Further Considerations on Newton's Methods

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Discussion at the symposium, and subsequent correspondence with participants, have raised a series of critical questions that seem to me to merit discussion. The issues raised have also led me to consider further some of the literature (most notably, discussions by Alan Shapiro and by A. I. Sabra) commenting on Newton's work and on related matters in the history of optics . What was initially intended as a brief supplement to the foregoing paper has thus evolved into a new article of considerable length.[1]

§1. In **MMN** I state, with respect to the *Experimentum Crucis* in Newton's first published paper, the "New Theory about Light and Colors":

> In a terminology that Newton proceeds immediately to introduce, he claims to have established a distinction—that is, an actually existing one: a distinction *in rerum natura*—between "uniform" or "homogeneous" light, and "difform" or "heterogeneous" light; to have established, moreover, that there is an infinite variety of kinds of the former; and that ordinary sunlight is a "heterogeneous mixture" of rays of a continuous range of refrangibilities.

In a comment upon this passage, it was suggested that the words "uniform," "homogeneous" (or "homogeneal"), "difform," and "heterogeneous" (or "heterogeneal") are either not used at all by Newton in that paper, or are used just "incidentally," and have no place in his "formal definitions"; and suggested further that Newton does not call white light a "heterogeneous mixture of differently refrangible rays," but a "confused aggregate of rays indued with all sorts of colors." All this was seen as characteristic of a philosopher's carelessness with historical details.

As to the first point: that the words in question do occur in Newton's paper I have now documented in n. 23 to **MMN;** we must consider whether they are used more than "incidentally." (The issue about "formal definitions" is unclear: in the course of the "New Theory," Newton introduces terminology as he goes along; there are no formal definitions, designated as such.) I learn from subsequent correspondence that the objection in question concerned whether the terms in dispute were applied by Newton, in the

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[1]To avoid frequent use of such phrases as "my paper," I shall refer in what follows to that paper as **MMN**. Further, in citing works already cited in **MMN** I shall use the short-form references already introduced there.

"New Theory," to *colors.* Let us review the matter, with careful attention to Newton's words and to the structure of his exposition.

I have quoted in **MMN** the sentence with which Newton sums up his conclusion from the *Experimentum Crucis;* let me repeat it here (for its place in the text, see **MMN**, n. 22): "And so the true cause of the length of that Image was detected to be no other, then that *Light* consists of *Rays differently refrangible,* which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall." This is directly followed by what Newton characterizes as a "digression," beginning as follows:

> When I had understood this, I left off my aforesaid Glass-works [namely, "the grinding of Optick Glasses of other figures than *Spherical*"];[2] for I saw, that the perfection of Telescopes was hitherto limited, not so much for want of glasses truly figured according to the prescriptions of Optick Authors, (which all men have hitherto imagined,) as because that Light it self is a *Heterogeneal mixture of differently refrangible Rays.*

There follows an estimate of the magnitude of chromatic aberration; and the paragraph ends with the sentence:

> And consequently, the object-glass of any Telescope cannot collect all the rays, which come from one point of an object so as to make them convene at its *focus* in less room then in a circular space, whose diameter is the 50th part of the Diameter of its Aperture; which is an irregularity, some hundreds of times greater, then a circularly figured *Lens,* of so small a section as the Object glasses of long Telescopes are, would cause by the unfitness of its figure, were Light *uniform.*

This passage is, to be sure (as I have said), part of a digression (which continues with some remarks upon Newton's work on the reflecting telescope, and on the possibility of a reflecting microscope). It does, however, follow immediately upon the "crucial experiment"; and it does introduce the terms "homogeneous" on the one hand, and "uniform" on the other—both, it should be noted, in italics.[3] I

[2]Correspondence, I, p. 92; Papers and Letters, pp. 53-57.

[3]Italics in the printed text correspond to underlining in the original manuscript (see *Correspondence,* I, p. xxxi). Note that the words in italics other than those under consideration here ("heterogeneous," "uniform," and the characterization of "light itself")—namely, "focus" and "lens"—are Latin; the mathematical term "sine" is also italicized in this paragraph (and elsewhere in

shall return presently to the question whether the terms introduced in this digression are used only "incidentally," or have crucial significance—in this paper of Newton's—not only for his theory of the diverse refrangibility of light, but also for that of color; first, however, I wish to consider the statement about the character of "light itself."

I said that Newton claims to have established that *ordinary sunlight* is a "heterogeneous mixture of differently refrangible rays." Newton actually gives this characterization of "light itself." What light is he talking about?—I should have thought the point entirely obvious; but since contention has arisen, it seems best to err on the side rather of the pedantic than the laconic.

In the first place, Newton cannot be speaking of light in the abstract, or light generically. For whereas one could well say that light, generically, *comprises* differently refrangible rays, only some particular variety of light can be said to be a *mixture.* Newton obviously does not mean to say that *every* kind of light is such a mixture, because he has found a way of separating out—to a reasonable approximation—rays that have all the same degree of refrangibility. Thus, the light he means to characterize has to be light of a specific kind.

On general principles we might easily infer what kind of light would be meant in such a statement as Newton's. For paradigmatic, or standard, "light" was understood by everyone at the time to be ordinary sunlight: white light—that of normal illumination by day—was what was regarded by all theorists as the simple, basic, undisturbed light, of which colors were produced by "modification" or disturbance of some kind. But if there were any doubt, the context would remove it: the passage concerns aberration in telescopes; and telescopes are used to look either at terrestrial objects, which are seen by sunlight, or—and this is Newton's more particular interest—heavenly bodies. The moon and planets are of course seen by sunlight; as is the sun (when one uses the telescope to cast an image of it). This leaves the fixed stars. To be quite precise, then, my formulation of Newton's claim ought perhaps to have referred to "ordinary sunlight and starlight." In any case, ordinary sunlight is certainly *included* in Newton's characterization—and that is in fact all I claimed. (Note that my statement did not use the word "white," any more than Newton's does; for as I have noted in **MMN**, I was careful to follow him in avoiding any mention of color in discussing the crucial experiment itself.)

After the digression on the problem of improving optical instruments, there occur these three paragraphs, introducing the second main part of the "New Theory," Newton's doctrine of colors (which is then expounded in thirteen numbered paragraphs):

> 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: So that of those,

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the paper). It is clear that Newton's underscoring of the words here under discussion (in contrast with his underscoring of Latin or established technical terms) is intended to emphasize them.

which are alike incident on the same medium, some shall be more refracted than others, and that not by any virtue of the glass, or other external cause, but from a predisposition, which every particular Ray hath to suffer a particular degree of Refraction.

I shall now proceed to acquaint you with another more notable difformity in its Rays, wherein the *Origin of Colours* is infolded.[4] A naturalist would scearce expect to see ye science of those become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without suspicion of doubt. To continue the historicall narration of these experiments would make a discourse too tedious & confused, & therefore I shall rather lay down the *Doctrine* first, and then, for its examination, give you an instance or two of the *Experiments,* as a specimen of the rest.

The Doctrine you will find comprehended and illustrated in the following propositions.[5]

In the first of these paragraphs, Newton repeats his summary of the conclusion he claims to follow from the *Experimentum Crucis,* in the terminology he has introduced in the first sentence of the digression. In the second paragraph, he announces the transition to *another part* of his theory—that concerning *the relation of color to the rays of light.* It should be especially noted that the last quoted sentence—"The Doctrine you will find comprehended . . . in the following propositions"—refers, not to the whole "new theory about light and colors," but only to that part which is specifically concerned with color. I have already noted that one chief point of contention in the objections to **MMN** that I have so far referred to concerns the relevance of the terms introduced by Newton in the passages already quoted to the theory of color. I shall return to this issue in some detail in §4 below; for the present, let me simply remark that the propositions about colors one and all presuppose the result of the crucial experiment (note well: the doctrine of colors does not *follow from* the crucial experiment, but it does *rest upon* the result of the latter). The words "homogeneous" and "heterogeneous" are, to be sure, not repeated in these propositions; Newton does, however, refer in them to various "sorts of rays" (and I have noted in **MMN** the semantical connection of those words with the word "sort"), assigning to each sort both a "species of color" and a "degree of refrangibility"—which two, he says, are indissolubly connected.

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[4]Oldenburg prints "unfolded."—The next two sentences of this paragraph, and its last sentence through the words "& therefore," are omitted by Oldenburg (cf. **MMN**, n. 45); his text continues: "Concerning which I shall lay down the *Doctrine* first, [etc.]."

[5]*Correspondence,* I, pp. 96-97; *Papers and Letters,* p. 53.

Moreover, he does use the word "difform" (which, with its contrary "uniform," also has a connection I have noted with the term "sort" on the one hand and with the distinction "heterogeneous/homogeneous" on the other), not only in the introductory paragraph quoted just above, but in numbered paragraph 9, where we find the sentence: "For, the difform Rays, by their unequal Refractions, are made to diverge towards several parts of the *Retina,* and there express the Images of things coloured, as in the former case they did the Suns Image upon a wall."

Let this suffice, then, as a provisional indication—pending the more detailed discussion to come—that Newton's summary of his conclusion from the crucial experiment is an essential part of the "new theory about light and colors," and that the terms he introduces in giving that summary are by no means "incidental" or "casual," but are intimately connected with the ideas involved and the terms used in the propositions about colors.

§2. A class of questions bearing more specifically upon Newton's methods concerned my discussion of three matters that I regard as intimately connected: Newton's conception of what constitutes a "mathematical science"; the nature of what Newton calls "experimental proof"; and the nature of what he calls "physical certainty." I shall try now to clarify further (and to document the grounds for) my interpretation of Newton's position on these subjects.

One point that was raised I confess I find rather puzzling; namely, to what I have said concerning Newton's conception of a mathematical science, it was objected that there existed in Newton's time a common, standard view about "mixed mathematics"—a term used for such applied mathematical subjects as optics, astronomy, geography, etc.[6] That is quite fine; but it seems in no way to contradict the view I have expressed—unless it be supposed that these sciences were regarded at the time as carrying "mathematical certainty." But in the first place, that is surely hard to assume of any of the subjects listed; and in the second place, whatever the *general* view at the time may have been, Newton's *own* view is clear and unequivocal. To repeat what we have already seen above: Newton says in his paper, "A naturalist would scearce expect to see ye science of those [*sc.,* "Colours"] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks." In his reply to Hooke, he explains:

> I said indeed that the *Science of Colours was Mathematicall & as certain as any other part of Optiques;* but who knows not that Optiques & many other Mathematicall Sciences de-

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[6]The *Oxford English Dictionary* quotes a definition from the 1706 edition, by John Kersey, of Edward Phillips, *The New World of English Words: or, a General Dictionary:* "*Mixt Mathematicks* are those Arts and Sciences which treat of the Properties of Quantity, apply'd to material Beings, or sensible Objects; as Astronomy, Geography, Navigation, Dialling, Surveying, Graphing, &c."

pend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall:* Whence the Propositions themselves can be esteemed no more then *Physical Principles* of a Science.

That, I have said, is clear and unequivocal. Is it accurate? A passage quoted in §1 above, the transition to the science of color, *does* explicate the statement about the certainty of this science by the declaration: "For what I shall tell concerning them is . . . evinced by the mediation of experiments concluding directly & wthout any suspicion of doubt." Newton is thus *demonstrably* not prevaricating in what he says to Hooke, but pointing accurately to what he had said, and explaining what he meant by it. For Newton, the "certainty" of mixed mathematics rests upon experiments, and is thus "physical"—not "mathematical"—certainty. And at least by the time of the *Principia,* as I have noted, this statement holds even of geometry—a view that was emphatically not the standard one at the time.[7]

But what is the character of this experimental evidence for the theory of color? It was suggested, in the symposium discussion, that I had failed to represent properly the role, in Newton's conception of scientific evidence, of "crucial experiments." Now, I have certainly not disputed that this is a category of experiment that Newton recognized and valued; in fact, I have cited the *Experimentum Crucis* of Newton's paper on light and colors, and have defended the claims he made for it. What I did, on the other hand, deny is that Newton *demanded* a crucial experiment *for every conclusion*—a single experiment that should decide the issue; and I also denied that Newton rests his case for his conclusions, in general, exclusively upon a "deduction" (that is, a single clear chain of inferences) from a set of experiments.

Of these contentions, the second is, I think, more problematic than the first. Let me consider them in sequence, first in connection with the paper on light and colors (and the controversial writings that paper gave rise to), and then a little more generally.

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[7]It is perhaps worth pointing out as well that what Newton characterizes as purely "mathematical"—and not "philosophical"—in the *Principia*—is the contents of Books I and II (see for this the introductory paragraph to Book III). The Laws of Motion, upon which those two books rest, formally *precede* Book I; and the Scholium to the Laws and their corollaries makes it clear that the Laws rest upon experimental evidence. They are thus not themselves "mathematical," but "physical" (or "philosophical"). The mathematical character of Books I and II consists precisely in the fact that they contain *mathematical demonstrations from the Laws,* without (except in a series of "philosophical" scholiums) any matter depending upon new experimental or observational evidence.

As to the reliance on crucial experiments in all cases, I think the matter is put beyond doubt by the second part of Newton's paper, which assuredly does not rely upon a crucial experiment. We have seen that Newton tells Oldenburg his conclusions about color rest upon "the mediation of experiments concluding directly and without any suspicion of doubt"; but tells him, also, that the number of these experiments is rather large (something clearly implied by his statement that the "historicall narration of these experiments" would "make a discourse too tedious and confused" for a letter). Something of the nature of the array of experiments upon which Newton bases those conclusions can be learned from paragraph 3 of the section on colors:

> 3. The species of colour, and degree of Refrangibility proper to any particular sort of Rays, is not mutable by Refraction, nor by Reflection from natural bodies, nor by any other cause, that I could yet observe. When any one sort of Rays hath been well parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it. I have refracted it with Prismes and reflected it with Bodies, which in Day-light were of other colours; I have intercepted it with the coloured film of Air interceding two compressed plates of glass; transmitted it through coloured Mediums, and through Mediums irradiated with other sort of Rays, and diversly terminated it. It would by contracting or dilating become more brisk, or faint, and by the loss of many Rays, in some cases very obscure and dark; but I could never see it changed *in specie.*

Obviously, then, the conclusion—stated in Newton's first numbered paragraph—that "Colours are not *Qualifications of light,* derived from Refractions, or Reflections of natural Bodies (as 'tis generally believed,) but *Original* and *connate properties,*" is inferred from the consistent failure of his attempts to modify the colors of homogeneous light, by all the optical processes he could think of trying. He goes on, in paragraph 4, to recount other processes, by which "seeming transmutations of Colours may be made," and to explain those apparent transmutations by the circumstance that the light in question was heterogeneous—so that its composition might be changed, and thereby its color. (Newton does not here use the word "heterogeneous"—any more than he uses the word "homogeneous" in paragraph 3, where instead he spoke of "any one sort of Rays . . . well parted from the rest"—but speaks rather of "a mixture of divers sorts of Rays.")

Now, I think that this same example partially illustrates my point about "deduction" from experiments. In the case of the *Experimentum Crucis,* it is quite reasonable to speak of such a "deduction"—not, of course, in the strict logical acceptation of the term that is now usual, but in a broader sense (and one certainly licensed by common usage). In the argument of paragraphs 3 and 4, however, the force of the conclusion from experiment rests upon the number and variety of ways in which attempts were made, unsuccessfully, to modify the properties—refrangibility as well as color—of homogeneous light; and upon the fact that whenever actual modifications of perceived colors *were* achieved, the lights in question could be shown to have been composite. It seems less natural to call an inference of this sort a "deduction." However, I shall not place a great deal of weight upon this argument; it is possible that Newton would after all have used the word "deduction" here (and indeed a passage quoted below indicates that he does so use it).[8] For the present, therefore, let this part of the question be put in abeyance; I shall return to it presently.

But first let us consider the remaining part of the threefold issue I have defined: the nature of what Newton calls "physical certainty." I have said in **MMN** that Newton considered such "certainties" to be *corrigible* or *revisable.* This was vigorously disputed in discussion, and it was maintained that not until well into the eighteenth century do there occur in Newton's writings any indications of a commitment to revisability. In particular, it was argued that the sentence I quote from the preface to (the first edition of) the *Principia* is unimpressive so far as this issue is concerned: "I hope the principles here laid down will afford some light either to that, or some truer, method of philosophy" (a statement that does not, to be sure, refer to particular scientific conclusions, but which, as applied to Newton's whole program for scientific investigation, seems to me very impressive indeed in its abstention from dogmatic claims to possession of the one right road).

Now, in my own view there is a rather convincing array of evidence that Newton held in principle to the revisability of conclusions based upon experiment—and therefore of *all* conclusions in physics—at the time of his first paper. Let me cite first the passage on certainty in Newton's reply to Hooke, given in part earlier. The beginning of that passage—which I have not yet quoted, either in **MMN** or in the present paper—is as follows: "In the last place I should take notice of a casuall expression [a "casual expression," that is, on the part of Hooke, in his critique of Newton's paper] wch intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematicall Demonstrations.*" From this, and from the words "but" and "no more th[a]n" in the statement already quoted—"Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physical:* Whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science"—it is entirely clear that Newton must regard "physical certainty," which is derived from experimental evidence, as *less* than the "certainty" of "mathematical demonstrations."

Lest it be argued that this notion of a "certainty" less than complete is unintelligible, and an abuse of language—that, therefore, Newton must be regarded as merely quibbling when he suggests such a distinction—let me cite the example of a contemporary of Newton's whose integrity I think will be generally admitted. The index (prepared by the author himself) to Locke's *Essay concerning Human Understanding* has, under the word "Certainty," a subhead that reads: "Sensible knowledge the utmost

[8]See n. 12 below, and the place in the text to which it is attached.

Certainty we have of Existence"; and at the place in the text to which the index refers us, we find: *"The notice we have by our Senses, of the existence of Things without* us, though it be not altogether so certain, as our intuitive Knowledge, or the Deductions of our Reason . . . ; yet it is an assurance that *deserves the name of Knowledge."* [9] And, after several pages of discussion of the point: *"[T]he certainty of* Things existing *in rerum Naturâ,* when we have the testimony of our Senses for it, is not only *as great* as our frame can attain to, but *as our Condition needs."* [10] Thus Locke, like Newton, speaks explicitly of a *certainty that is less than complete certainty.*

That physical principles have a lower degree of certainty than purely mathematical ones may, however, not seem necessarily to imply the corrigibility of the former. For instance, it might be suggested that even if corrigibility is denied, there will remain the distinction that the falsity of a physical principle is *conceivable* in a sense in which that of a result of mathematical demonstration is not. It is therefore pertinent to consider a series of more concrete statements by Newton concerning the methodological status of his conclusions.

In a letter to Oldenburg of July 6 1672 (three and a half weeks after he had sent to the latter his reply to Hooke), Newton proposes a way of proceeding to settle the chief points in controversy:

> In the meane while give me leave to insinuate that I cannot think it effectuall for determining truth to examin the severall ways by wch Phænomena may be explained, unless where there can be a perfect enumeration of all those ways.[11] You know the proper Method for inquiring after the properties of things is to deduce them from Experiments. And I told you that the Theory wch I propounded was evinced to me, *not by inferring tis thus because not otherwise,* that is not by deducing it onely from a confutation of contrary suppositions, but *by deriving it from Experiments concluding positively & directly.* The way therefore to examin it is by considering whether the experiments wch I propound do prove those parts of the Theory to wch they are applyed, *or by prosecuting other experiments wch the Theory may suggest for its examination.* [12]

The last clause—which I have italicized for emphasis (all other emphases are Newton's own)—*clearly,* I submit, implies that *even if the experiments already performed do* "prove those parts

[9]John Locke, *An Essay concerning Human Understanding,* ed. Peter H. Nidditch (Oxford: Clarendon Press, corrected ed., 1979), pp. 725, 631.

[10]Ibid., p. 634.

[11]Note again with what consistency it is the appeal to hypotheses—"ways by which phenomena may be explained"—*for determining truth* that Newton deprecates.

