4 BETWEEN BOOKS AND ARTICLES

NEWTON FACES CONTROVERSY

The appearance of the scientific journal in 1665 did not immediately displace books as the primary means of communicating scientific findings. Books remained the more substantial source for scientific information for many years, interacting with the emerging journals. Currently we have only an impressionistic overview of this transformation, as expressed by A. J. Meadows: "Major research continued to be written up in monograph form throughout the eighteenth century, but the habit began to die out in the nineteenth century, at least among the physical sciences" (*Communication in Science* 67). This broad-stroke characterization carries some broad-stroke truth, but a few pieces of information suggest a much more complex picture that needs investigation.

Even during the late seventeenth century some major findings first appeared in the *Philosophical Transactions of the Royal Society* rather than in books, such as Anton Leeuwenhoek's microscopical investigations and some of Boyle's vacuum experiments. Indeed Leeuwenhoek published exclusively through correspondence printed in journals, primarily in the *Transactions* beginning in the 1670s. His books were only collections of his letters (*DSB* 8:126–30). Other lesser seventeenth- and eighteenth-century scientists, such as Desaguliers (*DSB* 4:43–46) and Hauksbee (*DSB* 6:169–75) published primarily in journals. Certainly, as discussed in the previous chapter, the genre of experimental report developed fairly rapidly toward the presentation of primary research, with the generic features being shaped by the dynamics of controversy that would only attend primary publication for a professional audience. As we shall see in a later chapter, the journal article appears in fact to have from early on played an important role in organizing the scientific research community. Further, there seems to have been a great proliferation of journals during the eighteenth century. According to Kronick, the number of active, substantive scientific journals in Europe increased from 7 in 1710 to 27 in 1750 and 118 in 1790 (89).

On the other hand, at the end of the nineteenth century, some journals, including *Physical Review,* still carried book reviews, treating the books under review as major research contributions. Even well into the new physics of the twentieth century, books like Arnold Sommerfeld's *Atembau und Spektralinien* (going through six German editions) and Linus Pauling's *The Nature of the Chemical Bond* presented major theoretical advances as well as primary reports of research.

The more closely one looks at the shift from book to article science, the more the story seems a complex one, with different findings and different kinds of work going to different venues. Nor will Kuhn's association of mature science with journal science fully sort out the complex historical facts. The first two hundred pages of the first volume of the *Dictionary of Scientific Biography,* for example, reveal many exceptions to the expected overall pattern. For example, the eighteenth-century naturalist Michel Adanson, mathematician-physicists André Ampère and Franz Aepinus, and chemist Franz Karl Achard had mixed patterns of articles and books cited for their primary findings. The same mixed pattern pertains in the cases of twentieth-century astronomer Eugen Antoniadi, chemist Richard Anschuetz, paleobiologist Othenio Abel, and radio physicist Edward Appleton. The twentieth-century astronomer Robert Aitken made his most important contribution in book form, and the eighteenth-century polymath José Antonio Alzate y Ramírez contributed through journals. The data for nineteenth-century contributors are even more unpredictable, by date or by specialty.

Moreover, the forms of books and articles are not always distinct and insulated from each other. Although journal articles started off as generally quite short, some became rather long, such as Robert Boyle's "New Pneumatical Experiments about Respiration," which during 1670 filled most of issues 62 and 63 in the fifth volume of the *Transactions*. Such long articles resembled pamphlets of the period in form. By the eighteenth century the long article became common, with volume 90, for example, comprised of only 18 articles, averaging over twenty-five pages in length each. Moreover, Kronick reports some eighteenth-century journals that bear close resemblance to books, with each issue devoted to a single topic, and perhaps written by a single author (92). Similarly, books early show the influence of article styles of experimental presentation and adopt new functions to coordinate with journal publication, as might be observed in Joseph Priestley's *History and Present State of Electricity* (1775).

Thus there seem to be many kinds of books and many kinds of articles with complex relationships to each other. Much historical and textual work remains to be done before a clear picture can emerge.

The following is one attempt to look at an early moment in the book-article dialectic, shedding light on the dynamics and form of both book and article publication at the time.[1] We will consider how Isaac Newton—an intelligent, rhetorically sensitive, creative, and highly motivated individual—understood the two forms and made linguistic choices on the basis of his understanding. Moreover, we will see how he reconsidered his rhetorical problem and strategy, on the basis of readers' responses expressed within a structured communications forum. His reconsiderations influenced both book and article forms. Thus the story is of active reshaping of the form of communication with long-range impact on generic resources and expectations.

Newton's Optical Publications

From a biographical perspective, Newton seems to have dallied only once with journal publication, got burned badly, and never returned.[2] That is, he first published his optical findings in a 1672 *Transactions* article, entitled "A New Theory of Light and Colours," which sparked a controversy with much of the correspondence printed in later issues of the *Transactions;* afterward Newton refused to publish in journals and withheld further publication of his optical findings for thirty years until the *Opticks* appeared in 1704.

But from the perspective of the history of the journal, the "New Theory" article is the earliest significant finding published in the *Transactions,* and is treated as an exemplary piece of scientific writing.[3] Thus Newton's biography suggests that article publication was a failure for Newton, who found the book a more congenial medium, while the history of science judges the article a success. However, a closer examination of Newton's papers reveals the biographical and historical judgments as consistent and related. Newton, perceiving journal publication as a platform, created a forceful statement, but the bitter experience of controversy taught him that journal publication meant entry into an agonistic forum. To address this newly perceived situation, he developed new rhetorical resources to answer criticisms in following issues of the *Transactions.* These rhetorical innovations provided a mode of argument that shaped his book presentation and provided a model for fu-

1. James Paradis, "Montaigne, Boyle, and the Essay of Experience," examines another closely related moment in the early history of the relationship between longer book forms and the shorter article form. He finds the roots of the article in Montaigne's invention of the essay, which for many reasons appealed to the empirical skepticism of the Royal Society.

2. See, for example, Westfall, *Never at Rest,* chapter 7.

3. See, for example, both Cohen's and Kuhn's introductions to Cohen's edition of *Isaac Newton's Papers.*

ture scientific publication by others. The form of compelling argument he developed relied on creating a closed system of experience, perception, thought, and representation that reduced opposing arguments to error. The closed system Newton developed was his own, framed by the worlds represented in his powerful books. Only later was the scientific community to develop the means to construct communally developed closed systems; nonetheless, the Newtonian model of argument provided a powerful way of arguing for general truths from empirical experience. Newton shaped the science that came after him on many levels.

More specifically, this chapter will examine the different forms Newton used to describe his prismatic experiments and related findings about the spectral colors and the composition of white light. This work, forming the matter of book 1 of the *Opticks*, is the most deeply documented of Newton's optical investigations and has appeared in the most forms, including the forms occasioned by controversy. The material which composes books 2 and 3 of the *Opticks* has a shorter and less documented experimental history, has not undergone so many literary transformations by Newton, nor has it faced such extensive public controversy, requiring Newton's defence.[4] Moreover, in the *Opticks*, book 1 is presented confidently and compellingly, whereas books 2 and 3 are presented with greater hesitancy, noncompelling speculation, and openendedness—indicating Newton's inability to harness the latter material to his newly minted conception of compelling scientific argumentation, realized in book 1. The judgment of history seems to have born out Newton's rhetorical judgment, for the argument of book 1 still stands, whereas in the last two centuries only the observations and not the theoretical arguments of the latter books are given scientific credence.

We currently have, depending on how you count, at least seven significantly different versions of the material of book 1 by Newton's hand:

1. entries in his private notebook, *Questiones quaedam Philosophicae*, circa 1664 (Add. 3996);[5]
2. a private manuscript, "Of Colours," circa 1666 (Add 3975);[6]
3. university lectures, first version, circa 1670–71;
4. university lectures, second version, prepared with intent to publish in book form, circa 1671–72;[7]

4. For a discussion of the material leading to the second book of the *Opticks* see Westfall, "Isaac Newton's Coloured Circles twixt two Contiguous Glasses," 13–14.

5. McGuire and Tamny have edited these notebooks under the title *Certain Philosophical Questions: Newton's Trinity Notebook*. I have used this edition throughout.

6. Also in McGuire and Tamny, 466–89.

7. Both versions of the university lectures, in Latin, are published with English trans-

5. a letter to Oldenburg, dated February 1672, (*Correspondence* 1: 92–102) published in slightly edited form in the *Transactions* of 19 February 1672, under the title "A New Theory of Light and Colours";
6. consequent exchanges of correspondence, via Oldenburg, much of it published in the *Transactions*, 1672–76. The details of these exchanges will be provided later;
7. *Opticks*, Book 1, written circa 1690, including multiple extant drafts in English and a partial draft in Latin; published during Newton's life in 1704, 1717, and 1721.

Newton also reported that an additional book-length manuscript on the subject, presumably written after the 1672–76 controversy, was destroyed by fire before work began on the circa 1690 draft.[8]

The story of Newton as a self-conscious and flexible writer revealed in these documents as well as other Newton papers is a rich one, which I hope in future publications to be able to lay out with all the detail and leisure it deserves. Here I discuss only those events and textual transformations that shed light on the dynamic interaction between book and article publication as experienced by Newton.

The basic claims that Newton presents in these various forms were set by the first university lectures, even though later controversy and developments of the argument would cause some drawing back, some further elaboration, and some further precision. The simple substance is the now familiar observation that light of different colors is refracted to different degrees when passed through a prism. Thus light composed of a combination of colors, such as white light, upon passing through a prism will be broken into its various component colors, displayed as a spectrum. The modern understanding is that color is only our perception of light waves of different wavelengths. Thus we can easily conceive of the difference between color produced by light of a single wavelength and color produced by light of a number of wavelengths. At the time of Newton, color was seen as a unitary phenomenon. Newton's association of color with differing refractive indices (or as he called it refrangibilities) and consequent need to distinguish between simple and compound colors created conceptual difficulties for his contemporaries. Much of the controversy and Newton's rhetorical innovation hinges, in fact, on this problem.

lation in a modern edition as *The Optical Papers of Isaac Newton*, vol. 1, edited by Alan Shapiro. The introduction, pages 16–20, discusses the dating of the two versions.

8. In *Never At Rest*, Westfall dates work on this manuscript to 1677–78, with the fire in 1678 (276–78).

Student Explorations in Optics

Prior to the "New Theory" article, Newton's formulations of his prismatic investigations were free of the exigencies of open public debate. The first mention of a prismatic experiment comes in the middle of a private notebook kept by Newton while a student at Cambridge, circa 1664–65. The earliest part of the notebook consists only of summary notes in Latin and Greek of the required reading in his scholastic curriculum, but in the middle he turns to independent contemporary reading. Not only are these later notes in English, but they tend to represent Newton's own thinking and experiences set in motion by the reading.

Among the notes on many subjects, Newton speculates on the nature of light. These speculations are set in motion by his reading of Boyle and Descartes on the subject, and perhaps by his attention at Isaac Barrow's lectures (McGuire 241–44). In his notes Newton develops a mechanical, corpuscular description of light and he includes a diagram of a light particle moving through ether (384–85), paralleling an earlier diagram he had made of a body moving through water (366–69). He follows these speculations with several observations from his experiences and some queries (386–89). It is in the context of this speculative, theoretical, private musing about commonly experienced phenomena, as inspired by his reading, that we must interpret his accounts of prismatic experiments some pages later in the notebook.

His first prismatic experiment is presented only as a proposal, in the imperative mode: "Try if two prismas, ye one casting blue upon ye other's red, doe not produce a white" (430). He continues with a diagram and more than a dozen additional similar combinations (432–33). His comments thereafter are highly speculative and theoretical, giving an interpretation based on the speed of moving light globuli affecting both the amount of refraction and the impact on the optic nerve. A chain of reasoning follows, in which is embedded an experiment he clearly represents himself as having done: viewing through a prism a thread—half its length colored red, the other half blue. One half appears higher than the other. After three more pages of theoretical speculation, this set of notes trails off into a set of diverse observations about colors exhibited under varying situations (432–45). Another more extensive list of observations of colors in various situations appears later in the notebook (452–65).

