the mathematics and the physics cannot be separated cleanly: in Leibniz's case the physical assumptions he made about the mechanistic cause of gravity determined the mathematics he chose to analyse it with.

Following from this, the second variable is the awareness of individual readers that there is more than one way to mathematise force. In the section below on Gregory, I will outline the two distinct approaches to the mathematical analysis of force that Newton used in the first edition of the *Principia*, and note that he developed a third during the 1690s. Leibniz developed a fourth. Importantly, all four can be used to derive the inverse square law. None of these, therefore, should be seen as the "correct" method of analysis to explain orbital motion. There were many available approaches, as some of Newton's readers acknowledged. Leibniz's system demonstrates his awareness of this plurality, and Gregory was able to switch flexibly between the alternatives.

The third variable is the extent to which Newton's readers were persuaded by his rejection of centrifugal force in favour of his new conception of centripetal force. It will be clear from the primary evidence that overwhelmingly they were not. Huygens in his correspondence was notably unwilling to relinquish his conception of centrifugal force. Leibniz's mathematisation of force consisted of a mixture of centripetal and centrifugal force. And Gregory, remarkably, stated very clearly his view that all of Newton's orbital force proofs in Sections 2 and 3 of Book 1 could in principle be rewritten in terms of centrifugal rather than centripetal forces. In this way he accepted the validity of Newton's mathematical arguments without committing to his ontological assumption of the reality of centripetal forces. In short, although the retrospective narrative is that Newton's realisation of centripetal force was one of the major conceptual achievements of the *Principia*, the primary evidence shows that his contemporaries were not persuaded by it at the time of publication.

The fourth and final variable is extremely straightforward: it is simply to acknowledge that Newton's peers did not read large sections of his book, and therefore commonly formed their opinion about the physical reality of forces without having read all the mathematical arguments Newton put forward in their favour. In this respect, it is important to emphasise the distinction between gravitation as a centrally directed pull on (for example) the earth towards the sun, and universal gravitation as a mutually attractive force pulling both the earth towards the sun and the sun towards the earth. The derivations of Kepler's Laws and the inverse square law in Sections 2 and 3 of Book 1 assume only a centrally directed gravitational pull, and as outlined in Chapter 1, these passages were frequently studied by Newton's peers. But these opening sections do not anywhere refer to the existence of universal gravitation. It is only towards the end of Book 1, in Sections 11–14, that Newton considers what will happen if the orbiting body pulls back on the body around which it orbits. Indeed, Newton opens Section 11 with a long paragraph articulating exactly this distinction, and explaining that over the following pages "pergo motum exponere corporum se mutuo trahentium." But few of Newton's early readers ever saw this passage. Almost none of them studied the subsequent sections in any detail. Halley made brief comments on a few passages; Clerke reported that he had tried but struggled to understand Section 11; Flamsteed mentioned it once in a letter from 1695; Leibniz appears to have read Proposition 66 closely and skim-read the rest, but without checking the mathematics; and Gregory only got to the end of Book 1 in his second phase of reading at the end of 1692.[248] This means that there is no evidence that any readers undertook a detailed study of Newton's mathematical demonstrations concerning universal gravitation in the five years following publication. Most significantly, the two readers commonly cited by the secondary literature as having bemoaned the lack of a causal mechanism never read these results. The narrative that contemporaries opposed universal gravitation on the grounds of its physical improbability needs to be contextualised by the apparent fact that they did so without having studied the mathematical arguments in favour of its existence.

5.4.1 Fatio's causal speculations, independent of his study of the text

The first of these two readers is Fatio. I have already demonstrated that Fatio engaged with the *Principia* by dutifully working through many of the proofs in

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[248] CUL, MS Add. 3965, 99; Clerke to Newton, 7 Nov. 1687, *NC*, 2:491–96; Flamsteed to Newton, 29 Jan. 1695, *Correspondence*, 4:78; *Excerpts*, 491–99.

order, discussing them with their author, seeking clarification, challenging, and suggesting improvements. I showed in Chapter 4 that Fatio engaged particularly critically with Newton's articulation of geometrical limits in his method of first and last ratios. It is notable therefore that there is no record of an equivalent negotiation concerning Newton's mathematisation of force. There are many instances of Fatio querying or tweaking Newton's proofs, but – unlike Gregory or Leibniz, as will shortly become evident – at no stage did he question the fundamental aspects of his analysis of orbital motion.

Instead, Fatio's main concern with the nature of force in the *Principia* was to establish a physical mechanism for gravitation. In the late 1680s and early 1690s he proposed a number of causal mechanisms, both in private correspondence and to the Royal Society. He first addressed the Royal Society on the cause of gravity on 27 June 1688, five weeks after being nominated as a member, when he proposed a mechanism based on something like a Cartesian vortex, with orbital motion maintained by swirling particles of aether. Almost two years later, in February 1690, he returned to Gresham College to read the abstract of his tract *De la Cause de la Pesanteur*, which he submitted in full the following month. This provided a mechanism not just for centripetal orbital forces but for universal gravitation, proposing that the movement of aethereal corpuscles between two bodies could account for their being drawn towards each other according to an inverse-square law. Fatio continued to revise this theory for the next fifty years. He communicated it to Leibniz, regularly discussed it in correspondence with Huygens, and (almost certainly wrongly) thought that Newton approved of it.[249] However, a comparison with the chronology presented in Chapter 1 shows a disjunction between the development of Fatio's physical theories and his readings of the *Principia*. His presentation of 1688 was almost certainly made before he had engaged in any meaningful study of the text. His initial version of the tract *De la Cause de la Pesanteur* was composed shortly before Newton's stay with him in London in May 1690, and must therefore have been written prior to their discussion of the proofs in Sections 1–4 of Book 1, and before Fatio worked through Sections 5–9. Furthermore, there is no evidence that he ever studied the mathematical results concerning universal gravitation in Sections 11–14. His reading notes and marginalia strongly suggest that his programme of careful study came to a halt before he reached these passages. So Fatio did not develop a mechanism for universal gravitation because he had been persuaded of its physical reality by the mathematical arguments Newton provided: he cannot possibly have been, because (so far as we can tell) he never read them. Rather, he appears to have made his physical speculations independently of his study of the text. Fatio did accept the existence of mutual attractive forces, but the primary evidence shows that he must have been convinced of this by some means other than studying Newton's book. It wasn't reading the *Principia* that persuaded Fatio of the physical reality of universal gravitation.