[12]*Correspondence,* I, p. 209; *Papers and Letters,* p. 93.

of the theory to which they are applied," it can still be proper to "prosecute other experiments which the theory may suggest for its examination"; or, as I have said in **MMN,** that the process of "proving" a theory by experiments in principle never ends. The proposition is repeated a little later in this same letter, after Newton formulates a set of "Queries" to guide this further experimental examination (here again I have italicized a clause for emphasis):

> To determin by experiments these & such like Queries wch involve the propounded Theory seemes the most proper & direct way to a conclusion. And therefore I could wish all objections were suspended, taken from Hypotheses or any other Heads than these two; Of showing the insufficiency of experiments to determin these Queries or prove any other parts of my Theory, by assigning the flaws & defects in my Conclusions drawn from them; *Or of producing other Experiments wch directly contradict me, if any such may seem to occur.* [13]

In his reply to Hooke, in the longest of the sections (that devoted to the proposition "That whiteness is a mixture of all colours"),[14] after arguing that an experiment he had described in his paper provides satisfactory evidence for this claim—an experiment, namely, in which the dispersed light was made to converge by a convex lens, producing the appearance of white where the light converged, but with colors visible both before and behind the place of convergence (and at the place of convergence as well, when some part of the dispersed light was intercepted at the lens)—Newton adds: "But if there be yet any doubting, tis better to put the event on further *circumstances* of the *experiment,* then to acquiesce in the possibility of an Hypotheticall Explication." He then suggests a modification of the experiment, in which a toothed wheel is used to intercept some of the colors; if the wheel is made to rotate slowly, the image at the convergent point appears successively in different colors; but if the rotation is sufficiently rapid, the image appears white. Since such a procedure is described in the later version of Newton's *Lectiones opticæ,* but not in the earlier one,[15] it seems quite likely that this beautiful and important experiment (which embodies the principle later incorporated in the color disk or

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[13]*Correspondence,* I, p. 210; *Papers and Letters,* p. 94.

[14]*Correspondence,* I, pp. 181-186; *Papers and Letters,* pp. 128-134. (This is §9 in Newton's numeration, §10 in Oldenburg's—the latter having assigned the number §3 to what in Newton's original were the second and third paragraphs of §2.)

[15]Shapiro (ed.), *The Optical Papers of Newton,* I, p. 31 (end of the synopsis of Lectures 4, 5—corresponding to Lectures II, 4-7). Note that the earlier version of the lectures, described by Shapiro (ibid., p. xv) as "an untitled first draft (now University Library, Cambridge, Add. MS 4002)," is printed by him with the title *Lectiones opticæ;* the later version—the one actually published in 1729 as *Lectiones opticæ* —is printed by Shapiro under the title *Optica.*

top—first described, I believe, as an instrument for exact experiments, by Young, and employed to important effect by Maxwell)[16] was devised by Newton for the express purpose of providing further evidence in response to Hooke's objections.

Now, of course, the mere fact that a man looks for further evidence to convince doubters is not sufficient to establish that he regards his conclusions as subject to correction. But what we have seen here is that Newton (1) repeatedly and emphatically asserts that the way to resolve any doubts is to put them to experimental test, and (2) actually suggests such tests (in fact he devised a great number of them). Thus his practice corresponds to his profession. And that profession obviously implies that *if* further experimental tests lead to results in conflict with a theory, then that theory not only may but *must* be in some way revised. This implication is made explicit by Newton in his first reply to Huygens's reservations, written in Apriul 1673, where he says:

> As to the contents of his letter I conceive my former answer to the Quære about the number of colours is sufficient, wch was to this effect, that all colours cannot practically be derived out of Yellow and Blew, & consequently that those Hypotheses are fals wch imply they may. If you ask wt colours cannot be derived out of Yellow & Blew, I answer none of those wch I defined to be originall and if he can show by experiment how they may I will acknowledg my self in an error.[17]

Perhaps it will be objected that the last clause is merely rhetorical—that Newton had no genuine doubt that further experiments would support his theory. To this, my own response is—*of course* he had no genuine doubt! That is precisely what Newton means by calling the propositions of his New Theory

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[16]See Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts* (London, 1807), pp. 440-441; James Clerk Maxwell, "On the Theory of Colours in relation to Colour-Blindness" and "Experiments on Colour, as perceived by the Eye, with remarks on Colour-Blindness," in *The Scientific Papers of James Clerk Maxwell,* ed. W. D. Niven (Cambridge University Press, 1890), vol. I, pp. 119-125, 126-154.—I have said that I believe Young was the first to describe such a contrivance as an instrument for exact experiments; but Newton himself, three paragraphs after his proposal of the toothed wheel, actually describes a color top: "There may also be produced the like dirty colour [that is, a dusky white: "such a grey or dirty colour as may be made by mixing white & black together"] by mixing severall *Painters colours* together. And the same may be effected by painting a *Top* such as Boys play with) of divers colours, for when it is made to circulate by whipping it will appear of such a dirty colour." Clearly, however, this does not envisage anything like the full potentialities of the color top.

[17]*Correspondence,* I, p. 264; *Papers and Letters,* p. 143.

"certainties," and by his statement that they are "evinced by the mediation of experiments concluding directly & without any suspicion of doubt." But to say that there is "no suspicion of doubt" is not quite to say that there is no conceivable way in which doubts might arise from future experiments.

There is one place, at any rate, in which Newton actually expresses a (small but significant) doubt of his own. This, too, occurs in his first reply to Huygens, where it immediately follows a rather sarcastic remark which it will be of some interest to examine later. The whole passage is as follows (emphasis added here):

> If therefore M. Hugens would conclude any thing, he must show how white may be produced out of two uncompounded colours;[18] wch when he hath done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind, because as I remember I once tryed by graduall succession the mixture of all paires of uncompounded colours, & though some of them were paler & nearer to white then others yet none could be truly called white. *But it being some years since this tryall was made, I remember not well the circumstances, & therefore recommend it to others to be tryed again.* [19]

Although it is perhaps tangential to the matters at issue, it seems worth pointing out how perspicacious that last remark is: the one point in this extended discussion on which Newton professes a doubt, and recommends further investigation, is almost the only point on which he was in fact mistaken. To be sure, Newton never corrected this error; but the difficulty of the problem must also be understood. As late as 1852, Helmholtz concluded, after a careful experimental study, that although the impression of white can indeed be produced by a suitably proportioned mixture of a pair of spectral colors, there is only one such pair (more precisely, one pair of rather narrow spectral bands) that can do it—namely (after all!) a yellow and a(n indigo) blue.[20] It was only under the stimulus of an article

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[18]Of course Newton had never denied that white can be produced by mixing two suitably *compounded* colors.

[19]*Correspondence,* I, p. 265; *Papers and Letters,* p. 145. (In the published exchange with Huygens, the identity of the latter is concealed: Oldenburg identifies his first letter—*Papers and Letters,* p. 136—as "written by an ingenious person from Paris"; and Newton is made to call him "Monsieur *N.* " In Newton's original letter to Oldenburg, however, he is referred to as "M. Hugens.")

[20]Hermann Helmholtz, "Ueber die Theorie der zusammengesetzten Farben," *Annalen der Physik und Chemie*, 2nd ser., **87** (1852), pp. 45-66. The conclusion referred to is stated (on p. 55) as follows: "Die auffallendste und von den bisherigen Ansichten abweichendste Thatsache ist die, daß unter den Farben des Spectrums nur zwei vorkommen, welche zusammen reines Weiß geben, also Complementarfarben

published soon after by Hermann Grassmann—who showed, by a quite striking topological argument, that a few simple premises (most of them rather well-confirmed, although one was in fact defective) strictly imply the existence of a complement to every spectral color—that Helmholtz undertook a further inquiry with improved techniques, and confirmed Grassmann's principal predictions (explaining at the same time the failure of his earlier experimental procedure to detect the other cases of complementary colors).[21]

So far, then, the evidence I have been reviewing tends to show both that Newton exercised rather sensitive discrimination in the degree of assurance he attached to the conclusions he presented, and that he was open in principle to continued experimental testing, or "proof," and professed himself willing to confess error if such experiments showed him wrong.[22] Against this, in the symposium discussion,

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sind, und daß dieß Gelb und Indigblau sind, zwei Farben, aus deren Verbindung man bisher fast immer Grün entspringen ließ. Das Gelb, was man zu dieser Mischung gebraucht, ist ein sehr schmaler Strich im Spectrum, zwischen den Linien *D* und *E* gelegen, und etwa dreimal so weit von *E* als von *D* entfernt, ein Gelb, welches weder in das Orange, noch in das Grünliche zieht und unter den Pigmenten am besten durch das chromsaure Bleioxyd (Chromgelb) wiedergegeben wird. Das dazu gehörige Blau hat eine grössere Breite und umfaßt die Abstufungen dieser Farbe, welche *Newton* und *Fraunhofer* als Indigo bezeichnen, etwa von der Mitte zwischen den Linien *F* und *G* bis gegen *G* hin. Unter den Farbstoffen giebt dunkles Ultramarin diese Farbe aber besser wieder, als das mehr violette Indigo."

[21]Hermann Grassmann, "Zur Theorie der Farbenmischung," *Annalen der Physik und Chemie,* 2nd ser., **89** (1853), pp. 69-84; Helmholtz, "Ueber die Zusammensetzung von Spectralfarben," ibid., **94** (1855), pp. 1-28.—Although this latter paper of Helmholtz's appears in the volume of the *Annalen* dated 1855, it is cited in Helmholtz's *Handbuch der physiologischen Optik* (see the translation, from the third edition, under the title *Treatise on Physiological Optics*, tr. James P. C. Southall [reprinted New York: Dover Publications, 1962], vol. 2, p. 164) under the year 1853; indicating a remarkably rapid response to Grassmann's article!—Grassmann's analysis, as to its chief part, has one flaw: he supposes that the colors of the physical spectrum constitute the perceptual (topological) full circle of maximally saturated colors—that is, he supposes that the purples, which form the line-segment that joins the extreme violet to the extreme red, belong to the physical spectrum. Because this is not true, it is not the case that every spectral color has a complement that is itself a spectral color; the correct statement is that every color of maximal saturation has a complement of maximal saturation. Beyond this, Grassmann errs in supposing that there is a natural metric geometry of the color-space, in which the spectral colors lie on a *metric* circle (in other words, are equidistant from white). Helmholtz offers cogent criticism of both these points.

[22]This evidence seems to me to tell against Kuhn's remark (*Newton's Papers and Letters,* p. 39) that "Newton's fear of exposure and the correlated compulsion to be invariably and entirely immune to

a passage was cited in the last of the letters published by Oldenburg in the lengthy controversial correspondence, initiated by Francis Hall (*alias* Linus or Line) in September 1674, and continued by him, and (after his death in November 1675) by his successors John Gascoines and Anthony Lucas, until March 1678. The passage in question occurs in a letter from Newton to Oldenburg dated 18 August 1676 (the subsequent exchanges were not published in the *Philosophical Transactions*); it reads as follows:

> Concerning Mr Lucas's other experiments, I am much obliged to him that he would take these things so far into consideration, & be at so much pains for examining them; & I thank him so much the more because he is ye first yt has sent me an experimental examination of them. By this I may presume he really desires to know what truth there is in these matters. But yet it will conduce to his more speedy & full satisfaction if he a little change ye method wch he has propounded, & instead of a multitude of things try only the *Experimentum Crucis.* For it is not number of Expts, but weight to be regarded; & where one will do, what need of many?[23]

To evaluate the significance of this passage—and especially of its last sentence—for the matter here at issue, it is important to take account, first of one general point, and then of the fuller context of the quoted paragraph. The general point (which I should think beyond serious controversy) is that there is not a perfect democracy of experiments; in implying that one well-designed and well-conducted experiment is more informative than a multitude of ill-designed or ill-conducted ones, Newton is saying no more than what everyone competent to judge scientific procedure will agree with. As to the fuller context: here is what immediately follows the paragraph just quoted:

> Had I thought more requisite, I could have added more. For before I wrote my first letter to you about colours [that is, of course, the "New Theory" of 1672] I had taken much pains in trying experiments about them & written a tractate on that subject wherein I had set down at large the principall of ye experiments I had tryed; amongst which there happened to be the principal of those experiments which Mr Lucas has now sent me. And as for ye Experiments set down in my first letter to you, they were only such as I thought convenient to select out of that Tractate.

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criticism show throughout the controversial writings." (Newton's insistence that he was right in matters in which in fact he *was* so, and in which he had ample evidence to show this, surely cannot be taken as a sign of any such fear or compulsion.)

[23]*Correspondence,* vol. II, ed. H. W. Turnbull (Cambridge University Press, 1960), pp. 79-80; *Papers and Letters,* pp. 173-174.

> But suppose those had been my whole store, yet Mr Lucas should not have grownded his discourse upon a supposition of my want of experiments till he had examined those few. *For if any of those be demonstrative,* they will need no assistants nor leave room for further disputing about what they demonstrate [emphasis here added].
>
> The main thing he goes about to examin is ye *different refrangibility* of light. And this I demonstrated by ye *Experimentum Crucis.* Now it his demonstration be good, there needs no further examination of ye thing; if not good ye fault of it is to be shewn, for ye only way to examin a demonstrated proposition is to examin ye demonstration. Let that expt therefore be examined in ye first place, & that wch it proves be acknowledged, & then if Mr Lucas want my assistance to unfold ye difficulties wch he fancies to be in ye experiments he has propounded, he shall freely have it; for then I presume a few words may make them plain to him: whereas should I be drawn from a demonstrative experiment to begin with those, it might create us both ye trouble of a long dispute, & by ye multitude of words cloude rather then clear up ye truth. For if it has already cost us so much trouble to agree upon ye matter of fact in ye first & plainest experiment, & yet we are not fully agreed: what an endless trouble might it create us, if we should give ourselves up to dispute upon every argument that occurs, & what would become of truth in such a tedious dispute.

One thing should be quite clear from this: Newton—after nearly two years of a very frustrating controversy, begun by a man whom Kuhn calls "Newton's bitterest and least intelligent critic,"[24] in the

[24]*Newton's Papers and Letters,* p. 34. Line's first letter (*Correspondence,* I, pp. 317-319; *Papers and Letters,* pp. 148-150), which in effect denied that Newton could have performed the experiment he said he did, drew from the latter, in a reply to Oldenburg (*Correspondence,* I, pp. 328-329), first the declaration, "I am sorry you put your self to ye trouble of transcribing Fr. Linus's conjecture, since (besides yt it needs no answer) I have long since determined to concern myself no further about ye promotion of Philosophy"; and then this paragraph:

> If you think fit you may to prevent Fr. Linus's slurring himself in print wth his wide conjecture, direct him to ye scheme in my second answer to P. Pardies & signify (but not from me) that ye experiment as it is represented was tryed in clear days, & ye Prism placed close to ye hole in ye window so yt ye light had no room to diverge, & ye coloured image made not parallel (as in his conjecture) but transvers to ye axis of ye Prism.

That is not the language of a man "afraid of exposure" (cf. n. 22 above), but of one exasperated by unintelligent criticism—and wishing to avoid sterile public wrangles. Line's second letter argues that

Newton's original paper shows that the experiment *could not* have been done on clear days, nor with the prism near the hole, and that the image *could not* have been transverse to the axis of the prism, because the *results* reported in that paper are incompatible with these conditions. Oldenburg evidently showed this letter to Newton in London; for Newton later wrote to Oldenburg a reply to it (*Correspondence,* I, pp. 356-358) beginning, "Sr—When you shew'd me Mr Line's second Letter, I remember I told you, yt I thought an answer in writing would be very insignificant because ye dispute was not about any ratiocination, but my veracity in relating an experiment, wch he denies will succeed as it is described in my printed letters: For this is to be decided not by discourse, but new tryall of ye experiment."

Despite his exasperation, Newton does take the trouble to go on to give some detailed advice on how the experiments ought to be managed; and some three and a half months later, when he had had an opportunity to read Line's letter again after Oldenburg had printed it, he wrote a further reply, commenting on most of Line's specific objections (*Correspondence,* I, pp. 421-425; *Papers and Letters,* pp. 157-162). This reply of Newton's is the occasion of another remark by Kuhn that deserves some comment. Kuhn writes (*Newton's Papers and Letters,* p. 34):

> The reported shape [of the spectrum] leaves a puzzle illustrative of the nature of Newton's genius. Though the spectrum described cries aloud for the interpretation that Newton provided, it is very doubtful that he saw any such shape. Only the central 2-inch strip of his 2 5/8-inch-wide spectrum was illuminated uniformly by light from the disk of the sun. The balance of the width of the spectrum consisted of a penumbral region in which the various colors gradually shaded off into the black. Since the eye can distinguish red much farther into the penumbral region than it can distinguish blue, Newton probably saw a figure appreciably narrower and more pointed at the blue end than at the red. This is the shape that Newton's bitterest and least intelligent critic, Franciscus Linus, described, and this is the only one of Linus's criticisms to which Newton never responded.

One can construct an argument of a scholastic sort to suggest that something must be amiss here: If what Kuhn says about what Newton "probably saw" is right, why should such a virtuoso experimenter—and critic of Newton certainly more intelligent and, if perhaps less bitter than Line (although this is debatable), yet at any rate not lacking in forcefulness—as Robert Hooke, have reported to the Royal Society (I have quoted him in **MMN**) that he has himself "by many hundreds of trials" confirmed the truth of the experiments Newton "hath alledged"? Why should other critics—Pardies, Huygens—not have found this discrepancy? Why should Kuhn not have wondered at this?

Turning from scholastic arguments to physics (and elementary perceptual psychology), one easily sees that there is something drastically wrong with Kuhn's analysis. One minor point: if the hole, as was 1/4 inch in diameter, as Newton tells us, and the breadth of the image 2 5/8 inches, then the central fully illuminated region was a little wider than Kuhn states: 2 1/8 rather than 2 inches. (In fact, it must have been a little more than 2 1/8 inches: for the penumbra, which shades continuously into darkness, is never visible in its entirety; and therefore the actual image, including the complete penumbra, must have been slightly wider than what Newton could see to measure.) Now, the sensitivity of the eye to light is greatest towards the middle part of the spectrum, and falls off (gradually at first) in both directions. The boundaries of the visible image, therefore, although certainly not sharply defined, will be confined to strips 1/4 inch wide on each side of the central portion, itself 2 1/8 inches wide; will be sensibly straight for some distance near the middle; and then will taper towards each end. According to Newton, the "utmost length" of the spectrum (which of course fades out gradually at each end—so that the ends themselves, as Newton remarks, are ill-defined) was 13 1/4 inches. Now, an image that tapers both ways, over a total length of roughly 13 inches, from a(n ill-defined) breadth of about 2 5/8 inches down to 2 1/8 inches (or slightly more), will look very much like an oblong terminated by parallel straight sides; it will certainly not look like what Line describes (*Correspondence,* I, p. 335; *Papers and Letters,* p. 151). First, speaking of the semicircular ends that Newton attributes (somewhat hesitantly) to the spectrum (he says—*Correspondence,* I, p. 92; *Papers and Letters,* p. 48—that "at the ends, the decay of light was so gradual, that it was difficult to determine justly, what was their figure; yet they seemed *semicircular* "), Line asserts that these semicircular ends are never seen when the experiment is done in the way Newton claims to have done it. He then states (emphases supplied here), "But if . . . the Image bee made so much longer then broad *(as easily it may, by turning the Prisme a little about its Axis)* neere 5 times as long as broad then *the one end thereof will runne out into a sharp Cone or Pyramis like the flame of a candle,* and the other into a Cone somewhat more blunt." There is simply no way that the image in Newton's own experiment could have had this character—that of "a sharp cone or pyramis"; the numbers I have cited above are clearly incompatible with such a shape. On the other hand, Line tells us explicitly that he produced this shape by rotating the prism out of the position of minimum deviation (which position Newton had prescribed). With that rotated position, the sun's image in homogeneous light will be an oval, more or less drawn out in the direction of the deviation of the rays, and with a sharper end in the direction of the deviation. (This, indeed, was the entire point of the objection raised by Pardies: not having noticed that Newton described his experiment as performed with the prism in the position of minimum deviation, Pardies says that the ordinary laws of refraction will make the image oblong.) Since the spectrum is formed by a continuous series of such ovals—growing longer and sharper in the direction of the more refrangible rays—the more sharply tapered ends (and most sharply tapered in the violet) are fully explained by course of which Newton had to defend himself, by citing chapter and verse, against charges that his original paper did not say what it in fact did say[25]—was in the first place somewhat relieved to see the controversy taken over by a man who was willing actually to perform pertinent experiments; but in the second place was reluctant to become mired in an endless proliferation of details, and urged a *methodical* examination. (Unfortunately Newton's hope for a clarification with Lucas was never to be realized.)[26]

Another point about the above quotation is perhaps more significant for the matter here at issue—namely, Newton's repeated references to the *demonstrative* character of the *Experimentum Crucis.* That is a very unusual term for Newton to apply to an experiment (or to a series of experiments either): there may possibly be other instances, but I myself am not aware of any. Newton's characteristic term for the evidentiary contribution of experiments that support a proposition is *proof*—a word he never, so far as I am aware, uses in connection with mathematical argumentation. For

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this circumstance. Kuhn, in a note to his comment cited above, remarks the difference in the conditions of Line's experiment, but dismisses it as irrelevant: "Although the position of Linus's prism was different from that of Newton's, the 'sharp cone or pyramis' described by Linus is due to the same penumbral effects that must have caused the sides of Newton's spectrum to deviate from parallelism." This is simply wrong.

Finally—to complement the scholastic argument and the physical/perceptual one with a hermeneutical comment on that text upon which Kuhn bases his suggestion that Newton was here evading a criticism—it seems to me not in the least surprising that Newton should deem it unnecessary to explain a contrast with his own observations, when this occurred under conditions different from those he had specified.