In 1666 Newton reorganized and expanded these notes into a more coherent private document entitled, "Of Colours." The twenty-two folio sheets, divided into sixty-four numbered experiments and comments, contain fifty prismatic and related observations (number 6–55). The organizing principle here, rather than being the associations of explanatory theorizing, is the apparent similarity of observed phenomena. This is solely an account of actual observations and experiments, until the end when dissection of an eye leads to speculations about the operation of the visual faculty.

Although the mode is now empirical rather than speculative and the theoretical literature inciting the investigation has now dropped from sight, the ordering of observations is still exploratory, as one experiment suggests another of similar format or pursuing a related idea. Topics of recurrent interest keep reemerging, but in no obviously planned manner nor with any clear argumentative order. Descriptions remain largely brief and qualitative. Newton has not yet sorted out what he has into an ordering theory.

Professorial Expositions

After these first student explorations, the next record of Newton's prismatic investigations consists of his lectures delivered at Cambridge University under the terms of the Lucasian Chair of Mathematics and Natural Philosophy, which he took up in late 1669 (replacing Isaac Barrow, who had stepped down in his favor). Manuscripts of these lectures were deposited, according to the terms of the chair, at the university library some time later—the first version perhaps in 1672 and a revised version perhaps in 1674 (*Optical Papers* 1:19). It is unclear how intensively Newton carried out prismatic investigations between 1666 and 1669, but his responsibilities as newly appointed chair occasioned a new formulation of what he had learned to that point.

This formulation was shaped by the situation and goals of the university lecture. The authoritative voice of the professor, introducing students into a coherent and comprehensive understanding of a subject leaves little room for serious challenge. The usual authority relations of the classroom that acknowledge the lecturer as the unquestioned source of knowledge, were further supported by both the dispirited intellectual atmosphere at Cambridge at the time and Newton's already established campus reputation for brilliance (Westfall, *Never at Rest*, 185–95). Newton's lectures, consequently, were expository in organization and tone, rather than persuasive or argumentative.

By the time of the lectures, Newton was no longer uncertain about the meaning of his experiments: different colors are differently refrangible—that is, they suffer different amounts of refraction when passing from one medium to another. This meaning beomes the center of his expository organization of both versions. In the first lecture, immediately after a motivating introduction, Newton presents this basic principle through a schematic diagram not attached to any specific experiment (48; 282). The rest of the text follows as an explanation and elaboration of that opening principle. Topics are presented sequentially, generally moving from the simple to the complex, divided into separate lectures and further divided by section headlines. About half of the exposition is mathematical, offering geometric demonstrations, derivations, and calculations. Proofs serve as elaborations rather than arguments. The other half is experimental, using the experiments to demonstrate features and consequences of the basic principle.

Because both mathematics and experiments are presented as elaborations of a consistent and coherent explanation and because these elaborations are so extensive (the first version comprised of eighteen lectures, and the second comprised of thirty-one), Newton can rely on the massiveness of the overall vision as a device both of persuasion and pleasure. Typically, the lecturer comments at one point, "I now repeat the experiment, however, so that I may pursue its various features that are no less pleasant for the experimenter than they are informative for our purpose" (63). Alternative theories are dismissed rapidly, in passing, steamrollered by the weight of the exposition and the lecturer's authority.

Newton's Perception of Journal Publication

Through his private journals and then his lectures, Newton had produced confident formulations, coherently connecting many experimental details and mathematical elaborations around a central principle. Yet the rhetorical situations of journal and lecture had not necessitated that Newton prepare a public argument persuasive to other experienced and confident natural philosophers holding contrary beliefs. When a student talks to his notebook and a monopoly professor talks to his class, the speaker in satisfying himself, satisfies all relevant critics.

Although not prepared for the contentiousness he was to meet, Newton nonetheless perceived journal publication as presenting a new kind of rhetorical situation, for he chose an entirely different form of presentation, as we will examine below. But before we examine the rhetorical understanding realized in the "New Theory" article, we should examine evidence indicating Newton's perception of publication in the *Transactions*.

First we have some reading notes made by Newton on the first twenty-four issues of the *Transactions* (Add 3958). These notes seem to date from a single period, probably 1668–69. The notes consisting of thirteen pages of close handwriting summarize all the articles in the issues. The summaries range from only a five-word general description to a three-hundred-word discussion. In general Newton gives fairly detailed attention to concrete observations, findings, and inventions, no matter what the subject, even if rather far removed from his apparent interests, such as whales found in Bermuda or ores found in Germany. But he is especially attentive to all claims about lenses, telescopes, and astronomical observations. On the other hand, he is generally rather brief on theoretical or speculative articles. Thus he seems to treat the *Transactions* as a repository of concrete reports. In only a few cases does he comment on these reports—they are simply taken as reported facts. In answer to Boyle's article on hydrostatics in issue 10, Newton comments "Descartes answer to this unsatisfactory," without giving reasons for his judgment. More notably he adds a twenty-four line parenthesis to his summary of Wallis' account of diurnal and annual motion in issue 16, giving his own opposed account: "Saith Dr. Wallis (But I observe . . .)." He offers no arguments, just his contrary account. His comments in neither case suggest that he felt that his opposition needed support through close argumentation.

If he read the *Transactions* as a collection of concrete facts, he may well have seen publication in it as an opportunity to present his own findings in preview of the book version of his lectures he was preparing. Oldenburg first wrote to Newton on 2 January 1672 requesting additional information about his reflecting telescope, a version of which had been brought down to London at the end of 1671 by Barrow and demonstrated before the Royal Society in late December (*Correspondence* 1:29).[9] There had been no prior contact between Newton and the Royal Society as far as we know except for Newton's reading of the *Transactions*.

Newton provided the requested details about his telescope in a letter of 6 January (79–81). On 18 January he sent a follow-up letter, adding further details about the telescope, but also including a promise of "an accompt of a Philosophicall discovery wch induced mee to the making of the said Telescope, & wch I doubt not but will prove much more gratefull than the communication of that instrument, being in my judgment the oddest if not most considerable detection wch hath hitherto beene

9. Several other letters published in Newton's *Correspondence* indicate the wide fame of his reflecting telescope in this period before publication of its details (1:4, 5, 72, 78, 88, 89).

made in the operations of Nature" (82–83). In a return letter of 20 January, Oldenburg responded to all the particulars of Newton's letter except this last one (83). Yet in a 29 January letter, Newton renewed his promise: "I hope I shall get some spare howers to send you also suddenly that accompt wch I promised in my last letter" (84). He fulfilled the promise in a letter of 6 February, which with a few editorial changes became the "New Theory" article (92–102).

Newton's persistence in pressing unrequested material on Oldenburg suggests that Newton saw in the Royal Society interest in his telescope, expressed in Oldenburg's letters, an opportunity to publicize what he considered a more significant finding. Having been offered an open door, he was prepared to make most of it. Moreover, before any of this interest in him had been expressed, he had already indicated his intention of publishing his nearly completed revised lectures.[10] Thus we must consider the "New Theory" article not as a preliminary finding of a work in progress but as a summary announcement of a much larger, essentially completed work.

Newton saw this completed work as true, consistent, massive, and important, but even more he saw it as concrete fact. The confidence and coherence of the lectures, presenting original work as what we would now call textbook knowledge—chosen by him as his initial topic in the only lectures on mathematics and natural philosophy being given at Cambridge University—combined with his hardly modest characterization of his findings in the letter to Oldenburg quoted above, suggest the depth of his conviction. Moreover, in the controversies to follow he was repeatedly to insist his claims were not hypotheses, but fact. A look at the character of his prismatic work can offer some insight into his sense of concrete conviction. His theory of colors—that the white light entering the prism is composed of all the colors that separate in the prism because of different degrees of refraction—is clearly a second order abstraction from simpler observations, such as that white light entering a prism emerges multicolored. Yet having once postulated that theory, Newton not only could explain a wide range of results, he could construct endless other experiments that always work out correctly. He could prismatically analyze and recombine light in a dazzling array of ways. And he did so, as he reported in his notebook, lectures, and later *Opticks*. This plethora of evidence and manipulation of the phenomenon can plausibly leave one, as it apparently did Newton, with a concrete

10. Alan Shapiro, in the introduction to *Optical Papers* 1:18, gives the evidence for Newton's intentions to publish the lectures in 1672.

sense that one knows exactly what is going on, that one's hands literally hold the phenomenon.

The Discovery Account as Rhetorical Strategy: The Opening of the "New Theory" Article

Newton's perception of the *Transactions* as a vehicle for concrete findings and his sense of the facticity of his own findings frame his solution of how to represent his claims in a letter to Oldenburg, which as he well understood would likely appear in print.[11] His overall rhetorical problem is to give an account of his findings so that they appear as concrete fact, as real as an earthquake or ore found in Germany, even though the events that made these facts visible to Newton occurred in a private laboratory as the result of speculative ponderings and active experimental manipulations. Moreover, the conclusions that he wishes to present as facts are based on complex interrelated statements, forming a detailed, elaborated picture with implications for many related phenomena, as he spelled out in his lectures.

Newton attempts to make his findings appear as concrete facts by establishing in a discovery narrative his own authority as a proper observer of concrete facts. This narrative presents him stumbling across a natural fact, as one would stumble across a rock. Then the narrative presents him as pursuing the oddity of this fact in a systematic way until he completes a proper description of the concrete fact. The article begins:

> Sir,
> To perform my late promise to you, I shall without further ceremony acquaint you, that in the beginning of the Year 1666 (at which time I applyed myself to the grinding of Optick glasses of other figures than Spherical,) I procured me a Triangular glass-Prisme, to try therewith the celebrated Phaenomena of Colours. And in order thereto having darkened my chamber, and made a small hole in my window-shuts, to let in a convenient quantity of the Suns light, I placed my Prisme at his entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours

11. In the correspondence over the account of the reflecting telescope, Oldenburg has already requested Newton's permission to publish (20 January; 83) and Newton had replied that he was "willing to submit my private considerations in any thing that may be thought of publick concernment" (29 January; 84).

> produced thereby; but after a while applying my self to consider them more circumspectly, I became surprised to see them in an oblong form; which according to the received laws of refraction, I expected should have been circular.
>
> . . .
>
> Comparing the length of this coloured spectrum with its breadth, I found it about five times greater; a disproportion so extravagent, that it excited me to a more then ordinary curiousity of examining, from whence it might proceed. (92)

The narrative continues his pursuit of the cause of this elongation for three pages until he reaches one experiment (which he calls "the experimentum crucis") that gets to the bottom of the matter.

The personal account of stumbling across an unusual fact was a common one used in the early *Transactions,* such as in the accounts in the first volume of the luminescent pickled mackerel and the putrefaction of maydew, as described in the previous chapter. Since Newton had taken notes on and summarized a number of such articles, imitating that model need not have been a highly reflective act.

