# Newton Did Change His Views from Certainty to Probability

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Kirsten Walsh has criticized my interpretation of Newton's view on the certainty of his new science of color, namely, that it shifted through his long career from one of certainty to probabilism. I defend my view by further considering his statements on certainty in his *Optical Lectures*, "New Theory about Light and Colors," Query 31 of the *Opticks*, and Rule 4 added to the *Principia* in 1726.

There is little disagreement that at the beginning of his career, Newton made some strong, and even extreme, statements about the mathematical nature and certainty of his optical theories. I have argued that late in his long career, Newton publicly modified his approach and in Query 31 in the *Opticks* claimed only a high degree of probability. Kirsten Walsh (2017) has rejected my interpretation and argues that no shift occurred in Newton's attitude to certainty. Indeed, Walsh holds that the "aim of certainty, then, was an enduring feature of Newton's methodology" (866). I do not fundamentally differ with her on the last point—throughout his career Newton laid claim to a higher degree of certainty than his contemporaries—but I differ with her on evaluating Newton's claims as to the level of certainty that he had achieved and on whether he modified his early views on certainty and adopted probabilism. There is an inherent tension—if not a direct conflict—between Newton's early commitment to mathematical certainty and his later one to experimental induction, as Pulte (2019) has observed. I should clarify that by using the word "probable" and

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words derived from it, such as "probability," the existence of a formal philosophical or mathematical theory is not implied but only—following contemporary usage—something that was likely or more likely to be true than not. The probable was definitely less certain than deductive and mathematical conclusions.

Although I prefer to treat Newton's views on certainty in chronological order, I will turn first to Walsh's opening and principal argument concerning a passage that Newton added to Query 31 in the *Opticks* near the end of his career. I hold that this passage represents his transition to probabilism, and she denies it by considering it as equivalent to the fourth rule of reasoning in the *Principia*. I reject such an equivalence. In the following sections I return to the beginning of the story and consider various passages in Newton's writings from the early 1670s, including his *Optical Lectures*, "New Theory about Light and Colors," "Observations" on the colors of thin films, and his response to Robert Hooke's criticism of his views on certainty.

***Opticks* Query 31 and *Principia* Rule 4.** In the second English edition of the *Opticks* (1717), Newton added a passage to Query 31 on the role of the method of analysis and synthesis in his experimental philosophy:[1]

> This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. *And if no Exception occur from 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 1952, 404, emphasis added)

Walsh (2017, 868) calls the concluding two sentences that I have placed in italics *P* and states that it bears striking similarities to Newton's fourth rule of reasoning—which is correct—but it should be noted that *P* is only a small part of the passage from Query 31. The relation of Query 31 and Rule 4 is considered in more detail below. Newton added Rule 4 to the third edition of the *Principia* 9 years later in 1726:

1. In Shapiro (2004, 189–99), I discuss the development of this paragraph in Query 31 from its first appearance as Query 23 in the Latin translation of 1706 and then in its revision for the second English edition of 1717.

> Rule 4. In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true (*aut accurate aut quamproxime*) notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact (*accuratiores*) or liable to exceptions.
>
> This rule should be followed so that arguments based on induction may not be nullified by hypotheses. (Newton 1999, 796)

Walsh presents a novel reading of Rule 4. First, she takes Newton's "exactly or very nearly true (*aut accurate aut quamproxime*)," which she states is usually interpreted as highly probable, to mean "true." She bases this on Newton's use of *quamproxime* arguments in the *Principia*, as interpreted by Ducheyne (2012, 81 and 118).[2] It is important to note that Rule 4 tells us only to consider the propositions to be true or very nearly true and not that they are necessarily so. Second, she rejects the probabilist interpretation that new evidence either increases or decreases our credence in the proposition. Rather, she proposes that contrary evidence does not reduce our credence but restricts the scope of the proposition or "de-generalizes" it.

The second point may be a valid interpretation for contrary evidence, but she says nothing about confirming or positive evidence for either the proposition or its exception. Surely, in induction this increases the likelihood or probability of the proposition, and thus it increases our credence. Indeed, Rule 4 states that with new "phenomena" we should take the proposition to be "more exact." Since we already considered the proposition to be "either exactly or very nearly true," new confirming evidence will surely make the proposition still more exact; that is, the induction has become stronger.[3]

Walsh (2017, 871) states that Newton expressed the idea that contrary evidence does not reduce credence, or truth, but restricts the scope of a proposition "much more clearly" in drafts for the *Principia*: "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). It should first be noted that these two sentences are not part of a continuous, coherent passage in Newton's manuscript but are separated by nine folios. The first sentence tells us that if a proposition is inaccurate, it should be corrected

2. Smith (2002) stresses the importance of how in the *Principia* Newton proceeds by successive approximations to results that are *quamproxime*, true or very nearly true. Smith (2012, 373) notes that Newton uses the phrase *quamproxime* 139 times in the *Principia*.