[25]See *Correspondence,* I, pp. 421-425; *Papers and Letters,* pp. 157-162.

[26]The subsequent exchanges with Lucas may be followed in vol. II of the *Correspondence.* For an instructive evaluation cf. the following, from a letter of Hooke to Newton dated May 25 1678 (*Correspondence,* II, p. 265): "I saw also in Mr Auberys hand another Letter from Mr Lucas of wch I suppose he will give you an account. I much admire your patience that you will trouble your self wth such an extravagantly impertinent—, who never will yeald be the matter never soe plain."

Hooke at this time was endeavoring to maintain good relations with Newton. Nevertheless, it would be unjust to him to suppose that his strongly expressed view is mere flattery. Newton's letters to Lucas betray increasing exasperation; and just at this point (or perhaps six months later—see ibid., p. 269, Letter 226 and n.) he breaks off the correspondence. Yet he had indeed exhibited the "patience" to continue the discussion over a period of several years, during which he explained himself repeatedly and at length. As for the "impertinence" of the objections of Lucas, the text of the exchange bears sufficient witness to that.

the latter he reserves the word "demonstration"; and, as I have said, I know of no other instance in which he uses that term for an experiment. This, I think, is directly related to the most unusual role of the crucial experiment in Newton's work (and in his view of the normal processes of scientific reasoning).

In what sense—and *of* what—is the *Experimentum Crucis* demonstrative? I have in effect commented on this already, in **MMN**; and that comment agrees exactly with what Newton says in the passage now under consideration: "The main thing he [Lucas] goes about to examin is ye *different refrangibility* of light [N.B.: Newton's own emphasis]. And this I demonstrated by ye *Experimentum Crucis*." Now, this claim will be found to satisfy even the stringent standard of Karl Popper, who argues that experiments can never provide warrant for the typical propositions of science, because the latter are *universal:* they cannot be established by a finite amount of evidence; but they can be *refuted* by a *single* counterexample. We have here to do, however, with an *existential* statement: that *there are* kinds of light that differ in refrangibility; and—as I have remarked in **MMN**—Newton's *Experimentum Crucis* actually *produces* (examples of) such kinds, and *exhibits* their difference of refrangibility. The very unusual language Newton uses in this connection, then, testifies to his acute sensitivity to the methodological principle involved. And it is no cause for wonder if, in *this* context, Newton says of the experiments, "[W]here one will do, what need of many?" It surely does not follow that one will always do; it surely does not follow that there is *never* need of many. And—here the principle of continuing "proof" applies even to the *Experimentum Crucis*—there is the possibility, however remote, that the experiment itself may somehow prove to have been faulty (or—following Hume rather than Newton in this—that its counterpart may simply fail in the future). Newton of course has no fear of such failure (and none, for that matter, of the failure of the other conclusions which, although not "demonstrated," he regards as "proved" and established "without any suspicion of doubt"). But in the discussion with Line and his successors, he does not appeal to the authority of his own experiment, or its confirmation by the Royal Society (that is, by Hooke), as a historical fact: he advises his correspondents on the manner in which they may carry out this "demonstrative" experiment *for themselves.*

There are two other passages that seem to me of great significance in establishing Newton's view of the process of experimental (or, more generally, empirical) testing of scientific propositions. The first I have cited in **MMN**: Newton's statement, in his reply to Hooke, of the grounds of certainty of his "mathematical" science of color: "[I]f those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in wch severall colours are originally inherent; I suppose the *Science of Colours* will be granted *Mathematicall* & as certain as any part of *Optiques.* And that this may be done I have good reason to believe, *because* *ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.* "[27]

The second passage occurs in a letter from Newton to Leibniz, dated October 16 1693 (and, thus, more than twenty-one years later than the reply to Hooke on the optical questions). Leibniz had written to Newton on March 7, [28] and in the course of his letter had said, "I do not doubt that you have weighed what that other most great mathematician Christiaan Huygens has remarked about your [results] in the appendix of his book on the cause of light and weight." The reference is to Huygens's publication of 1690, containing both his *Traité de la Lumière* and his *Discours de la Cause de la Pesanteur.* In a late addition to the latter, Huygens had made a series of comments on Newton's *Principia,* expressing his very great admiration of that work, but also defending his theory of light against Newton's criticisms, and stating his own dissent from Newton's theory of universal gravitation (in contradistinction to the limited theory, which Huygens accepted, that the planets and satellites are acted on by forces of gravity—towards the sun and planets respectively—varying in each case inversely as the square of the distance from the central body). This dissent was based in part upon Huygens's view that "mechanical explanation" is a *sine qua non* for physics, and his opinion that such explanation of *universal* gravitation is impossible; in part upon an (implicit) criticism of Newton's application of the third law of motion to the gravitational situation—a rejection, that is, of Newton's assumption that gravitation is *an interaction between the gravitating body and the central body.* [29] This pair of considerations is adduced by Huygens in the following remark, referring to a difference between Newton's and Huygens's own estimates of the degree of oblateness of the earth:

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[27]*Correspondence,* I, pp. 187-188; the last emphasis added here.

[28]*Correspondence,* vol. III, ed. H. W. Turnbull (Cambridge University Press, 1961), pp. 257-258 (Latin), 258-259 (English translation). In quoting from Leibniz's letter—and, a little later, from Newton's reply—I deviate slightly from Turnbull's translations.

[29]I have discussed this point, and alluded to Huygens's criticism and Newton's response, in two earlier papers: "Newtonian Space-Time," *The Texas Quarterly* **10** (1967) (see pp. 179-180)—also in Robert Palter, ed., *The* Annus Mirabilis *of Sir Isaac Newton* (Cambridge, Mass.: MIT Press, 1970) (see pp. 263-264); and "On the Notion of Field in Newton, Maxwell, and Beyond," in Roger H. Stuewer, ed., *Historical and Philosophical Perspectives of Science* (Minnesota Studies in the Philosophy of Science, vol. V; Minneapolis: University of Minnesota Press, 1970) (see p. 269). I return to the point also in a forthcoming paper, "On Locke, 'the great Huygenius, and the incomparable Mr. Newton'," to appear in Phillip Bricker and R. I. G. Hughes, eds., *Philosophical Perspectives on Newtonian Science* (Cambridge, Mass.: MIT Press).

> I do not agree with a Principle that he assumes in this calculation and elsewhere; namely, that all the little parts that one can imagine in two or more different bodies attract or tend to approach one another. This I cannot admit, because I believe I see clearly that the cause of such an attraction is in no way explicable by any principle of Mechanics or rules of motion; as I am moreover not persuaded of the necessity of the mutual attraction of entire bodies [Huygens means, e.g., such bodies as sun and planet, or earth and moon]; having shown that, even if there were no earth at all, bodies would still, by what one calls their weight, tend towards a center.[30]

Huygens's point here is that he has described a mechanism by which a material medium pressing upon ordinary bodies could impel them towards a center—without any assumption that a body (the earth or the sun, say) occupies that center. In this case the third law would imply the occurrence of forces of reaction *upon that pressing matter,* not upon the central body (if any such should exist); thus, the application of the third law made by Newton in his argument for universal gravitation would be incorrect—and, what is more, there would be no universal gravitation.

What, then, was Newton's response? On the basis of the view that, for Newton, principles are *definitively established* by "deduction from phenomena" (and also on the view that Newton was subject to a "fear of exposure and the correlated compulsion to be invariably and entirely immune to criticism"),[31] one might expect a defense of the reasoning by which, in Book III of the *Principia,* he arrives at the law of universal gravitation (which is expressed in Proposition VII and its second corollary)—that is, of just the reasoning that Huygens's remarks impugn; for (cf. the remarks of William Harper, below, on this subject) that reasoning has a claim to be regarded as the example *par excellence* of such a "deduction." But here is what Newton actually says in his response to Leibniz:

> The remarks of that most great man Huygens about my [results] are acute *[ingeniosa].* . . . But . . . since celestial motions are more regular than if they arose from vortices [such as Descartes, and (although in a quite different way) Huygens too, assumed—the "multilateral" vortices of the latter were what he proposed as the cause of weight)], and observe other laws . . . ; *and since all the phenomena of the heavens and of the sea follow accurately, so far as I am aware, from gravity alone acting according to the laws I have described,* and nature is most simple; I myself have judged that all other causes are to be rejected and that the heavens are to be stripped as far as may be of all matter lest the mo-

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[30]*Œuvres complètes de Christiaan Huygens,* published by the Société hollandaise des Sciences, vol. XXI (The Hague: Martinus Nijhoff, 1944), p. 471.

[31]See above, n. 22.

tions of the planets and comets be impeded or rendered irregular. But if meanwhile someone explains gravity together with all its laws by the action of some subtle matter, and shows that the motions of the planets and comets will not be disturbed by this matter, I shall be far from objecting.[32]

The passage I have here emphasized is an exact counterpart of the one I have stressed in Newton's reply to Hooke: Newton says that his confidence in the law of gravitation—a law he had "deduced from the phenomena"—rests upon the fact that *all the phenomena of the heavens and of the sea* (and, most especially, one might remark, an impressive array of such phenomena that did *not* enter into the initial "deduction") follow accurately from that law. Furthermore, this is certainly not a mere afterthought, written to persuade Leibniz and Huygens; for Book III of the *Principia* is itself an eloquent testimony to the concern of Newton for the most extensive "proof" he could obtain for his theory from the further study of phenomena.

This point is worth pursuing a little. In the first place, it is well known that the problem of the gravitational attraction of a spherically symmetric mass-distribution was a critical one for Newton. (Cajori once put forward the view—now no longer tenable—that this problem was responsible for a twenty years' delay in Newton's public announcement of the law of gravitation.)[33] Now, the proposition that states the solution to this problem is Proposition VIII of Book III of the *Principia;* thus, it is formulated *after* the statement of the law of gravitation (as, indeed, logic requires: a proposition about the gravitational effect of a given mass-distribution could hardly be established without appeal to that law). The result requires no mathematical argument at the point at which it occurs in Book III—the purely mathematical theorem about the effect of an inverse-square force with a spherically symmetric source-distribution has already been demonstrated in Book I. Here is what Newton has to say, in the place where that argument would normally occur:

> After I had found that the force of gravity towards a whole planet did arise from, and was compounded of the forces of gravity towards all its parts; and towards every one part, was in the reciprocal proportion of the squares of the distances from the part: I was yet in

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[32]*Correspondence,* III, pp. 285-286 (Latin), 286-287 (English translation).

[33]See §40 in the appendix to Cajori's edition of the *Principia* (pp. 663-664), and the article of Cajori there referred to. The history of Newton's discovery of the theory of gravitation is now much better known than when Cajori wrote. A general picture can be obtained from the relevant portion of Westfall, *Never at Rest* (pp. 381ff. in ch. 9, together with ch. 10). It has become quite clear that the idea of a universal law of gravitation only occurred to Newton in the mid-1680's, while he was working on the treatise that ultimately became the *Principia.*

> doubt, whether that reciprocal proportion did accurately hold, or but nearly so, in the total force compounded of so many partial ones. For it might be that the proportion which accurately enough took place in greater distances, should be wide of the truth near the surface of the planet, where the distances of the particles are unequal, and their situations dissimilar. But by the help of prop. 75. and 76. book I. and their corollaries, I was at last satisfy'd of the truth of the proposition, as it now lies before us.[34]

Now, the point of Cajori's suggestion was that this accurate holding of the inverse square law right down to the surface of the earth is essential for the comparison of the force on the moon in its orbit with the terrestrial force of weight, which is the first fundamental step towards Newton's law of gravitation. More precisely: In Proposition IV of Book III, Newton (by applying the inverse square law to the moon, in a thought-experiment in which the moon is brought down to the earth's surface) concludes that the force on the moon in its orbit is just its weight—and, correspondingly, that the force of weight itself varies inversely with the square of the distance of a body from the center towards which that weight tends. There are some subtleties and some difficulties of detail in the actual argument from the relevant phenomena, but let us here pass them by; we may say with at least rough accuracy that Newton's calculation, in Proposition IV, is justified by astronomical evidence that *the forces on planets and satellites* vary inversely with the squares of their distances *from that focus of their respective orbits towards which their accelerations are directed.* Thus (again, setting aside some complicating details), no difficulty of principle is presented at *this* point in the "deduction from phenomena" by the issue that is settled in Proposition VIII. However, in the transition from Proposition IV to Proposition VII a fundamental transformation of the theory under development has occurred: by the time he has reached Proposition VII, Newton has concluded that the phenomena of the solar system are the result, not simply of inverse square forces towards each of the principal foci, but—more fundamentally—of such forces towards *every particle of matter* as a *separate* center. With this transformation, it is far from clear *prima facie* that the theory is even *consistent* with the phenomena from which it has been "deduced." And the demonstration of Proposition VIII is the first step—clearly signalized as such by Newton—in the establishment of that consistency.

It is only a first step. Another is taken in Proposition XIII: "The Planets move in ellipses which have their common focus in the centre of the Sun; and, by radii drawn to that centre, they describe areas proportional to the times of description." In the course of the argument for this proposition, Newton, having estimated the perturbative effect of the actions of the planets upon one another in order to ver-

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[34]I follow Motte's original translation of 1729; see Sir Isaac Newton, *The Mathematical Principles of Natural Philosophy*, trans. (from the third edition) by Andrew Motte (reprinted London: Dawsons of Pall Mall, 1968), vol. II, pp. 226-227.

ify that they may be disregarded, actually concludes something a bit different: namely, that in the case of the action of Jupiter upon Saturn this effect is (relative to the observational techniques of his own time) *not* entirely negligible: indeed, that it is "so sensible that astronomers are puz[z]led with it."[35]

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[35]Let me take the occasion here to correct an error of my own. In the paper "Newtonian Space-Time" (cited in n. 29 above—see pp. 180-181 in *The Texas Quarterly;* pp. 264-265 in *The* Annus Mirabilis *of Sir Isaac Newton*), I raised the question why the occurrence of *observable* perturbations, predicted by Newton's theory, which had previously *"puzzled astronomers,"* should not have convinced Huygens (at least) of the "mutual attractions of entire bodies"; and pointed out that the statement that the perturbation of Saturn's motion by the action of Jupiter is "sensible" did not occur in the first edition of the *Principia.* In the second edition, such a statement occurs both in Corollary 3 of Proposition V, Book III (no such Corollary appears in the first edition), and in the discussion of Proposition XIII—where the remark about "puzzling astronomers" is found. I pointed out further that Newton had written to Flamsteed in December 1684 (*Correspondence,* II, pp. 406-407), inquiring about a possible deviation of Saturn's motion near its conjunction with Jupiter, and had received a reply (ibid., pp. 408-409) that was somewhat indecisive, but essentially negative. I then cited a memorandum of David Gregory, dated 4 May 1694 (Newton, *Correspondence,* III, p. 314 [Latin], 318 [English translation]), to the effect that the actions upon one another of Jupiter and Saturn "were manifest" at those planets' "most recent conjunction," and that this observation had led to "Corrections of the Orbits of Saturn and Jupiter by Halley and Flamsteed, which were afterwards perceived to be worthless." In commenting upon all this, I expressed some surprise that "neither Flamsteed, to whom Newton had addressed his inquiry several years before, nor Halley, Newton's good friend, had thought at first of the effect predicted by Newton."

Now, in the case of Halley in particular, such a lapse is almost incredible: Halley, after all, was the devoted sponsor of the *Principia.* But it turns out that I was misled by a mistranslation of the Latin by Turnbull. The phrase *[Saturn]i et [Jov]is proxime preterita conjunctione* is rendered by the latter as "at [their] very recent conjunction"; and although I had given my own more accurate reading, "at [those planets'] most recent conjunction," I assumed that Turnbull was correct in construing that ambiguous phrase (the ambiguity being the same in Latin as in English) as he had; assumed, therefore, that a conjunction had been observed near the date of Gregory's note—that is, some time in the early 1690's. But this is quite wrong. A series of three geocentric conjunctions of Jupiter and Saturn (first in direct motion, then retrograde, then again direct) had in fact occurred from October 1682, to May 1683 (see the letter of Arthur Storer to Newton of April 26 1683: *Correspondence,* II, pp. 389-390, with n. 14, p. 394). There was, therefore, a heliocentric conjunction during this interval; and since the period for such

Thus the establishment of the consistency of the theory with the phenomena from which it was inferred is only approximate, and terminates in an actual (although small) *deviation* from the previously assumed phenomena—which deviation, however, agrees with the *actual* phenomena. In December 1684—quite early, therefore, in the period during which the *Principia* was developed—Newton had written to Flamsteed inquiring about the possible occurrence of observable planetary perturbations.[36] The pursuit of this general theme—that of detailed consequences of the theory of gravitation, and their comparison with observations—characterizes almost all the remainder of the *Principia,* and much of the remainder of Newton's active scientific life. And this is what I have meant in saying that Newton's pithy response to Leibniz—namely, that his confidence in the theory rests upon the fact that "all the phænomena of the heavens and of the sea" follow precisely, so far as he is aware, from that theory (rests, in other words, on the continuing successful

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conjunctions is nearly twenty years, this was the "most recent conjunction" referred to by Gregory: not "very recent," but some eleven years past.

Further clarification of this episode—of the circumstances, first, of the new determinations of the orbits by Halley and Flamsteed; and then, still more interesting, those of the discovery that such corrections were useless and that the discrepant observations were due to the perturbation predicted by Newton—wouod surely be very desirable. I have found no discussion of the matter in the historical literature. (For instance, I. B. Cohen, in his commentary on the *Principia* in its three editions, based upon his and Koyré's comprehensive edition recording all substantive revisions, does not mention the changes that introduced allusions to observable perturbations; and, although he discusses the memoranda made by Gregory—on the basis of discussions with Newton—in the early nineties, when Gregory former was working on the project of a second edition, he omits the memorandum I have spoken of here.—See I. Bernard Cohen, *Introduction to Newton's 'Principia'* [Cambridge, Mass.: Harvard University Press, 1978], ch. vii, §§11-15.)

[36]See the preceding note.—It is clear from this inquiry that by December 1684 Newton was in possession, if not already of the law of universal gravitation, at least of the theory that the planets are attracted to one another. (Caution: this neither implies universal gravitation, nor establishes that Newton had at this point thought of applying the third law of motion to a pair of planets—or to a planet or satellite and its central body. For if one simply assumes that each planet—or, still more cautiously, each planet that has satellites—is a gravitational center, towards which *all bodies have weight,* it will follow that the planets have weight towards each other. Thus one might imagine Huygens, for example, being persuaded of the "mutual attraction of entire bodies," while still rejecting the universal gravitational interaction of all particles of matter.)

"proof" of the theory by phenomena)—is no mere afterthought, or rhetorical ploy to persuade Leibniz and Huygens, but a veracious account of his own steady scientific practice.[37]

§2.1 That the science developed in Book III of the *Principia* qualifies as "mathematical" in the sense explained by Newton in his reply to Hooke, I presume no one would question. An objection was raised in discussion at the symposium, however, concerning the science of color itself, concerning which Newton was there speaking: it was said that Newton's claim was false; that neither in his paper, nor in the *Optical Lectures,* did Newton actually *compute* phenomena of colors. This is a view that has also been argued in print;[38] it requires some attention here.

Some distinctions are needed. In the first place, Newton quite clearly did make use of geometrical reasoning—for example, in predicting (or explaining the observation) that whereas a single prism produces a linearly dispersed spectrum of the various colors, a second refraction by a prism placed at right angles to the first and to the mid-ray of the first-refracted beam does not further disperse the col-

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[37]One very late instance of this is worth mentioning. Westfall (*Never at Rest,* pp. 799-800) relates a series of incidents from Newton's late years as evidence that "Newton could no longer sustain a penetrating reexamination of his work." I do not question that this is true, nor that the evidence is pertinent. But one of the events (or anecdotes) shows something else as well. "Molyneux," Westfall says,

> told Conduitt that he, together with Graham and Bradley, had accidentally discovered a nutation of the earth's axis while they were attempting to observe stellar parallax. Unable to account for the nutation, they were convinced that it undermined the whole Newtonian philosophy [that is, of course, the whole theory of gravity]. Molyneux did not know how to break such news to Newton and did so very gradually and gently, only to meet the same lack of interest which Stirling had found; 'all S^{r} I. said in answer, was, it may be so, there is no arguing against facts & experiments.' It is true that Newton's treatment of precession implied nutations whatever Molyneux thought, but the story expresses indifference rather than quiet confidence.

I think this story does clearly show that Newton by this time lacked the interest, energy, and power of concentration to rethink a difficult problem of mathematical physics; but to me it appears also that the attitude Newton expresses concerning the relation of theoretical principles to observational test is the same one he had consistently stated—and followed in practice—from his earliest scientific maturity.