This earlier part of the article relies heavily on the language of personal thought and agency as it unfolds the attempts of a baffled investigator to come to terms with a robustly visible phenomenon. The first person followed by an active verb forms the armature of most sentences: "I suspected," "I thought," "I took another Prisme," "I then proceeded to examine more critically," "Having made these observations, I first computed from them." At key moments he offers quantitative descriptions of his experiments, switching to third person existential statements: "Its distance from the hole or Prisme was 22 feet; its utmost length 13¼ inches. . . ." But even experimental quantities are framed by his limited agency: "The refractions . . . were as near, as I could make them, equal and consequently about 54 deg. 4′" (93)

The orderliness with which he pursues and isolates the phenomenon gives rhetorical warrant to the degree of facticity of language Newton allows himself in this section. That is, the credibility of the investigation helps establish the credibility of the fact and the credibility of the investigator. The procedure Newton presents himself as following, moreover, is exactly that of exclusions, as prescribed by Bacon: "What the sciences stand in need of is a form of induction which shall analyse experience and take it to pieces, and by a due process of exclusion and rejection lead to an inevitable conclusion" (Great Instauration B, 1, 137).[12] Newton, in

12. Sabra in *Theories of Light from Descartes to Newton* gives an exemplary explanation of Bacon's method of exclusions which Newton presents himself as following (175–84).

an orderly narrative, presents himself as analyzing the possible causes of the elongation of the prismatic image, and rejecting them one by one, until he settles on the final, inevitable cause as revealed by the experimentum crucis. He examines and excludes causation from the varying thickness of the parts of the prism, unevenness of the glass, and the breadth of the sun's image before he finally examines the differing refrangibility of the several colors. By presenting himself as acting as any proper Baconian should, Newton establishes an authority which he will rely on in the latter part of the article.

Most interestingly, Newton's persuasive structure here seems in many respects a close precursor of the kind of articles appearing a hundred years later in volumes 60 and 70 of the *Transactions,* as I have discussed in the previous chapter. Then the rhetorical problem had seemed rather similar to that perceived by Newton: presenting work done out of sight of peers, that gave novel accounts of newly found anomalous phenomena. In those cases, the narrative of the scientist operating under procedures, as any proper scientist might and ought to have done, is the main rhetorical resource to establish the credibility of the events and conclusions. Strikingly, Newton also offers a demonstration experiment at the end, although truncated, just as some of the later writers do. Whether this congruence is a matter of Newton serving as a model or similarity of rhetorical situation suggesting similar rhetorical strategy remains unclear.

What is clear is that much of Newton's account of his investigation in the "New Theory" article differs from details of his earlier accounts. In viewing these differences we need keep in mind that Newton was writing a number of years after the event when memory of dates and sequence may have faded and more significantly after his memory may have been restructured around later meanings. Yet parts of this autobiographical rewriting may reflect a conscious rhetorical strategy adopted for the current account.

Also we need distinguish between accounts of individual experimental events and accounts of the contexts—intellectual, emotional, autobiographical, sequential—in which the experimental report might be placed. The differences we are about to look at all develop contexts concerning the order, motivation, and interpretation of experiments—but not the actual results. As we shall see, the ensuing controversy leads Newton to focus increasingly detailed attention on the experimental events and on the superstructure of claims that can be constructed on those events, rather than on the kinds of contexts in which the events occurred. Thus the kinds of issues in which we see distortion here, fade from importance in later versions.

The first difference is over the dating of the first prismatic experiments. The article dates the purchase of prisms and experiments to 1666, but McGuire and Tamny date the notebook account of Newton's early prismatic experiments in late 1664 (13).[13] The 1666 date shortens the discovery period, emphasizing the lucky-find interpretation and obscuring the longer term interest.

More significantly, Newton represents his motives and attitudes in beginning this work as different from those evident in the notebooks. In the notebooks a theoretical investigation of the motion of particles and light as a form of corpuscular motion very clearly motivates the early experiments. Here, however, Newton presents himself as being moved by the phenomenon of colors itself and as having an attitude of naive wonder at the spectacle of nature: "a very pleasing divertisement to view the vivid and intense colours produced thereby" (92). He presents his observations as incidental to an interest in grinding nonspherical lenses. He does not present himself as trying to find out anything in particular until he stumbles across the surprise of an oblong projection, rather than the expected circular projection. Thus he presents himself as the Baconian collector, free of prior theoretical impulse, being only led into inquiry by the observed facts themselves.

Discrepancies also appear about the sequencing of experiments. According to the article, his first experiment was projecting a narrowed beam of sunlight through the prism against a wall, almost immediately leading to the discovery of the oblong projection. The notebooks describe no such experiment involving projection. The two experiments presented in the notebooks involved looking through the prism at bicolored objects (432–35). The later 1666 paper, "Of Colours," does record a pair of projection experiments, producing the oblong image (#7 and #8), but again only after a pair of experiments (#6) looking through the prism at a bicolored line and a bicolored thread. In the lectures, the projection experiment is presented first of the actual experiments, but not with the claim that it was chronologically first. Newton instead gives the pedagogical rationale that it was the experiment that enabled him to figure out what was happening and would therefore be most helpful to others' understanding (50–53; 284–85). The implication is, of course, that this experiment was preceded by others less easily intelligible.

The article then represents a series of experiments as following al-

13. Westfall also doubts the 1666 purchase date and offers evidence for earlier dating of the interest in lens-grinding (*Never at Rest,* 156n) and speculates on the possibility of both earlier and later dating for the purchase of prisms (157–58). In any event Newton would have had to possess some prisms before 1666 to have carried out the experiments reported in the notebook.

most immediately to eliminate possible causes of the effect—in order: thickness, termination with shadow or darkness, unevenness of glass, the curving of the light corpuscles. No such follow-up projection experiments are reported in the notebook, and only a pair in the 1666 paper (#15 and #16), involving a square water-filled prism rather than a triangular glass one. They also appear only after an intervening sequence of experiments, unrelated to the sequence discussed in the "New Theory" article. Further on, another experiment (#24) bears some similarity in method to one of the experiments reported in the article, but the context is entirely different, with Newton looking for color rather than shape. Nor is the finding reported in the later article even recorded in the earlier paper. In fact, the "New Theory" article very carefully separates the issue of differing refraction from that of colors (as does the later version of the optical lectures), but no such separation appears in the private notebooks or the manuscript "Of Colours." These earlier documents are more directly concerned with colors, with differing refrangibility appearing only in explanation of color phenomena.

Further, Newton in the article presents himself as withholding interpretation and belief concerning differing refraction until after the experimentum crucis, while in "Of Colours" after reporting only two very similar projection experiments (#7 and #8), Newton quickly announces his conclusion of differential refraction: "And therefore if theire sines of incidence (out of glass into air) be ye same, theire sines of refraction will generally bee in ye proportion of 285 to 286, & for ye most extreamely red & blew rays, they will be as 130 to 131+" (468). Having achieved closure, Newton moves immediately on to a different sequence of look-through experiments.

In the early paper, a series of experiments resembling what Newton later labelled the experimentum crucis, is presented much after the conclusion of differential refraction (#44–#46). These experiments are, morever, treated as a separate series with no explicit connection to the oblong observation. In the lectures, Newton also describes two experimental arrangements similar, but not exactly the same, to that of the experimentum crucis. Moreover, these variants appear in subordinate positions in the exposition, elaborating different propositions than in the "New Theory" article (96–97; 134–35; 448–51; 496–97).[14]

14. Lohne also discusses the ephemeral appearance of the experimentum crucis as a persuasive device in the "New Theory" article ("Experimentum Crucis"). Lohne also points out, that although Newton nowhere else in his optical writings uses the crucial experiment as a form of argument, the experiment so designated in the "New Theory" article becomes emblematic for his optical findings. As an emblem, the illustration of

These many discrepancies strongly suggest that Newton's discovery account was deliberately shaped for this occasion, to create the appearance of the discovery of a naturally found object, described by proper Baconian procedures. This does not mean Newton is lying about what he has found or is fabricating results. It is the sequence of thoughts and experiments that were fabricated. In manipulating the context in which he places his results, Newton revealed awareness that not only must he be convinced of the factual truth of what he has to say, but he must make it appear so to others. As we shall discuss below, the strategy he first chose to create that appearance did not forestall the kinds of criticism journal publication made possible. But in showing awareness of the rhetorical necessity of persuasion, Newton was setting himself on the path that would lead to a more compelling form of argument.

From Discovery to Theory: The Latter Part of the Article

Two sections of the article following the discovery account are easily identified, although unmarked by formal divisions or headings. An account of the invention of the reflecting telescope and a general exposition of the doctrine of colors solidify and extend the conclusions in the narrative.

The presentation of the invention of reflecting telescope as a direct consequence of his discovery of differential refraction helps reinforce the sense of concrete reality of the finding. First, it makes it clear that Newton was so sure of his discovery that he gave up his attempt to solve chromatic aberation through nonspherical lenses and set out on a whole new line of invention. Second, because the reflecting telescope not only worked but was a current sensation, it added certain persuasive force to the refraction findings, as though such a wonder could not be invented without that theory. (This persuasive connection is not only not necessitated by logic, Cassegrain had already independently discovered the reflecting telescope, without needing the push of a theory of differential refraction.)[15] Finally, bringing in the reflecting telescope in a subordi-

the experiment remains in increasingly schematic (and imprecise) versions in later optical publications, such as the 1722 Paris edition of the *Opticks* ("The Increasing Corruption of Newton's Diagrams").

15. Moreover, Newton's analysis of the incorrigibility of dispersion in lenses was in itself faulted, as shown by John Dolland a century later. The story of Newton's construction of his faulted argument is described in Shapiro, "Newton's Achromatic Dispersion Law," and Bechler, "Newton's Search" and "A Less Agreeable Matter."

nate way graphically emphasizes Newton's judgments about the greater importance of the refraction findings: if the telescope is considered of consequence how much more important is this doctrine.

The last section shifts from a discovery/invention narrative into a list of abstract propositions, supported by only limited concrete material. Newton calls these propositions stated authoritatively a "doctrine" and makes little attempt to persuade. For his and the propositions' authority, he relies on his credentials for proper method established in the earlier discovery narrative. Whereas the first two sections bear some resemblance to other articles in the *Transactions*, this last section seems in direct contrast with the stated principles and general practice of the journal.

To see how this shift is accomplished and the nature and consequences of this shift, we need first look at the turning point between the second and third sections. The second, just-completed section on the telescope is presented as a continuation of the chronological narrative of the opening, with Newton seemingly just turning his attention from the fundamental discovery to the technological consequence. This narrative continues through observations with the telescope, the current presentation of the telescope in London, and future plans for a reflecting microscope.

At this point Newton switches organization (from narrative to expository list), vantage point (from first person active to third object existential), level of discussion (from discovery and invention process to general claims) and specific topic (from differential refraction to colors) while seeming to be simply continuing his prior discussion. He does this by labelling the telescope narrative a digression and using the concluding sentence of the prior discovery narrative as an assumption for a generalized exposition. That narrative ended with a general statement, which as we have discussed is made to appear a natural experimental fact. The experimental particularity is now, however, left behind, as Newton treats the claim as a general principle which sets the terms for another general statement to be elaborated:

> But to return from this digression, I told you, that light is not similar, or homogeneal, but consists of difform rays, some of which are more refrangible than others. . . .
>
> I shall now proceed to acquaint you with another more notable difformity in rays, wherein the Origin of Colours is infolded. . . .
>
> The Doctrine you will find comprehended and illustrated in the following propositions. . . . (96–97)

The Doctrine is then elaborated in thirteen numbered general propositions with only passing reference to experiments or other empirical evidence. The vocabulary is general and statements are lawlike:

> 1. As the Rays of light differ in degrees of Refrangibility, so they also differ in their disposition to exhibit this or that particular colour. . . .
> 2. To the same degree of Refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility. . . .
> 3. The species of colour, and the degree of Refrangibility proper to any particular sort of rays, is not mutable by Refraction, nor by Reflection from natural bodies. . . . (97)

There is a logical and expository sequencing among these statements as Newton elaborates the difference between pure prismatic colors and mixed colors, leading to an explanation of white as a compound, the functioning of the prism, the appearance of the rainbow, and several other related phenomena. After the end of the numbered list appears a statement of even greater theoretical character and generality about the nature of light itself, following on the proposition that light is a quality and not a modification. This generalization revives his earliest speculations on the corpuscular character of light as raised in his notebooks:

> These things being so, it can be no longer disputed, whether there be colours in the dark, nor whether they be qualities of the object we see, no nor perhaps, whether Light be a Body. For, since Colours are the qualities of Light, having its Rays for their intire and immediate subject, how can we think those Rays qualities also, unless one quality may be the subject of and sustain another; which in effect is to call it a Substance. (100)

Newton seems careful to have excluded this deduction from his list of propositions of Doctrine, but neither does he label it a theory, speculation, or hypothesis. Rather he treats it as indisputable fact, a necessary consequence. Except for one qualifying "perhaps" (which will be discussed below) he has been rather careful to avoid any language admitting of uncertainty. In the next paragraph he in fact breaks off the discussion when he feels himself on less firm ground: "And I shall not mingle conjectures with certainties." Even the descriptive title appearing in the *Transactions* "A New Theory of Light and Colours" is Oldenburg's editorial addition.