5.4.2 Huygens' emphasis on Cartesian vortices and centrifugal motion

The second reader whose concern for a causal mechanism is routinely emphasised by the literature is Huygens. The received narrative that Huygens

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[249] See the account in Gangebin "De la Cause de la Pesanteur."

could not accept action at a distance and so rejected the physical reality of universal gravitation is certainly borne out by the primary evidence. He labelled Newton's idea in a letter to l'Hôpital "le principe peu vraisemblable de l'attraction,"[250] and its improbability is a recurrent theme of his correspondence with Leibniz, particularly for the two years from the summer of 1692.[251] In November 1690, he wrote the following:

> Pour ce qui est de la Cause du Reflus que donne Mr. Newton, je ne m'en contente nullement, ni de toutes ses autres Theories qu'il bastit sur son Principe d'attraction, qui me paroit absurde, ainsi que je l'ay desia temoignè dans l'Addition au Discours de la Pesanteur. Et je me suis souvent etonnè, comment il s'est pu donner la peine de faire tant de recherches et de calculs difficiles, qui n'ont pour fondement que ce mesme principe. Je m'accommode beaucoup mieux de son Explication des Cometes et de leur queues; et quoyque la chose ne soit pas sans cette grande difficultè, que vous remarquez fort bien, je ne trouve encore rien de meilleur que ce qu'il en dit, qui vaut mieux incomparablement, que ce qu'en a imaginè des Cartes.[252]

> As for the Cause of the Tides given by Mr. Newton, I am by no means satisfied with it, nor with all his other Theories which he builds on his Principle of attraction, which seems absurd to me, as I have already testified in the supplement to the Discourse on Gravity. And I have often been amazed, how he could have taken the trouble to do so much research and difficult calculations, which are based only on this same principle. I get along much better with his Explanation of Comets and their tails; and although the thing is not without this great difficulty, as you

[250] Huygens to l'Hôpital, 29 Dec. 1692, *OC* 10:354.
[251] See, for example, Leibniz to Huygens, 11 Apr. 1692, *OC,* 10:283–6; Huygens to Leibniz, 11 Jul. 1692, *OC,* 10:296–9; Leibniz to Huygens, 26 Sep. 1692, *OC,* 10:316–21; Huygens to Leibniz, 12 Jan. 1693, *OC*, 10:383–9.
[252] Huygens to Leibniz, 18 Nov. 1690, *OC*, 9:536.

observe very well, I still do not find anything better than what he says about it, which is incomparably better than what Descartes imagined.

The “Discours de la Cause de la Pesanteur” to which he refers was published at the start of 1690 as an appendix to his “Traité de la lumière,” and proposed a mechanism for a centrally directed force that would maintain a planetary body in an orbit. As demonstrated in Chapter 1, Huygens had by this stage already undertaken all the detailed study of the proofs of the *Principia* of which evidence remains, and his “Discours” does not attempt to supply a mechanism for mutual attraction, only a centrally-directed force. It seems that Huygens therefore denied universal gravitation without reading Newton’s arguments in favour of it.

Huygens’ reference to Descartes in this letter is representative of a wider habit, and the overall impression from his correspondence is that Huygens considered the primary achievement of the *Principia* to be its refutation of Cartesian vortices. In a short note he made about orbits in December 1688 he wrote that

> Hasce omnes difficultates abstulit Clar. vir. Neutonus, simul cum vorticibus Cartesianis; docuitque planetas retineri in orbitis suis gravitatione versus solem. Et excentricos necessario fieri figurae Ellipticae.[253]
>
> The great Mr Newton has removed all of these difficulties, along with Cartesian vortices; he has shown that the planets are held in their orbits by gravitation towards the sun, and that eccentric orbits necessarily become elliptical figures.

In a note on comets made in 1689 Huygens recorded that

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[253] *OC*, 21:143.

> Je suis maintenant presque du sentiment de M^{r}. Newton qui veut que les Cometes tournent en des Ellipses fort oblongues autour du Soleil, qui fait l'un des foiers. Cela devient probable apres qu'il a ostè les tourbillons de des Cartes, qui d'ailleurs ne s'accommodoient point avec plusieurs phenomenes des mouvements planetaires.[254]

> I am now almost of the opinion of Mr Newton who wants the Comets to turn in very elongated Ellipses around the Sun, which is one of the foci. This becomes likely after he has set aside the vortices of Descartes, which, moreover, do not accommodate themselves with several phenomena of planetary movements.

And in his exchanges with Leibniz his concern is to discover from his correspondent "si vous n'aviez pas rejettè les Tourbillons de des Cartes apres avoir vu le livre de Mr. Newton."[255]

It is also noticeable that Huygens did not reject the language of centrifugal forces in his correspondence, and did not appear to register that Newton had done so either. This is demonstrated in a letter about the nature of gravity to Leibniz in the summer of 1692, which is suffused with the vocabulary of "la force centrifuge" and "la vertu centrifuge."[256] In a letter from early 1690 quizzing Leibniz on his own system of planetary motion, after repeating his view that Cartesian vortices are "superflus," Huygens again asked his correspondent what he had made of "le Systeme de Mr. Newton où le mouvement des Planetes s'explique par la pesanteur vers le Soleil et la 'vis centrifuga,' qui se contrebalancent."[257] This misrepresentation of Newton's analysis is revealing, and, confirms the overall pattern of Huygens' engagement with the *Principia*. He

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[254] *OC*, 19:310.
[255] Huygens to Leibniz, 24 Aug. 1690, *OC*, 9:470–3.
[256] Huygens to Leibniz, 11 Jul. 1692, *OC*, 10:296–304.
[257] Huygens to Leibniz, 8 Feb. 1690, *OC*, 9:366–368.

undertook little detailed examination of the book, and processed its claims in terms of his own pre-existing conceptual framework, not the one Newton presented: he verified the limit proofs by recreating them with infinitesimals, and judged its dynamics in terms of Cartesian vortices and centrifugal forces.