[38]See Alan E. Shapiro, "Experiment and mathematics in Newton's theory of color," *Physics Today* **37** No. 9 (September 1984), pp. 34-42.

ors into a(n approximate) square, but gives an *oblique, linear* image (with the several colors in their expected sequence).[39] This prediction—or explanation—obviously could be made quantitative; but even if it is presented (as I have done here) only qualitatively, it nonetheless constitutes an example of what Newton speaks of: the determination, by a mathematician, of phenomena of colors, "by computing *or demonstrating* after what manner & how much those refractions doe separate or mingle the rays in wch severall colours are originally inherent."[40] In this sense of "computing or demonstrating"—i.e., determining by *mathematical reasoning* from his principles—there are in fact numerous instances in the *Optical Lectures* of such predictions or explanations of phenomena.

A second relevant distinction concerns just *how much* of the explanation or prediction is "computational," or, more generally, "mathematical." Many of the relevant phenomena involve composite colors. Newton does not seem, at this date, to have been in possession of any means of "computing" what color would result from a given combination of simple colors; this, therefore, is something that would have to be presupposed as determined by empirical investigation before the actual appearances of colors in a given experimental situation could be predicted. Similarly, one would have to suppose given, in addition to the general principles stated in Newton's paper, a table of empirically determined indices of refraction of a sufficient variety of media for a sufficient variety of homogeneous lights (with the colors of the latter specified). It does not seem to me unreasonable on Newton's part to have taken it for granted that his readers would understand the need for such supplementary empirical tables as a basis for mathematical computations of colors; the corresponding necessity in such recognized

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[39]See *Lectiones opticæ,* in Shapiro (ed.), *The Optical Papers of Newton,* I, pp. 88-91; *Optica,* ibid., pp. 306-309, 440-443; and cf. Newton, *Opticks,* pp. 34-45.

[40]It is possible that Shapiro's negative opinion is influenced by a too restricted view of the "mathematical"; for in the paper cited in n. 38 above (see p. 37 of that paper), he writes as follows:

> [According to Newton] there is a one-to-one correspondence between refrangibility and color. Newton implicitly assumes that this correspondence is independent of the refracting material. This proposition allows the possibility of constructing a mathematical theory of color, for it in principle assigns a metric to color. However, because it is proved and applied only qualitatively, namely, by observing that red rays are always refracted the least, violet the most, and so on, it could not be strictly established or mathematically formulated. Nonetheless, in the "New Theory" Newton (erroneously) asserts that the correspondence "is very precise and strict."

But of course it is entirely possible to reason mathematically about (non-metrical) ordering relations; nor is it clear why it should be supposed that such relationships cannot be "strictly established."

examples of "mixed mathematics" as astronomy—or, indeed, optics itself—was certainly familiar.[41] In short, as I read Newton's claim in that passage, and compare it with what he was actually in a position to do—and actually did do—it seems to me that his claim was absolutely and fully justified.

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[41]Shapiro (ibid., pp. 38-40), places great emphasis upon Newton's proposed general dispersion formula(s) as a central part of the "mathematical theory of color," and maintains that by the time of the *Opticks* Newton had abandoned the idea of such a mathematical theory, at least in part because of uncertainty about the correct dispersion formula. J. A. Lohne had earlier expressed the view—cf. his "Newton's 'Proof' of the Sine Law and his Mathematical Principles of Colors," *Archive for History of Exact Sciences* **1** (1961), pp. 389-405; see p. 397—that "Newton . . . proposed to erect this mathematical science of colors on a dispersion law." To me this seems rather odd. It is of course true that the possession of a general law that should determine the index of refraction of an arbitrary medium for an arbitrary spectral color on the basis of its index of refraction for a given ("standard") color and the corresponding indices of a given ("standard") medium—and this is the character of Newton's proposed dispersion law(s)—would contribute to the strength of the theory; would, one might say, make it "more mathematical." However, it is apparent from the characterization I have just given that such a theory would still require empirical data (not just the general principles enunciated by Newton)—namely, all the indices of refraction of the "standard" medium, and the index of refraction of the medium in question for the "standard" color. Again, as noted in the text above, such a requirement of empirically determined parameters in a "mixed mathematical" science is something that may be taken to go without saying. Not even Descartes, with his deprecation of experiment as a means of establishing such general quantitative *laws* as the law of refraction (cf. his discussion of the problem of the "anaclastic curve" in Rule VIII of the *Rules for the Direction of the Mind*), ever claimed to be able to determine actual indices of refraction without recourse to measurement. It is therefore very puzzling to me that anyone would take the establishment of a general dispersion law as the *sine qua non* of a "mathematical science" of colors.

What makes this still more puzzling is the fact that these writers make no difficulty about the lack, for Newton (at the time of the *Optical Lectures* and the "New Theory"), of any means of computing the results of color mixing; for this seems more intimately connected to what one might expect in a "mathematical theory of colors." Such a method was, in fact, eventually developed by Newton—the "center of gravity" construction described by him in the *Opticks*, Book I, Part II, Proposition VI, of which he says, "This Rule I conceive accurate enough for practice, though not mathematically accurate," but which later proved of quite fundamental importance for the understanding of the physiological psychology of color perception; it furnishes the foundation for the work of Young, Graßmann, Helmholtz, and Maxwell, referred to in nn. 16, 20, and 21 above. In this sense, one might well argue

§3. The preceding section has grown to great length—although not, I think, length disproportionate to the importance of its subject. There still remains an aspect of that subject that it seems proper to assign a separate section of its own, since it concerns chiefly not the historical evidence for the position I have ascribed to Newton, but rather the philosophical interpretation (and perhaps also the philosophical assessment) of that position.

Namely, it may be asked—and the question was indeed raised at the symposium—in what way Newton's position then differs, according to me, from a straightforward hypothetico-deductive account of scientific reasoning. What I have said about Newton, it was argued, sounds more appropriate to Huygens.

Now, I do believe that Newton's conception of scientific validation is much closer than usually supposed to what we now call, in a general way, the hypothetico-deductive one; this seems to me to follow clearly from the evidence I have now cited at some length. But I also think there is an important difference—let us say, *within* the bounds of the general hypothetico-deductive view—between Newton and Huygens. For the latter, as I have remarked in **MMN**, the essential task of physics is to devise "mechanical" *hypotheses* for the explanation of phenomena—which hypotheses are then to be tested by testing their consequences. Any "mechanical" hypothesis that succeeds in accounting for the known phenomena is thus acceptable—has, one may say, "standing" in physics; and any proposition, however well it may conform to known phenomena, that poses serious obstacles to "mechanical" explanation is to that extent suspect, and lacks standing—indeed, if the proposition appears to be *impossible* to reconcile with "mechanical" explanation, it is flatly to be rejected.

I do not suppose that anyone will quarrel with the statement that these last principles of method differ sharply from Newton's. To say this, however, leaves open the possibility that the only difference between Huygens and Newton in this regard is that the latter has a more liberal view towards hypotheses than the former—a view receptive of hypotheses that fail to meet Huygens's stringent standard of mechanical explanation. And this is surely a hard thing to accept, in the light of Newton's expressly negative position concerning the "standing" of hypotheses. So I must now explain how I under-

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that, far from having been forced to abandon the idea of a "mathematical science of colors" in the *Opticks,* Newton had in that work enormously deepened that mathematical science.

Let me take the occasion to remark that it would be of the greatest interest if some indication were found, in Newton's remaining optical papers, of the method by which Newton arrived at his construction for determining composite colors. To do this would involve experimenting upon mixtures of colors *in given proportions;* and that is by no means a straightforward thing to do. I have remarked above that one of the experiments Newton suggests in his reply to Hooke involves the color top. Could he eventually have used such an instrument for this purpose—thus anticipating the work of Maxwell?

stand this negative position; to explain, in other words, how (in my opinion) Newton's view is indeed in one way more liberal, but in another way far more restrictive than that of Huygens. In preparing to do so, I have found myself led to reconsider just what it is that Newton meant by a "deduction" of propositions "from phenomena."

It is quite clear that Newton regarded as such deductions both his inference from the *Experimentum Crucis* and his argument for the law of universal gravitation. It is *prima facie* plausible in the latter case to take the "deduction" to consist just in the reasoning that leads from the *Phænomena* to Proposition VII. If one does so take it; and if one accepts the conclusion contended for above, that Newton did not regard the law of gravitation as adequately supported until he had proceeded to the further "proof" represented by the main body of Book III; then one is likely to conclude that "deduction from the phenomena" in general for Newton is something that has *heuristic* but not *probative* force—that it is an "abductive" procedure, in the sense of that expression due to C. S. Peirce—rather than a mode of argument that *establishes* its conclusions. I have myself long been inclined to view the matter this way; but I think now, as a result of reflecting further upon the challenge made in the symposium discussion, that this cannot be quite right, and that Newton's position—or at any rate, what is *implied* by his several statements (whether or not he fully appreciated the implication)—is more subtle and interesting.

In a famous passage near the end of the General Scholium to the *Principia* Newton tells us that "whatever is not deduced from the phenomena, is to be called an *hypothesis;* and hypotheses whether metaphysical, or physical, or of occult qualities, or mechanical, have no place in *experimental philosophy.*" He adds: "In this philosophy propositions are deduced from phenomena, and rendered general by induction."[42] To this, written in early 1713,[43] there correspond two passages that first appeared in the Latin translation of the *Opticks,* in 1706;[44] I quote them from the final English edition. There we

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[42]In the interest of strict verbal accuracy, I here translate more literally than did Motte, who not only adds a few words, but introduces a stylistic variation in the second quoted sentence, replacing Newton's "deduced" by "inferr'd." The original reads: "Quicquid enim ex phænomenis non deducitur, *hypothesis* vocanda est; & hypotheses seu metaphysicæ, seu physicæ, seu qualitatum occultarum, seu mechanicæ, in *philosophia experimentalis* locum non habent. In hac philosophia propositiones deducuntur ex phænomenis, & redduntur generales per inductionem." See *Isaac Newton's Philosophiæ Naturalis Principia Mathematica,* 3rd ed. with variant readings, ed. Alexandre Koyré and I. Bernard Cohen (Cambridge, Mass.: Harvard University Press, 1972), vol. II, p. 764.

[43]See Cohen, *Introduction to Newton's Principia,* p. 240.

[44]Some confusion on this point may result from a statement by I. B. Cohen in his preface to the Dover Publications reprint of the *Opticks.* On p. xxxiii of that edition, Cohen says: "The Latin version . . . was issued . . . in 1706; to it were added Queries 17-23 in which Newton discussed the nature of the

read, in Query 28, that "the oldest and most celebrated Philosophers of *Greece* and *Phœnicia* " made "the Gravity of Atoms" one of "the first Principles of their Philosophy"; but that, in contrast:

> Later Philosophers banish the Consideration of such a Cause out of natural Philosophy, feigning Hypotheses for explaining all things mechanically, and referring other Causes to Metaphysics: Whereas the main Business of natural Philosophy is to argue from Phænomena without feigning Hypotheses, and to deduce Causes from Effects, till we come to the very first Cause . . . .[45]

And—the fullest statement we have of Newton's view of the proper mode of reasoning in science—in Query 31:

> As in Mathematicks, so in Natural Philosophy, the Investigation of difficult things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception occur from Experiments, it may then begin to be pronounced with such Exceptions as occur.[46]

It is noteworthy that this last passage makes "induction"—by which inferences from experiments, or observations, or phenomena, are "rendered general"—a *part* of the "Method of Analysis" by virtue of which propositions count as something more than "hypotheses." In the passage from the General

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aether." This is incorrect. The queries concerned principally with the ether—eight in number—were in fact introduced in the second English edition of the *Opticks* (1717), where they are inserted as Queries 17-24; the seven queries introduced in the Latin edition (somewhat modified in the light of the new discussion of the ether) are there renumbered as Queries 25-31. See Cohen, *Introduction to Newton's Principia,* p. 259, n. 2, where this is explained (but without any reference to Cohen's earlier, erroneous statement); and for a fuller discussion Westfall, *Never at Rest,* pp. 643ff.

[45]*Opticks* (Dover ed.), p. 369.

[46]Ibid., p. 404.

Scholium, Motte has it that "particular propositions are inferr'd from the phænomena, and *afterwards* render'd general by induction" (my emphasis); but the word "afterwards" is Motte's, not Newton's. It is also clear that each of these passages insists that *only* propositions that are "deduced from phenomena" can qualify as more than hypotheses. But to suppose this to mean that Newton wished to exclude altogether from scientific consideration any proposition that has been *initially hit upon* otherwise than as the outcome of a systematic argument from experience is to attribute to him a view that is (a) entirely unreasonable—in fact, impossible to apply generally; (b) (perhaps I should say, *therefore*) incompatible with his own practice; and (c) discordant with other statements he made, both early in his career and late, as I have shown above. Some commentators are of course entirely happy to conclude that Newton's preachment is indeed unreasonable, incompatible with his practice, and inconsistent in itself. I believe, however, that a simple interpretation is possible that makes of Newton's methodological position a coherent whole, consonant with his general practice (although, as I also believe, there are significant lapses), and philosophically bot defensible and interesting.

The nub of the matter is this: The view that Newton's "deduction" amounts to a kind of Peircian abduction, and is therefore to be *distinguished from* "proof" in Newton's sense, involves a stronger separation than I think Newton himself would make between what has more recently been called the "context of discovery" and the "context of verification (or confirmation)." I do not mean that a distinction like the latter (which, however much discredit has come upon it through anti-logical-empiricist criticism, seems to me—if it is not made too rigid—quite useful) has no place at all in Newton's scheme of science; on the contrary, the non-"feigning" use he allows for hypotheses is one precisely restricted to "contexts of discovery" (or, perhaps better, of investigation). But this last remark implies that it is just in such purely heuristic contexts that Newton would *not* insist upon "deduction from the phenomena." On the other hand, Newton would surely not call a proposition that is being used as a guide to investigation—such, for instance, as the propositions that give affirmative answers to the Queries of the *Opticks*—a "discovery." It is only when such a proposition has achieved some reasonably satisfactory degree of "proof" that he would allow it to have been "discovered," or (a word more characteristic of him) "detected," from phenomena.

But then there is only one way to understand how Newton's position can be more restrictive than Huygens's: this stringency can only lie in what he regards as a satisfactory degree of proof. Just this is clearly implied by a phrase in his first paper, when, speaking of the part of his "New Theory" that concerns colors, he writes: "[W]hat I shall tell concerning them in not an Hypotheses but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise *or because is satisfies all phænomena (the Philosophers universall Topick,)* but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt."[47] Since, on the other hand, as I have argued above, the propositions of this part of the theory are indeed established—and can only be so—by an abundance of experimental tests, and since Newton himself makes no bones about this (indeed, proclaims it with pride), it follows that (if he is consistent) he must distinguish between simply "satisfying all phenomena," and being *adequately* "proved" by phenomena.

There is a well known difficulty about such a position: we do not possess a systematic, generally accepted theory of inductive evidence that can provide an exact analysis of the required distinction. And of course there are philosophers of science—Popper, for example—who have concluded that there is simply no such thing as inductive *reasoning.* It is obviously beyond the scope of the present discussion to argue the philosophical issue at any length; but two points are worth making. First, the lack of a systematic theory (and even the possibility that we shall never have such a theory) cannot of itself exclude actual (if rough) inductive argumentation—any more than the lack, up to a century ago, of a systematic theory of mathematical inference made the actual conduct of mathematical reasoning impossible. And second, whatever one may think of the philosophical cogency of the notion of "an adequate degree of empirical support," it cannot be regarded as controversial that such a notion has been entertained by philosophers and scientists; to doubt or even to reject the notion is quite compatible with recognition that it formed part of Newton's conception of science. (I have argued in **MMN** not only that Newton held such a notion, but that he succeeded to a remarkable degree in separating the "imagined" from the "proved," the "conjectures" from the "certainties"; and this claim does, of course, presuppose a favorable verdict upon the philosophical notion itself.)

I should like to pursue this point just a little further, citing an example from another physicist whose philosophical merits I rate very high. When Maxwell, in 1855-6, put forward some heuristic ideas towards a radically new way of thinking about the phenomena of electricity and magnetism (based upon the conceptions of Faraday), he concluded the main body of his paper with some remarks on the relation of his suggestions to the then reigning theory (that of Wilhelm Weber). "In these six laws," he wrote, "I have endeavoured to express the idea which I believe to be the mathematical foundation of the modes of thought indicated in the *Experimental Researches.* I do not think that it contains even the shadow of a true physical theory; in fact, its chief merit as a temporary instrument of research is that it does not, even in appearance, *account for* anything."[48] He then proceeds to acknowledge the existence of "a professedly physical theory of electro-dynamics, which is so elegant, so mathematical, and so entirely different from anything in this paper, that I must state its axioms, at the risk

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[47]See n. 5 above. (The passage containing the words here emphasized does not appear in the version published by Oldenburg—cf. n. 4 above.)

[48]James Clerk Maxwell, "On Faraday's Lines of Force," in W. D. Niven, ed., *The Scientific Papers of James Clerk Maxwell* (Cambridge University Press, 1890), vol. I, p. 207 (emphasis in original).

of repeating what ought to be well known." After presenting the four basic assumptions of Weber's theory, and remarking that from them there follow the known laws both of the forces between and the induction of currents, Maxwell continues: "Here then is a really physical theory, satisfying the required conditions [that is, in Newton's phrase, "satisfying all phenomena"] better perhaps than any yet invented, and put forth by a philosopher whose experimental researches form an ample foundation for his mathematical investigations"; and asks: "What is the use then of imagining an electro-tonic state of which we have no distinctly physical conception, instead of a formula of attraction which we can readily understand?"[49]

Maxwell's answer to his own question contains three clauses, of which I am here interested in just one. He says: "I do not think that we have any right at present to understand the action of electricity, and I hold that the chief merit of a temporary theory is, that it shall guide experiment, without impeding the progress of the true theory when it appears." This is in effect to maintain that the theory of Weber, although it accommodates all the phenomena then known, is nevertheless not adequately supported by the evidence, because *the known phenomena are* (should one say, "demonstrably"?) *insufficient to afford a basis for a full theory of electromagnetism.* It is a claim quite precisely parallel to Newton's judgment that the known phenomena of optics remained inadequate, even at the end of his career, to ground a fully satisfactory theory of light.

The difference from Huygens, then, lies in this: that Huygens, who considers mechanical explanation a *sine qua non,* believes that such an explanation is acceptable when no known phenomena contradict it; whereas Newton on the one hand rejects the necessity of such an explanation, and on the other hand demands more of the agreement of theory with phenomena: he demands that the phenomena by which the theory is "proved" be in some sense *proportionate to the content of the theory*—that, in some sense, they really test all the main parts of the theory. Again the case of Maxwell and Weber may be instructive. It was an essential assumption of Weber's theory—without which its explanation of the forces between currents, and of the induction of currents, would fail—that all electric currents consist in an equal flow of positively charged particles in one direction and negatively charged particles in the other. This hypothesis about the physical constitution of the current had no foundation whatever in direct experimental evidence (Maxwell himself was unwilling to postulate even that current in general consists in motion of charged matter); and, of course, it was eventually proved to be quite wrong.

One final remark about the present issue, and in particular about the contrast between Newton and Huygens: I have argued in **MMN** that Newton had conceived a very new kind of program of scientific investigation. A most crucial point about this program is Newton's optimism (which may be usefully contrasted, for example, with the pessimism in such matters of the "skeptical chemist" Boyle and of the "official" doctrine of Locke's *Essay*) concerning *the eventual reach of experimental investigation*

[49]Ibid., p. 208.

itself. [50] For example, the line of investigation in Books II and III of the *Opticks* is designed not only to perfect the theory of light—it is directed also towards obtaining information both about the structure of matter (concerning which Newton—perhaps somewhat rashly; without a doubt prematurely; and in part explicitly as a matter of "conjecture"—states a series of very radical conclusions in Book II of that work),[51] and about the forces between microscopic, perhaps even elementary, particles.[52] The connec-

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[50]Some discussion of the relation between Newton's view of science and that of Locke—and of late influences of Newton upon Locke, traceable in the *Essay* and in other writings, that led to statements by the latter in striking contrast with what I have called his "official" doctrine—will be found in the forthcoming paper cited in n. 29 above.

[51]These conclusions are stated by Newton in Propositions II-VII of Book II, Part III, of the *Opticks.* The most radical of them is that by far the greatest part of the volume of even the densest bodies consists of empty space. This view is first hinted at in the statement of Proposition III (where, however, the hint is blurred by the suggestion that the spaces between "the parts of opake and colour'd bodies" may be not quite empty, but "replenish'd with Mediums of other Densities"). It is then greatly reinforced by the discussion of Proposition Viii, where Newton argues for the extreme porosity of all bodies, and sketches a theory of a hierarchical structure of matter—out of particles composed of smaller particles, themselves composed of still smaller particles, and so one. He supposes, by way of example, that at each level of this hierarchy a composite particle consists half of particles of the next lower level, and half of spaces between those constituents; concluding thus: "And if in any gross Body there be, for instance, three such degrees of Particles, the least of which are solid; this Body will have seven times more Pores than solid Parts. But if there be four such degrees of Particles, the least of which are solid, the Body will have fifteen times more Pores than solid Parts. If there be five degrees, the Body will have one and thirty times more Pores than solid Parts. If six degrees, the Body will have sixty and three times more Pores than solid Parts. And so on perpetually. And there are other ways of conceiving how Bodies may be exceedingly porous. But what is really their inward Frame is not yet known to us."