Newton never uses the word theory or an equivalent. The term doctrine, which Newton does use to describe his generalizations, avoids any possibility of questioning or uncertainty. In his original letter to

Oldenburg, Newton is even more explicit about the facticity of his generalizations, but Oldenburg in his one major substantive editorial change[16] deleted this passage, appearing near the beginning of the third section:

> A naturalist would scearce expect to see ye science of those [Origin of Colours] become mathematicall, & yet I dare affirm there is as much certainty in it as 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 phaenomena (The Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt. To continue the historicall narration of these experiments would make a discourse too tedious & confused, & therefore. (96–97)

Then, as in the published text, he continues "I shall lay down the Doctrine First, and then, for its examination, give you an instance or two of the Experiments, as a specimen for the rest" (97).

This excised passage not only asserts Newton's certainty of his claims, but also characterizes the claims, gives his grounds for belief and explicitly discusses his strategy of presentation. Both the character of the claims—that they are mathematical—and the grounds for his belief—direct experimental proof—are nowhere in evidence in the article and can only be considered plausible in light of his manuscript of the optical lectures, which he anticipated publishing. Those lectures include extensive mathematical derivations, proofs, and calculations as well as pages of experimental demonstrations. Although they are not arranged argumentatively as a definitive proof, they carry the enormous weight of a coherent and empirically responsible system, as discussed earlier. In this article, however, we have little more than Newton's word to go on, relying primarily on the credibility he has established in the first two parts. Even the experimental evidence he calls into play is only sketched in a passing phrase, again throwing us back on his credibility for accuracy, method, and interpretation. Only a single demonstration experiment is described in any detail. A demonstration experiment is of course very different in character than a proof by experiment. The demonstration experiment simply puts the phenomenon on display; it does not resolve any question nor directly argue for any proposition.

His reasons for adopting this strategy are apparent and admitted.

16. The only other changes were Oldenburg's deletion of the words "& others" and the signature (*Correspondence* 1:102).

First, he would need a book (his lectures) to lay out his full system and evidence—that I take to be part of the meaning of his phrase "too tedious." But the other part of his reasons is suggested by the continuation "& too confused." That is, although he has been able to recount some of the experiments in the first part of the article to suggest an orderly discovery procedure of differing refractions, he cannot create as neat and pointed a story out of this other half of his claims. His lectures, because aiming at a complete exposition, do have a structure, but not an argumentative one—they are tedious and argumentatively confused in the accepted, pedagogically useful, academic sense. Here he has neither the space nor the appropriate relation with his audience to be the tedious professor.

He seems rather to have a collegial estimation of his readers, relying on them to fill in the necessary details. He reveals his assumption about readers being able to grasp the consequences and implications of his claims just before the demonstration experiment, when he states "I see the discourse it self will lead to divers experiments sufficient for its examination. And therefore I shall not trouble you further than to describe one of those, which I have already insinuated" (100). Reasonable readers should be able to follow his lead properly on their own. In a letter written to Oldenburg four days later (on 10 February), Newton confirms this perception of his readers and his relation to them: "I designed [the letter] onely to those that know how to improve upon the hints of things, & therefore to shun tediousnesse omitted many such remarques & experiments as might be collected by considering the assigned laws of refractions" (109).

Thus the persuasiveness of the whole seems to rely on a confidence Newton's voice maintains about the facticity of the specific events and general claims made. The first part of the article narrates the discovery of a general claim as a natural fact stumbled across and described through proper method. The last part presents an entire system of claims based only on the authority Newton has established earlier and the anticipation that, having read this article, readers will go out and see exactly what Newton saw. The article ends with an invitation that others indeed do that. Although Newton raises the possibility of admitting error, the article ends with a self-assured final clause: "If anything seem to be defective, or to thwart this relation, I may have an opportunity of giving further direction about it, or of acknowledging my errors, if I have made any" (102).

First Responses and Newton's Answers

The rhetorical strategy of establishing personal authority to underpin broad claims (technically known in rhetoric as the argument from ethos) seemed to have worked when Newton's letter was read aloud at the Royal Society meeting of 8 February, for it was met with general approbation. Oldenburg reports in a letter later that day to Newton, that the account was "mett both with a singular attention and an uncommon applause, insomuch that after they had order'd me to returne to you very solmne and ample thankes in their name" (107).

As soon as the account met more careful inspection, however, it came under question from many quarters, most immediately by Robert Hooke who took a copy home and within a few hours had written a long critique which he read at the next meeting of the society on 15 February. The controversy over the "New Theory" article, initiated by Hooke, lasted four years, into 1676, and seems to fall into three periods. In response to each set of criticisms, Newton develops a related set of rhetorical strategies, such that by the close of the period, the main features of the presentation of the *Opticks*, Book 1, are set.

The first set of criticisms, as outlined in the table below, are immediate responses to the reading of the text, in either manuscript or printed version. These were all initiated within two months of the article's publication. Newton had access to them all before writing a response to any of them, and Newton's answers were published in the *Transactions* before the end of the year.

Critic	*Date of criticism*	*Date Newton answered*	*Date criticism published*	*Date answer published*
Robert Hooke	Feb. 15	June 11	—[17]	Nov. 18
Robert Moray	?	April 13	May 20	May 20
Ignace	April 9	April 13	June 17	June 17
Pardies	May 11	June 10	July 15	July 15
	June 30	—	July 15	—
Generalized response to all three				
	—	July 6	—	July 15

Of this first round of controversy, Hooke's criticism was most significant, done first, yet answered in print last. Newton's attempt to formu-

17. Although at the time Hooke's critique was only read aloud and then circulated in manuscript, it has since been published in Birch (10–15), Newton *Papers* (110–15), and Newton *Correspondence* 1 (110–15).

late a proper answer to him influences all the other responses Newton makes during this period. That is, Newton received Hooke's critique (20 February) two weeks to the day after dispatching the original article. He immediately promised a rapid reply. (116) According to a letter of 19 March from Newton to Oldenburg, Newton appears to have drafted some comments, which he did not find sufficient (122).[18] He did not send Oldenburg his completed comments until June. In the meantime, while several times renewing his promise to answer Hooke forthwith and otherwise making reference to a task obviously very much on his mind (see *Correspondence* 1:137, 155, 159), he received and answered two other sets of correspondence on the same subject. When he finally replied to Hooke he relied on all the rhetorical tactics he had developed in the interim correspondence. He then reduced these rhetorical lessons to a single strategy embodied in a list of queries proposed in a letter to Oldenburg, which Oldenburg printed long before the reply to Hooke. In order to analyze the development of rhetorical tactics, we will examine the correspondences in the order of Newton's answers.

Newton first answered Sir Robert Moray, the first president of the Royal Society (1660–62) and a continued active member. Moray had proposed a series of four experiments to be carried out by Newton. The purpose of these variations of Newton's reported experiments is not spelled out, but they seem aimed at establishing whether Newton's results may have arisen from other causes or may have been contaminated in some fashion. Newton handled these proposals by spelling out in

18. Newton was probably referring to the manuscript on folios 445 to 447 in Add 3970. This manuscript reflects several of the features of Newton's eventual response, such as the appeal to the common ground of plain inquiry, the calculation of the relative errors of refracting and reflecting telescopes, the attempt to distance himself from the corpuscular hypothesis, the exploration of analogies, and an attempt to distinguish between compounded and uncompounded light. In this early draft, however, Newton's attempt to disown the mention of corpuscularity is awkward and involuted, his distinction between compounded and uncompounded light is not as crisply drawn, and his use of analogy is not contained by his later-developed argument that arguing by analogies is futile. Thus there is no attempt to switch the discussion from theoretical grounds to empirical ones, although he does complain that Hooke seems to be more concerned with asserting his hypothesis than inquiring after the truth. By recognizing his rhetorical problem in trying to put a wedge between Hooke's commitment to his hypothesis and the evaluation of Newton's own claims, Newton is only a step away from finding the rhetorical solution of discrediting hypotheses. Nevertheless, in this early attempt to answer Hooke, Newton tries to meet Hooke more on Hooke's own grounds. In the final version, discussed later in this chapter, Newton's newly developed strategies of disowning hypothetical discussion and reducing issues to empirical questions allows him to distance himself from the complaints Hooke makes and to mount more elaborate and forceful arguments against Hooke.

greater detail the methods and results of relevant experiments briefly mentioned in the earlier article and adding to them other relevant experiments he had already done but had not mentioned in the article. In this manner he demonstrated that he had already taken Moray's concerns into consideration (*Correspondence* 1:136–39; *Transactions* 7:4059–62).

In the first letter, dated 9 April, Ignace Gaston Pardies (professor at College Clermont in Paris and a committed Cartesian) objected to Newton's theory on the grounds that the experimental results were explicable by existing laws of optics and that certain other common experiments contradicted Newton's conclusions, which Pardies labelled a hypothesis. The largest part of the letter offers a geometrical derivation showing that according to received principles the expected shape of the image projected through a prism in Newton's experiment should be an oblong; therefore, Newton's results are unsurprising and do not require any new theory. Newton responded to this quickly (on 13 April) and simply, by adding an experimental detail he neglected to put in the article (but was in the account of the experiment in the lectures) and by redoing Pardies' geometric derivation (again paralleling an expansion in the lectures). To Pardies' other criticisms, he gives further detailed explanation and interpretation of experiments he had done and the common experiments mentioned by Pardies (*Correspondence* 1:140–44; *Transactions* 7:4091–93).

Here, as in the response to Moray, Newton is discovering the limitations of the elliptical style he had adopted for the article, and is returning to the fuller exposition of the lectures. As students may need full details as part of their education so that they can comprehend fully, so do one's peers, for although they are likely to fill in the details on their own, they are likely to do it in their own way, according to their own lights. Newton is discovering he cannot rely on shared visions and shared experience. Although he still insists that he is not here hypothesizing, he does willingly label his claims a "theory." By categorizing the phenomena he presents as "certain properties of light, which, now discovered, I think not difficult to prove" (144), Newton shows a nascent rhetorical awareness that discovery is different than proof, and that proof requires its own set of arguments.

Pardies' second letter (of 11 May) accepted all of Newton's added details and elaborations, but still denied his conclusions. Pardies claimed that alternate hypotheses explained the results equally well. Thus, although Pardies apologized at the end for calling Newton's conclusions hypotheses, Pardies still called them theories and considered them no more firm than the hypotheses of other people. More substantively, he treated Newton's claims as hypotheses by arguing there was no necessary link between the empirical evidence and the general claim (*Correspondence* 1:156–59; *Transactions* 7:5012).

Newton's reply to the second letter takes up the direct challenge of the concept of hypotheses and admits three alternative hypotheses which he considers legitimate (*Correspondence* 1:163–71; *Transactions* 7:5014–18; translation from Baddam 1:375–79). He even goes so far as to suggest ways of amending Hooke's, Descartes's, and Grimaldi's hypotheses to be consistent with his results. Thus he argues that there is no end to hypotheses: "since numerous hypotheses can be devised, which shall seem to overcome new difficulties." Newton claims that his doctrine is different in kind—for he has reduced the issues to empirical ones. This claim may not be precisely accurate, for we have already seen how much his early work was speculative, how his later experiments were driven by concerns arising from speculation, and how these early speculations creep into the article presentation. We will see later how he backtracks on these points to maintain his nonhypothesizing stance. Yet we can see the drift of a rhetorical strategy which attempts as far as feasible to reduce all questions to empirical issues.