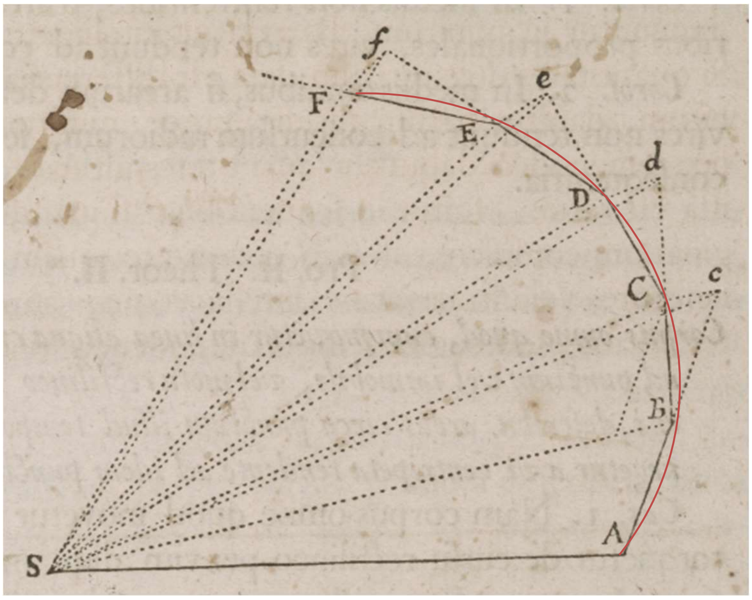
5.4.3 Gregory's analytical flexibility

Gregory's aim in studying the *Principia* was to verify Newton's proofs rather than to establish a causal mechanism for gravitation, and his *Notae* scrutinise the integrity of the book's mathematical arguments more than the viability of his physical conclusions. His comments on the mathematics, however, are highly revealing of his approach to the physics, because they show that he recognised that there was more than one way of mathematising force, and more than one ontology consistent with Newton's analysis. As to the ontology he favoured – and in particular whether he thought the forces on a body in orbit should be viewed as centripetal or centrifugal – Gregory appears in his *Notae* to have remained explicitly non-committal.

Gregory's awareness that force could be mathematised in more than one way reveals itself in the *Notae* because Newton himself tackled the problem of orbital motion using two distinct methods in the *Principia*. These are usually labelled the "Polygonal" and "Parabolic" approximations respectively.[258] In the polygonal approximation, a curved orbital path is approximated by an inscribed polygon. The orbiting body is considered to move along the rectilinear sides of the polygon in turn with constant velocity, and at each vertex (all of which lie on the approximated curve), it receives an instantaneous force which immediately

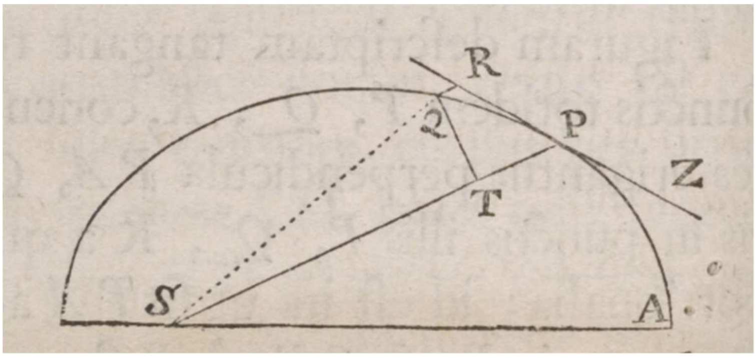
[258] See Brackenridge, *The Key to Newton's Dynamics*, 24–35. Brackenridge also explains a third method, the "circular" approximation, which was employed in the second edition of 1713.

alters its direction and speed, such that it continues its motion with a new constant velocity along the next side of the polygon. The polygonal approximation is therefore intrinsically associated with a conception of force as an instantaneous impulse, or “blow”; and it decomposes the motion into elements of constant velocity. The polygonal approximation for orbital motion was first used by Newton in the 1660s, and provided the foundation for his early studies on circular motion, but it appears very rarely in the *Principia* itself. Easily the best-known instance is Proposition 1 of Book 1, which may briefly be considered by way of an example. Proposition 1 proves Kepler’s area law, that any body moving under the action of a centrally-directed force will sweep out equal areas in equal times. Here is the construction Newton uses for the proof:



The body moves from *A* to *F* along the red orbital path (which is not marked on the printed diagram in the book), and the centre towards which the forces are directed is labelled *S*. The curve is approximated by the inscribed polygon *ABCDEF*, and the body is considered to move rectilinearly along each of the sides *AB*, *BC*, *CD*, etc. in turn. At each of the vertices *B*, *C*, *D*, etc. the body receives an instantaneous impulsive force directed towards *S*, such that it is diverted along the next side of the polygon. The magnitudes of the impulsive forces are represented by the displacements *Cc*, *Dd*, *Ee* etc. It is a simple matter in Proposition 1 to prove geometrically that the areas of the triangles *SAB*, *SBC*, *SCD* etc. are all equal, and thus that Kepler's area law must hold for any body orbiting under the action of a centrally-directed force.

In the parabolic approximation, on the other hand, primary importance is given to the tangent to the curved orbit. At any given moment in the orbit, consideration is given to the tangential path the body would continue along if there were no centripetal force. The centripetal force has the effect of deflecting and accelerating the body away from this tangent, and the size of this accelerated deflection can be used to measure the size of the force. This deflection is computed using Galileo's laws of kinematics: the centripetal force is considered to act continuously and accelerate the body away from the tangent, and the laws of kinematics dictate that it thus moves in a parabolic arc. We notice that the parabolic approximation is therefore intrinsically associated with a conception of force as a continuous pull, rather than the succession of impulses implied by the polygonal approximation. The majority of the orbit proofs in the first edition of the *Principia* employ this parabolic approximation. For example, Proposition 6 concerns a body moving in any curved orbit whatsoever, under the action of a centrally directed force. Here is the diagram Newton uses:



The body at point *P* is understood to be orbiting along the curve anticlockwise around the point *S*, towards which the force is directed. If this centripetal force were not to act, the body would continue along the tangent towards *R*. But the centripetal force does act, and is considered to pull continuously on the body parallel to *PS* as it moves away from *P*, deflecting the body from the tangent such that it arrives at the point *Q*. Newton uses the Galilean laws of kinematics to compute the size of the deflection *QR*; and because he is using the Galilean laws of kinematics and the force is considered to be a continuous pull on the body parallel to *PS*, the orbital curve between *P* and *Q* is approximated by a section of parabolic arc.

Most of Newton's demonstrations employ the parabolic approximation, and so most of the *Notae* show Gregory successfully assimilating this style of proof. However, Newton's Scholium after Proposition 4 of Book 1 is unusual, and Gregory's commentary on this result demonstrates us that he was able to switch between the two methods of analysis. Proposition 4 concerns the centripetal force required to maintain motion in a circle. In modern terms, it proves that $F = v^2/r$, and it does so using the standard parabolic approximation. Uniquely among Newton's proofs, however, the short Scholium that follows contains an alternative derivation of the same conclusion using a polygonal analysis. In translation, here is the final paragraph of the Scholium in the first edition:

> The preceding results can also be demonstrated in the following way. In any circle, suppose that a polygon of any number of sides is described. And if a body moving with a given velocity along the sides of the polygon is reflected from the circle at each one of its corners, the force with which it impinges on the circle at each reflection will be as its velocity, and therefore the sum of the forces in a given time will be as that velocity and the number of reflections jointly, that is (if the polygon is specified) as the length described in that given time and the same length divided by the radius of the circle jointly, that is as the square of that length divided by the radius. And therefore if the polygon, with its sides diminished indefinitely, coincides with the circle, it will be as the square of the arc described in the given time divided by the radius. This is the force with which the body urges the circle, and the opposite force with which the circle continually repels the body toward the centre, is equal to this.