3. Below in the paragraph containing n. 6, I consider two other texts that show that when more confirming experiments are carried out, the induction becomes stronger.

by carrying out more observations and more accurate ones. This will make the proposition stronger, as we will see in the next paragraph. In a footnote Walsh states that the second sentence is Ducheyne's paraphrase, but I would call it an interpretation. In my translation of the Latin text published by Ducheyne (2012, 119), it states that "arguments by induction are not demonstrations. Yet they are stronger than hypotheses, and they must be taken for general, except insofar as exceptions drawn from experiment occur. Thus when no exceptions of this kind occur, they must be declared generally."[4] This passage states that in arguments by induction, the arguments are taken as general except when experimental exceptions arise. In each passage Newton once again rejects arguments by hypotheses.

My concern here, however, is not with Rule 4 but with certainty and probability in Query 31. Before I leave that rule, it should be noted that Biener (2018, 6) has recently argued that Rule 4 is best understood as a defense of simple induction from instances: Newton's "demand that some propositions not be taken as strictly true and that all propositions be subject to future correction merely indicates his recognition that inductions can be made stronger/weaker and more/less general by new findings." It is important to see Rule 4 as primarily reasserting Newton's lifelong rejection of hypotheses opposed to demonstrated propositions, and not about evidence. Cohen (1971, 259–60) and Biener (2018, 6–7) have shown that Rule 4 originated in Newton's correspondence in 1713 with Roger Cotes, the editor of the second edition of the *Principia*, over objections to Book III, Proposition 5 based on hypotheses. Not only in Rule 4 but in all but one—Newton's reply to Hooke—of the passages concerning certainty cited in this response to Walsh Newton is determined to exclude hypotheses.[5]

Returning to Query 31, it is essential to consider the sentence immediately preceding the passage that Walsh designated as *P*: "And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general" (Newton 1952, 404). Now we learn that conclusions or propositions established by induction are stronger, the more general the induction. By "stronger" Newton clearly means that they become more likely or more probable as they approach becoming

4. I present a clean version—i.e., I omit deletions and indications of additions—of Ducheyne's transcription of Newton's text: "Argumenta per Inductionem non sunt Demonstrationes. ffortiora tamen sunt quam Hypotheses: & pro generalibus haberi debent nisi quatenus exceptiones ab experimentis desumptæ occurrant. Ideoque ubi nullæ occurrunt ejusmodi exceptiones, generaliter ennunciandæ sunt."

5. See chap. 1.2 in Shapiro (1993) on Newton's views on the relation of certainty and hypotheses.

universally true. In a draft preface for the *Principia* from about the same time (1716–18 according to D. T. Whiteside), Newton makes it clear that by a proposition becoming stronger, he means it is supported by or deduced from a greater number of experiments or phenomena: "In experimental philosophy hypotheses are not considered. Arguments here are taken from experiments by induction. And an argument by induction, although not a perfect demonstration is nonetheless stronger [*fortius*] than an argument by hypothesis alone, and the more numerous the experiments or phenomena from which it is deduced the stronger [*fortius*] it proves to be. Therefore in this treatise we do not feign hypotheses nor arguments taken from them, since they give way to arguments by induction" (1981, 452 n. 34, my translation).[6] Thus, the more experiments and observations, the stronger a proposition becomes. And once again Newton rejects hypotheses in favor of strong arguments by induction.

By omitting the sentence in Query 31 immediately preceding *P*, Walsh's comparison of Query 31 and Rule 4 is not particularly meaningful, because in Query 31 Newton's concern is with induction and the strength of propositions and not with their truth or certainty. Further restricting the significance of her comparison of *P* and the fourth rule, it is not evident to me that declaring a conclusion or proposition to "be pronounced generally" is, as Walsh writes (2017, 868–69), "similar" to declaring that it "should be considered either exactly or very nearly true," especially with her strong reading of "either exactly or very nearly true" as "true."