Following this strategy, Newton takes his problem in this particular letter to translate the issues raised by Pardies into concrete empirical issues to be determined by experiment. "To lay aside all hypotheses" he considers the substantive force of the disagreement: "the whole force of the objections will lie in this, that colours may be lengthened out by some certain diffusion beyond the hole, which does not come from the unequal refraction of light or of the independent paths of light." Having redefined the issue so, Newton then recounts the experimentum crucis from the original article in greater detail and more concretely, with greater explanation of the meaning of the event. Moreover, he points to a procedural detail which Pardies may not have been aware of and which would lead to different results and different interpretation.

Whether or not crucial experiments are philosophically a valid and certain procedure, and whether or not they actually prove to be persuasive in the majority of disputes, in this particular case reduction of theoretical issues to empirical ones determined by a crucial experiment, elaborated adequately for all parties to share an understanding of the event, turned out to be a useful rhetorical strategy. Pardies replied soon thereafter,

> I am quite satisfied with Mr. Newton's new answer to me. The last scruple which I had, about the Experimentum Crucis, is fully removed. And I now clearly perceive by his figure what I did not before understand. When the experiment was performed after

> his manner, every thing succeeded, and I have nothing further to desire. (*Correspondence* 1:205–6; *Transactions* 7:5018; translation from Baddam 1:379.)

Such a successful outcome would certainly reinforce Newton's faith in the rhetorical power of detailed experimental accounts.

Answering Hooke

Robert Hooke's critique, although written almost immediately after Newton's paper was first read before the Royal Society, was by far the most difficult, penetrating, and challenging. In strategy the critique resembles Pardies' second letter, characterizing Newton's claims as hypothesis, no superior to a number of equally plausible hypotheses; however, Hooke scrutinizes in greater specificity Newton's corporeal assumptions, his own alternative wave hypothesis, and detailed points of divergence between the two. Following in order the numbered list of claims in Newton's paper, Hooke deems some of Newton's claims consistent with his own hypothesis, but offers explanations of some of the results to demonstrate that his hypothesis is of greater explanatory power than Newton's. Moreover, he denies that the experimentum crucis is indeed crucial in distinguishing between the two hypotheses, whereas Pardies only expressed some procedural uncertainties about the experiment. I leave out of discussion of Hooke's critique and Newton's response, technical issues concerning telescopes.

To answer, Newton adopts a strategy similar to the one he chose for Pardies' second letter: denying that his claims are hypothetical, discrediting hypotheses as a mode of investigation, then reducing the issues to empirical ones, and finally reestablishing the experimentum crucis. However, because of the intensity and specificity of Hooke's challenge, Newton must work harder and add new twists to the argument to achieve the same effects.

First, because Hooke more pointedly identifies the speculative remains of prior hypothesizing—the corpuscularity argument near the end of the article—Newton must distance himself from his comments. A simple denial of hypothesizing is not enough. He argues that this entire late passage was couched by a "perhaps" which identifies its hypothetical character and sets it apart from the main body of his more solid findings, which are discussed in terms independent of the alleged hypothesis (*Correspondence* 1:171–72; *Transactions* 7:5086). His invocation of the "perhaps" is a weak argument, for the word in the original article is proceeded by "it can be no longer disputed" and followed by related claims in equally urging language: "which in effect is to call it" and "we have as good reason to believe" (*Correspondence* 1:100; *Transactions* 7:3085). Moreover, the article continues with another more detailed set of questions based on the corpuscularity assumption before breaking off the discussion as entering into conjecture. Newton's defense that in the body of the article he avoided terms based on corpuscular assumptions seems equally suspect. The terms he says avoided all concern with the issue of perception, which is not a topic in the article, although he does discuss the issue in his notebooks. The public article gave him no occasion to use the assumption-laden vocabulary.

Whatever the strength or weakness of the defenses, Newton's face-saving and backpedaling aims to separate his main claims from anything he cannot identify as experimentally grounded. In all future presentations of the optical findings he was to avoid any language that would raise the specter of corporeality.[19]

But damage was done to Newton's position, and Newton felt it necessary to reconcile his doctrine with those details Hooke claimed were better accounted for by his own hypothesis. This was particularly important since Hooke had claimed that his gave a better account of color dispersion in layered plates, which seemed back then and still seems now, much easier to explain as a wave phenomenon. Newton explained how secondary wave phenomena arose by movement of corpuscles through the ether, as stones thrown into ponds create waves.

This attempt to reconcile wave phenomena with a particle account became the basis of Newton's explanation of rings in Book 2 of the *Opticks*, which we will not examine here.[20] In the current context, however, two points are significant. First, the discussion of rings, and thus the necessity of discussing wave phenomena, particularly in the cumbersome way Newton had to in order to reconcile it with his other conclusions, was separated out from the basic theory of refraction and colors. Once again he establishes clarity around an issue by distancing it from

19. Hooke began a response to Newton's answer in an unfinished letter (Newton *Correspondence* 1:198–205). It is uncertain whether Newton or Lord Brouncker, the current president of the Royal Society, ever saw the letter. In it, however, Hooke beards Newton at some length for having relied on the corpuscular hypothesis in the "New Theory" article. By ostensibly excusing himself for the mistake in attributing the corpuscular hypothesis to the article, Hooke introduces extensive textual evidence to show how the hypothesis appears to be taken. These apparent references were, of course, the cause of his "mistake."

20. In "Uneasily Fitful Reflections on Fits of Easy Transmission," Richard Westfall provides an enlightening account of Newton's corpuscular explanation of wave phenomena.

all troublesome matters. Second, the "fits of easy reflection" theory that derives from this formulation never achieves the crispness of presentation that the theory of refraction and colors does. That is, Newton is never able to reduce the material to a closely linked network of generalizations and empirical results of compelling character. In Book 2 there remains a large explanatory and hypothetical middle between claims and results, with consequences for the structuring of the argument.

In this answer to Hooke, despite having to address the comparison of hypotheses, Newton must distance his main claims from the discussion of rings, lest his whole set of claims be tainted with the brush of uncertainty. So he introduces the comparison of hypotheses with a denial of responsibility for what follows: "But supposing I had propounded that Hypothesis . . ." And he ends the comparison by disowning hypothetical discussion as unnecessary for his doctrine: "But whatever be the advantages or disadvantages of this Hypothesis, I hope I may be excused from taking it up, since I do not think it needful to explicate my Doctrine by any Hypothesis at all" (*Correspondence* 1:174–77; *Transactions* 7:5087–91). Between these two disclaimers, Newton uses an analogy with sound phenomena to suggest that Hooke's hypothesis is consistent with his doctrine and provides a plausible alternative, up to a point. That point is when the analogy reveals a patent absurdity.

Newton uses the breakdown of the analogy to discredit hypothetical discussion and move on to his experimental discussion: "You see therefore, how much it is besides the business in hand to dispute about Hypotheses. For which reason I shall now in the last place proceed to abstract the difficulties in the Animadversor's discourse, and without having regard to any Hypothesis, consider them in general terms" (177, 5091).

Newton uses here what strikes modern ears as strange locutions to talk about empirical results in contrast to hypotheses. To us terms like *abstracting* and *general* seem associated with theories and hypotheses, instead of being opposed to them. Newton has also used similar language both earlier in this response and in his second response to Pardies, so it cannot be dismissed as a chance usage. What Newton seems to be meaning here and in similar contexts is that concrete implications of a general character can be abstracted, or pulled out of hypotheses and treated empirically separate from the larger explanatory systems of the hypotheses. These generalities are in the form of empirical claims, and are thus open to empirical tests. Thus although certain generalizations may have their origin in corporeal hypotheses, they can be cast in empirical terms and tested in ways that do not explicitly invoke corporeality.[21] He continues in this passage immediately to propose a series of empirical questions, that are the result of this process of abstracting:

> 1. Whether the unequal Refractions, made without respect to any inequality of incidence, be caused by the different refrangibility of the several Rays; or by the splitting, breaking or dissipating the same Ray into diverging parts?
> 2. Whether there be more than two sorts of Colours?
> 3. Whether Whiteness be a mixture of all Colours? (178; 5092)

As in the second response to Pardies, the strategy is to reduce all issues to empirical ones. The effect of this, however, as Newton's language is beginning to recognize, is to create another level of claim, in between the large explanatory hypothesis and the specific empirical result. This claim is a generalization based on results and is to be held specifically responsible to empirical results. The premium is to be placed, in fact, on establishing as strict a link between the result and the claim as possible. Newton will work out further implications of this middle level empirical generalization for the form of his argument in the later exchange with Huygens, but here he already begins an amplification and reorganization of his materials around general empirical questions and their answers.

With respect to the first proposed question, Newton adds experimental and interpretive details to support his claim that the unequal refractions were not caused by factors other than differing refrangibilities. He recounts a number of experiments directly relevant to the claim but not reported in the original article and spells out their direct meaning with respect to the claim. The added detail of interpretation is of equal importance to the added detail of the account.

With respect to the second question concerning Hooke's claim of only two fundamental colors, Newton gives two arguments. The first argument is not an empirical reduction; quite the contrary, it is a theoretical argument of why Hooke's results could not be precisely as he reports them. Newton recognizes the weakness of this first form of argument

21. In modern terms, this is Newton's first attempted solution to the problem of translatability and gaining some measure of intertheory agreement on empirical grounds. The continuing difficulties Newton encountered in gaining agreement suggests the difficulties in translation between theoretical systems. On the other hand, the success of his later solution of building mutual understanding and experience from first principles on up suggests the possibilities of intertheory agreement based on carefully constructed empirical grounds.

by putting greater weight on his second, as revealed in his transition to the second point: "But, supposing that all Colours might according to this experiment, be produced out of two by mixture; yet it follows not, that these two are the only Original colours" (180; 5095).

The second argument is indeed the more essential, developing a fundamental distinction between simple and compound colors. Although Newton had made the interpretive distinction in the original article, he had not applied it to the kinds of experiments Hooke, and later Huygens, discuss. Only by making the application of this distinction to such experiments intelligible and persuasive could Newton establish publically what he believed was the proper interpretation of the results. The manner of Newton's handling of this point is particularly important, for he finds it necessary to make this point repeatedly, and each time he finds a crisper way of articulating it. The final way he finds, in the Huygens exchange, will provide a structural model for Book 1 of the *Opticks*.

In articulating his position more clearly, he also refines the particulars of his claims, in response to forceful objections concerning the kind of light accounted for by his theory. The clarifying of the definitions of simple and compound colors, makes clearer the issue of the character of various forms of white light, including the sun's. As Alan Shapiro elucidates in "The Evolving Structure of Newton's Theory of White Light and Color," the challenges to his earlier formulations, by himself and others, forced him to withdraw from broader claims about all white light and about the sun's light. Even the final narrowed definitions of the *Opticks* contain some irregularities which troubled Newton, but which he was unable to resolve (see also Shapiro, "Experiment and Mathematics," and Westfall, "Development"). Interestingly, his reformulations are often accompanied by statements that he is only clarifying or elaborating an earlier formulation, rather than that he is retracting a position. Whether this is simply a face-saving ploy or a reflection of the psychological experience of being forced to a more precise statement is difficult to disambiguate. The terms of a claim can be refined through an agonistic struggle, and the refinements may have substantive consequences for the claim. By clarifying terms you can clarify them to yourself as well as to others, and that clarification may make distinctions visible that were not visible before. Is a prior obscurity rightly perceived as error? In any event, the refined claim is an improvement in being more defensible, given the current means of argument and use of empirical evidence. By allying itself more closely to the available empirical evidence, the improved claim is actively relying on passive constraints for its force, as Fleck argues is characteristic of modern science.