Newton's explanation here deserves brief consideration. The penultimate sentence ("ut quadratum arcus dato tempore descripti applicatum ad radium") recognisably contains the v^2/r that is to be derived, and Newton is clearly establishing a polygonal approximation in this Scholium, rather than the parabolic approximation used in the main proof. Following through sentence by sentence, his claim that the force with which the body strikes the circle at each vertex of the polygon is proportional to the velocity is intuitively plausible to a modern reader, if force is thought of as an instantaneous impulse proportional to the change of momentum. The sum of all these forces in a given time will therefore be proportional to the product of the velocity and the number of reflections. But the argument in the text then makes a leap: it is not obvious why the number of reflections is proportional to the length described in a given time divided by the radius. Once this is granted, the v^2/r result follows easily. But Newton does not explain why this step holds.

His reading notes show that this also puzzled Gregory. This is what he wrote in his *Notae*:

> Esse vero numerum reflectionum ut longitudo applicata ad radium ita ostenditur
>
> tot sunt reflexiones quot sunt latera polygoni dato tempore percursa, sed ob polygonum regulare, latus est ut radius, quare latera‸sive longitudo in dato tempore sunt ut radius et numerus laterum conjunctim id est ut radius et numerus reflexionum conjunctim, id est longitudo est ut rectangulus sub numero reflexionum et radio unde longitudo applicata ad radium est numerus reflexionum[259]
>
> That the number of reflections is indeed as the length divided by the radius is shown as follows.
>
> There are as many reflections as there are sides of the polygon run through in a given time; but on account of it being a regular polygon, the side is as the radius, hence the sides‸or total distance in a given time are as the radius and the number of sides jointly, that is as the radius and the number of reflections jointly, that is the total distance is as the product of the number of reflections and the radius, whence the total distance divided by the radius is the number of reflections.

Gregory has here provided the missing explanation. In a given time, the number of reflections is obviously equal to the number of sides of the polygon run through. But because it is a regular polygon, if the size of the circle is varied as everything else is kept the same, the length of each side is proportional to the radius. The total distance described along the circumference of the polygon in a

[259] RS, MS210, 2v.

given time – which is equal to the number of sides run through multiplied by the length of each side – is therefore proportional to the number of reflections multiplied by the radius. Whence the number of reflections is proportional to the total distance described divided by the radius, as required.

Other instances in which readers supplied arguments missing from the text were considered in Chapter 4, but this example additionally demonstrates Gregory's ability confidently to switch between alternative mathematisations of centripetal force. Although Newton very rarely uses the polygonal approximation in the *Principia*, and supplies almost no other examples on which to model a proof, Gregory was sufficiently flexible to be able to construct a polygonal argument for himself when the text failed to provide it. He recognised that there was more than one way of mathematising force, and was able to adapt accordingly when reading the text.

There is one further observation to be made about Gregory's response to Newton's mathematisation of force, which is briefly stated but of considerable significance. Having worked through the central orbit theorems in Section 2 and the short Scholium that follows, he wrote the following:

> vellem Auctor Theorema conderet de vi centrifuga thoremati quinto de vi centripeta simile quod non adeo arduum futurum reor eadem enim Lemmata in sectione prima doctrinae de vi centrifuga inservirent.[260]

> I would like the author to compose a theorem about centrifugal force similar to Theorem 5 about centripetal force, which indeed I think is not going to be not difficult, for the same lemmas in the first section would serve the doctrine of centrifugal force.

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[260] RS, MS210, 4v.

To this note he subsequently added these words:

> Unde facillime deduci poterunt Theoremata de vi centrifuga Horologio Oscillatorio D: Huygenii subnexa
>
> Whence the theorems added about centrifugal force in Mr Huygens' 'Horologium Oscillatorium' will very easily be able to be deduced

And squeezed alongside in the margin, he has written this:

> immo theorema idem theor. 5 inservit, nam actioni qua centrum retinet et trahit corpus in orbita ne in tangentem exeat aequalis est reactio qua corpus trahit centrum hoc est corporis vi centrifuga q.e.d.
>
> On the contrary, a theorem the same as Theorem 5 serves, for to the action with which the centre holds back and drags the body in an orbit so that it does not go out along the tangent, there is an equal reaction by with which the body drags the centre, that is the centrifugal force of the body. Q.E.D.

The catalyst for these notes is the final words of the Scholium, "vi centripeta in centrifugam versa" ("with the centripetal force turned into a centrifugal force"). In the Scholium these words are used to describe how the geometric construction used to determine the force governing elliptical motion could be altered to determine the force governing hyperbolic motion. But reading this phrase prompted Gregory to make a much broader remark. Theorem 5 is the alternative label for Proposition 6, which provides the geometric measure of centripetal force Newton uses in all the subsequent demonstrations: Gregory is therefore explicitly stating his desire that alternative proofs be provided of all the orbit proofs, expressed in terms of centrifugal rather than centrifugal force. He thought that the proofs in Section 2 could, and should, be re-written in terms of centrifugal rather than centripetal forces. Whether this reflected his own ontological preference for centrifugal over centripetal forces, or whether he merely wished the reader of Newton’s book to have the freedom to make their own choice, is not clear. My interpretation of the notes above is that Gregory wishes not to commit to a position on the physical reality of centripetal as opposed to centrifugal forces. The first note appears to suggest that whether the proofs are phrased in terms of centripetal or centrifugal forces is a matter of mathematical convenience, and that they have equal ontological validity. He clearly states his view that the method of first and last ratios is just as applicable to either. The final marginal note complicates matters, since it appears erroneously to identify centrifugal force with the pull exerted by the orbiting body on the larger one at the centre. But Gregory’s writing in this final note is somewhat difficult to read: further research needs to be done into the development of Gregory’s own views about the mathematisation of force before drawing firm conclusions.

However, Gregory’s awareness that there is more than one possible mathematisation of centripetal force resonates with his view that there is more than one possible interpretation of the underlying physics. He recognised that the analysis provided by Newton was only one of the many possible mathematisations of central force. Multiple proofs of the orbital laws were possible, with multiple underlying ontologies. In the face of such variety, he appears in the *Notae* to have remained non-committal. In other words, and despite his extremely close reading of the text, Gregory was not persuaded that Newton’s analysis of orbital forces was the only valid possibility.

5.4.4 Leibniz’s alternative in the *Tentamen*

Bertoloni Meli's analysis of Leibniz's response to the *Principia*, and his account of the book's influence on his own mathematisation of orbital forces in the *Tentamen* of February 1689, reveals that, like other readers, Leibniz was not persuaded by Newton's mathematisation of force. My purpose in this final section is to show how this disagreement was manifested in his processing of the text.

