Newton: From Certainty to Probability?

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Newton's earliest publications contained scandalous epistemological claims: not only did he aim for certainty; he also claimed success. Some commentators argue that Newton ultimately gave up claims of certainty in favor of a high degree of probability. I argue that no such shift occurred. I examine the evidence of a probabilistic shift: a passage from query 23/31 of the *Opticks* and rule 4 of the *Principia*. Neither passage supports a probabilistic approach to natural philosophy. The aim of certainty, then, was an enduring feature of Newton's methodology.

1. Introduction. Newton's first optical paper (February 1672) contained a scandalous epistemological statement: not only did he aim for certain theories; he also claimed success. Some of Newton's modern commentators argue that Newton eventually relinquished his claims of certainty, admitting only a high degree of probability. This view has been perpetuated by Alan Shapiro (e.g., 1989, 225; 1993, 14) and endorsed more recently by Niccolò Guicciardini (2011, 20). Shapiro views Newton's probabilist shift as gradual and somewhat private. He argues that the first shift occurred less than 6 months after writing that first optical paper—on the basis of the criticisms (Shapiro 1989, 228–29). However, he argues, Newton's first public statement of probabilism did not occur until many years later (Shapiro, pers. comm.). "Only in the last decades of his life," Shapiro (1993, 14) writes, "did [Newton] accept the probabilism of his contemporaries" (see also Shapiro 1989, 225).

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Gradual though it may have been, this is a significant shift in Newton's thought. Newton's notion of certainty permeates his views, not only on the aim of science but also on the nature of evidence, the nature of scientific reasoning, and how he saw his achievements in mathematics and natural philosophy (e.g., Walsh 2011). Shapiro has identified two key pieces of textual evidence for this shift: the first, a response to Hooke in June 1672; and the second, an explicit statement of probabilism in query 23/31 of the *Opticks* (introduced in 1717).[1]

I argue that Newton remained committed to certainty. First, I examine the relevant passage from query 23/31 of the *Opticks* and a related passage, rule 4, from the *Principia*. I argue that neither of these passages should be interpreted as supporting probabilism. Second, I examine the relevant passage from Newton's correspondence with Hooke in 1672. I argue that this passage expresses a similarly qualified notion of certainty as that which Newton expressed two years earlier, in his *Optical Lectures* in 1670. Finally, I characterize Newton's notion of certainty as 'compelled assent' and conclude that this was an enduring feature of Newton's methodology.

2. 1717: Newton's First Public Statement of Probabilism? That Newton claimed some kind of certainty in his early work (in his first optical paper, for instance) is uncontroversial. Exactly what this 'certainty' amounted to is less clear. I offer an account of this in the final section. For now, let us put this issue to one side and focus on Newton's apparent shift to probabilism. In this section, I will assess Shapiro's claim that a passage from query 23/31 should be interpreted as probabilism. First, I introduce the relevant passage from query 23/31 and a related passage from the *Principia*—rule 4—clarifying the standard (probabilistic) interpretation of these passages. I then offer an alternative interpretation, based on the application of rule 4 to Newton's argument for universal gravitation: rule 4 advises us to adjust the scope of generalizations, not our credence. Applying this thinking to the original passage, I conclude that this is not a statement of probabilism and so does not represent a shift in Newton's thinking about certainty.

2.1. The Probabilist Interpretation of Query 23/31 and Rule 4. In 1717, Newton published the second edition of the *Opticks*, in which he relabeled query 23 as query 31 and expanded the methodological discussion.[2] Here, Newton described his methods of *analysis* and *synthesis* (or *composition*). Newton's method of analysis involved drawing general propositions from observations and experiments and, more generally, inferring causes from ef-

1. It is interesting to note that Guicciardini (2011, 20, 14) endorses the first shift but not the second.

2. I follow convention in referring to this as query 23/31.

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fects. The method of composition is the inverse: we assume the causes and infer their effects. The ordering of these methods was important: the method of composition should always follow the method of analysis, using the causes discovered by analysis to explain their effects, thus "explaining the Phænomena" and "proving the Explanations" (Newton 1730/1952, 405).[3]

Expanding on his method of analysis, Newton (1730/1952, 404) wrote the following statement, which I will refer to as 'P':

> If no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur.