Newton recognizes the difficulty of communicating the distinction between simple and compound colors in the sentence introducing the substantive discussion: "But, because I suspect by some circumstances, that the distinction might not be rightly apprehended, I shall once more declare it, and further explain it by examples." He begins the substantive discussion by defining the two terms directly and contiguously so to make evident the contrast: "That colour is primary or original, which cannot by any art be changed, and whose Rays are not alike refrangible: and that compounded, which is changeable into other colours, and whose rays are not alike refrangible" (180; 5095). In the original article this distinction had been made descriptively over two substantial paragraphs, rather than as tightly contrasted definitions. Newton now continues to provide empirical prismatic tests to distinguish between the two kinds of colors. He then offers an experimental confirmation of the distinction by proposing that if two objects of apparently the same color are so tested and found to be distinguishable, then the colors must be of two kinds.

In this manner Newton has presented a general issue, reduced it to an empirical question, then organized the experimental material so as to present a direct line of reasoning tying the empirical to the general.

The last query presents no such complicated problems of tying claim to evidence, for Hooke had presented a rather straightforward empirical challenge: "Methinks, all the coloured bodies in the world compounded together should not make a white body, and I should be glad to see an experiment of that kind done" (Birch 14; Newton *Correspondence* 1:114). After reiterating an experiment already reported and examining the difficulties which Hooke would have in explaining away that result, Newton offers at least twenty-one other experiments and observations to the same point.

After this inductive pummelling of Hooke, Newton simply reasserts the importance and soundness of the experimentum crucis, without adding new substantive discussion.

Concluding the First Round

In dealing with the queries and objections of Moray, Pardies, and Hooke, Newton has learned to reorganize his discussion to argue specific claims or positions. Moreover, he has found it most useful to tie his discussions as closely as possible to empirical results. Even reasoning processes are to be supported by empirical procedures at each step.

As a conclusion to his response to this first set of challenges, Newton sends Oldenburg a set of experiments which he considers appropriate for testing what he now calls his theory. Although this list of experiments was published on 15 July, 1672, along with his response to the second Pardies letter and before his response to Hooke, it was written almost a month after he had finished all the other responses (see table, p. 100). His continued use of the term *theory* (first used in responding to Pardies) in place of the original term *doctrine* indicates his recognition that he is offering a higher level of claim beyond simple descriptions of experimental fact, yet still to be distinguished from hypothesis. He wishes, moreover, to remove discussion of his theory from confutation among a variety of opinions, by having all interested parties suspend all objections deriving from hypotheses and convincing themselves by "Experiments concluding positively and directly," as he claims to have done (218; 4044).

To that end, he reduces the relevant issues to a series of eight experimental questions. Although he provides no answer to these questions here and gives no indication that he has indeed satisfied himself on these issues, we know from his other writings that he already has confirming results for all of these issues. By leaving the questions apparently open, he takes the stance of letting the facts speak for themselves. However sure he is of the facts, nonetheless, Newton will come to distrust even this rhetorical strategy, for future controversy was to convince him that people won't read the experimental facts correctly unless he reads the facts to them. Indeed he has already begun to have second thoughts about the elliptical approach he has taken. In a letter of 8 July 1672, Newton writes to Oldenburg:

> Touching the Theory of Colours I am apt to believe that some of the experiments may seem obscure by reason of the brevity wherewith I writ them wch should have been described more largely & explained with schemes if they had been then intended for the publick. (212)

The Second and Decisive Round

The first round of controversy had set all the wheels of a new style in motion, but the style had not yet found its settled form. For that, Christian Huygens' challenge the following year was necessary. Huygens originally had been favorably impressed with Newton's article, but had an increasing number of questions with time. In his fourth letter to Newton, transmitted by Oldenburg on 18 January, 1673, and published in the *Transactions* of 21 July, Huygens offers serious criticism. He takes his lead from Newton's reduction of the controversy to empirical issues by pressing one of Hooke's earlier empirical proposals:

> I have seen, how Mr. Newton endeavours to maintain his new Theory concerning Colours. Me Thinks, that the most important Objection, which is to be made against him by way of Quaerre, is that, whether there be more than two sorts of Colours. (255; 6086)

Huygens then claims that yellow and blue can combine to form all other colors including white and proposes an experiment (which he admits to not having yet done, for the thought came to him just as he was writing).

Whether two colors might combine to form all others was not a question Newton thought essential to his findings and is not in his list of eight queries, nor is it considered anywhere in Newton's writings except in response to Hooke and Huygens. As we have seen, Newton considered experiments demonstrating this flawed because they did not distinguish between compound and simple colors. Once that distinction is accepted, the importance of such experiments for the validity of his theory evaporates. Yet because such proposals were such a sticking point with his opponents, because they did not see the import of the distinction, Newton had to lead his readers through the distinction.[22]

Newton's first response to Huygens was written on 3 April, 1673, but not published until 6 October due to an editorial mistake. It begins with a methodological point, suggesting that using compound colors to compound again will only lead to confusion and that one must begin with simple colors. Thus he insists on his distinction as a necessary methodological consideration even prior to its interpretive use. That is, while he still leaves the empirical confirmation to the reader, he recognizes more and more how that empirical experience must be led and con-

22. It also was apparently an unpleasant challenge that Newton first attempted to evade by withdrawing from the Royal Society. In a letter of 8 March 1673, to Oldenburg, Newton first suggested that Huygen's critique needed no response, since it was part of a private correspondence of Oldenburg and Huygens (despite the well-established practice by that time of reading the private correspondence at Royal Society meetings and printing it in the *Transactions*). However, after promising to respond if Oldenburg pressed the issue, Newton requested to resign from the society, for he lived too far from London to take advantage of the meetings (262). Oldenburg answered on 13 March by excusing Newton from paying dues (263). The matter was then dropped, and within three weeks Newton sent his first reply to Huygens' critique.

strained so as to produce the proper results. Experiments must be done properly, embedding the proper assumptions and in the proper order. This further implies, so that the assumptions do not appear simply a priori, that the experiments establishing assumptions must be done first. In this case the experiments establishing the distinction between compound and simple colors logically must precede experiments on the production of other colors. The later experiments are to be constrained by the conclusions of the earlier.

Starting as Newton did with an immediate sense of the concrete and self-evident facticity of his findings, Newton has been discovering that empirical experience is a variable thing. His readers did not immediately understand that certain claims implied certain prior completed experiments nor that other experiments follow by immediate implication (as in the Moray case). Nor did they always perform the experiment in the way Newton had, which led to varying results and disagreements (as in the Pardies case). Nor did they even do the experiments Newton proposes (as is evident in Hooke's critique and following letters). Nor did they even do experiments they themselves thought up (as in this first letter from Huygens). Since Newton himself was convinced that anyone who went out and did the proper experiments could not doubt the concrete truth of his doctrine, it is not odd that he would get increasingly frustrated with what he might consider the readers' obtuseness in getting the experiments right. On the other hand, he is realizing that he must provide much more detailed instructions—of logical procedure, sequencing, and interpretation, as well as of apparatus and procedure—in order that they get the experience right. In Newton's words to Huygens we find both his attempt to challenge better experimental procedure from his critics and his increasing disillusion with the certainty of this happening:

> This, I confess, will prove a tedious and difficult task to do it as it ought to be done; but I could not be satisfied, till I had gone through it. However, I only propound it, and leave every man to his own method. (264; 6108)

Before discussing concrete experiments in answer to Huygens, Newton argues by analogy about how implausible Huygens' hypotheses are, but again immediately disowns this analogical discussion as being irrelevant to his purpose in exhibiting concrete phenomena—as he had done with Pardies and Hooke. Newton then discusses how Huygen's experiments out to be done properly and how they ought to be interpreted. In passing, he elaborates some of his own experiments, which, although producing results resembling Huygens', point to substantially different conclusions. Thus he ends with an experimental challenge as to what Huygens must do, and even that result, if obtained, Newton promises to show is not what it appears:

> If therefore M. Hugens would conclude anything, he must shew, how White may be produced out of two uncompounded colours; wch when he hath done, I will further tell him why he can conclude nothing from that. (265; 6110)

Newton further goes on to claim he had tried that experiment and had not been able to get the results that Huygens would wish for.

In a letter 10 June (published 6 October), Huygens responds by backing off from further disagreement, except to bite at the bait that Newton had offered:

> I list not to dispute. But what means it, I pray, that he saith; Though I should shew him, that White could be produced if only two Un-compounded colors, yet I could conclude nothing from that. Yet he hath affirm'd in p. 3083 of the Transactions, that to compose the White, all primitive colors are necessary. (288; 6112)

At this point the discussion is now focussed on a single interpretive issue, which is Newton's task to make clear and unquestionable. Newton in his answer of 23 June (published 21 July) explains his position three times, ending with an argument in the form of a compelling mathematical derivation.

Newton had been thinking about such a format since having completed the first round of controversy, which ended in him reducing his claim to a series of empirical queries. Shortly after receiving the list of queries from Newton on 7 July 1672, Oldenburg had requested Newton in a letter of 16 July to elaborate on the appropriate experiments. On 21 September Newton replied to this request belatedly:

> I drew up a series of such Expts on designe to reduce ye Theory of colours to Propositions & prove each Proposition from one or more of those Expts by the assistance of common notices set down in the form of Definitions & Axioms in imitation of the Method by wch Mathematitians are wont to prove their doctrines. And that occasioned my suspension of an answer, in hopes my next should have conteined the said designe. But before it was finished falling upon some other business, of wch I have my hands full, I was obliged to lay it aside. (237–38)

In this mathematical form of proof Newton sees a way of compelling assent and ending controversy.

In presenting his answer to Huygens on this highly focused issue, he has his first opportunity to display this new rhetorical strategy. Nonetheless, it takes Newton three levels of presentation in this one letter to reach the mathematical form he seeks. That is, he presents his main point in three different ways before the issue can be turned to one of mathematical argumentation from first principles and supporting statements. He starts with a direct answer to the issue at hand cast in general form. He then turns the answer into a general position which he supports by experiment and then uses to analyze Huygens' proposed results. Only then does he derive his conclusions from first principles. That is, he must lead Huygens from a hostile theoretical position, focused on a particular point in contention, through an alternative answer instantiated in an experiment, to a reconception of the original experiment. Only then can the exact meaning and full implications of the original experiment be made accessible by placing it within a rigorously drawn new system. The procedure seems to be to compel the hostile Huygens to take Newton's system seriously in its own terms instead of seeing it just as a proposal competitive with the Cartesian one held by Huygens.

The first statement of Newton's response is presented in a few general sentences of direct answer. The issue is why Huygens could not conclude anything from the compounding of white from two colors. Newton answers because "such a white would . . . have different properties from the white . . . of ye Sun's immediate light, of ye ordinary objects of our senses, & of all white Phaenomena that have hitherto falln under my observation" (291; 6087). Moreover, those differences of property would support his theory, for they would reveal how ordinary whites are produced by more than two colors.

To explain this difference more precisely, Newton must shift to the second level of his argument. This shift is well marked by a transitional sentence: "But to let you understand . . . I shall lay down this position" (291; 6088). This shift of argumentative level is accompanied by a change in discourse focus, organizational pattern, and graphic layout. The position he offers is italicized and separated from the surrounding text. It becomes the central focus of the following three paragraphs, organized as experimental demonstration, deduction of consequences, and application to the Huygens' experiment.