The explicitly "conjectural" conclusion is found in Proposition VII; but in this whole series of propositions Newton seems to me to underestimate seriously the extent to which his reasoning rests on a hypothetical foundation (namely, on a most conjectural theory of the mode of propagation of light through transparent bodies and of its reflection by opaque colored bodies). On the other hand, the concluding statement of the passage just quoted shows clearly both that Newton realized that he was dealing with a subject about which he really knew very little, and—in his use of the important little word "yet"—that he envisaged progress far beyond what he had attained.

[52]A small beginning on the program of learning about such forces is suggested by Propositions IX and X of *Opticks,* Book II, Part III. Such a program dominates many of the Queries of Book III of that work (the passage I have quoted in **MMN** concerning electrical attraction occurs in a context of this kind). It is

noteworthy that this whole set of queries—the earliest of which speculate about *forces* between rays of light and ordinary bodies—is printed as surrogate for experimental investigations Newton had originally planned to complete Book III. In the observations of phenomena of "inflection" with which that Book opens, Newton has convinced himself that forces must be operative between bodies and light that are attractive at some distances, repulsive at others (cf. Query 3: "Are not the Rays of light in passing by the edges and sides of Bodies, bent several times backwards and forwards, with a motion like that of an Eel? And do not the three Fringes of colour'd Light above-mention'd arise from three such bendings?"). Since Newton believed in the fundamental identity of the (ultimate) particles of light and of ordinary matter (and very likely thought the particles of light themselves to be elementary particles), and since a force that is attractive at some distances, repulsive at others, was in his view indicated by the exigencies of a theory of matter, it seems to me quite evident that the possibility of an experimental investigation of both fundamental structure and fundamental forces was to Newton by no means a remote dream.—For the identity of particles of light and of matter, cf., e.g., Query 30. That the particles of light are elementary is not something that, to my knowledge, Newton ever suggests explicitly—in fact, I know of no place in which he suggests *anything* definite about which particles, or which forces, may be fundamental ones. But his account of the traversal of a transparent body by a ray of light does at least imply that the latter, if taken to be a particle, must occupy a position far down in the succession of deeper and deeper levels of analysis of the components of matter; for in the discussion of this hierarchical structure (see the preceding note), Newton tells us (*Opticks*, Dover ed., p. 268): "The Rays of Light, whether they be very small Bodies projected, or only Motion or Force propagated, are moved in right lines; and whenever a Ray of Light is by any Obstacle turned out of its rectilinear way, it will never return into the same rectilinear way, unless perhaps by very great accident. And yet Light is transmitted through pellucid Bodies in right Lines to very great distances." It follows, then, that if the rays are bodies, they must be small enough to pass through the pores of a level sufficiently deep that the pores at that level preponderate enormously over the occupied regions.

It ought to be said that if Newton was over-optimistic about the immediate prospects for his program, the general lines of the research he envisaged have striking similarities with the lines along which in our own century information of the kind he sought has actually been obtained. For example, with the help of a more adequate theory of light than Newton's, and of a newly discovered part of the (quasi-)optical spectrum (namely, X-rays), it became really possible to gain information about the micro-structure of bodies (the determination of crystal structure by "optical" probes of matter: X-ray diffraction). And the study of the scattering patterns of beams of more or less fundamental particles to obtain bot structural and dynamical information has—starting from the time of Rutherford's determination of the nuclear structure of atoms with the help of α-particle scattering—become one of the major contemporary modes of experimental physics.

tion between this optimism about the prospects for definite information, on the one hand, and Newton's more stringent demands upon the evidence for theories, on the other, is obvious.

To summarize the conclusions I draw from this lengthy train of reflections, the salient features of the position I have attributed to Newton may be put as follows;

(a) Newton rejected any decisive *a priori* commitment to a special mode of scientific explanation. Two corollaries of this rejection are (i) that, in comparison with his contemporaries generally (and with Huygens in particular), he admitted a far wider class of possible theories into serious scientific consideration; and (ii) that the mere fact that a suggested theory, compatible with known phenomena, explained those phenomena in a mode generally esteemed desirable or necessary (to wit, "mechanically") did not, in his eyes, suffice to give that theory high scientific standing.

(b) The greater stringency of evidence demanded by (a)(ii) above implies a philosophical commitment to the view that the strength of evidence can be estimated in a way that goes beyond simple consistency of theory and phenomena, to take account of the adequacy with which the essential parts of a theory have been put to "proof." A theory counts as having been *adequately* "proved by"—or, in other words, as having (in Newton's sense of the words) been "deduced from"—phenomena, only when (not just the number of those phenomena is large enough, but) their range is sufficient to allow one, in the light of reasoning that connects the phenomena with the theory, to conclude that the main components of the theory have undergone genuine test.—Of course, in the absence of a systematized theory of the strength of evidence, the application of this principle in specific cases has to rest strongly upon individual judgment.

(c) Both of the preceding principles of method (the openness of investigation to the "detection" of scientific principles of unexpected and novel form, and the demand that principles be stringently tested before they are admitted to a "place" in experimental philosophy) are related to, perhaps one should even say dependent upon, the view that the reach of experiment itself—guided by theory already developed and "proved," and aided in the interpretation of its results by such theory—can extend to domains of nature that were ordinarily supposed to lie beyond that reach.[53]

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[53]What I have here referred to as the "ordinary" supposition about the possible reach of experiment underlies both the "hypotheticism" of Huygens and the "official" skepticism of Locke in the *Essay concerning Human Understanding.* For instance, in Book IV, ch. iii, §26 of the latter (Nidditch ed., pp. 556-557) Locke—having said in the just preceding section that "whilst we are destitute of Senses acute enough, to discover the minute Particles of Bodies, and to give us *Ideas* of their mechanical Affections, we must be content to be ignorant of their properties and Affections"—makes this declaration:

> And therefore I am apt to doubt that, how far soever humane Industry may advance useful and *experimental* Philosophy *in physical Things, scientifical* will still be out of our reach . . . . By the Colour, Figure, Taste, and Smell, and other sensible qualities, we have as clear, and distinct *Ideas* of Sage and Hemlock, as we have of a Circle and a Triangle: But having no *Ideas* of the particular primary Qualities of the minute parts of either of these Plants, nor of other Bodies which we would apply them to, we cannot tell what effects they will produce; Nor when we see those Effects, can we so much as guess, much less know, their manner of production. Thus having no *Ideas* of the particular mechanical Affections of the minute parts of Bodies, that are within our view and reach, we are ignorant of their Constitutions, Powers, and Operations: and of Bodies more remote, we are yet more ignorant not knowing so much as their very outward Shapes or the sensible and grosser parts of their Constitutions.

In the preceding note I pointed out that in a domain in which Newton overestimated the short-range prospects of obtaining "hidden" information—just the domain, indeed, that Locke has referred to: that of "the minute parts of Bodies"—the *kind* of technique he envisaged did eventually prove fruitful in obtaining such information; namely, after theory had progressed sufficiently far, and after previously unknown natural processes had been discovered. But it is also germane to note that Newton himself actually succeeded in obtaining some such information about minute constitution. For Newton, in the investigations reported in Book II of the *Opticks* (and earlier in his communication to the Royal Society of December 1675), measured what we now call the *wave lengths* of homogeneous lights. Such lengths are not only far more minute than anything Locke would suppose to be "within our reach," they are just at the boundary of what can be made visible at all by purely optical means. Of course, this last fact has no bearing upon Newton's measurement, since the latter is in any case indirect—there could be no question of "seeing" a wave length of light. It is, however, very striking that the significance of this length as defining the limit of what can be made visible was recognized by Newton, as a result of this very investigation: he concluded that a particle smaller than the length in question cannot reflect light (and is in this sense "transparent"). Speaking of (presumed} composite corpuscles somewhat larger than this that enter into the constitution of bodies, he says (*Opticks,* Dover ed., p. 262): "[I]t will add much to our Satisfaction, if those Corpuscles can be discover'd with Microscopes; which if we shall at length attain to, I fear it will be the utmost improvement of this Sense. For it seems impossible to see the more secret and noble Works of Nature within the Corpuscles by reason of their transparency."

Newton, of course, did not interpret the length he determined as a wave length (although the nature of the phenomenon did indicate clearly to him that this length was associated with some kind of spatial periodicity). Thus we have not only a remarkable case of the finding out of a hidden

§4. I wish now to return to the substantive consideration of Newton's "New Theory" (that is, of the theory contained in his letter of February 6 1672), and its relation to the further explanations he offered to Hooke and to Huygens; for subsequent correspondence, and a reconsideration of writings of recent historians of the subject , has led me to conclude that the controversy that arose at the symposium turns on fairly deep issues of interpretation—issues that concern, in part, exactly what Newton's theory itself asserts, but that concern also how *any* account of a scientific theory ought to be read.

One objection that I have received to my own reading of Newton includes the following passage:

> You begin your critique . . . by explaining of the two sorts of rays (a simple and compound green "that their colors are 'the same in *Specie* '—in appearance." (Incidentally, your etymology is incomplete, for while "species" does mean "appearance" in Latin, it also means "sort" or "kind"; and according to the *OED* the phrase "in specie" always has the latter meaning. In any event, this is of no consequence, for in either case it means "hue.") That you have to introduce the concept of appearance or hue . . . shows that Newton did change his definitions of simple and compound colors in his successive replies to Hooke and Huygens. Newton . . . came to realize that his concepts of simple and compound colors were defined in terms of refrangibility and not visual appearance or hue; he makes this point especially clear to Hooke by explaining the test of refracting two identically appearing green rays through a prism. In his letter to Huygens he then introduces the terms homogeneous and heterogeneous *colors* which are defined in terms of homogeneous and heterogeneous *refrangibility.*

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attribute, but one in which this "detection" clearly did not rest upon a prior "conjecture." (A careful analysis of Newton's experiments and reasoning would show that the isolation of the length as a quantity of theoretical importance was quite independent of any specific theoretical interpretation of it—independent, e.g., of the interpretation Newton was led to make.) Further, we have an equally remarkable theoretical *conclusion* from the determination of this attribute—a conclusion that itself remains independent of a full theoretical "explanation," or understanding of the "nature," of light or of this attribute: namely, the absolute limit to the size of objects that can be seen by their reflection of (ordinary) light.

I have said at the beginning of **MMN** that E. A. Burtt was right on one point: that for a proper understanding of Newton's general remarks about method, it is necessary to study their exemplification in his work. The example I have just cited seems to me one of the most illuminating of all. I hope it may serve here both to clarify the principles of method I have been discussing, and to show how intimately and deeply they are connected with Newton's essential scientific achievements.

In **MMN** I remarked that Newton writes with a precision that exceeds that of most readers (which is, of course, not to say that he writes with the precision of a computer program); and that he has suffered from imprecise readers (who impute to him confusions based on their own misreadings). The first quoted sentence above mentions "the two sorts of rays (a simple and compound green)." But Newton never distinguishes rays into "two sorts—simple and compound." He distinguishes *light* into two main sorts, homogeneous and heterogeneous (which, on general principles of linguistic synonymy, could perfectly well be called "simple" and "compound," and which Newton *does* call "similar" or "uniform," and "difform"); the former of these (or any species thereof—"species" here in its classificatory sense) consists of rays that are *all alike,* the latter of rays that *differ* (in essential respects). Thus, when Newton says "I told you, that Light is not similar, or homogeneal, but consists of *difform* Rays," he does not expect his reader to conclude that each ray constituting light is "difform"—any more than when he earlier said that *"Light* consists of *Rays differently refrangible,"* he expected his readers to conclude that each ray of such light has several refrangibilities. The basic constituent, or element, of light, in the terminology of his paper, as in that of all his subsequent optical writings, is the ray; and the discovery he is communicating is that there are different kinds of rays, each with its own distinctive essential, or intrinsic, or—in his terminology—*"Original* and *connate properties."* In the first part of the paper (as I have already pointed out) Newton describes the isolation of these distinct kinds of light (i.e., of distinct *homogeneous* lights, each consisting of *rays of the same kind*); these having been separated by refraction, and thus distinguished by their differing refrangibility. In the second part of the paper—and again I quote Newton's own very clear words—he describes "another . . . difformity in [the] Rays, wherein the *Origin of Colours* is infolded." Again, the "difformity"—or heterogeneity—is a difference *among* the *rays*, or elements, of light; and it turns out from Newton's first three propositions of the second part that the property of the rays that "infolds the *Origin of Colours*" is strictly and indissolubly correlated with their refrangibility.

To return, then, to the comments under discussion: the phrase "a simple and compound green" just cannot, in the light of Newton's first paper—never mind his later explanations to Hooke and Huygens—stand correctly in apposition to "the two sorts of rays"; a *sort of ray* with (a disposition to exhibit) a *color that is a compound green* is, in Newton's clearly (if not formally) defined terminology, a self-contradictory notion. (This will be discussed more fully below.) Now, as to the next point, concerning the etymology and meaning of the word "species," of course I am aware of its use for "sort" or "kind." The question is, how does Newton use that word in his discussion of light and colors? The *OED* is cited as indicating that the phrase "in specie" was used only in the latter sense. It may be relevant to note here that Newton *italicizes* that expression; one might take this as a sign that he was expressly using the phrase *as Latin,* in a sense not usual in English. The point is a bit refined, and I should not wish to rest my case upon it; but it can perhaps serve as auxiliary support for that case—which is based upon the wider context of Newton's words. Here are the passages, in the "New Theory," in which the term "species" or the phrase *"in specie"* (or "in *Specie* ") occurs; first, Proposition 3:

> 3. The species of colour, and degree of Refrangibility proper to any particular sort of Rays, is not mutable by Refraction, nor by Reflection from natural bodies, nor by any other cause, that I could yet observe. When any one sort of Rays hath been well parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it. I have refracted it with Prismes and reflected it with Bodies, which in Day-light were of other colours; I have intercepted it with the coloured film of Air interceding two compressed plates of glass; transmitted it through coloured Mediums, and through Mediums irradiated with other sorts of Rays, and diversly terminated it, and yet could never produce any new colour out of it. It would by contracting or dilating become more brisk, or faint, and by the loss of many Rays, in some cases very obscure and dark; but I could never see it changed *in specie.*

There is one further occurrence; but first let us consider this passage. In the first sentence, Newton mentions two things: the species of color of the rays of any particular sort, and the degree of refrangibility of those rays. He somewhat oddly uses the singular of the copula in making a predication about the conjunction of these two—presumably because he has already asserted, in Proposition 2, their indissoluble connection: "And this Analogy 'twixt colours, and refrangibility, is very precise and strict; The Rays always either exactly agreeing in both, or proportionally disagreeing in both." In the rest of the passage, the emphasis is entirely upon color (again, presumably, we are to understand that immutability of color through the various operations he details is associated with immutable refrangibility as well). Note that the "species" of color is characterized as proper to a particular "sort" of rays. But so far, "species" might just mean "sort"—of color on the one hand, of rays on the other (the two associated firmly). Indeed, there is no harm in here reading "species of color" to mean "sort of color" (understanding, as we ordinarily do, the property of color itself to be one of visual appearance). Now, however, consider the last sentence—in which, in contrast to the first, the word at issue is italicized in the original text. Newton here says that "it"—namely, "any one sort of Rays . . . well parted from those of other kinds": i.e., any homogeneous light—"would by contracting or dilating become more brisk, or faint, and . . . in some cases very obscure and dark"; but that he "could never *see* it changed *in specie.*" What could this possibly be taken to mean, except that the light, although changed in brightness, was never changed in *visual quality* ? Newton in fact says, in two consecutive sentences—of which the second qualifies the first, and makes the statement more precise—that out of such homogeneous light he "could never produce any new colour," and that he "could never see it [N.B.: the *light*—not the color] changed *in specie.*" From this it follows that he is using the last term as, in effect, synonymous with "in color"—again in the ordinary sense of color, as visual appearance. (It ought to be remarked that the quoted comment is not quite correct in saying that this means "hue," if the latter word is used in the technical sense it now bears; what is unchanged when homogeneous light merely undergoes changes in brightness is the two-dimensional quality later represented in Newton's planar color-diagram: the conjunction of hue and saturation.)

Perhaps this still seems overrefined and inconclusive. The objection goes on to say: "That you have to introduce the concept of appearance or hue . . . shows that Newton did change his definition of simple and compound colors." This seems to suggest that I have imported a concept, or a distinction, that Newton introduced only later. Yet the next sentence identifies this "change of definitions" with "a realization that his concepts of simple and compound colors were defined in terms of *refrangibility* and *not* visual appearance or hue." Since I, on the contrary, believe that Newton's first exposition of his "New Theory" already contains the full array of notions that he elaborates in more detail in his replies to Pardies, Hooke, and Huygens, I do not see how my use of one of those notions—and, indeed, that precise one that the objection claims to have been basic to the first account—can "show that Newton did change his definitions." Setting this aside, however, I may still seem, to those who agree with the quoted objection, simply to be asserting without cogent proof that Newton meant what I say he did. But what can the alternative be? "Newton," the objection maintains, "came to realize that his concepts of simple and compound colors were defined in terms of refrangibility and not visual appearance or hue." We must therefore consider how Newton characterizes these latter concepts in his letter of February 6.

Note first that the passage from that letter considered above does not concern the distinction of simple and compound colors (this is introduced in Proposition 5); and it surely does involve a conceptual distinction between—but with a "very precise and strict" correlation ("Analogy") between—color, or species of color, and refrangibility. This, at least, ought not to be controversial: two distinct properties of the rays of light have been identified, their refrangibility and their colorific disposition; the *Experimentum Crucis* has shown light to be difform in point of the first of these; further experiments show that this is strictly parallel to a second difformity, with respect to the second.

I have said that the concepts of simple and compound color are introduced in Proposition 5; but this refers back to Proposition 4, as providing the basis of the distinction between them. Proposition 4 itself comments upon, and qualifies, the statement in Proposition 3 that colors are not mutable "by any [cause] that [Newton] could yet observe":

> 4. Yet seeming transmutations of Colours may be made where there is any mixture of divers sorts of Rays. For in such mixtures, the component colours appear not, but, by their mutual allaying each other, constitute a midling colour. And therefore, if by refraction, or any other of the aforesaid causes, the difform Rays, latent in such a mixture, be separated,

there shall emerge colours different from the colour of the composition. Which colours are not New generated, but only made Apparent by being parted; for if they be again intirely mix't and blended together, they will again compose that colour which they did before separation. And for the same reason, Transmutations made by the convening of divers colours are not real; for when the difform Rays are again severed, they will exhibit the very same colours, which they did before they entered the composition; as you see, *Blew* and *Yellow* powders, when finely mixed, appear to the naked eye *Green,* and yet the colours of the Component corpuscles are not thereby really transmuted, but only blended. For when viewed with a good Microscope, they still appear *Blew* and *Yellow* interspersedly.

5. There are therefore two sorts of colours. The one original and simple, the other compounded of these. The Original or primary colours are, *Red, Yellow, Green, Blew,* and a *Violet-purple,* together with Orange, Indico, and an indefinite variety of Intermediate gradations.

And then, in the first sentence of Proposition 6, there occurs the remaining instance of the phrase "in specie":

6. The same colours in *Specie* with these Primary ones may also be produced by composition: For, a mixture of *Yellow* and *Blew* makes *Green;* of *Red* and *Yellow* makes *Orange;* of *Orange* and *Yellowish green* makes *yellow.*

In a passage I have criticized in **MMN**, Alan Shapiro quotes the first sentence of Proposition 3, and says that Newton has not defined—I take this to mean, not just *has not stated a formal definition,* but *has not made sufficiently clear*—"particular sorts of rays"; adding, "both greens, for example [that is, the "original and simple" green mentioned in Proposition 5, and the green produced by composition, mentioned in Proposition 6] could be called 'particular sorts of rays,' since, as he just told us, their color is the same." The objection I am now discussing was offered as a defense of Shapiro's position. But I am at a loss how to understand this. How could Newton at the same time have thought, as the objection holds, that the distinction between simple and compound colors was "*defined in terms of . . . hue,* and have stated *explicitly* in Proposition 6 that the same colors—or the same "in *Specie* "—as the primary ones could be "produced by composition"? Setting quite aside my own technical interpretation of the phrase *in specie* as meaning "in visual appearance," I do not see how anyone—let alone Newton—could think he had made a distinction, on the basis of visual appearance alone, between (in the terms Shapiro here uses) colors that are "the same."