It will be helpful to begin by comparing Newton's analysis of orbital motion in the *Principia* with Leibniz's in the *Tentamen*. Both analyses can be used to derive the required expressions for the centripetal forces needed to maintain circular and elliptical orbits, but they arrive at these results in different ways. In the opening sections of Book 1, Newton assumes the existence of only one type of force, a centripetal force. Coupled with a law of linear inertia, centripetal forces are sufficient for Newton to explain orbital motion. A body will continue to move in a straight line unless deflected from it, and a force continually acting towards a fixed point will continually deflect the body into some sort of orbit. The shape of the orbit will depend on the size of this deflecting force. As explained in the previous section, Newton's mathematical analysis of centripetal forces in the *Principia* mostly uses a parabolic approximation, whereby the force pulls continuously on the body to deflect it from its inertial path. The body therefore accelerates along each deflection. In Sections 2 and 3 of Book 1 Newton uses this analysis to determine the centripetal force required to maintain orbits in a variety of shapes. In the text of the *Principia* Newton famously refused to hypothesise about the physical cause of the centripetal forces sustaining planetary motion.

Leibniz's explanation of orbital motion in the *Tentamen*, on the other hand, assumes the existence of not one but three forces. First, a transverse component – the *circulatio harmonica* – sweeps the body around the orbit in a circular motion, with speed inversely proportional to the distance from the centre (thus accounting for Kepler's Area Law). The *circulatio harmonica* generates, second, a centrifugal force (or "conatus") propelling the body outwards, which Leibniz proved to be inversely proportional to the cube of the distance from the centre.[261] And, third, the "solicitation of gravity" provides a pull towards the centre. So the transverse component (the *circulatio harmonica*) moves the body around the orbit at the same time as the centrifugal and centripetal components combine to produce a radial component (the *motus paracentricus*) which moves the body either towards or away from the centre of the orbit, depending on their relative sizes. While in principle the transverse *circulatio harmonica* and the radial *motus paracentricus* could presumably combine to produce orbits of many shapes, Leibniz – unlike Newton – only considered circular and elliptical motion. And unlike Newton, Leibniz analyses the radial forces using not a parabolic but a polygonal approximation, whereby a force acts impulsively on the body at each vertex to deflect it from its inertial path. Leibniz considers these impulsive forces to be acting at regular time intervals, with the body moving uniformly along each deflection, in contrast to Newton's accelerated deflections. And unlike Newton's analysis of orbital motion, Leibniz's was intrinsically bound up with what he understood to be its physical cause. Following Descartes, Leibniz assumed the existence of a swirling vortex which carried the planets along, and it was the vortex that provided the *circulatio*

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[261] Guicciardini, *Reading the Principia*, 150.

harmonica. This transverse component was lacking from Newton's analysis: for Leibniz, its role in his mathematical proofs was intrinsically linked to his commitment to the physical existence of Cartesian vortices.

These differences in analytical approach are manifested in Leibniz's engagement with the text of the *Principia*. In Chapter 1 it was noted that, of the orbital force theorems Newton presented at the start of Book 1, Leibniz made detailed studies of only three: Proposition 4 (which considers a circular orbit), Proposition 10 (an elliptical orbit with the force directed to the centre) and Proposition 11 (an elliptical orbit with the force directed towards a focus). Proposition 4 depends on Lemma 11, and although Leibniz denied the general validity of Lemma 11, he accepted its result in the specific case of a circle, so was able successfully to reconstruct Newton's proof in his *Marginalia*.[262] Propositions 10 and 11, however, depend on Proposition 6, which presented Leibniz with a more significant problem. Proposition 6 introduces the parabolic approximation, and so – unsurprisingly – Leibniz denied it. Specifically, he objected to Newton's assumption that bodies accelerate along each deflection from the inertial path. In his analysis, Leibniz preferred to assume that bodies moved along each deflection with constant velocity. So while Newton uses Galileo's laws of kinematics to conclude that the deflection is proportional to the square of the time, Leibniz understands the deflection to be proportional to the time itself. In the geometrical construction used in Proposition 6, the deflection is QR, and the time (because it is proportional to the area traced out, by Kepler's Law) is represented by $SP \times QT$. This means that Newton – whose

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[262] *Marginalia*, 53.

parabolic approximation considers a continuous force accelerating a body for a given duration of time – assumes that:

$$\begin{aligned} \text{deflection} &= \text{force} \times \text{time}^2 \\ QR &= \text{force} \times (SP \times QT)^2 \\ \text{force} &= \frac{QR}{(SP \times QT)^2} \end{aligned}$$

This is the result of Proposition 6. But according to Leibniz – whose polygonal approximation assumes an impulsive force acting instantaneously on a body, which then moves uniformly for a given duration of time – this should be:

$$\begin{aligned} \text{deflection} &= \text{force} \times \text{time} \\ QR &= \text{force} \times (SP \times QT) \\ \text{force} &= \frac{QR}{(SP \times QT)} \end{aligned}$$

Leibniz therefore rejected Proposition 6. Although the margins alongside this result in his copy of the first edition are otherwise completely blank, he changed Newton's text from

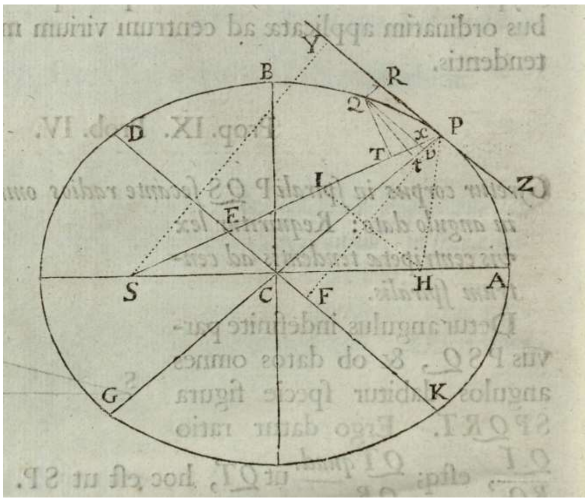
> lineola nascens *QR*, dato tempore, est ut vis centripeta (per Leg. II.) & data vi, ut quadratum temporis (per Lem X.)

to

lineola nascens *QR*, dato eodem manente tempore, est ut velocitas centripeta (per Leg. II.) & data velocitate, ut tempus (per Lem. X.)[263]

This alteration matches the first lines of the proportions we have just given almost exactly. Because (in anachronistic vocabulary) the magnitude of an impulsive force is equal to the change of momentum, he is able to use “velocitas centripeta” where I have written “force,” but otherwise these changes show how Leibniz’s different analysis of orbital motion was reflected in his reading of Newton’s book.