The sentence style is also particularly interesting here, in light of Newton's expressed intent in developing a mathematical type of argument. Earlier in recounting experiments, Newton had most frequently adopted a first-person past-tense narrative, although for the demonstration experiment at the end of the "New Theory" article he had adopted general imperative instructions; e.g., "In a darkened room, make a hole in the shut of a window . . ." (100; 3085). Here, although he claims to have done this experiment and many like ones, and although he is using it in support of a claim rather than as a demonstration, he again uses the second-person imperative mode. This casts the responsibility for doing the experiment back on the reader, as he had been trying to do with his queries. The instructions, however, have the advantage over the queries of leading the reader more strongly and precisely.

Newton takes charge even further by commanding not only the actions but also the interpretive process, as is done in a geometrical demonstration: "Let α represent an oblong piece of white paper" (291; 6088). Newton had of course used such language of mental command when engaged in geometric derivations and analyses of optical phenomena during his lectures, but here this is being applied directly to the experiment. This strategy of interpretive command further has the advantages of increasing the appearance of generality to the claims and lending the universal force of geometry. Moreover, it then presents the results of the experiment in the precise form and mode for the continuation of a geometric argument. With no change of tone, the second paragraph deduces conclusions, which are then immediately applied to Huygens' proposed experiment, which is treated as an abstract geometrical problem, since Newton does not consider Huygens' results plausible. Moreover, this hypothetical geometrical problem is described in the exact same style of the actual experiment ("suppose that A represents . . ."). Even the same diagram and reference letters used to describe the actual experiment are reused for the hypothetical.

Newton has succeeded in integrating an actual experiment into a general geometrically styled argument. Doing so, he has eliminated the need for the interpretive arguments he has needed earlier to make clear the import of the experiments. Plus he has found a way of totally divorcing his claims from his explanatory hypotheses, which he kept finding himself tempted to discuss and then having to disown as irrelevant. That is, the experiments don't find a meaning in any external explanatory scheme, but only within the scheme in which they are serving as cogs. Moreover, since the language of presentation is so tightly linked into that immediate scheme, no loose linguistic ends suggest switching to any analogical or explanatory mode of discourse. The geometrical precision ensures that its own boundaries are maintained. And finally, the geometrical argument in support of general propositions removes the local and direct confrontation with specific opponents. The text is addressed to a general proposition rather than against Hooke or Huygens.

To complete this translation into a geometrical argument, Newton follows this three-paragraph demonstration with a brief return to personal confrontation to indicate that the points being made in this abstracted form are exactly the same points he had made in earlier presentations. Specific references and appeals to comparison attempt to establish that this is the position he has been maintaining all along, although not quite in this general form. This cross-reference also serves as a personal character defense against Huygens' comment that Newton "maintains his doctrine with some concern." Newton here suggests that the whole problem has been the lack of the readers' comprehension, and he has only been explaining previous answers to people who were not able to see his points.

Having succeeded in translating the point at contention into a geometrically styled argument of the lemma sort, and having established and elaborated that lemma, Newton has changed the level of discourse. Now he can begin to lay out his whole system in this general mathematical form, therby indicating the precise meaning of the current claim. Again he recognizes the transition through a single sentence similar in syntax and phrasing to the transitional sentence cited earlier:

> However, since there seems to have happened some misunderstanding between us, I shall indeavor to explain my self a little further in these things according to the following method. (292; 6089)

This last level of mathematicization of the argument is further recognized by the organization and labelling of the parts: five numbered items under the italicized, separated, centered heading *Definitions* and nine numbered items under *Propositions*. Each definition consists of a single naming statement: e.g., "I call that Light homogeneal, similar or uniform whose rays are equally refrangible" (292; 6090). Similarly, each proposition statement consists of a single-sentence claim followed by one or more sentences of proof. The proof is sometimes experimental, as after the first two propositions. And sometimes the proof is deductive: e.g., "by Def. 1. & 3. & Prop. 2. & 3" (293; 6091).

Round Three: Reducing Disagreement to Error through System

Newton had now satisfactorily solved how to present his optical findings in a compelling manner within a critical forum of competing researchers. The remaining exchanges of letters required no rethinking or reformulation of argument, only a reiteration of existing statements. In this last set of exchanges with Francis Line and his students, Newton heavily cross-references his previous statements, using them as an articulated, coherent system which, when properly read, can answer all relevant questions and problems. In this constant pointing back to previous experiments and arguments, Newton displays increasing irritation with the inability of some readers to carry out proper experiments, to make appropriate judgments, or even to read his original text correctly. These developments strengthen Newton's rhetorical strategy of leading the readers very carefully down an intellectual and experiential path, controlling both the reasoning and experience of the reader. In what modern literary theory would call a closed text, Newton does the thinking and experimenting for the reader, with the reader needing only to comprehend each step as he is presented with it.

This last round of correspondence was initiated by a letter of Francis Line to Oldenburg at the end of 1674, doubting Newton's account of the first experiment in the "New Theory" article. Oldenburg replies, under Newton's instructions, by referring to Newton's second answer to Pardies (328; *Transactions* 9:219). In a second letter, Line persists in claiming that his own results differ from Newton's and questions specific lines from Newton's earlier papers. A supporting letter follows from one of Line's students, John Gascoines. Newton responds by giving increasingly detailed and directive instructions, heavily interspersed with exact-page cross—references. Newton also cautions about specific possible errors. For example,

> 1. Then, he is to get a Prism with an angle about 60 to 65 degrees, N. 80, p. 3077, and p. 3086. If the angle be about 63 degrees, as that was which I made use of N. 80. p. 3077, he will find all things succeed exactly as I described them there. But it be bigger or less, as 30, 40, 50, 70 degrees, the refraction will be accordingly bigger or less, and consequently the Image longer or shorter. . . . But he must be sure to place the prism so, that the refraction be made by the two planes which comprehend this angle. I could almost suspect, by considering some circumstances in Mr. Linus's Letter, that his error was in this point, he expecting the Image should become as long by a little refraction as by a great one; which yet being too gross an error to be suspected of any Optician, I say nothing of it, but only hint this to Mr. Gascoin, that he may examine all things. (419; *Transactions* 10:560)

The only slightly veiled irritation of the last sentence reinforces the impression, given by the simplified and directive instructions, that Newton by now is impatient with what he perceives as experimental and intellectual imcompetence. This impatience abates only slightly in the next exchange, when Anthony Lucas takes over from Gascoines. Lucas grants the substance of Newton's last answer, but raises a new issue, over the exact proportions of measurements resulting from the experiment in question. Lucas then provides an account of some other experiments which he claims contradicts Newton's theories. Newton, praising Lucas for being serious enough to actually do the experiments and taking some care over them, reciprocates by reporting fresh measurements to suggest how the quantitative results can be reconciled. Newton, however, simply dismisses Lucas' new experiments as beside the point and based on misunderstandings. Newton points back to his already published experimentum crucis as definitive.

Most interestingly, however, Newton here mentions for the first time in any letter for publication his completed book on the subject. The mention is to establish he already has considered and explained the kinds of experiments Lucas reports. While arguing here that only the experimentum crucis is important, Newton is yet coming to recognize the persuasive force of the entire system to answer all objections and to demonstrate how all related phenomena are to be accounted for.

> Had I thought more requisite, I could have added more [experiments]: For before I wrote my first Letter to you about Colours, I had taken much pains in trying Experiments about them, and written a Tractate on that subject, wherin I had set down at large the principal of the Experiments I had tried; amongst which there happened to be the principal of those Experiments which Mr. Lucas has now sent me. (174; 703)

Having worked out a full system of claims, representations, and arguments, and having a plethora of experiments, observations, and phenomena reconciled to that system, Newton reduces disagreement to error—errors in reading and errors in conceiving, carrying out, and interpreting experiments.[23] In further correspondence not published in the *Transactions*, Newton with increasing impatience identifies Lucas'

23. Although Newtonian system gained authority in England, it did not do so in continental Europe, where a different conceptual/empirical/rhetorical/social climate reigned. There the objections excluded in England through Newton's narrowing of issues and experience remained alive, as described in Henry Guerlac, *Newton on the Continent*. The rhetorical interchange between Newtonian England and the continent is explored in part in Schaffer, but interesting questions remain to be studied concerning the interaction of the two distinctive rhetorical systems.

"mistakes" against the authority of his entire theory (see, for example, *Correspondence* 2:254–60, 262–63). Newton, finding Lucas incorrigible, finally breaks off entirely in a letter to John Aubrey who had taken over Oldenburg's role as intermediary and editor.

> Mr. Aubrey
> I understand you have a letter from Mr. Lucas for me. Pray forbear to send me anything more of that nature. (*Correspondence* 2:269)

The Juggernaut as Persuasion: Book 1 of the *Opticks*

Newton was never again to publish optical results in a journal, nor was he to publish anything else in the *Transactions* or any other journal, except for a minor piece in 1701 on a scale of temperatures. He was to present his major physical findings only within the complete and comprehensive argumentative systems of the *Opticks* and the *Principia*. Moreover, not wishing to rekindle any of the controversies (or misunderstandings, as he saw them), he was not to publish the *Opticks* until 1704, even though in 1677–78 he was on the verge of publishing an earlier version based on the controversy correspondence until a fire in his rooms destroyed the manuscript, and even though he had essentially completed the final version by around 1694.

That final version totally scraps the expository structure and much of the content of the previously completed book of his optical lectures and adopts the argumentative structure that we have seen developing in the correspondence published in the *Transactions*. The book, in the manner of a Euclidean tract, moves from definition to axiom to propositions. The propositions, supported by experimental proofs, are sequentially arranged to create an ironclad deductive argument, as revealed by the organization, the hierarchical ordering of claims, the internal numbering system, and the graphic layout. The beginning of the analytical table of contents prepared by Duane H. D. Roller for the 1931 reissue serve as sufficient example of the structure and organization.

ANALYTICAL TABLE OF CONTENTS*

THE FIRST BOOK OF OPTICKS

Book I, Part I

*Prepared by Duane H. D. Roller.

The complete system is presented as a logical and empirical juggernaut, with every step in the reasoning backed up with carefully described experimental experiences precisely related to the formal proposition. As Newton states in the opening sentence of the text:

> My Design in this book is not to explain the properties of light by Hypotheses, but to propose and prove them by Reason and Experiments.

The reader is moved down a path of reasoning and vicarious, virtual experience through the experiments reported. The placement of the experimental descriptions within the developing framework also makes it more likely that the experiments will be understood, performed, and interpreted in the manner intended by Newton, if the reader wishes to move from the virtual, literary experience to the laboratory.

This control of reason and experience within a tightly developing network of claims, experimental representations, and deductions is well illustrated in his elaboration of "Prop. II. Theor. II. The light of the Sun consists of Rays differently Refrangible" through experiments numbers 3 through 10. The announcement of the theorem is immediately followed by the subheadings "The PROOF by Experiments. Exper. 3." The text proper begins "In a very dark Chamber, at a round Hole, about one third Part of an Inch broad, made in the Shut of a Window, I placed a Glass Prism" (26).

Experiment 3 is a much more detailed account of the experiment described at the beginning of the "New Theory" article, resulting in the elongated image. Here, however, the experiment is detached from any discovery account. It is presented only to establish the result. Both the methods of obtaining the results and the results themselves are told in far greater detail and precision than in any previous presentation. For example, the description of the solar image, which in the article was only a sentence long, here is given almost a page. Not only is the immediate image described, but all the variations that occurred as Newton rotated the prism. Not only does this description answer possible questions about what occurred, but it recreates the experience with sufficient narrative intensity for the reader to imagine the event. Througout this and other experiments, Newton emphasizes the care he took, the places where mistakes might occur and which therefore required even greater care, and the many variations and trials he ran in order to avoid error and anticipate all disagreements.

Further, to establish the result as important, Newton presents a full geometrical derivation of what the results should have been given traditional optics. Thus the reader is carefully held in tow, to see what Newton wants him to see in detail, to be silenced on all possible objections, and to find the proper meanings in the experimental experience.