We might be forgiven for interpreting P in hypothetico-deductive terms, as it seems to suggest that the epistemic status of a theory ought to be sensitive to new evidence. Indeed, according to Shapiro, this is Newton's first public probabilistic statement.

This is a mistake. To understand why, we should see how Newton applied P in practice. Although P was not invoked explicitly in the *Opticks*, the same notion can be found in another late addition to Newton's work, rule 4 of the *Principia*.[4]

In the third edition of the *Principia*, Newton (1726/1999, 796) added a fourth 'rule for philosophising':[5]

> Rule 4. *In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions.*
>
> This rule should be followed so that arguments based on induction may not be nullified by hypotheses.

The similarities between this rule and P are striking: that new evidence can make a proposition "either more exact or liable to exceptions" is similar

3. In this passage, Newton interchangeably referred to the methods of composition and synthesis. For discussion, see Guicciardini (2011, 74–78) and Ducheyne (2012, 6–8, 18–25).

4. While the *Opticks* and the *Principia* ostensibly deal with different subject matter, query 23/31 goes beyond optics to discuss various forces, motions, and systems, including universal gravitation. Moreover, query 23/31 discusses the other rules for philosophizing—explicitly drawing methodological connections between the *Principia* and the *Opticks*. So I believe I am justified in drawing this comparison.

5. The rules act as explanatory constraints, or heuristics, enabling Newton to make inferences from phenomena and theorems to further theorems.

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to pronouncing the conclusion either "generally" or "with such Exceptions as occur."

Rule 4 looks like a pragmatic response to the problem of induction: Newton's recognition that uncertainty was introduced by ampliative inference, that is, generalizing from particulars.[6] The phrase "very nearly true" could mean 'highly probable'. So, it might seem reasonable to put the following hypothetico-deductive gloss on rule 4:

> If our best theory fits all the known facts, then we should act as though it is highly probable (i.e., tentatively accept it) until more facts either support or refute it.

I take it that an interpretation of this kind is the source of Shapiro's claim.[7] However, I argue that this is a misinterpretation of rule 4. Rule 4 was employed explicitly in the *Principia*—in the argument for universal gravitation. This use should tell us how to interpret the rule itself and, by extension, P.

2.2. Rule 4 and Newton's Argument for Universal Gravitation. In the *Principia*, Newton (1726/1999, 805) explicitly employed rule 4 only once, in his argument for universal gravitation, proposition 5, book 3:

> Proposition 5. *The circumjovial planets gravitate toward Jupiter, the circumsaturnian planets gravitate toward Saturn, and the circumsolar planets gravitate toward the sun, and by the force of their gravity they are always drawn back from rectilinear motions and kept in curvilinear orbits.*

The argument takes the following form:

P1. In system S_1, effect E is caused by G.
P2. The same effect E also occurs in systems S_2, S_3, . . ., S_n.
P3. Whenever the same effect occurs, we should infer the same cause.
P4. G is sufficient to cause E in systems S_2, S_3, . . ., S_n.
P5. If G is sufficient to cause effect E in systems S_2, S_3, . . ., S_n, then we should infer G and no other causes of E.

C. In systems S_2, S_3, . . ., S_n, effect E is caused by G and no other causes (from P1, P2, P3, P4, and P5).

6. For an account of Newton's method of 'inductive gradualism', see Ducheyne (2005).

7. Indeed, Shapiro's (1993, 200) hypothetico-deductive reading of the development of Newton's theory of fits suggests that he supports the hypothetico-deductive interpretation of Newton's method more generally.

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Newton's argument for universal gravitation proceeds step by step from the motion of the moon with respect to the earth, the motions of the moons of Jupiter and Saturn with respect to Jupiter and Saturn, and the motions of the planets with respect to the sun, to the forces producing those motions. Thus, in this argument, system S_1 refers to the two-body earth-moon system. Systems S_2, S_3, . . . , S_n refer respectively to two-body systems involving Jupiter and each of its moons, Saturn and each of its moons, and the sun and each of the planets.