*An instance of a proposition with an exception*.—In accounts of Rule 4 or Query 31, I do not recall anyone bringing forth a specific example of the statement of a proposition in which Newton posited an exception or made a proposition less general. I will therefore give an instance in Proposition 10, Book II, Part III of the *Opticks*: "If Light be swifter in Bodies than in Vacuo, in the proportion of the Sines which measure the Refraction of the Bodies, the Forces of the Bodies to reflect and refract Light, are very nearly proportional to the densities of the same Bodies; excepting that unctuous and sulphureous Bodies refract more than others of the same density" (Newton 1952, 270). Newton included a table with measured and calculated values, such as the index of refraction, density, and refractive power, made on 22 substances. To a modern, critical spirit the proposition may not appear to meet Newton's usual high standard of evidence and may not be particularly

6. "In Philosophia experimentali hypotheses non considerantur. Argumenta hic desumuntur ab experimentis per Inductionem. Et argumentum ab inductione, licet Demonstratio perfecta non sit tamen fortius est quam argumentum ab Hypothesi sola[,] et quo plura sint experimenta vel Phænomena a quibus deducitur eo fortius evadit. Hypotheses igitur in hoc Tractatu non fingimus neque argumenta inde desumimus, cum cedant argumentis ab inductione."

convincing. If, however, he had carried out more observations—say, another 22—which turned out to agree with the proposition and the exception, both the proposition and its exception would become "stronger," that is, more likely or probable.

**Optical Lectures.** As Lucasian Professor of Mathematics at the University of Cambridge, Newton delivered his inaugural set of lectures on optics in which he set forth his new theory of color. He completed a written version of his lectures by about October 1671 and revised them by February 1672, when he sent his "New Theory about Light and Colors" to the Royal Society.[7] Walsh (2017, 875) writes that in his *Optical Lectures* Newton "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*)—a science that yields perfectly accurate knowledge of the world." She also refers to his "mathematico-experimental method" (876).

Walsh does not appreciate the full import of this key passage in the *Lectures*. It is not just another methodological statement by Newton but rather his proclamation of a program for the mathematization of science.[8] This was his inaugural course of lectures as Lucasian Professor, and in the first place he felt compelled to explain and justify why the professor of mathematics was lecturing on color, for color was then part of the philosophy course, not mathematics. Newton begins this passage by explaining, "lest I seem to have exceeded the bounds of my position while I undertake to treat the nature of colors, which are thought not to pertain to mathematics, it will not be useless if I again recall the reason for this pursuit" (1984, 87).

In what was still a Scholastic curriculum at Cambridge, physics (*physica*) was the qualitative Aristotelian discipline dealing with forms, substances, and qualities and not the modern discipline of that same name. Just a few years earlier, as an undergraduate at Cambridge, Newton had sat through lectures on Aristotelian natural philosophy, and his notes survive (see, e.g., Westfall 1980, 81–87; Buchwald and Feingold 2013, 13–19). And, as he explains later in the paragraph, color, as a quality, was treated in the physics course. A few paragraphs earlier, at the beginning of this lecture, Newton had mocked the Peripatetic treatment of color as "highly absurd and ridiculous" (1984, 83). He justifies his treatment of color by his discovery of

7. On dating the composition of the *Lectures*, see Newton (1984, 18–19). The two versions do not differ in the passages cited in this section.

8. Shapiro (1984) discusses the *Optical Lectures* and Newton's aim of the mathematization of the science of color.

unequal refrangibility and an exact, mathematical correspondence between color and refraction, so that they cannot be treated separately:

> The generation of colors includes so much geometry, and the understanding of colors is supported by so much evidence, that for their sake I can thus attempt to extend the bounds of mathematics somewhat, just as astronomy, geography, navigation, optics, and mechanics are considered mathematical sciences even if they deal with physical things: the heavens, earth, seas, light, and local motion. Thus although colors may belong to physics [*ad Physicam*], the science [*scientia*] 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—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. (Newton 1984, 87)

Newton concludes his declaration to reform the practice of natural science by mocking both mathematicians and natural philosophers: "Hence the former [geometers] shall not entirely spend their time in speculations of no value to human life, nor shall the latter [those devoted to natural science], 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" (1984, 87–89). By "conjectures and probabilities," Newton is referring to the hypothetical explanations propounded by Cartesians, mechanical philosophers, and many in the Royal Society.[9] In a less moribund institution this lecture by a 27-year-old professor might have caused a stir, but his lectures were poorly attended and, after having revised them, Newton decided not to publish them.