But it seems to me that the case is perfectly clear from Newton's text, with no talmudic straining. The distinction *announced* in Proposition 5 has in effect already been *used*—the very *words* have been used—in Propositions 1 and 4. Proposition 4 refers to "seeming transmutations of Colours" that "may be made where there is an *mixture* of divers sorts of Rays"; to the "component colours" of such a mixture; and to "the colour of the composition"—thus the notion of "compounded" colors is plainly adumbrated here. And Proposition 1 speaks of the colors *of the rays of light*—more precisely, of "their disposition to exhibit this or that particular colour"—as *Original* and *connate properties,* which in divers Rays are divers." Thus we have the following *explicitly* in Newton's paper: (1) Light consists of difform rays, with different refrangibilities. (2) Rays of different refrangibility differ also in their dispositions to exhibit color. (3) These colors (dispositions) are "original and connate" properties of the rays. (4) The association of color and refrangibility is "very precise and strict"—that is, one-to-one: "to the same refrangibility ever the same color, and to the same color ever the same refrangibility" (this is in fact not strictly true). (5) The colors and refrangibilities of "rays of a particular sort" are not mutable. (6) "Seeming transmutations" may be made, by composition (mixture) or separation of "divers sorts of Rays." (Note that "seeming"—i.e., "apparent"—may here be construed to mean both "in appearance to the senses," and "*merely* apparent"—i.e., not [physically] genuine; but this subtlety is not crucial to the case.) (7) From all this, Newton says it follows that—"*therefore*"—there are two sorts of colors: original and simple (or "primary") on the one hand; compounded on the other. (8) The former and the latter sorts can be (*at least* visually—but, I would argue, also at most visually) the same.

I believe that if all this is considered in the way one ought to read a scientific treatise (indeed, in the way one ought to read any sort of exposition)—namely, not in the way one reads a computer program or a legal contract (looking for possible "bugs" or "loopholes"), but with the aim of understanding the view being propounded—one will come to the following conclusions: (1) What Newton calls "original" colors are, taken as properties of the rays themselves, those "dispositions to exhibit" color that he says are "*Original* and *connate properties*" of the rays; and, in a derivative sense, the visual qualities those rays are "disposed to exhibit." The same original color is constantly associated with rays of the same refrangibility, different ones with rays of different refrangibilities (see Newton's Proposition 1). (2) What he calls "compounded" colors are those produced by "mixtures of divers sorts of Rays." In this phrase, "divers sorts of Rays" can only mean the same thing as "difform Rays"—namely, such as possess *different "original and connate" properties.* (It is not only the dispositions to exhibit color that are "original and connate," although it is only these properties that Newton explicitly singles out as such. The phrase means "belonging by nature," "intrinsic"—we should remember that for Newton the semantical connection between "origin," i.e. "beginning or source," and Greek *ἀρχή* ["principle"], was a quite live one. Thus, the refrangibilities of the rays are also original and connate—as are any analogous properties they may turn out to have: for instance, their different "reflexibilities" [mentioned by

Newton in his first reply to Huygens], or the characteristic length assigned to them by Newton from his investigation of the "coloured film of Air interceding two compressed plates of glass" [the phenomenon mentioned in Proposition 3, the result communicated only five years later].) (3) According to Proposition 6, "original" and "compounded" colors *cannot* always be distinguished from one another by the eye. According to Proposition 4—which provides the immediate basis for the distinction introduced, with a "therefore," in Proposition 5—"in mixtures *the component colours appear not"* (another clear indication that the conceptual distinction of simple from compound is not based upon visual appearance); but these component colors can be *"made Apparent by being parted"* (namely, "by refraction, *or any other of the aforesaid causes"*—the most germane [see Proposition 3] is "[interception by] the coloured film of Air interceding two compressed plates of glass"; but reflection by ordinary colored bodies [ibid.] is also pertinent). Therefore (4) *such decomposability is the criterion of compositeness.* This differs from the "later definitions" offered to Hooke and Huygens only by being more general. Moreover, the generality is perfectly well justified: since the basic notion of "rays of the same sort" is that of rays *agreeing completely in their intrinsic properties,* separability by any means whatever—resulting in lights having distinguishable intrinsic properties—is a legitimate criterion of compositeness. On the other hand, the narrower criterion offered to Hooke and to Huygens provides a more effective operational criterion; and it, too, is legitimate, given Newton's conclusion that refrangibility—although not the only intrinsic property distinguishing the rays—is connected by a "precise and strict analogy" with the other such properties. (Note that the breakdown I have referred to in the one-to-one correlation of refrangibility with color does not invalidate this point; for refrangibility proves to be a *finer* criterion than visual appearance: the relation of the former to the latter is many-to-one.)[54]

Just one terminological point in the original letter of February 6 is a little obscure: namely, Newton's use of the word "color" itself. Or rather, I should say, that use is not genuinely obscure, but it

[54]There are really two points involved in this correction to Newton. First, there is the matter of perceptual thresholds: physically, there is a *continuum* of kinds of homogeneous light, and a corresponding continuum of refrangibilities; but of course the eye and brain are not capable of actually distinguishing infinitely many color-qualities. Second, it appears, from the closer study of the perceptual relationships made towards the end of the nineteenth century by Helmholtz's student Arthur König, that at each end of the visible spectrum there is an entire segment over which hue and saturation are constant—presumably because, in each of these regions, only one of the three basic absorptive processes responsible for visual sensation continues to occur. (On this second point, see the color chart due to König and Conrad Dieterici, reproduced in the second [German] edition of Helmholtz's *Handbuch*—Hermann von Helmholtz, *Handbuch der Physiologischen Optik* [2nd ed., rev.; Hamburg, 1896], p. 340—and the remark there made: "Die Farben jenseits *B* im äußersten Roth und jenseits *G* im Violet fallen in die Endpunkte der Curve zusammen."

is ambiguous (the ambiguity being fairly transparent). I have said that Newton does not call the simple and the composite green "the same color," but only "the same in *Specie*." This is strictly correct (I have quoted the full text already). But in the earlier propositions Newton refers to "color" *simpliciter;* and he pretty clearly uses the term in several senses (indeed, just the same senses that he so carefully distinguishes in the "Definition" I have quoted, in **MMN,** from Book I Part II of the *Opticks*). But ambiguity is not the same as confusion. If one reads the word "color," wherever it is used either of *rays,* or more generally of *light* (whether simple or composite), to mean a "disposition to exhibit" some "particular color"; if one takes the word in this last phrase—referring as it does to that which is "exhibited" (or, from the other end, "perceived")—to mean the visual impression produced (on my technical reading, the *species*); and if one then distinguishes the "dispositions" of homogeneous and heterogeneous lights (which may be dispositions to exhibit the same *species*) as themselves "original, primary, and simple" dispositions (that is, intrinsic properties of elementary physical constituents of light) or "composite" (that is, dispositions resulting from the combined effect—the "mutual allaying"—of diverse elementary constituents); then Newton's entire discussion is perfectly coherent. Since these are really just the distinctions of usage Newton eventually makes explicit (the most crucial one just four months later, in his reply to Hooke) it seems to me simply perverse to read Newton's responses to Hooke and Huygens as signalizing a change in the foundations, or a significant revision, or indeed anything more than a more fully explicit *exposition* of his original theory.

§5. The deepest substantive issue about Newton's theory and its status to arise from discussion at the symposium is connected with a view that has been put forward and defended at some length by Alan Shapiro: namely, that although the "New Theory" did indeed establish the compositeness of sunlight "with respect to refrangibility," Newton failed to "prove" adequately—not only in the "New Theory," but *ever*—that sunlight is "compounded of all the spectral colors." It is of course clear that the disputed proposition does follow from the compositeness of sunlight "with respect to refrangibility" *conjoined with* the proposition that color and refrangibility are rigidly associated. But it is Shapiro's view that Newton never succeeded in proving the latter proposition. In his paper "The Evolving Structure of Newton's Theory of White Light and Color," Shapiro writes as follows:

> The problem of establishing the innateness and immutability of color is altogether different from that of refrangibility: first, Newton had no mathematical law to describe color changes; and second, the color of the sun's incident light appears totally different before the first refraction and ever after, once it has been resolved into colors. As Newton himself was ultimately to recognize, it is empirically impossible to prove what may be called the strong principle of color immutability for the colors of the sun's light at the first refraction, since the colors are not perceptible before the first refraction and so may not be compared

> with the colors after that refraction to see if they have changed. Indeed, if they are compared, it seems as if they have changed. The weak principle, establishing color immutability after the first refraction, can be readily proved; it is this weak principle that Newton demonstrated above in Proposition 3. The principle of color immutability appealed to Newton because he hoped to use it to prove that the colors are innate to the sun's direct light. . . . The principle of color immutability is an important one, incorporated into modern optics; it substantially improved Newton's theory, but its proof, alas, eluded him. While three centuries later it may seem obvious that the principle of color immutability cannot be empirically proved, it is, as we shall see, not such a simple matter. Newton carefully probed the problem and sought various alternative formulations and indirect proofs before he recognized the futility of his efforts and abandoned it altogether in the *Opticks.* And so he was never able to prove, at least according to his own rigorous standards, that the sun's light is compounded.[55]

By the last remark, I take it, Shapiro means to say that Newton failed to prove, according to his own rigorous standards, that sunlight is "compounded of all the simple colors"—since his paper explicitly focuses on the theory of color, "leaving the problem of unequal refrangibility as much as possible in the background"; and adds: "Newton himself never doubted that he had successfully demonstrated that rays of unequal refrangibility are innate to the sun's light, nor in demonstrating this did he encounter any particular intractable conceptual problems as he did with color. . . . The validity of the sine law of refraction and the immutability of degree of refrangibility can then in principle be empirically confirmed or rejected by a series of measurements of the angles of incidence and refraction."[56]

There is something odd about the statement that Newton eventually realized the futility of his efforts to "prove" the principle of immutability, and abandoned the attempt in the *Opticks;* for later in the paper, Shapiro says: "Innateness was not dropped from the *Opticks,* just the attempt to prove it; likewise immutability remained, but it was demonstrated only in the weak form for homogeneous light. Newton, it seems, finally recognized that he could not rigorously demonstrate innateness."[57] But the principles in question, immutability and innateness, have their place in the *Opticks* in Book I, and are therefore covered by Newton's general statement of aim at the beginning of Book I: "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."[58] What then are we to conclude? Is it that Newton failed—and knew he had

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[55]Shapiro, *Isis* **71** (1980), pp. 215-216.

[56]Ibid., pp. 214-215.

[57]Ibid., p. 232.

[58]*Opticks,* Dover ed., p. 1.

failed—to accomplish his design? And in that case, was he deliberately prevaricating by stating what his design was, allowing his readers to suppose that the book *accomplished* its design, while he himself was aware that it did not? This is a supposition quite compatible with the very dark view of Newton's character that has come to be widely shared among historians (a subject I shall return to at the end). Yet against it there stands some evidence I have presented above, suggesting that although Newton sometimes (I emphasize: in my opinion, not often) underestimated the problematic character of his conclusions, he was on the whole straightforward in acknowledging such a character where he perceived it.[59] It seems to me, therefore, that it would be more reasonable—and certainly more charitable—to suppose that Newton himself thought the "proofs" presented in the *Opticks* satisfactory.[60]

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[59]A notable case is the very important Proposition VI of *Opticks,* Book I, Part I, asserting the strict adherence to Snell's law of each kind of homogeneous light. Newton refers—with little detail—to some measurements and rough calculations that, he claims, seem to confirm the correctness of the law of the constancy of the ratio of sines for each kind of homogeneous light (for "every Ray considered apart," as he puts it—*Opticks,* Dover ed., p. 75). That this experimental "proof" was less than adequate he implies in his summary of the state of affairs; for referring to what he had determined by measurement he says: "from [this] the Proportions of the Sines being derived, they come out equal, so far as by viewing the Spectrums, and using some mathematical Reasoning I could estimate. For I did not make an accurate Computation" (ibid., p. 79). He then proceeds to show that the law in question will indeed hold strictly, *upon a certain hypothesis* (namely, the assumption about refraction that is characteristic of the corpuscular theory of light). Again, that this is indeed a hypothesis—a *supposition*—is stated explicitly by Newton: "so then the Proposition holds true in every Ray apart, so far as appears by Experiment. And that it is accurately true, may be demonstrated upon this Supposition. *That Bodies refract Light by acting upon its Rays in Lines perpendicular to their Surfaces.*" Newton here, then, disguises neither the shortcomings of his experimental "proof," nor the hypothetical basis of his theoretical demonstration.

[60]Shapiro bases his conclusion that Newton realized his failure on the fact that his unpublished papers contain a number of more or less elaborate arguments that he failed to invoke in the *Opticks* (*Isis* **71** [1980], pp. 230-233). I think anyone who has gone through the process of developing a systematic exposition of a mathematical or scientific theory will recognize that experimenting with alternative forms of argument is quite normal, and that the abandonment of one or another form need not imply its incorrectness. In the present case—where the propositions had met with strong resistance—such attempts to find the clearest and most persuasive line of reasoning are all the more understandable. It is therefore entirely plausible that Newton simply concluded that his more elaborate arguments were in principle unnecessary, and in practice unlikely to be more persuasive than

But this is incidental to the main issue, which, here, is not what Newton thought about his own success, but what we ought to think. Shapiro says that "the principle of color immutability is an important one, incorporated into modern optics"; but he also says that this principle "cannot be empirically proved." The question obviously arises, how a principle that cannot be "empirically proved" obtains its standing in modern optics; and how close Newton came to establishing for it whatever the latter kind of standing is. The only clue I have found to Shapiro's view is afforded by the following passage:

> When he turned to composing the *Opticks* Newton was still confronted with the problem of establishing that colors are immutable and thereby that they are innate to the sun's direct light. Though this proof ultimately eluded him, it was not for want of trying. From his view of color mixing and color vision—namely, the rays of light, like colored powders, do not interact with one another, only their sensations do—we can appreciate why the principle of immutability seemed almost axiomatic to Newton. Undoubtedly this was buttressed by, or perhaps even derived from, his corpuscular view of light. If it were assumed that various light corpuscles or atoms according to their size or other innate properties corresponded to different colored rays, and these did not interact with one another, then the light corpuscles, or rays, would unalterably maintain their properties through all mixing, reflecting, or refracting. But to establish immutability and innateness in this way, while perhaps tempting, would violate Newton's prohibition against hypothetical physics . . . .[61]

Then, after reviewing a series of arguments sketched by Newton in manuscripts preparatory to the *Opticks* but not included in the latter work, he continues:

> After a thirty-year quest, Newton finally abandoned his attempt to prove, as a "most rigid consequence," that colors are innate to the sun's light. Nonetheless, following some initial resistance the theory was widely accepted, and we should briefly consider the reasons why it was adopted. I think Newton's own early assessment of the resistance to his theory is in large part the true one: the paradox of considering the apparently pure, simple, and homogeneous white light of the sun to be a heterogeneous mixture of an indefinite number of colors. The other major stumbling block to the acceptance of his theory was a failure to appreciate the significance of his discovery of unequal refrangibility. Once the

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those he presented in the published work; it in no way follows that he regarded the latter as insufficient.

[61]Ibid., p. 230.

former prejudice was overcome and the latter discovery acknowledged, there was little choice or difficulty for a mechanical philosopher but to accept that colors were innate to the sun's direct light. Moreover, Newton's theory of the color of natural bodies . . . also reinforced the acceptance of innateness. Finally, since white light could be decomposed into irresolvable colors and in turn be compounded from those colors, then by the traditions of Western natural philosophy the colors, and not white, ought to be considered elementary and simple.[62]

These passages seem to refer to three kinds of basis that might support the principle in question. The first Shapiro calls "hypothetical physics": in effect, the introduction of assumptions from which the principle follows. Shapiro remarks that "to establish immutability and innateness in this way . . . would violate Newton's prohibitions against hypothetical physics." But the situation is far worse than that. A once-celebrated parody review of various mathematical methods, applied to the problem of "hunting lions in Africa" as a paradigm, began with the Hilbert, or axiomatic, method: "Axiom 1. There is a lion in the veldt. Axiom 2. If there is a lion in the veldt, there is a lion in the cage." In short: unless one has some satisfactory *grounds* for the assumptions introduced, one has—quite apart from Newton's special views—established nothing. "Hypothetical physics" is not a way of establishing propositions; it is only a way of reducing the problem of grounding those propositions to the problem of grounding the hypotheses. (Note that Huygens's hypothetico-deductive mode of argument is something quite different: Huygens says that it is the *hypotheses* that are to be established, by *empirical evidence for their consequences.*)

The second kind of basis Shapiro suggests is the removal of stumbling blocks and overcoming of prejudices. That clearly was important, since, as we have seen, the stumbling blocks and prejudices did exist; but their removal could suffice at most to gain for Newton's views an *unprejudiced hearing.* Shapiro says that once the prejudice about white light was overcome and the discovery of unequal refrangibility acknowledged, "there was little choice or difficulty for a mechanical philosopher but to accept that colors were innate to the sun's direct light." It is somewhat hard to know how to take this. The phrase "little choice or difficulty but to accept" is unhappy; but it must mean "little difficulty in the way of accepting, and—further—little choice but to accept." The latter comes pretty close, after all, to an acknowledgment that Newton had succeeded in "proving" his principle. But there remains the qualifying phrase, "for a mechanical philosopher"; and this is a restriction that Newton would not have happily admitted, since he thought his results independent of the "mechanical philosophy" itself. Nor does the restriction comport with our own wish to understand how *we* should view Newton's evidence for his principle—since we no longer hold the "mechanical philosophy" of the seventeenth

[62]Ibid., pp. 233-234.

(or even that of the nineteenth) century. Beyond this, I myself do not understand the basis for Shapiro's assertion. If the empirical evidence (and the arguments from that evidence) offered by Newton did not suffice as grounds for his principle, what was it in the "mechanical philosophy" that, added to that evidence and those arguments, left one little choice but to accept that principle?

Shapiro's last suggested basis is extremely general: "Finally, since white light could be decomposed into irresolvable colors and in turn could be compounded from those colors, then *by the traditions of Western natural philosophy* the colors, and not the white, ought to be considered elementary and simple." Again, this is a little hard to interpret. The phrase I have emphasized suggests a small obeisance to cultural relativism, but perhaps that issue may be waived for present purposes. The more serious trouble is that the explanatory clause—that governed by "since"—is seriously inadequate, and that the argument, taken at face value, is vitiated by an *ignoratio elenchi.* The explanatory clause certainly departs from Newton's terminology. To repeat an old point: Newton would never have said that white *light* can be decomposed into, and compounded from, *colors;* rather, white light is decomposable into, and can be compounded from, homogeneous (and thus "irresolvable") *lights,* each of which has its characteristic (and thus in a *derivative* sense "irresolvable") color. This, then, is conceded to have been established *before* the appeal to "the traditions of Western natural philosophy"; and the latter are said to license the further conclusion that "the colors, and not the white, ought to be considered elementary and simple." But the issue—after the prejudice that sunlight is "pure, simple, and homogeneous" in physical constitution (as it is in appearance) had been overcome—was not that. The issue, in Shapiro's words, was to establish "that colors are immutable and thereby that they are innate to the sun's direct light"; where, by the phrase "innate to," I take it that he means "inherent—or present—in." How does the proposition that the colors—or the homogeneous lights—are "elementary and simple" show that those colors are *present in* sunlight (in any other sense than that the homogeneous lights whose colors they are compose sunlight); or that those colors are *immutable?* (Remember that no one who held white light itself to be "pure, simple, and homogeneous" thought that white was immutable—on the contrary, the received view was that colors are *modifications* of white.)

It seems to me, then, that Shapiro has failed to present a convincing analysis of what Newton had and had not achieved, in respect of his doctrine about white light and colors, or of the reasons why "following some initial resistance the theory was widely accepted." Before reconsidering what I have called the main issue here—what we ought to think about Newton's success—I want to call attention to one special peculiarity of Shapiro's view that I have not so far commented on. In trying to understand (what I take to be) his view that Newton had "proved" his claim about "the composition of sunlight with respect to refrangibility," but had not proved his claim that "sunlight is compounded of all the simple spectral colors," I have found that I could only explain this difference to myself by placing stress upon the question whether *the colors themselves* are actually *present in the white;* and by trying then to base an objection upon the circumstance that there is no way actually to observe, *in* the sun's direct light, the colors of the alleged "colored constituents." This conjecture seems to be supported, for instance, by Shapiro's statement (already quoted) that "it is empirically impossible to prove what may be called the strong principle of color immutability for the colors of the sun's light at the first refraction, since the colors are not perceptible before the first refraction and so may not be compared with the colors after that refraction to see if they have changed."

Now, it should be observed that this kind of argument is susceptible of extraordinarily wide application. It can be used to cast doubt upon what, in ordinary usage, would be described as observable, in any circumstances that exclude actual observation. This last consideration made it possible for Berkeley to use the argument in question to deny the reality of any perceptual object whatever, except at such times as such objects are in fact being perceived (since the circumstance of not being perceived *ipso facto* excludes observation). I rather doubt that Shapiro intends his argument to be carried to such an extreme; and I think we may reasonably set aside what may be regarded as philosophical extravagances here, and restrict the scope of that argument to what might be called "observability as conceived by common sense." The trouble is, I do not see how this can be done in a way that continues to exclude the "colors in the sunlight" as beyond empirical detection, but that admits the detectability of the compositeness of sunlight "with respect to refrangibility."