In the scholium following proposition 5, Newton (1726/1999, 806) wrote, "Hitherto we have called 'centripetal' that force by which celestial bodies are kept in their orbits. It is now established that this force is gravity, and therefore we shall call it gravity from now on. For the cause of the centripetal force by which the moon is kept in its orbit ought to be extended to all the planets, by rules 1, 2, and 4." Rules 1 and 2 tell us not to postulate more causes than are sufficient for the effect (P5) and that we should assume that effects of the same kind have causes of the same kind (P3). Rule 4 tells us that (*a*) we ought to regard proposition 5 as "either exactly or very nearly true," (*b*) new observational evidence may make proposition 5 "either more exact or liable to exceptions," and (*c*) proposition 5 may not be refuted by "contrary hypotheses." I noted above that rule 4 seems to invite us to regard propositions as highly probable until other evidence either increases or decreases our credence in the proposition.

I now suggest an alternative reading of this rule, addressing the three parts of rule 4 in turn.

2.2.1. We Ought to Regard Proposition 5 as "Either Exactly or Very Nearly True." This tells us what epistemic attitude we should take toward propositions. The phrase "either exactly or very nearly true" is usually interpreted as 'highly probable'. However, I now argue for an alternative reading of this passage. In the original Latin, the relevant phrase in rule 4 appears as "pro veris aut accurate aut quamproxime haberi debent" (Newton 1726/1972, 555). In Latin, *quamproxime* literally means 'closest (*proxime*) in the highest possible degree (*quam*)'. In their translation of the *Principia* (Newton 1726/1999), Cohen and Whitman translate the phrase as 'very nearly'. Indeed, out of context, this seems to be a reasonable translation. However, many commentators agree that this does not capture the strength and significance of the phrase when used by Newton. For example, Ducheyne (2012, 82 n. 151, 118 n. 51) has made a strong case for the translation "as most closely as possible," whereas Smith (e.g., 2002) prefers to treat *quamproxime* as a technical term.[8]

8. On the role of '*quamproxime* propositions' in the *Principia*, see Smith (2002, 155–56) and Ducheyne (2012, 82–92).

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So what does *quamproxime* mean in this context? Newton approached universal gravitation via a series of increasingly complex model systems. In book 1 of the *Principia*, he modeled the laws of motion in a one-body system, establishing that a body would display perfect Keplerian motion when acted on by an inverse square centripetal force. This is because, in a one-body system, there is no mutual attraction. But, once he added a second body, he found that, if a body displays Keplerian motion *quamproxime*, then it is maintained by a centripetal force directed *quamproxime* towards a central body (Newton 1726/1999, 448). That is, the consequent holds *quamproxime* as long as the antecedent holds *quamproxime*. *Quamproxime* captures the degree to which orbits are perturbed by other bodies in the system.

In short, this phrase tells us that we should take this proposition as true, which is to say it captures the essential dynamics of the system.

2.2.2. New Observational Evidence May Make Proposition 5 "*Either More Exact or Liable to Exceptions.*" This tells us what to do in the face of new evidence. The probabilist reading says that new evidence either increases or decreases our credence in the proposition. I prefer a different interpretation: if exceptions to proposition 5 occur, instead of reducing our credence in the proposition, we should add detail (thus, making it more accurate) or degeneralize the proposition (thus, restricting the domain to which it applies). Degeneralizing a proposition does not reduce certainty; rather, it reduces the proposition's scope while maintaining certainty.

Newton expressed this idea much more clearly in draft material: "If a proposition gathered by induction is not sufficiently accurate, then it should be corrected, not by introducing (ad hoc) hypotheses, but by more widely and accurately observed phenomena of nature. If this turns out impossible, however, then the proposition should be de-generalized" (Ducheyne 2012, 119).[9] Thus, if faced with 'contrary instances', we should reduce the domain to which the proposition applies: it is true, but true of fewer instances.