However, despite rejecting Proposition 6, Leibniz was still able to verify the conclusions of Propositions 10 and 11. He did so by constructing his own derivations that did not rely on Proposition 6. Proposition 10 proves that the force required to maintain an elliptical orbit if the force is directed towards the centre of the ellipse is directly proportional to the distance from the centre:



Newton’s proof uses a series of geometrical arguments to show that for this construction

[263] Ibid., 55.

$$\frac{QR}{Qt^2 \times PC^2} = \frac{PC}{2BC^2 \times CA^2}$$

Since $QR/(Qt^2 \times PC^2)$ was the quantity Newton derived in Proposition 6 as his measure of centripetal force, and since for any given ellipse both *BC* and *CA* are fixed, this allows him to conclude that the force is proportional to *PC*, as required. Leibniz, however, does not recognise $QR/(Qt^2 \times PC^2)$ as a measure of centripetal force. In the *Marginalia*, alongside Newton's invocation of Proposition 6, Leibniz writes

> ergo vis centripeta est in composita ratione directa ipsius *PV* [=*QR*] spatii, et reciproca ipsius *PC.QT* temporis (non quadrati temporis ut vult Coroll. Th. 5)[264]

He then proceeds to deduce Newton's conclusion from within his own analytical framework. As Fellmann's transcription shows, he first infers from the constancy of *BC* and *CA* that *QR* is proportional to $Qt^2 \times PC^3$. Since for Leibniz the force is proportional to the deflection divided by the time (which by Kepler's Area Law is proportional to $Qt \times PC$) he is therefore able to establish that the force is proportional to $Qt \times PC^2$. Having done so, however, he ignores this result and instead goes back a step to his previous expression for *QR*:

> sin vero tempora aequalia, vis centripeta erit ut *PV* [=*QR*], seu ut $PC^3 \cdot QT^2$. sed quia *QT* reciproce ut *PC* in casu aequalis temporis, erit impetus novus aequalibus temporum intervallis impressus, seu vis centripeta in ratione distantiae directa.

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264 Ibid., 56. 'Theorem 5' is the alternative title Newton uses for Proposition 6.

That is, Leibniz now chooses to lean on his assumption that as the body moves around the orbit the successive impulsive forces occur at a regular frequency. They are thus separated by equal intervals of time. Under this condition, the forces will be proportional to the deflections *QR*, which is to say, proportional to $PC^3 \times QT^2$. And because the times are all equal, $PC \times QT$ will be constant, which means that the forces will be proportional to *PC*, as Newton claims. In short: by assuming equal time intervals between the impulses, Leibniz can measure the force by the size of the deflections *QR*, which in Proposition 10 are proportional to the distances from the centre *PC*.

The *Marginalia* suggest that he performed an extremely similar manoeuvre when reading the inverse-square law in Proposition 11. As the reader may again verify from Fellmann's transcription,[265] Leibniz followed the text right up to the penultimate stage of the proof, agreeing with Newton that

$$\frac{L \times QR}{QT^2} = \frac{2PC}{Gv}$$

where *L* is the latus rectum (and the diagram is the same as that provided for Proposition 10). From here, Newton noted the equality of 2*PC* and *Gv*, and then multiplied both sides by SP^2/QR to get

$$L \times SP^2 = \frac{SP^2 \times QT^2}{QR}$$

[265] Ibid., 57–8.

From Proposition 6 – and because *L* is fixed – Newton was thus able to conclude that the force is inversely proportional to the square of the distance. Leibniz, however, was able to take a much more straightforward final step. He also acknowledged the constancy of *L* and the equality of 2*PC* and *Gv*, and then simply wrote "Ergo *QR* sunt ut QT^2, seu ut $1 : SP^2$." Although he has explained his thought process less explicitly, this note is consistent with the approach he took in Proposition 10: he once again assumes that the impulsive forces act at regular intervals, under which condition the force is proportional to *QR*. The expression for the deflection can therefore legitimately be taken to be a measure of the force, which means that the force is inversely proportional to the square of the distance.

According to the chronology established by Bertoloni Meli, Leibniz returned to Newton's book in 1689, the year after his initial *Marginalia*. When he revisited Proposition 6, he recorded the following short note in his first set of *Excerpts*:

> Aestimat vim ~~percussionis~~ centripetam per *RQ* ductam in 1 : in quadratum temporis seu 1 : $\overline{PS \cdot QT}^2$ scilicet spatium *QR* semper est in ratione vis centripetae seu celeritatis et ~~temporis~~ quadrati temporis nempe in summis istis elementis.[266]

This note reinforces the observations we have already made. It is apparant that Leibniz's instinct is to think of forces acting impulsively, initially writing "percussionis" instead of "centripetam" and "temporis" for "quadrati temporis," and identifying force with speed ("vis centripetae seu celeritatis"). Bertoloni Meli sees in these changes "Leibniz's acceptance of the proportionality between QR

[266] *Excerpts*, 481, 486.

and time squared,"[267] but I am not so sure. The *Excerpts* consist largely of transcriptions or paraphrases of Newton's text, and it is not clear to me that by changing "temporis" to "quadrati temporis" Leibniz was necessarily agreeing with Newton, so much as recording what he wrote. He let "celeratis" stand, after all, which indicates that he had not fully acknowledged the differences. Indeed, an "acceptance" of the proportionality between *QR* and time squared would constitute an admission that Newton's analysis of orbital motion was preferable to his own, which Bertoloni Meli would surely not recognise. I think a fairer interpretation is that Leibniz tried to process Newton's orbital proofs using his own conceptual framework, and that both this note and the *Marginalia* reflect the disjunction that thereby arose. Just as he did with the method of first and last ratios, and notwithstanding Bertoloni Meli's account of how the publication of the *Principia* stimulated the development of his own analysis of orbital motion, Leibniz judged Newton's arguments not in the terms in which they were presented, but by the extent to which they corresponded to conclusions he could establish with his own.