What was the nature of Newton's "proof"—indeed, *"demonstration"*—by means of the crucial experiment, that sunlight is heterogeneous? The answer we have already seen: he produced, "at the first refraction," lights that had a wider range of directions than is compatible with Snell's law; and then, isolating from among these the light propagated in a particular direction, he showed that in its subsequent behavior that light is refracted without further dispersion, and each such light with its own distinct refrangibility (compatible with the initial deviation of the part of the beam that yielded it). It is of course also true that the lights so produced "exhibited" diverse colors; and that, according to the many experiments whose results Newton summarizes in the second part of his paper, these in their subsequent behavior continue always to "exhibit" a fixed color, the same as that with which they emerged from the prism (just as they continue to manifest a fixed refrangibility, corresponding to the direction in which they emerged). Are these considerations not strictly parallel? Are there convincing grounds for rejecting Newton's argument about color while accepting his argument about refrangibility? If it is alleged against the former that the diverse colors are not perceptible until after the emergence of the light from the first prism, and that we therefore have no direct assurance that the colors were not *created*—rather than merely separated out—by that prism, are we not equally free to suppose that the first prism, in *disturbing* and *breaking* the incident light, created new kinds of light not actually present in the direct light of the sun? Shapiro bases his assertion that the two cases are "altogether different" upon two considerations: "first, Newton had no mathematical law to describe color changes; and second, the color of the sun's incident light appears totally different before the first refraction and ever after, once it has been resolved into colors." It is not clear how the possession of a mathematical law affects the matter (and Newton did—eventually—obtain such a law, in the form of his barycentric calculus of colors); and as to the second point, it is also true that the sun's incident light behaves quite differently before the first refraction (at which dispersion occurs) and ever after, once it has been resolved into lights homogeneous "with respect to refrangibility."

I am left, then, with this diagnosis of Shapiro's position: Refrangibility is a property he *does not expect* to be directly observable—it is detected only in the *behavior* of light; and even at the first prism itself the sun's light is variously refracted (although one doesn't directly see whether that is the result of a "change" or a mere "separation of pre-existing parts"). Color on the other hand is a property he does think of as directly observable, and it is not observed in the sunlight. But this way of thinking misses one of Newton's most acute points. I have documented over and over his emphasis—beginning in the "New Theory" paper itself—upon the *dispositional* character of the "colors" (more properly: the "colorific properties") of light. It ought to be clearly understood that one does not, strictly speaking—but in the ordinary sense of the word 'see'—*see light.* The night sky, for example, is dark, although in any direction one looks it is (sufficiently high up) traversed continually by sunlight—as one "sees," for example, when the moon happens to cross the region towards which one is looking. The visual experience of "seeing" occurs *when light impinges upon the retina.* This we ordinarily call *seeing the object from which the light comes* (whether by reflection or by emission). In special circumstances, when we know that no object is present in the place towards which which we look, we speak of a "virtual image." Whenever we do speak, in ordinary discourse, of "seeing a beam of light," we are in fact "seeing" (as I have put it above, "strictly speaking, but in the ordinary sense of the word 'see'") a series of reflecting motes organized roughly along the direction of propagation of the light. All this is of course elementary; but it is easily overlooked. Newton certainly did not overlook it. For him, the "colors present in the white" are nothing more nor less than the dispositions of the various "parts" of the light severally to produce sensations of the various spectral ("primary, original, and simple") colors. And the refrangibilities present in the sunlight are the dispositions of the various "parts" of that light to be refracted each in its own particular degree. The cases are genuinely parallel: *neither* the colorific character *nor* the refrangibility is "directly" observable; and if the one conclusion is to be called "impossible to prove empirically," and to be established only by appeal to something like "the traditions of Western natural philosophy," the other should be regarded in the same way.

§6. That is not the end of the matter; the issue having been raised, we ought to consider a little further whether Newton has satisfactorily established that the colors—and the refrangibilities as well—are "in the sunlight," not "created by the prism."

On this subject, A. I. Sabra has argued strongly in the negative.[63] Sabra's treatment is based upon a discussion of the character of natural light given by G.-L. Gouy in 1886.[64] What Gouy there argues (taking for granted, of course, the wave theory of light) is that ordinary composite light—white light, for example—is propagated in such a way that the optical state at any given point varies *irregularly* (and, in particular, aperiodically), and that the component simple harmonic oscillations (thus, the "homogeneous" constituents) are "present" only in the mathematical sense: that, by Fourier analysis, such an irregular motion can (under very general conditions) be represented as a superposition of simple harmonic oscillations. After discussing the formation of fringes of color when light so constituted is subjected to interferometry, Gouy concludes as follows:

> Thus the existence of interference fringes of great path difference, in the case of sources with continuous spectra and of white light, does not at all imply the regularity of the incident light. That regularity exists in the spectrum, but it is the spectral apparatus that produces it, in separating more or less completely the diverse simple motions which, until then, had none but a purely analytic existence
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> To consider an extreme case, by way of example, one may regard white light as formed by a series of altogether irregular impulses, or of incessantly perturbed vibrations, analogous to the motion of trepidation which, for some physicists, constitutes the calorific motion. This quite plausible hypothesis permits a simple explanation of the perfect continuity of the

[63]See A. I. Sabra, *Theories of Light from Descartes to Newton* (London: Oldbourne, 1967); reprinted, Cambridge University Press, 1981). pp. 280-282. (As we shall see, Sabra's argument is not restricted to the issue of the compositeness of white out of colors, but concerns equally the compositeness of sunlight "with respect to refrangibility.")

[64][Georges-Louis] Gouy, "Sur le mouvement lumineux," *Journal de physique théorique et appliqué*, 2nd ser., **5** (1886), pp. 354-362. Sabra cites the author as "M. Gouy"; and indeed, the heading of the paper reads: "Sur le mouvement lumineux; par M. Gouy." It should be noted that the index to the journal volume, which lists most authors by surname and initials, gives the author of this paper simply as "Gouy"; an idiosyncrasy that is repeated for many years of the journal in question—to which Gouy was a frequent contributor—and is also to be found in successive volumes of the *comptes rendu* of the Académie des Sciences. In the latter, however, from vol. 157 (1913), Gouy is listed in the index with initial "G."; and after his election as a corresponding member of the academy (session of Nov. 25 1901), one can verify from the membership list that his full prenomen was Georges-Louis. The letter "M." stands simply for "Monsieur."

spectrum, quite difficult to understand if one considers the luminous source as producing distinct series of regular vibrations.[65]

Sabra treats Gouy's remarks as a substantial vindication of Hooke against Newton:

> Now Hooke did not in 1672 endow his vibrations or waves with the property of periodicity. Nevertheless, his fundamental idea of the 'coalescence' or composition of vibrations which he envisaged in his Considerations upon Newton's theory came very near to M. Gouy's[66] representation of white light in wave terms. Gouy showed that the motion of white light can be represented as the sum or superposition of an infinite number of waves each corresponding to one of the spectrum colours. As it happens, the composed motion in this representation turns out to be a single *pulse,* such as that produced by a pistol shot or an electric spark. Using Fourier's theorem it could be shown that the Fourier analysis of such a pulse yields a continuous spectrum such as that produced by a dispersive apparatus. *Fourier analysis* is, of course, a mathematical concept; and the regularities produced by the dispersive apparatus (for example, the prism) would not exist in a real or physical sense in white light. What would exist in that sense in white light would be the pulses sent out by the luminous source at irregular intervals. The point in common between Gouy's interpretation and Hooke's idea is that, in both, the colours are considered to be generated by the prism; and in both, Newton's doctrine of the original difformity of white light has no place.[67]

It ought to be pointed out that there is a serious technical inaccuracy in this account of what Gouy "showed." It is not the case that the composed motion in Gouy's representation "turns out to be" a *single pulse;* Gouy himself, in the passage quoted, gives—as *alternatives*—an *altogether irregular series* of pulses, or a *series of vibrations that is constantly perturbed.* What Gouy showed is that there is a much wider range of possibilities for the detailed constitution of white light than previous superficial consideration had supposed; but "a single pulse" is not among these.

This error is not merely incidental. Sabra quotes the following passage from Newton's response to Hooke—a passage in which Newton seeks to indicate how, if wave (or pressure) hypotheses can be made compatible with the rectilinear propagation of light, the doctrine of his New Theory should not be seen as inimical to them:

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[65]See the paper cited in n. 64 above, p. 362.

[66]Cf. n. 64 above.

[67]Sabra, *Theories of Light,* p. 280.

> But yet if any man can think it possible [that a pressure or motion in a medium may be propagated in straight lines], he must at least allow that those motions or endeavours to motion caused in ye Æther by the severall parts of any lucid body wch differ in size figure & agitation, must necessarily be unequall. Which is enough to denominate light an aggregate of difform rays according to any of these Hypotheses. And if those originall inequalities may suffice to difference the rays in colour & refrangibility, I see no reason why they that adhere to any of those Hypotheses, should seek for other causes of these effects, unlesse (to use Mr Hook's argument) they will multiply entities without necessity.

He then proceeds as follows:

> Thus, for Newton, the original difformity of white light should be preserved in any case. Hooke, on the other hand, was inclined towards a different view. Being convinced, as we have seen, of the uniformity (homogeneity, simplicity) of white light, he would rather have the various waves unite *and compose one undifferentiated motion, a pulse.*[68]

The single "undifferentiated" pulse is essential here: it represents the "uniformity" attributed by Hooke to white light. But that is entirely incompatible with Gouy's analysis. It is in fact true that a single pulse will have a continuous Fourier spectrum; but it cannot represent a beam of white light, for the obvious reason that it is not a steady but a transient process. A *series* of pulses can indeed represent white light; but then, Gouy makes clear, only if those pulses are *"tout á fait irregulières."* So Sabra's rescue of Hooke's positive theory turns crucially upon what may look at first like a trivial verbal slip, but is in fact a fundamental misunderstanding.

As to the alleged similarity of Gouy's discussion of white light to Hooke's estimate of Newton's theory as "unproved," the issue is more subtle—but in my own opinion, equally unfavorable to Sabra's interpretation. Sabra summarizes his view lucidly: "The point in common between Gouy's interpretation and Hooke's idea is that, in both, the colors are considered to be generated by the prism; and in both, Newton's doctrine of the original difformity of white light has no place."[69] He proceeds to quote further from Newton on Hooke, and to draw conclusions:

> . . . though I can easily imagin how unlike motions may crosse one another, yet I cannot well conceive how they should coalesce into one *uniforme* motion [the pulse], & then part again

[68]Ibid., pp. 279-280 (my emphasis).

[69]Ibid., p. 281.

> [by refraction] & recover their former unlikenesse; notwithstanding that I conjecture the ways by wch Mr Hook may endeavour to explain it.

That from Newton. Sabra continues:

> This passage should leave no doubt as to Newton's conception of white light: even if it were possible to explain the properties of light in wave terms (as he was willing to assume for a while), it must be allowed that the unlike motions, the waves corresponding to the various colours, should keep their individual characteristics unaltered when they mix and compose white light. The function of the prism then consisted in simply *separating* what was already differentiated though blended, not in *generating* new characteristics.[70]

Next, Sabra proceeds to quote E. T. Whittaker to the effect that Gouy's discussion is compatible with Newton's theory; and to reject Whittaker's view:

> Since it is my purpose to differentiate between Gouy's interpretation (qualitatively conjectured in Hooke's idea of the coalescence of vibrations) and Newton's conception of white light, it must be observed that, from Newton's last passage just quoted, it is quite clear that he understood white light as a mixture in which the rays of various colours preserve, in Whittaker's words, 'their separate existence and identity unaltered within the compound'. Not to appreciate this would be to miss the point at issue between Newton and Hooke. To adopt Gouy's representation implies a rejection of Newton's particular understanding of the compositeness of white light.[71]

I have said that the issue here is subtle. It involves at least two more or less delicate questions: one concerning the exact content of Newton's conception of the "mixture" or "compound" that is white light; the other concerning the exact character of "Gouy's representations," and, more generally, of the best conception of the nature of white light from the point of view of modern physics—let us say (putting the quantum theory out of sight for the main part), the point of view of the electromagnetic wave theory of light. Further, we ought to reconsider, in connection with these questions, the adequacy of the empirical evidence cited by Newton to support the correctness of his conception.

As to Newton's conception: Sabra contends, on the basis of his own quotation, that for Newton, if light is a wave process, "it must be allowed that the unlike motions, the waves corresponding to the

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[70]Ibid., pp. 281-282.

[71]Ibid., p. 282.

various colours, should keep their individual characteristics unaltered when they mix and compose white light." This, however, is certainly not what Newton has said. He says—and the emphasis is his—"I cannot well conceive how they [the "unlike motions"] should coalesce into one *uniform* motion, & then part again & recover their former unlikenesse."

It is equally certain that Newton, here, is essentially right: his statement does require a little supplementation to make it rigorously correct, but in the context of the whole discussion, and of the evidence previously cited by Newton, what is missing from his formulation is the kind of thing that may reasonably be taken to be tacitly understood; indeed, that it would be *un*reasonable *not* to take as understood.

It s of course possible for "unlike" motions, superposed, to yield a "uniform" one. To be sure, the terms in quotation marks have not really been defined here; but let us say, for example: possible for two wave systems of fluctuating amplitudes and periods to combine to form one of uniform amplitude and period. By the same token, it is possible for the uniform wave to be resolved into the two systems out of which it has been composed. But then, Newton does not deny this; he even says, after the words I have just cited and at the end of Sabra's quotation, that despite his objection he is able to "conjecture the ways by wch Mr Hook may endeavour to explain" what he himself has not dismissed as impossible, but said only that he "cannot well conceive" how it should happen. Now, if a "uniform" light is broken up by the prism into unlike components—and in a definite way—the unlikeness must somehow be contributed by the prism; it is *ex hypothesi* not contained in the light (the "uniform motion"). To use a terminology introduced only in our own century, the light does not contain the *information* represented by the spectrum. The problem, therefore, for Hooke's theory, is to explain (a) how it is that, as in an important experiment described by Newton before the *Experimentum Crucis,* a prism in reversed position exactly *erases* the information contributed by the first refraction (this is the reverse of the problem posed by Newton, which was how the uniform result of the coalescence of unlike motions can yield up its "unlike" constituent parts once again), and (b), conversely (corresponding to the problem Newton posed, but in modified form), how it is that a prism applied to a light beam (whether white or colored in appearance) that has been formed by the coalescence of a number of beams of the kind Newton has identified as "homogeneal," but in which the information corresponding to the history of that genesis has been *lost,* nevertheless always produces from that incident beam "unlike" refracted ones *exactly agreeing to those out of which the incident beam was composed.* To say that it is hard to conceive (more, that one "cannot well conceive") how this should happen is surely right.

But that is not the end of it. The further evidence summarized by Newton in the second part of his paper has all tended to the result that the properties of the "homogeneal" lights obtained by refraction—in particular, both their refrangibility and their color; but, *as Newton already knew at the time* (although he did not publish his results for some years), also their characteristic "length" ("Interval of fits"; our *wave length*) in a given medium—are retained by them in all circumstances in which they are not combined with lights of a different sort. Newton further knew that "homogeneal" lights of the very same sorts as he obtained by refraction are also produced by reflection from thin films of air. In the language of "information" used in the preceding paragraph: all the optical procedures available to Newton extracted from—or contributed to—white light *exactly the same* information; from the "homogeneal" lights *no more,* and *no other,* information could be extracted—or constructed—than that represented by the specific homogeneal light itself; and from a "heterogeneous" light *exactly that information, no more and no other,* could be extracted, or made—not only by (say) a prism of a given material and form, but by *any* prism, or *any optical means whatever*—than had entered into the genesis of that light.[72] It surely seems a warranted conclusion, by "the standards of Western natural philosophy," or perhaps by the standards of any rational discourse in empirical matters, that the information contained severally in the distinct sorts of "homogeneal" light used to constitute a beam still exists—in some form or other—in the beam so composed. And if a light obtained by the "coalescence" of homogeneous beams is indistinguishable from sunlight, not only in appearance but by any physical or perceptual test, it seems warranted to conclude that sunlight too contains the information in question.

I wish to suggest that the formulation I have just given is an accurate representation, although in terms not available to Newton, of the content of his claim—I mean, expressly, of the content *intended by him* when he claimed—that the sun's light (for example—but more generally, any of his "heterogeneous" lights) *contains* the homogeneous constituents out of which it has been made, or into which it can be resolved. (However, as we shall see, the content of this claim does not exhaust the content of Newton's *theory;* there is a further crucial point to be made.)

How can this interpretation *of Newton's own intention* be established? In the first place, as I have already remarked, and contrary to what Sabra has stated, the interpretation is clearly not *contradicted* by what Newton has said—not, at least, by the passage cited by Sabra as contradicting it. (And I should argue further—and appeal to the judgment of a careful reader—that the interpretation even agrees rather well with Newton's formulation in that passage.) But there are other considerations which, in my estimation, taken all together, support my reading very strongly.

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[72]It is instructive to compare this with the case of *polarization* of light. We may take a plane-polarized beam as analogous to a "homogeneal" light. But the composition of beams polarized in different planes will yield, in general, an elliptically polarized one (or in special cases a new plane-polarized beam, polarized in yet another plane); and analysis of the latter can be made to yield still other plane-polarized beams, with still other planes of polarization. The conditions established experimentally by Newton for the composition/decomposition according to refrangibility-color-frequency (or wave length) are here not satisfied, and one correspondingly does not conclude that the variously polarized constituents are "present" in the light made from them.

As background to these considerations, a distinction made by Newton in a letter to Oldenburg of February 15 1675/6 should be borne in mind. At the session of the Royal Society of February 3 of that year, after the reading of the major part of Newton's lengthy manuscript containing both a hypothesis about light and a detailed account of the phenomena and theory of the colors of thin films,[73] a discussion occurred, reported as follows in Thomas Birch's *History of the Royal Society of London:*

> This being read, occasion was taken to discourse of Mr. Newton's theory itself, and to debate, whether the rays of light, which, though alike incident in the same medium, yet exhibit different colours, may not reasonably be said to owe that exhibition of different colours to the several degrees of the velocity of pulses, rather than, as Mr. Newton thought, to the several connate degrees of refrangibility in the rays themselves?[74]

The terms of that discussion ignored the clarification Newton had made nearly three years before, in his first reply to Huygens (dated April 3 1673): "I would not be understood as if their difference [*sc.* that of colors] consisted in the different refrangibility of those rays. For that different refrangibility conduces to their production no otherwise then by separating the rays whose qualities they are. Whence it is that the same rays exhibit the same colors when separated by any other means; as by their different reflexibility; a quality not yet discoursed of."[75] His response in the present case shows

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[73]Newton's manuscript, in two parts—"An Hypothesis explaining the Properties of Light discoursed of in my severall Papers," and "Discourse of Observations"—was sent to Oldenburg with a letter of December 7 1675—see Newton, *Correspondence,* I, pp. 362-392, which contains the complete text of the "Hypothesis" and a précis of the "Discourse"; Thomas Birch, *The History of the Royal Society of London* (London, 1756), vol. III, pp. 247-305, giving the complete texts as read to the Society with some interesting circumstances of the ensuing discussions; Newton, *Papers and Letters,* pp. 177-235, in which the cited pages of Birch are reproduced. The "Hypothesis" was read at the sessions of December 9 and 16; the "Discourse" (described in Birch as "Mr. Newton's discourse, containing such observations, as conduce to further discoveries for completing his theory of light and colours, especially as to the constitution of natural bodies, on which their colours or transparency depend") at the sessions of January 20, February 3, and February 10.

[74]Birch, *History,* p. 295; Newton, *Papers and Letters,* p. 225.

[75]Newton, *Correspondence,* I, pp. 264-265. Newton's reference to the separation of rays "by their different reflexibility" alludes to the colors of thin films (which Newton takes to show that "fits of easy reflection" occur, for different species of homogeneous light, at different intervals); he did not "disourse of" this until the occasion in 1675/6 now under discussion.

him in patient rather than irrascible mood: without so much as mentioning that he had made the distinction before, he repeats it now:

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I thank you for giving me notice of ye objection wch some have made. If I understand it right they meane that colour may proceed from the different velocities wch ætheriall pulses or rays of light may have as they come immediately from ye Sun. But if this be their meaning, they propound not an objection but an Hypothesis to explain my Theory. For ye better understanding of this I shall desire you to consider that I do not put ye different refrangibility of rays to be ye internal or essential cause of colours, but only the means whereby rays of different colours are separated. Neither do I say what is that cause, either of colour or of different refrangibility, but leave these to be explained by Hypotheses . . . .