2.2.3. Proposition 5 May Not Be Refuted by "*Contrary Hypotheses.*" This claim instructs us in the treatment of 'contrary hypotheses'. A theory's uncertainty could be sourced in *countervailing evidence* or *competing theories*. That proposition 5 may not be refuted by 'contrary hypotheses' is often interpreted as denying that competing theories can change our credence in the present theory. Take Achinstein's (2013, 74–75) reading, for example: "There may be another hypothesis, incompatible with yours, from which the same phenomena can be derived and explained. If so, then, according

9. Ducheyne's translation and paraphrasing of draft material relating to the *Principia*, 2nd ed.

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to the hypothetico-deductivist, that hypothesis would also be established. But two incompatible hypotheses can't both be established on the basis of the same phenomena. From the 'mere possibility' of a hypothesis—that is, from the mere fact that it entails and explains a range of phenomena—you cannot conclude that it is true." Similarly, Harper (2011, 109) takes contrary hypotheses as alternative propositions that are not "sufficiently supported empirically to be counted as serious rivals." Such interpretations, we shall see, miss the point. Newton tells us that hypotheses must not influence our epistemic attitude toward the theory (i.e., proposition 5). That is, according to rule 4, only phenomena (i.e., observed motions of celestial bodies) can influence the universality of proposition 5.

My interpretation of this claim turns on Newton's (unique) distinction between 'theories' and 'hypotheses'. Although well established among Newton scholars, the distinction is not well understood, so it is worth covering here.

In Newton's methodology, theories and hypotheses deal with different subject matter, have different epistemic statuses, and perform different roles. Theories systematize the observable, measurable properties of things; hypotheses describe the (unobservable) nature of things. Theories are inferred from observation and experiment; hypotheses are speculative. For example, Newton saw universal gravitation as a theory, since it was inferred from celestial and terrestrial observations, and systematised those observations. However, an explanation of the nature and cause of gravity would be a hypothesis, since it concerns the unobservable, and is speculative rather than inferred from experiment. The distinction is nicely captured in a draft letter from Newton to Roger Cotes (March 1713): "One may suppose that bodies may by an unknown power be perpetually accelerated and so reject the first law of motion. One may suppose that God can create a penetrable body and so reject the impenetrability of matter. But to admit of such hypotheses in opposition to rational propositions founded upon phenomena by induction is to destroy all arguments taken from phenomena by induction and all principles founded upon such arguments" (Newton 2004, 120).

Recall that rule 4 tells us that hypotheses may not refute or alter "propositions gathered from phenomena," namely, theories. For Newton, theories were on epistemically surer footing than hypotheses because they were grounded in phenomena, whereas hypotheses were grounded in speculation. When faced with disagreement between hypotheses and theories, we should modify the hypothesis to fit the theory, not vice versa. Newton (2004, 120) explained this idea in the same letter to Cotes: "And therefore as I regard not hypotheses in explaining the phenomena of nature, so I regard them not in opposition to arguments founded upon phenomena by induction or to principles settled upon such arguments. In arguing for any principle or proposition from phenomena

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by induction, hypotheses are not to be considered. The argument holds good till some phenomenon can be produced against it."[10] So, according to rule 4:

1. In the absence of exceptions, we should take gravity to be universal.
2. If exceptions to universal gravitation are found, we should infer that the domain of the theory of gravitation is limited (i.e., not universal).
3. We should not allow our imagined possibilities (e.g., the possibility of a penetrable body) or assumptions about natural mechanisms (e.g., the implausibility of action at a distance) to have any influence on our epistemic attitude toward universal gravitation.

Rule 4, then, should not be read probabilistically. Newton uses it to claim that the theory of universal gravitation is certain, that exceptions should lead us to restrict the domain of the theory, and that ideas about the nature of gravitation, that is, hypotheses about it, should play no epistemic role. Let us apply this kind of thinking to query 23/31.

2.3. Revisiting Query 23/31. Recall P:

> If no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. (Newton 1730/1952, 404)

This passage is similar to part of rule 4 of the *Principia*: new observational evidence may make a theory "either more exact or liable to exceptions" (Newton 1726/1999, 796). From its application in the argument for universal gravitation (proposition 5), we have seen that rule 4 tells us, in the face of refutation, to update a theory's scope, not its epistemic warrant. Generalizations may be degeneralized, but degeneralization does not reduce credence. That is, it does not take us from certainty to probability. P should be read in the same way. Newton does not suggest that we lower our credence in the face of countervailing evidence; rather, we should maintain certainty but decrease scope.