The marginal note to this paragraph states that the propositions "are to be treated not hypothetically and probably, but by experiments or demonstratively" (Newton 1984, 87–89). Throughout this paragraph Newton repeatedly mentions mathematics and geometry (10 times), but other than for a reference to "evidence," he does not mention "experimental philosophy," "experimental techniques," or a "mathematico-experimental method," which are terms Walsh invokes (2017, 876). This is really a plea to extend the realm of mathematics to color and indeed to all of natural philosophy (*physica*), a

9. Newton is using the term "probability" or "probable" here in what was then its negative sense of "specious." *OED Online*, https://www.oed.com/view/Entry/151692?redirectedFrom=probable#eid (accessed June 10, 2019).

program advocated by his mentor and predecessor as Lucasian Professor, Isaac Barrow.[10]

Newton is claiming that his new experimental science of color is a mathematical science with its implicit claim to a high degree of certainty. Indeed, he sometimes seems to treat it as if it had the certainty of pure mathematics—a claim that was at odds with the general attitude of the Royal Society toward experimental science (see Ducheyne, 199–200). I believe that Newton's implicit conception of a mathematical science of color expressed here would serve as the basis for his explicit, excessive claims for the mathematical certainty of his theories a few months later in 1672. Contrary to my interpretation, Walsh (2017, 874) holds that in this paragraph in his *Lectures*, "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." Walsh is referring to Newton's response to Hooke's criticism in June 1672, which will be considered in the next section, but she does not explain what "moderate" certainty is.

As we will see in the next section, Newton's extreme claim to certainty in the "New Theory" is inconsistent with Walsh's claim that Newton's views on certainty were always moderate. She calls his statement there "scandalous" but essentially treats it as a transient anomaly, for she presents no explanation for this claim. Moreover, I will introduce another instance of Newton's claim of extreme certainty—his "Observations"—from this same period in early 1672 that Walsh does not mention at all. These will support my claim that Newton moderated his views on certainty from mathematical certainty to one of fallibility, and they will justify the reading of the *Optical Lectures* presented in this section. I turn now to Newton's "New Theory" and Hooke's criticism of Newton's idea of its mathematical certainty.

**"New Theory about Light and Colors," Claims to Certainty, and Newton's Response to Hooke's Criticism.** In his public announcement of his "New theory about light and colors," which Newton sent to the Royal Society in February 1672, he wrote: "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 & wthout any suspicion of doubt*" (Newton 1959–77, 1:96–97, italics added). Henry Oldenburg,

10. On Newton and Barrow, see Shapiro (1993, 31–37), Dear (1995, chap. 8), and Guicciardini (2009).

Secretary of the Royal Society and editor of the *Philosophical Transactions*, excised this passage in the published version, because it was in such sharp contrast to the Society's ideology. Before I turn to Hooke's challenge to Newton's claim to certainty, I will introduce another claim to great certainty from just a month or two later in early 1672.

In the spring of 1672 Newton composed a paper—known as his "Observations" or "Discourse of Observations"—on the colors of thin plates that primarily treats the phenomenon that became known as "Newton's rings." In the first version of his "Observations" that he had composed before his reply to Hooke in June and that he had intended to send to the Royal Society with his reply to Hooke, Newton made the most extreme of his claims for the certainty of his theory.[11] After presenting a physical explanation of the appearance of 'Newton's rings' when viewed through a prism, he asserted that "for confirmation of all this, I need alledge no more, than that it is mathematically demonstrable from my former principles. But I shall add, that they, which please to take the pains, may by the testimony of their senses be assured, that these explications are not hypothetical, but *infallibly true & genuine*" (Birch 1756–57/1968, 3:293, italics added).[12] When Newton revised the "Observations" for the *Opticks*, Book II, Part II (1952, 240), he toned down this assertion of mathematical certainty to make the unexceptionable claim that "now as all these things follow from the properties of Light by a mathematical way of reasoning, so the truth of them may be manifested by Experiments."

Hooke, who heard and refereed the "New Theory" and thus knew the unedited text, did not believe the theory was demonstrated with "absolute necessity," and he objected to Newton's claim that his theory was "soe certain as mathematicall Demonstrations" (Newton 1959–77, 1:113). Newton replied to Hooke on June 11, 1672:

> In the last place I should take notice of a casuall expression wch intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematicall Demonstrations*. I said indeed that the *Science of Colours was Mathematicall* & *as certain as any other part of Optiques*; but who knows not that 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. Now the evidence by wch I asserted the Propositions of colours

11. See Shapiro (1993, 59–60) for the "Observations."

12. There are two versions of his observations of the colors of thin plates, which eventually formed the bulk of Bk. II of the *Opticks*. One is from 1672, and the other was submitted to the Royal Society in December 1675. Birch publishes the later version, but the two versions are the same here (University Library Cambridge, Add. MS 3970, f. 525).