Newton is next careful not to require that the readers find too much meaning in the experiments. He marks the steps of the argument very carefully, nowhere leaving a gap in reasoning that a critical reader might use to undermine the argument. He in fact calls to the reader's attention the limits of the conclusions that can be drawn from each experiment. After a description of experiment 4, he comments,

> So then, by these two experiments it appears, that in Equal Incidences there is considerable inequality of Refractions. But whence this inequality arises . . . does not appear by these experiments, but will appear by those that follow. (34)

Experiment 5 is then prefaced by some comments on its design to indicate how it is aimed at demonstrating a specific point not demonstrated by the previous experiments. The experiment is then described in the language of a geometrical demonstration referring to a schematic diagram. "Illustration. Let S [Fig. 14, 15] represent the sun, F the hole in the window, ABC the first Prism, DH the second Prism . . ." (35).

From the time of his student notebooks Newton had used schematic diagrams to display his experiments and analyses, but here the incorporation of the experiments into a geometrical argument, and the consequent easy movement from experimental description to geometrical analysis, often in reference to the same diagram, gives these representations a special function within the argument. They are treated as both real and ideal, combining experience and reasoning in a step-wise construction of reality. Indeed, immediately following the presentation of experiment 5, a schematized analysis of the results occurred, using prior experimental results and geometrical derivations and assumptions. The reader is again carried one more step into a carefully constructed perception of an ideal/real world. By this point Newton had a practical sense of the modern concept that every observation was theory laden. He wanted to make sure that his experiments were seen through the proper theory loading.

After Newton has marched his reader through almost forty pages of narration and discussion, through all the steps of experience and reasoning creating a tactile and ideal proof of the theorem, he then sums up the argument to this point. The summation is not just a series of claims, however. It is a series of experiences that reveals a coherent world, a felt vision of a world that we have all shared, turned around, and shared again:

> Now seeing that in all this variety of Experiments, whether the Trial be made in Light reflected, and that either from natural

> bodies, as in the first and second Experiment, or specular, as in the ninth; or in light refracted, and that either before the unequally refracted Rays are by diverging separated from one another, and losing their whiteness which they have altogether, appear severally of several Colours, as in the fifth Experiment; or after they are separated from one another, and appear colour'd as in the sixth, seventh, and eighth Experiments; or in Light trajected through parallel Superficies, destroying each others Effects, as in the tenth Experiment. . . . It's manifest that the Sun's Light is an heterogeneous Mixture of Rays, some of which are constantly more refrangible than others, as was proposed. (62–63)

Newton has vicariously given us that same concrete feeling of holding the phenomenon in our hands and turning it over and over again.

Through this juggernaut of a system, Newton has been able to create an authority and certainty of argument that seems to go against the tendency of the period to find in empirical experiences only uncertainty and probabilities. Such tentativeness is evident in Hooke's and Huygens' insistence of maintaining alternative hypotheses in the correspondence examined here, and in Huygens' own work on optics, *Treatise on Light*. In the preface to that work, Huygens states that the empirical evidence he presents cannot produce certainty, although "It is always possible to obtain thereby to a degree of probability which very often is scarcely less than complete proof."

The persuasive historical accounts of the rise of uncertainty and probability by Hacking, B. Shapiro, Dear, and Paradis set off by contrast just what a powerful tour de force of argument Newton has created. In this sense Newton seems very much a man against his times, although his solution was to remake his times. Never satisfied with uncertainty in argument, once he shed his professorial authority, he sought authority through establishing his credentials as a proper Baconian investigator in the "New Theory." When that failed he moved toward the compelling claim, supported first through structured experiment, and then embedded in a massive system built from fundamentals.

The Effects of Compulsion

The controlled experience Newton created in the *Opticks*, moving the reader from first principles to a fully articulated and fully imagined system has a remarkable literary effect, as noted by many readers, both scientists and nonscientists. Marjorie Hope Nicholson's book *Newton Demands the Muse* documents the mighty force of the *Opticks* on the eighteenth-century literary imagination; almost all of the literary impact of the volume came from the first book. Albert Einstein in an introduction to a modern edition of the *Opticks* attests to the imaginative force of the work, which he sees as a portrait of Newton's mind:

> He stands before us strong, certain, and alone: his joy in creation and his minute precision are evident in every word and every figure. . . . It alone can afford us the enjoyment of a look at the personal activity of this unique man. (lix–lx)

Einstein was not alone in commenting on this experience of felt thought in reading this book. But this experience of the reader must not be taken naively as the actual fact of the writer. As the evidence reviewed in this chapter indicates, the book is far from the spontaneous workings of the creative mind. The book is a hard-won literary achievement forged through some trying literary wars. The texts that are closer to the spontaneous outpourings of Newton's mind, such as his student notebook, have hardly the compelling presence.

The compelling effect of Book 1 of the *Opticks* is rather evidence of how well, totally, and precisely Newton has gained control of the reader's reasoning and perception, so that he can make the reader go through turn by turn exactly as he wishes. In modern literary theory such a text is called a closed text as opposed to an open one that allows the reader greater freedom in providing alternative interpretive procedures and meanings, and projecting personal considerations on to the text (Eco). In the closed text we read only the author; in Book 1 of the *Opticks*, Newton powerfully grabs hold of our reason and experiences until we have seen exactly what he wants us to have seen, in both the concrete and cognitive senses of the word.

With the writer so closely shaping our experience of reading, it is inevitable that the author's voice should be compellingly powerful and the authorial presence imposing. The author has taken over our minds and we become subservient to the powerful directions laid down by the guide and master. Of course, we hand over our wills only to the extent that other firmly held beliefs and experiences are not violated in ways that cannot be and are not reconciled to the emerging vision. As we have seen, Newton is quite careful to recognize and deal with those places where common beliefs and experiences would likely pull the reader out of sympathy with the closed text and thereby remove the reader from the cognitive compulsion.

In this respect, it is important that the text provide an account of the phenomenon that encompasses all contemporary experiences and satisfactorily addresses all contemporary issues. Forceful criticisms must be attended to with a compelling answer or with a revised claim for the closed text to maintain its compulsion. And it must be able to weather the continuing experiences, experiments, and thought of the readers. Compelling scientific texts are embedded in nature and in science. A compelling text, whose end is an authoritative representation of the world, is not simply a textual matter. The text can only create a formulation that serves as a resting point for thoughts and experience of reader and writer. In the current context, the text must appear to be "the right answer."

In Books 2 and 3, where Newton felt (and modern scientific belief agrees) that he had not gotten to the bottom of the issues, he could not create this kind of compulsion. But when the contemporarily satisfying answer combines with a compelling form of argument, an intellectual network is established that seems to spread the presence of the author over a vast and certain domain. And that domain becomes defined by the terms of that intellectual network, making it hard to escape and establish contrary claims. Powerful arguments and experiences must be mounted to break through the compulsions of the earlier system.

Newton's encounters with criticism and opposition, some of which were recounted here, in all instances show his personal conviction and desire to sweep away all objections as ill-founded, if not ignorant. But only in this kind of form did he find the strong vehicle that really would push opposition off the stage, demonstrate the power of his claims, and leave him and his claims in the center spotlight. In his success we can recognize his great effect on the scientific community to follow. It was not just Newton's findings that dominated eighteenth-century science; it was his presence.

And it was his mode of argumentation that also dominated. I. B. Cohen in his analysis of the Newtonian style, which he argues set the tone for the science that followed, focuses almost solely on the *Principia*. He dismisses the *Opticks* as not amenable to the kind of tight, logical system-building with empirical consequences that he finds characteristic of the Newtonian style (*Newtonian Revolution*, 13-14; 134-35). But the kind of closer inspection of the *Opticks* and its literary history that we have carried on here suggests how much the style of the *Principia* may owe to Newton's rhetorical struggles and solutions in trying to shape the optical work. The *Opticks*, to be sure, does not contain the radical split between deduction and induction, between logic and empiricism, between mathematics and physics as there is in the *Principia*. But the *Opticks* does attempt an empirical argument with the same kind of compulsion as the mathematical-deductive argument. The choice of separating out the empirical elements into the final book of the *Principia* is only another option in the same kind of literary problem.

On the form of scientific argument developing in the journals, the solutions reached in Book 1 of the *Opticks* seem to have had a more immediate and powerful impact than the more abstract machine of the *Principia*. As we have seen, the form of Book 1 of the *Opticks* was a direct response to the rhetorical situation and rhetorical problems created by the emergence of the journal. In its rhetorical solutions it served as a precursor of many of the later developments in the scientific article that we examined in the last chapter. It seems that it took the community as a whole over a century to discover what Newton worked out in about a decade, from his first notebooks to his answer to Huygens. Even the way-stop of the failed experiment of discovery narrative used in the opening section of the "New Theory" article seemed to foreshadow the reliance on discovery accounts a century later in the *Transactions*.

Certainly Newton's final rhetorical conclusions seem to match very closely with those realized in the *Transactions* article of 1800 and after: (1) That experimental methods and results must be spelled out explicitly and in detail, both to allow replication and comparison of results and to create a plausible virtual experience for readers; (2) That the discourse must be organized around a central claim or sequential series of claims, and the experimental accounts should be structurally and logically subordinated to those claims to serve as a form of experimental proof; (3) That the coordinated series of claims and articles, incorporated into a coherent system, becomes a mutually supporting network framing a way of working, viewing, and thinking, so that reliance on the network becomes an essential cognitive and argumentative resource. Serious arguments can only be cast within the closed system that realizes the mode of perception, activity, thinking, and interchange. Arguments that step outside the closed system are no longer considered properly scientific.

The framework that Newton developed and relied on was entirely his own and was the system codified in his books, whereas ultimately the scientific community was to develop a communally constructed framework. But this was to require inventing not only the modern apparatus of citation and embedding of others' ideas, not only developing forms of theoretical argument, but also the invention of complex synthetic genres that allow the emergence of codified beliefs without hindering the argumentative and negotiative processes that occur in the research front articles—genres such as review articles, forums, handbooks, and textbooks. Much of this integrative machinery was not developed until the nineteenth and twentieth centuries. The late arrival of integrative machinery makes Newton's awareness of the necessity of a coherent system to provide a powerful account of phenomena all the more remarkable and his solution all the more powerful a resource. His individually conceived system, without the more modern integrative apparatus, both drove science that followed him and created difficulties for integrating viewpoints, discoveries, and claims from outside the system. One suspects that there are important correlations between the breakdown of the Newtonian systems and the emergence of new rhetorical devices both for mounting oppositional arguments and for creating integrated communal theory. Certainly the emergence of integrative machinery allows for more flexibility and modification of the communal system, allowing for changes in argument without stepping outside or causing breakdowns of the system.[24]

That Newton's mode of argument was a model as well as a precursor for later developments in the journal article is more than likely given the omnipresence in the eighteenth century of editions of the *Opticks* and the other evidence of wide circulation, greater than that associated with the more difficult *Principia*.[25] However, the details of the path of literary influence have yet to be drawn out to support this claim.

This single example of an individual working with book and article modes of publication hardly resolves the issue of the relationship between book and journal publishing, but it does begin to suggest the complications, particularly in a time of transition. In this one case the book, which at first was conceived as an extension of an expository series of lectures, became—through contact with the more intimate argument of journals—an argumentative system, shaping consciousness, reason, and experience to compel readers down an incontrovertible path. It appears likely that such a rhetorical style came to reside most fully and permanently in the journals; books gradually moved to other functions, popularizing and codifying the results of such arguments.

Whatever books and articles have become, and whatever relationship between them has developed, the result has been the consequence of individual writers making assessments of their perceived rhetorical situations, choosing among available resources and adding a few new tricks of their own. Books and articles are all the products of writers writing.

24. For one early step in the development of this integrative machinery see Bazerman, "How Natural Philosophers Can Cooperate."

25. For discussions of the popularity of the *Opticks* see Cohen's preface to his edition of the book, and Nicholson, chap. 1.