3. June 1672 and Newton's Early Optical Work. In this section, I assess Shapiro's claim that Newton's response to Robert Hooke (June 1672) represents the first stage in a gradual shift in Newton's thinking about certainty. First, I will get clearer on Shapiro's claim by contrasting Newton's February

10. This is the idea Newton (1726/1972, 584) expressed with his (in)famous phrase *hypotheses non fingo*.

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1672 statement with his response to Hooke in June 1672. We will see that, in June 1672, Newton's claims about certainty are apparently more moderate than the statement from February 1672. This lends prima facie support to Shapiro's position. However, I will challenge Shapiro's position with a methodological passage from Newton's *Optical Lectures*, completed in 1670. In this passage, Newton offers a moderate view, similar to the view he expressed in June 1672. This suggests that June 1672 does not mark a shift in Newton's thinking about certainty. Rather, Newton's notion of certainty was always somewhat moderate—but never probabilistic. I will close by arguing that Newton's moderate 'certainty' is *compelled assent*.

3.1. June 1672 and Newton's Response to Hooke. Let us consider Newton's (1959–77, 1:96–97) scandalous statement from February 1672: "A naturalist would scearce expect to see ye science of [colors] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all Phænomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt." These are surprising ideas. While the Royal Society valued epistemic responsibility (see, e.g., Locke 1690/1997 and Hooke 1665/1966, preface), Newton claimed certainty (apparently) without warrant. Indeed, the passage was omitted from the published version (see Newton 1672, 3077; 1959–77, 1:96–97). Hooke was one of the few of Newton's contemporaries who read the passage; and it jarred. He objected that Newton was not justified in claiming that his theory was mathematically certain.

Newton (1959–77, 1:187) responded that he had never claimed mathematical certainty: "I should take notice of a casuall expression wch intimates a greater certainty in these things then I ever promised, viz. The certainty of Mathematical Demonstrations." He contrasted 'mathematical certainty' with the certainty offered by the 'mathematical sciences'. While the former is achieved by reasoning to mathematical theorems from mathematical principles, the latter starts with physical principles and reasons mathematically to physical propositions. Newton claimed to have achieved this latter certainty: "Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall*: Whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science" (1:187). And from such physical principles, reasoning mathematically, Newton had derived a theory of color. Therefore, his science of colors was "*Mathematicall* & as certain as any part of Optiques" (1:188).

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In this explanation, Newton suggests a more moderate certainty—pointing out that the certainty possible in optics is limited. He explained that the science of colors, "Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles" (Newton 1959–77, 1:187). So, according to Newton, certainty in optics is limited by (at least) two things. First, a proposition's certainty is constrained by the certainty of its principles. Second, optical principles are experimental and hence not mathematically certain. It is easy to see why Shapiro interprets this as a subtle shift toward probabilism. In February 1672 Newton emphasized the mathematism and certainty of his theory, but in June 1672, he emphasized the lack of mathematical certainty and the experimental basis of the theory. In the following section, however, I argue that no such shift occurred.

3.2. Optical Lectures and Newton's Mathematico-Experimental Method. Newton's approach was based on the idea that mathematics is a bearer of certainty—one can reason deductively from certain axioms to theorems, without losing certainty. Moreover, this method of reasoning is applicable to natural philosophy: one can reason deductively from laws and principles to propositions in natural philosophy, without epistemic loss. So, if one can establish certain natural philosophical laws or principles, it is possible to reason mathematically to certain propositions. Furthermore, Newton thought it was possible to establish a certain principle via a single experiment.[11]

Newton first indicated such a method in his *Optical Lectures*, which were completed by 1670.[12] He argued that natural philosophy should combine the insights of experimental philosophy and geometry—experimental techniques can rigorously investigate nature, while mathematical techniques enable reasoning to sound conclusions. In this way, we achieve an exact science (*accurata scientia* [Newton 1984, 86, 438])—a science that yields perfectly accurate knowledge of the world. The passage is worth quoting in extenso:

> Thus although colours may belong to physics, the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning. Indeed, since an exact science of them seems to be one of the most difficult that philosophy is in need of, I hope to show—

11. In this mathematico-experimental methodology, Newton was almost certainly influenced by Isaac Barrow (Shapiro 1993; Guicciardini 2011; Dunlop 2012).

12. Newton lodged two different versions with the Cambridge University Library. For transcriptions and translations, see Newton (1984).