I wish to end with three final, interconnected remarks. First, the modern reader with the benefit of hindsight might form the view that the two approaches are not as different as they first appear. Leibniz considers each force to be an impulse, and he assumes that they act at regular intervals of time, and so he is effectively finding an expression for the impulse per unit time. But the impulse per unit time – which is to say, the rate of change of momentum – is the very definition of force in many modern articulations of Newton's Second Law. The

[267] Ibid., 486.

modern reader might therefore view Newton and Leibniz as providing different mathematical analyses of the same underlying dynamics. Second, it is important to observe that the different ways in which Newton and Leibniz analysed orbital dynamics were inextricably connected with the different ways in which they articulated infinitely small quantities. Proposition 6 quantifies force by the expression $QR/(SP \times QT)^2$, calculated in the limit as the points *Q* and *R* approach the point *P*. As this limit is approached, the values of *QR*, *SP* and *QT* all change continuously, and Newton's task for any given orbit is to evaluate $QR/(SP \times QT)^2$ in the limit of the geometrical construction. Leibniz, however, measures force with the expression $QR/(SP \times QT)$. Because he assumes that his forces act impulsively at regular intervals, he takes the time (given by $1/2 \times SP \times QT$) to be constant, under which condition the forces are proportional to *QR*. For Leibniz *QR*, *SP* and *QT* are static, infinitesimal quantities, whereas in Newton's proofs they decrease dynamically towards zero. Leibniz considers the impulsive forces that act at a series of points along the orbit, each separated by an equal, infinitesimal interval of time; Newton considers the continuous force applied at a single point on the orbit, in the limit as the interval of time for which it acts decreases towards zero. The mathematics and the physics cannot be separated. And third, the actors' own views of the differences between their respective analyses merits close attention. Taken as a whole this theme extends well beyond the scope of this thesis, and has been much more fully addressed by both Guicciardini and Bertoloni Meli. Guicciardini emphasises that while Leibniz "stated that the polygonal model is *preferable*," it remained the case that "both Leibniz and Newton considered the choice between the polygonal and the continuous model one of convention."[268] He writes that

> Leibniz's preference for the polygonal model and Newton's usage of the parabolic one do not reveal incommensurable approaches to the mathematization of natural philosophy. They emerge as choices motivated by different aims. Both Newton and Leibniz understood that these choices were a matter of convention. Newton was interested in the geometrical representability of motion and force: he thus drew figures which are approximations in terms of polygons, circles or parabolas of actual displacements of bodies in motion. Leibniz was interested in writing a differential equation of motion. He thus found it more useful to focus on the infinitesimal representation of trajectories.[269]

My reading of Bertoloni Meli is that he disagrees. While he acknowledges that "the choice of the specific polygon entails a degree of arbitrariness" and that in that sense "Leibniz's mathematical representations of curvilinear motion are fictitious," the polygonal analysis "corresponds in principle to physical actions in a way that the continuous curve does not." He says that the "mathematics mirrors the physical laws involved." That is, while the choice of any particular polygon to approximate the curve was somewhat arbitrary for Leibniz, the general choice of the polygonal approximation over the parabolic was not, because it corresponded with the physical reality of Cartesian vortices, and "his belief that phenomena were ultimately explicable in terms of impacts."[270] According to this interpretation, Leibniz's rejection of the parabolic model was not, in Guicciardini's terms, merely a matter of convention.

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[268] Guicciardini, *Reading the Principia*, 156. The emphasis is his.
[269] Ibid., 156.
[270] Bertoloni Meli, Equivalence and Priority: Newton versus Leibniz, 83.

Leibniz recorded his own views on the matter very clearly. In the margin alongside Proposition 10, he wrote the following:

> Vero cum autore sit vis centripeta ut *PV* seu *QR* directe, et quadratum temporis $PC^2.QT^2$ reciproce, ~~fiet~~ tunc erratur quidem, sed positis temporibus aequalibus, error evanescit, idemque prodit, ipsi et nobis[271]

That is, Leibniz thought Newton's proof was erroneous, and that his own demonstration made the error disappear. When he first read the book in 1688, Leibniz thought the orbital force proofs in the *Principia* were wrong. Once again the primary evidence reveals the paradox that has been repeated in many of the other readings of the text I have examined in this thesis: although he was not persuaded by the arguments he found in Newton's book, Leibniz agreed with his conclusions. Leibniz thought Newton used the wrong method and the wrong argument, but he thought that his result was right. Even though he processed the proofs from within a different conceptual framework, he concurred with Newton's expressions for the forces required to maintain circular and elliptical orbits. And it has been a recurring pattern that Newton's peers disagreed with the arguments they found on the page, but generated independent verifications of the same results to which they could willingly assent. Readers of the *Principia* routinely discovered what they perceived to be errors in the text, and by correcting them generated their own proofs of Newton's claims. "Error evanescit, idemque prodit, ipsi et nobis": these words provide a useful distillation of the themes with which I will conclude.

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[271] *Marginalia*, 56.

Conclusion: *Newtoni in verba*

Re-statement of findings

In this thesis we have seen that there is a large quantity of primary material documenting the reception of the *Principia* that has not been adequately analysed by the secondary literature, including manuscripts that record the responses to the text of Halley, Fatio, Gregory, Huygens, Clerke, Locke and Flamsteed. Although this material cannot be relied upon to provide a complete record of early readings – or even a representative record – it is reasonable to suggest that their contents are usefully indicative of early readings of Newton's text.

Careful analysis of this material shows that readers generally engaged with his text piecemeal, studying different passages in different orders in different ways. Most readers left most of the book unread. The only sections that appear to have been routinely studied were the Definitions, the Laws, the opening sections of Book 1 on orbital motion, and Book 3. But even within this pattern the level of scrutiny with which Newton's peers engaged with his text varied widely, with individual readers routinely skipping individual steps in the proofs. The extant documentation suggests that the importance of Halley and Fatio as early readers of the text has been understated by the secondary literature, that of Huygens has overstated, and Locke's three readings of the text have been persistently misrepresented. We only have evidence of one complete, sequential reading of the text (Gregory), although a few others appear to have been attempted (Fatio, possibly Halley, and arguably Leibniz and Locke).

There was a variety of ways in which readers engaged with the text, but we can usefully distinguish between mathematical readings of the *Principia*, in which readers attempted to verify the proofs, and non-mathematical readings, in which they did not. Some readers (Fatio, Leibniz) were able to switch between these two modes. In either case, the act of reading constituted an active process of reconstruction: Newton's readers picked up a pen and reconstructed the connected prose of his arguments in symbolic form. These reconstructions were often substantively different from the original arguments that appeared in the text, and used different notation, different concepts, and different logic. Readers assented to Newton's proofs to the extent to which they were successfully able to reconstruct arguments that supported his conclusions. Because these reconstructions were often substantively different from Newton's originals, this meant that they often assented to his claims but not the arguments he used to derive them.

Because studying the text entailed an active process of reconstruction, readers automatically generated what they considered to be improved versions of Newton's proofs. They altered, simplified and often extended the demonstrations printed in the text. Editing the *Principia* was thus inherent to the act of reading it. When circumstances allowed, readers communicated these alterations back to Newton, who often incorporated them verbatim into his annotated copy of the text. He reworded, reformulated and restructured his proofs in response to their feedback: Newton negotiated the validity of his arguments with his readers.