> 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. (Newton 1959–77, 1:187–88)

In distinguishing pure from mixed mathematics and granting that the latter can be only as certain as its physical principles, Newton was adopting the position of the mathematical tradition as expounded by Barrow. He still insisted that as a mathematical science his theory of color was as certain as geometrical optics and more certain than a qualitative or purely physical account. However, for the first time he admitted the fallibility of his experimental principles, or that they were only (highly) probable, and abandoned his strong claim for the certainty of experimental results. No longer would he assert, as he did in the "New Theory," that his theory was a "most rigid consequence" deduced from "experiments concluding directly & wthout any suspicion of doubt." In fact, a month later, when he closely paraphrased the methodological claims of the "New Theory," he modified them. He wrote Oldenburg in July (Newton 1959–77, 1:209) that he arrived at his theory "by deriving it from Experiments concluding positively & directly." It should, however, be noted that this section of Newton's reply to Hooke (on the certainty of his theory) was deleted from the published response by Oldenburg—presumably because he had earlier deleted the offending passage from the published "New Theory"—and thus it was little known.

The path from Newton's implicit assertion of mathematical certainty for his theory of color in his *Optical Lectures*, followed within a few months by explicit assertions of mathematical certainty in two papers and then his admission of the fallibility of his experimental theory in his letter to Hooke in June 1672, contradicts Walsh's contention that no change occurred in Newton's claims to certainty. Moreover, Newton himself subsequently modified both extreme claims to mathematical certainty.

**Newton's Late Consideration of Induction.** When I first expressed the views that Walsh is criticizing (Shapiro 1993), I had not recognized that Newton had introduced the language of induction into his considerations of the natural sciences quite late, namely, in the General Scholium of the *Principia* in 1713, in Query 31 in the second English edition of the *Opticks* in 1717, and in Rule 4 in the third edition of the *Principia* in 1726, as described in Shapiro (2004) and Biener (2018). Newton was led to the problem of induction in the natural sciences in his effort to respond to criticisms from Cartesians and Leibnizians by distinguishing his style of science—"experimental philosophy"—based on experiment and phenomena from their hypothetical science.[13]

13. In mathematics Newton had adopted the concept of induction from John Wallis quite early (Guicciardini 2009).

I now believe that there were two distinct phases in Newton's passage from mathematical certainty to probabilism. In the first, in his response to Hooke in June 1672 Newton recognized the limits of physical claims or "principles." There he explained that optics depends on physical principles as well as mathematical demonstrations. "And the absolute certainty of a Science," he continued in the passage quoted in the preceding section, "cannot exceed the certainty of its Principles. 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" (Newton 1959–77, 1:187). Newton does not here explain the specific reasons for the lesser certainty of physical principles, but he evidently has in mind the conventional view that the real physical world of measurements, experiments, and observations can never perfectly reflect a mathematical construct. In a letter in August 1676 to Henry Oldenburg for Anthony Lucas, one of the Jesuits in Liège who contested Newton's experimental results, Newton explained that if they followed his instructions, they should get results that agreed with his, "so nearly at least that there shall not remain any very considerable difference between us. For if some little difference should still remain, that need not trouble us any further seeing there may be many various circumstances wch may conduce to it" (2:79). He then gave examples of the circumstances. In one category was the inevitable differences in the experimental setup, such as the exact placement of the prisms or the continually varying apparent diameter of the sun through the course of the year. The other category was the "little errors" that arise in measurement.

The second phase of Newton's passage to probabilism was prompted by his new "experimental philosophy" in which he held that the propositions and conclusions of his science were derived from experiment and phenomena. This shift compelled him to confront the limitations of induction. As he conceded in Query 31, "the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general." Newton now talked about inductions that were stronger and more or less general. This represents a further move away from mathematical certainty to probabilism and to results that are judged to be more or less likely.

**Conclusion.** Walsh and I differ on whether Newton modified his views on certainty and adopted probabilism. Walsh argues that Newton's views were always the same "moderate" certainty, but she avoids accounting for his extreme claims for the mathematical certainty of his theory of color in two papers in early 1672. I hold that these claims followed from the views that Newton expressed on mathematical certainty in his *Optical Lectures* and subsequently modified in response to criticism by Hooke. Walsh also argues that Newton did not adopt probabilism in a passage in Query 31, as I claim, by equating that passage with Rule 4 in the *Principia*. I show that these two texts cannot properly be considered to be equivalent.

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