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> as it were, by my example—how valuable mathematics is in natural philosophy. I therefore urge geometers to investigate nature more rigorously, and those devoted to natural science to learn geometry first. Hence the former shall not entirely spend their time in speculations of no value to human life, nor shall the latter, while working assiduously with an absurd method, fail to reach their goal. But truly with the help of philosophical geometers and geometrical philosophers, instead of the conjectures and probabilities that are being blazoned about everywhere, we shall finally achieve a natural science supported by the greatest evidence. (Newton 1984, 87–89, 439)

In this passage, Newton contrasted the product of his mathematico-experimental method, "a natural science supported by the greatest evidence," with the "conjectures and probabilities" produced by less rigorous investigation and reasoning. Here, Newton explicitly presents his methodology in opposition to those who, "working assiduously with an absurd method," reach only probabilities rather than certainties. The passage shows us that what Newton had in mind, even in 1670, was a science of optics, grounded in experiment and observation, and mathematical only "insofar as [colors] are treated by mathematical reasoning." He did not claim to have achieved mathematical certainty but only the kind of certainty that could be achieved in natural philosophy from careful experiments and mathematical reasoning—just as in June 1672.

4. Closing Remarks: Certainty as 'Compelled Assent'. And so, Newton did not become a probabilist—he remained committed to the view that his methods produced certainty. But he did not mean mathematical certainty. What, then, is Newtonian certainty?

I close with a brief suggestion: Newton's certainty can be characterized as 'compelled assent'.[13] When Newton claimed certainty, it was because he thought the evidence compelled him undeniably to his conclusion. For example, in his first paper (February 1672), Newton (1959–77, 1:95) wrote, "And so the true cause . . . was *detected to be no other* then that *Light* consists of *Rays differently refrangible*" (emphasis added to "detected to be no other"). And to Hooke (June 1672), he wrote, "And that this whitenesse is produced onely by a successive intermixture of the colours without their being assimilated or reduced to any uniformity, is *certainly beyond all possibility of doubting*" (1:182–83; emphasis added). There was a social aspect to this as well: Newton expected that others would draw the same conclusion in the same context. For example, in his correspondence with the nat-

13. I develop this suggestion in Walsh (2017). The phrase 'compelled assent', which I have used on a few occasions in this essay, comes from Barbara Shapiro (1983, 29).

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ural philosopher Anthony Lucas (August 1676), Newton (1959–77, 2:79–80) wrote as follows: "[Let Lucas examine the experiments given.] For if any of those be demonstrative, they will need no assistants nor leave room for further disputing about what they demonstrate. The main thing he goes about to examin is ye *different refrangibility* of light. And this I demonstrated by ye *Experimentum Crucis*. Now if this demonstration be good, there needs no further examination of ye thing; if not good ye fault of it is to be shewn, for ye only way to examin a demonstrated proposition is to examin ye demonstration." So, as far as Newton was concerned, the *experimentum crucis* did not provide 'mere' empirical support; it proved that the "true cause" could be "no other" than the one he had identified (1:95). And others, having carried out the same experiment, should be compelled to accept the same conclusion.

This social aspect of Newton's epistemology can be found in the *Principia*. For example, in the scholium to the laws, he writes, "The principles I have set forth are accepted by mathematicians and confirmed by experiments of many kinds" (Newton 1726/1999, 424). And in his manuscript *De gravitatione*, Newton (2004, 12) writes, "I have undertaken to demonstrate its [i.e., the science of fluid dynamics] individual propositions from abstract principles, sufficiently well known to the student, strictly and geometrically." And a few lines later: "The foundations from which this science may be demonstrated are either definitions of certain words, or axioms and postulates no one denies" (12). In these passages, Newton tells us that the abstract principles of this science are supposed to be "sufficiently well known to the student" or "definitions of certain words, or axioms and postulates no one denies." The emphasis on students implies undisputed and fundamental principles—what a student learns by way of introduction to the field. Newton (1730/1952, 20) expressed a similar notion in his *Opticks* while introducing the axioms: "For what hath been generally agreed on I content my self to assume under the notion of Principles, in order to what I have farther to write. And this may suffice for an Introduction to Readers of quick Wit and good Understanding not yet versed in Opticks." There are, then, three components to 'compelled assent': for Newton, some proposition is certain when given (1) the evidence cited, someone of (2) sufficient rationality ('quick wit') and (3) background knowledge ('good understanding') would be forced to accept the truth of that proposition.

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