The book was "difficult" to read to the extent that its composition and style produced obstacles to this process of reconstruction. Newton's connected prose was often hard to parse and recreate in symbolic form. He often left gaps in his proofs. The conceptual frameworks in which individual readers reconstructed his proofs were sometimes unable to accommodate his arguments. The length of the book made complete readings impractical, discouraging readers from carefully examining all the steps in his arguments. For these reasons, even careful readers such as Gregory, Fatio and Halley were only able to give their assent after face-to-face meetings with the author, during which they were able to query, challenge, and haggle over the validity of Newton's arguments.

Responses to the novel mathematical methodology of the *Principia* varied. Some readers paid little attention to the underlying foundations (Flamsteed), others reinterpreted his geometrical limit proofs in terms of infinitesimals (Leibniz, Huygens), and many got the gist of his techniques but registered important misgivings with Newton's articulation of them (Halley, Clerke, Fatio, Gregory). Newton rewrote many of the proofs in Section 1 on the method of first and last ratios in response to his readers. Some were not persuaded by his limit definition in Lemma 1 because the logical structure of its proof was unclear, and others either objected to the articulation of or did not see the need for the unusual "microscope" style of proof he employed in Lemmas 6–9. Responses to the novel physical claims in the book were just as diverse. Some readers did not accept the foundational assumptions they found in the text, with Halley making material changes to the articulation of the Definitions and Laws. Many recognised that Newton's mathematisation of force was not the only viable alternative, and many either denied or were explicitly ambivalent about his rejection of centrifugal force. There is no evidence that any readers scrutinised the mathematical demonstrations concerning universal gravitation during the five years following publication.

There are therefore two overall conclusions for which this thesis has argued.

First, the arguments in the *Principia* did not succeed in persuading its readers. Most of Newton's readers left most of the book unread. When they did read it, they often did not understand it; when they did understand it, they often did not agree with it. They disputed his physical and mathematical methodologies, and haggled over the validity of his proofs. Even careful readers required face-to-face meetings with Newton to supplement the printed demonstrations. Every reader for whom there is reliable evidence objected to his articulation of the method of first and last ratios in Section 1 of Book 1, and every reader for whom there is reliable evidence objected to at least one step in his proof of the inverse-square law. The text of the *Principia* failed to persuade early readers of the validity of the arguments it contained. The most that can be said is that a very small number of readers gave qualified assent to a very small number of its results. The acclaim the secondary literature asserts it received in the summer of 1687 must therefore have been unwarranted: the immediate approval of the book when it was published could only have been founded in some cause other than assent to the mathematical demonstrations in the text.

The second conclusion points towards the longer-term acceptance of Newtonianism. Although the arguments in the *Principia* did not receive the assent of its early readers, Newton's conclusions often did. Readers granted assent to his claims according to whether they were successfully able to reconstruct arguments in their support. Very often these reconstructions were different from Newton's originals, and readers were only able to agree with his conclusions because they could provide their own arguments in support of his claims. Let it be clearly understood, however, that readers did not merely wish the book's demonstrations to be expressed differently: in many instances they thought that they were wrong. By proposing amended proofs, Newton's peers thus negotiated the validity of the arguments in his book. Even when they thought his arguments were defective, they granted assent to his conclusions if they were able to construct their own, improved demonstrations in support of his claims. In Leibniz's words from the close of the last chapter, "Error evanescit, idemque prodit, ipsi et nobis."

Suggestions for further research

These findings link with recent developments in the secondary literature a few ways. Feingold and Svorenčík's forthcoming publications may contain valuable information about families of manuscript annotations, and may reveal patterns in the distribution of edits to the text of the first edition. This information might allow the dates of individual edits to be established, and so enable a chronological reconstruction of the alterations to Newton's proofs. Tracing the modifications of his text in this way would provide a detailed account of the means by which the new mathematical knowledge in the *Principia* was made over the years after publication. Separately, the evidence presented in this thesis affirms the particular importance of David Gregory among Newton's early readers, as has been emphasised in the most recent papers of Guicciardini and Ducheyne. Gregory's role as Newton's principal reader, interpreter and evangelist calls for further scrutiny of both his *Notae* in the Royal Society and his archives in Scotland.

However, neither of these lines of enquiry promises to shed any light on the original *explanandum* of this thesis, the speed with which Newton's book is considered to have been acclaimed when it was published in the summer of 1687. The nearest I have found to a convincing explanation is in Iliffe's "Butter for Parsnips," whose emphasis on networks of trust in the reception of Newton's book is consistent with the evidence I have presented here. Iliffe proposes that less mathematically confident readers sought the help and reassurance of their more expert peers in accessing the technical details of Newton's proof, and that trust in the text radiated outwards through "concentric circles of competence" as a result. The evidence presented in this thesis provides the critical additional information that the innermost experts whose competence Iliffe says was trusted did not themselves verify Newton's proofs in the years following publication. Those experts who were able to access the text directly – Huygens, Fatio, Gregory – did not assent to the validity of its demonstrations, or at least did so in a complicated way over a protracted period of time. That is, the evidence I have presented here would appear to show that for some years after 1687 none of Newton's readers agreed with his mathematics. The acclaim the *Principia* is asserted to have received when it was published must, in this sense, have been unwarranted. If this conclusion is correct, it deserves to be stated much more boldly than is currently admitted by the secondary literature: when the Lucasian Professor announced in 1687 that he had solved the problems concerning cosmology and planetary motions which had occupied them for the previous decade, the Fellows of the Royal Society took Newton's word for it.

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Abbreviations

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CUL Cambridge University Library

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RS Royal Society, London

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The primary sources identified in this section are divided by author, starting with Newton and then proceeding alphabetically by surname.

Newton

Newton's annotated copy of the first edition, to which I make frequent reference in my text, is:

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Further commentary by Fatio on the *Principia*, not examined in this thesis, can be found at:

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Flamsteed

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Gregory

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Halley

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Cambridge, Cambridge University Library, MS Add. 3965, 94–99. Accessed 23 June 2021 at https://cudl.lib.cam.ac.uk/view/MS-ADD-03965/191.

Huygens

The notes Huygens made when reading the *Principia* are in the following locations in the Codices Hugeniani at Leiden University Library:

HUG 7 (Book G), 15–16.
HUG 26 (Chartae mechanicae), 85.

This archive has recently been digitised as the *Codices Hugeniani Online*. Accessed 19 June 2021 at https://primarysources.brillonline.com/browse/codices-hugeniani.

Many of these notes are published at various locations in the *Oeuvres Complètes* (cited in my text as *OC*):

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MS Locke c. 31, 99–100.
MS Locke c. 33, 19–20.
MS Locke d. 9.

The proof of the inverse-square law Newton sent to Locke is at MS Locke c. 31, 101–104.

The copy of the first edition given to him by Newton is:

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