This being apprehended, I presume you will easily see that you have not sent me an objection but only an Hypothesis to explain my Theory by. For to suppose different velocities of ye rays ye principle of colour is only to assign a cause of ye different colours wch rays are originally disposed to exhibit & do exhibit when separated by different refractions. And though this should be ye true essential cause of those different colours yet it hinders not but that the different refrangibility of ye rays may be their accidental cause by making a separation of pulses of different swiftness. Yea so far is this Hypothesis from contradicting me that if it be supposed it infers all my Theory. . . .

Were I to apply this Hypothesis to my notions I would say therefore yt ye slowest pulses being weakest are more easily turned out of ye way by any refracting superficies then ye swiftest, & so *cæteris paribus* are more refracted: and that ye Prism by refracting them more separates them from ye swiftest, & then they being freed from ye alloy of one another strike ye sence distinctly each wth their own motions apart & so beget sensations of colour different both from one another & from that wch they begat while mixed together; suppose ye swiftest ye strongest colour, red; and ye slowest ye weakest, blew.

To all this I might add concerning ye different swiftness of rays that I my self have formerly applied it to my notions in mentioning other Hypotheses, as you may see in my answer to Mr Hook sect. 4, & I think also in ye Hypothesis I lately sent you. I say I applied it in other Hypotheses; for in this of Mr Hook I think it is much more natural to suppose ye pulses equally swift & to differ only in bigness, because it is so in ye air, & ye laws of undulations are wthout doubt ye same in æther that they are in air.

> Having thus answered, as I conceive, your objection in particular; I shall now for a conclusion remind you of what I have formerly said in general to ye same purpose: so that I may at once cut off all objections that may be raised for ye future either from this or any other Hypothesis whatever. If you consider what I said both in my second letter to P. Pardies & in my answer to Mr Hook sect. 4 concerning ye application of all Hypotheses to my Theory you may thence gather this general rule. *That in any Hypothesis whence ye rays may be supposed to have any originall diversities, whether as to size or figure or motion or force or quality or any thing els imaginable wch may suffice to difference those rays in colour & refrangibility, there is no need to seek for other causes of these effects then those original diversities.* This rule being laid down, I argue thus. In any Hypothesis whatever, light as it comes from ye Sun must be supposed either homogeneal or heterogeneal. If ye last, then is that Hypothesis comprehended in this general rule & so cannot be against me: if the first then must refractions have a power to modify light so as to change it's colorifick qualification & refrangibility; wch is against experience.[76]

This passage—which may serve as another example of Newton's ability to entertain alternative hypotheses, and to apply constructive thought even to those he himself has rejected—does not, to be sure, bear expressly upon my present contention; for it does not specify the sense to be attached to the notion of an "aggregate of heterogeneal rays," in the hypothesis that identifies these rays as "pulses." However, the passage is not without a noteworthy implication on that point. Consider, namely, how one could possible conceive of a beam that was a mixture of "pulses *of different swiftness.*" Quite clearly, these could not remain stably separated from one another: for if the pulses are emitted at more or less random intervals, one behind the other, it must happen at least from time to time that a swifter pulse overtakes a slower one and passes through it. And such a thing is quite explicitly allowed by Newton in a passage quoted a little earlier, where, before saying that he cannot well conceive how unlike motions should first coalesce to a *uniform* one and afterwards part and recover their former unlikeness, he qualified this with the clause "though I can easily imagine how unlike motions may crosse one another."

Now consider Newton's explanation, in his reply to Hooke, of how it is that Hooke's view of the nature of light is not incompatible with Newton's own "New Theory." The passage has been alluded to, and a brief extract given, in **MMN**, n. 36; and it has also been quoted in part by Sabra, in a passage presented above; but now a fuller quotation is relevant:

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[76]Newton, *Correspondence,* I, pp. 417-420.

> In the second place I told you that Mr Hook's *Hypothesis* as to ye fundamentall part of it is not against me. The fundamentall supposition is, that the parts of bodies when brisquely agitated, do excite vibrations in the Æther, wch are propagated every way from those bodies in streight lines, & cause a sensation of light by beating & dashing against ye bottom of the eye, something after the manner that vibrations in the Air cause a sensation of Sound by beating against the Organs of hearing. Now the most free & naturall application of this Hypothesis to the solution of Phænomena I take to be this: That the agitated parts of bodies according to their severall sizes, figures, & motions, excite vibrations in the Æther of various depths or bignesses, wch being promiscuously propagated through that Medium to our eyes, effect in us a sensation of light of a white colour; but if by any meanes those of unequall bignesses be separated from one another, the largest beget a sensation of a Red colour, ye least or shortest of a deep Violet, & the intermediate ones of intermediate colours: Much after the manner that bodies according to their severall sizes shapes & motions, excite vibrations in the air of various bignesses, wch according to those bignesses make severall tones in sound.[77]

After some further discussion of this theory, followed by the objection that its fundamental assumption seems irreconcilable with the propagation of light in definite paths, Newton generalizes that objection and sums up:

> What I have said of this, may easily be applyed to all other *Mechanicall Hypotheses* in wch light is supposed to be caused by any pression or motion whatsoever excited in the Æther by the agitated parts of luminous Bodies. For it seems impossible that any of those motions or pressions can be propagated in streight lines without the like spreading every way into the shadowed Medium on wch they border. But yet if any man can think it possible, he must at least allow that *those motions or endeavours to motion caused in ye Æther by the severall parts of any lucid body wch differ in size figure & agitation, must necessarily be unequall. Which is enough to denominate light an aggregate of difform rays according to any of those Hypotheses.* And if those originall inequalities may suffice to difference the rays in colour & refrangibility, I see no reason why they that adhere to any of those Hypotheses, should seek for other causes of these effects, unlesse (to use Mr Hooks argument) they will multiply entities without necessity.[78]

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[77]Ibid., pp. 174-175; *Papers and Letters,* p. 120.

[78]*Correspondence,* I, p. 176; *Papers and Letters,* p. 121. (The last emphasis is my own.)

The two sentences I have emphasized, if taken quite strictly and literally, already suffice to settle the issue under discussion; for the proposition which, if allowed, is according to Newton "enough to denominate light an aggregate of difform rays," is certainly allowed by Gouy's hypothesis. But this may be thought inconclusive, since it assumes Newton to mean exactly what he says; and it is, after all, possible that he has spoken rashly—overlooking the possibility that the "inequality" of the motions caused by diverse particles may fail to preserve their "separate existence and identity within the compound." However, another part of the passage seems to me to have a more substantial bearing—a bearing that is, so to speak, more robust, turning not upon a particular phrase, but a physical conception.

Indeed, twice in that passage Newton makes an analogy of the theory he is suggesting (but not endorsing) to the theory already then generally accepted of the nature of sound and its propagation through the air. In particular, he mentions that "bodies according to their severall sizes shapes & motions, excite vibrations in the air of various bignesses, wch according to those bignesses make severall tones in sound." How, then, can Newton have conceived of the physical process at the eardrum of someone listening, for example, to a consort of viols? The listener hears simultaneously several lines of polyphonic music. In a given short period of time—let us say, the common duration of a single note of each musical line—several musical tones are distinguished by the ear. Each of these separately exists, in the air adjacent to the ear drum, as a vibration of a definite "bigness" (i.e., wave length). How, then, do these *coexist* in the air adjacent to the ear? Newton could hardly have supposed that the several homogeneous vibrations corresponding to the different tones occur *separately, in rapid succession;* for he knew that each tone is excited by the vibration of a string, and he knew that the bowed strings all vibrate throughout the interval in which the notes in question are played. There is therefore no alternative but to say that just as unlike motions may "crosse one another" without losing their distinct characters (which already implies that at the place of their crossing they must exist as a *single "composite" motion,* within which the "unlike" parts nevertheless retain, in a significant physical sense, their distinct identities), so those motions may combine within some spatio-temporal locus, as what may be called a "heterogeneous vibration," from which by a process of *physical separation* the "homogeneous" constituents may be extracted.

There is yet another, related, passage in the reply to Hooke that is perhaps even clearer in its implications for our question. Immediately after Newton's statement that he does not think it needful to explicate his doctrine by any hypothesis at all, he continues as follows:

> For if Light be considered abstractedly without respect to any Hypothesis, I can as easily conceive that ye severall parts of a shining body may emit rays of differing colours & other qualities, of all wch light is constituted, as that the severall parts of a false or uneven string, or of unevenly agitated water in a Brook or Cataract, or ye severall Pipes of an Organ inspired all at once, or all ye variety of sounding bodies in the world together,

should produce sounds of severall tones, & propagate them through ye Air confusedly intermixed. And if there were any naturall bodies wch could reflect sounds of one tone & stifle or transmit those of another; then as the Echo of a confused aggregate of all tones would be that particular tone wch ye echoing body is disposed to reflect; so since (even by Mr Hooks concessions) there are bodies apt to reflect rays of one colour & stifle or transmit those of another, I can as easily conceive that those bodies when illuminated by a mixture of all colours must appear of that colour onely wch they reflect.[79]

How can anyone read this, and suppose that Newton conceived of the "confused intermixture" of vibrations in the air caused by "all the variety of Sounding bodies in the world together" any otherwise than as what we should call a very complicated wave-form; at a given point, a very irregular vibration? And yet he envisages the possibility of a kind of resonating body that might *extract* from this confusion one of the component simple tones; and analogizes this to the case of light and color. To me, this passage by itself seems entirely conclusive against Sabra's interpretation of Newton's meaning.

In order now to assess whether the representation of white light suggested by Gouy can be regarded, in terms of the distinction Newton made in his comment on the discussion of February 3 1675/6, as "not an objection but an Hypothesis to explain [his] theory," we must examine somewhat closely what Gouy's representation is.

Gouy's discussion starts with the remark that the "immediate object" dealt with by the wave theory of light, in the explanations it gives of optical phenomena, is the "simple motion" (as he calls it): wave motion in which the state of the ether at each point varies with a simple harmonic oscillation.[80] That is, the basic regularities of wave optics are developed for this case; and application to the more complicated conditions that arise in reality is made by treating these as in some sense *composed* of "simple motions." But simple harmonic waves can never really occur: for a simple harmonic oscillation by its very nature has regularly varying phase and constant amplitude *over all time;* and although such a process can be conceived without contradiction, it first of all clearly does not occur in any optical process of ordinary interest (since conditions of illumination are not constant from eternity to eternity), and secondly is strictly excluded by the existence of sources of more or less random disturbance of the electromagnetic field (the "ether"). The usual response to this situation, Gouy says, is to suppose that there really are, for sufficiently long periods of time, states of simple harmonic oscillation *during those*

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[79]*Correspondence,* I, p. 177; *Papers and Letters,* p. 123.

[80]For bibliographic reference to Gouy's paper see n.64 above. Having described Gouy's starting point, I do not restrict myself in what follows to a report of his explicit arguments; on the contrary, although I do outline his principal considerations, I have felt free to supplement them with considerations he does not adduce.

times, but that these are occasionally perturbed (or, as a special case, extinguished). But this is a mere working assumption: there is no basis in principle for the proposition that the actual state of the ether has such a character (this point is effectively the same as I have just made in connection with Newton's views and his analogy of light with sound waves). Furthermore, as Gouy points out, the assumption fails to accomplish its objective, since it leaves unanalyzed the effects of those perturbations of the simple harmonic state—simply taking it for granted, without giving a reasoned mathematical discussion, that each separate long train of waves can be treated as if it were infinitely long, and that the interruptions of those trains of waves have no other consequence than to switch from the effect of one to the effect of the other.

Gouy's own treatment does not in fact altogether avoid the difficulties he has cited; for he bases his treatment on the representation of the optical state at a point by a *Fourier series*—and such a series represents a *strictly periodic* function (from eternity to eternity). Gouy takes the period of the function to be sufficiently great that the entire process of interest occurs within a single period, and assumes that what happens outside this very long time interval can be ignored in practice—an assumption of the kind that is regularly made in physics, and one much more innocent than the hypothesis Gouy has criticized. The point is in any case a minor one; for a more rigorous mathematical treatment can be based on the representation of the optical state by a Fourier integral rather than a Fourier series (something that is, moreover, necessary if we want to have a strictly continuous spectrum).

Unfortunately, there is a deeper and far more serious problem with Gouy's discussion. The predecessors he criticizes took it for granted that each of their (long but limited) "regular" trains of waves could be treated individually by the laws developed for simple harmonic waves. Quite analogously, Gouy takes for granted that *each Fourier component* of a complicated wave-form can be treated individually by those laws; and that the resulting optical effect can then be obtained by simply superposing these separate results. This, however, cannot be strictly true. For the effect of (say) a prism upon a beam of light must depend only upon the physical conditions within the body of the prism *during the time that beam traverses it.* But the spectral decomposition of a function is a *global* characteristic of the function; in particular, the spectral decomposition of a function that represents the variation of some physical state-quantity over time depends upon *the entire history of that quantity, from eternity to eternity.* Since the prism cannot "know" this total history—it certainly cannot "know," for example, when the light is going to be extinguished—it is clearly impossible for it to act independently upon the Fourier components.

There is therefore a fundamental flaw in Gouy's analysis. And yet, on the one hand, the reasons underlying his hypothesis about the constitution of ordinary light are convincing and sound; and on the other, physical optics does indeed treat the monochromatic constituents of a beam of light as if they behaved independently. (Shapiro's remark quoted above, "[T]he principle of color immutability is an important one, incorporated into modern optics," recognizes a part of this circumstance.) How is this to be understood?

The answer is that modern physics (whether in the form of the mechanical or electromagnetic wave theory in fully developed form, or in that of the quantum theory) does not regard light itself as a "fundamental" or "elementary" process.[81] For example, when an electromagnetic disturbance impinges upon a material medium, it is not true that the disturbance is propagated within that medium as a "refracted" wave, with a velocity determined by the index of refraction of the medium. On the contrary—according to the form(s) of electrodynamics that have reigned ever since the work of H. A. Lorentz—such a disturbance is always and everywhere propagated with the "universal" velocity c of light *in vacuo.* Both the (optical) *phase velocity,* characteristic of a monochromatic wave in the given medium and associated with its index of refraction, and the so-called *group velocity* (itself derived from the phase velocities of monochromatic components, and characteristic—in ordinary cases—of the transfer of measurable amounts of energy ["signals'] through the medium), arise only after a transition time during which the particles of the material medium, responding to the new disturbance, achieve a "quasi-stationary" oscillatory state. But such a state can only ensue if the incoming disturbance itself has some degree of regularity. Thus we see (and this result is characteristic of all optical processes involving interactions between light and matter; hence of all processes in which optical effects can be in any way detected) (a) that the fundamental laws governing the process are not those of optics—the latter being, rather, derivative from those fundamental laws; (b) that the "optical state" is of a special character, distinguished by a kind of stability (or "quasi-stationarity") that makes it possible to speak of magnitudes which in the most general states imaginable are simply not well-defined;[82] (c) that some special conditions are required if such an "optical state" is to set in at all.[83]

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[81]This statement may seem to conflict with the fact that *photons,* the quanta of light, are counted among the elementary particles. The fundamental laws that characterize photons are, however, not those of optics, but of (quantum) electrodynamics; for photons to "behave optically" (that is, for a process to be characterizable in terms of reflection, refraction, diffraction, etc.) is a quite special case.

[82]A comparison with thermodynamics is instructive: for such thermodynamic quantities as temperature or entropy to be well-defined, a many-particle system must satisfy certain conditions (whether those of thermodynamic equilibrium, as in classical thermodynamic theory, or the more general—but still in some degree restrictive—conditions considered by nonequilibrium thermodynamics.

[83]This situation was first clarified in two papers submitted together, and under the same title, to the *Annalen der Physik* in 1914—the first by Arnold Sommerfeld, the second by Léon Brillouin: A. Sommerfeld, "Über die Fortpflanzung des Lichtes in dispergierenden Medien," *Annalen der Physik*, 4th ser., **44** (1914), pp. 177-202; L. Brillouin, Über die Fortpflanzung des Lichtes in dispergierenden Medien," ibid., pp. 203-240. Sommerfeld had presented a sketch of some of the relevant considerations

several years earlier: A. Sommerfeld, "Ein Einwand gegen die Relativtheorie der Elektrodynamik und seine Beseitigung," ibid., **8** (1907), p. 841 (there follows—pp. 841-842—interesting discussion by [Wilhelm] Wien, [Ferdinand] Braun, [Th.] Des Coudres, and especially [Waldemar] Voigt, the last of whom contributes a rather significant clarifying remark). For a later especially instructive account see Arnold Sommerfeld, *Optics* (*Lectures on Theoretical Physics,* vol. IV), trans. Otto Laporte and Peter A. Moldauer (New York: Academic Press, 1954), pp. 114-123.

It should be noted that the clarification here described was motivated, not by the paradox that results from assuming independent behavior of Fourier components, but (as the titles of the papers indicate) by an issue that concerned the special theory of relativity, and that had been cited as an objection against the latter— namely, the problem posed by "anomalous dispersion," in which, according to classical optics, the speed of light in the dispersive medium is for some rays greater than that *in vacuo.*

There had also occurred, in the pages of the *Comptes Rendus* of the Paris Académie des Sciences in 1895, a discussion that began with an objection by Poincaré to the views of Gouy—views that had also been put forward by Lord Rayleigh (a little later than Gouy, but, it appears, independently), and had been adopted and developed by Arthur Schuster; Gouy and Schuster both replied to Poincaré. See H. Poincaré, "Sur le spectre cannelé," *Comptes Rendus de l'Académie des Sciences de Paris* **120** (1895), pp. 757-762; [G.-L.] Gouy, "Sur la régularité du mouvement lumineux," ibid., pp. 915-917; Arthur Schuster, "Sur les spectres cannelés," ibid., pp. 987-989. Poincaré's objection was based upon the fact that, in his opinion, Gouy's hypothesis implied that the spectroscopic image of a candle should be visible "after the candle has been extinguished and even before it was lit"; it is thus close to the objection I have described in the text above. However, as Gouy quite justly remarks, Poincaré does not say where in his view the fallacy of Gouy's own analysis lies; and indeed, in the alternative analysis Poincaré suggests, he himself makes full use of the assumption of independent behavior of the Fourier components—that is, just the assumption that leads to the impossible results. Gouy and Schuster both offer cogent responses to Poincaré's objection. Each independently remarks that a point on which the dispersed or diffracted light falls will be illuminated, not by light of a single frequency, but by some narrow frequency band. From this Gouy, in particular, concludes (p. 916): "Il faut donc, pour calculer la vibration réelle en un point donné du spectre, tenir compte des vibrations fictives que produiraient en ce point un grand nombre ou une infinité de mouvements simples, qui diffèrent de période, de phase et d'amplitude; rien ne s'oppose á ce que cette vibration rélle varie d'amplitude avec le temps, et s'annule lorsqu'il le faut."

The last clause of Gouy's statement is incorrect, if the superposed vibrations belong strictly to a limited band of frequencies; for by a fundamental theorem due to Paley and Wiener, a function whose spectrum is *bounded* cannot vanish except at isolated points, unless it vanishes everywhere; therefore, if some physical state is quiescent over an interval of time, the function representing the rate of change of that state must have Fourier components of arbitrarily high frequency. (Of course, the Paley-Wiener Theorem was not known in 1895.) However, this remark does not dispose of the matter; for when account is taken of the diffraction of light, one has to conclude that, strictly considered, each illuminated point will receive non-zero contributions of *all* frequencies—so these general considerations do not after all decide the issue, and the upshot of the discussion of 1895 (amended in the way I have just indicated) was to remove, at least to this extent, the sting of Poincaré's objection. Yet that discussion must, I think, be judged to have been inconclusive. It left open the question whether full cancellation of luminous flux outside the interval of illumination of the candle would always actually occur if Fourier components were treated independently; and it did not deal at all with the more fundamental problem I have emphasized, decisive clarification of which came only from the work of Sommerfeld already cited.

One more point ought to be made here. Sabra (*Theories of Light,* p. 281, n. 14) gives this account of the discussion among Poincaré, Gouy, and Schuster:

> Following the publication of Gouy's paper the scientific world witnessed a curious re-enactment of the dispute between Newton and his critics. Poincaré maintained that a high degree of regularity must be attributed to white light, while Schuster and Lord Rayleigh argued in support of Gouy's representation. (See R. W. Wood, [*Physical optics,* 2nd ed., New York, 1911], p. 650.) Needless to say, this repetition of the seventeenth-century controversy occurred on a definitely higher level of mathematical and experimental sophistication. It should, however, indicate that Newton's opponents were not simply motivated by their pigheadedness or their addiction to hypotheses. And, in any case, it clearly shows that Newton's experiments had not been sufficient to 'prove' his doctrine of white light. A parallel study of these two disputes, with due attention to their different contexts, would be illuminating.

Now, Poincaré's objection to Gouy's analysis in actual fact concerned a point made by Gouy about a classical experiment of Fizeau and Foucault. The latter had observed interference fringes, from white light, corresponding to path differences of the order of 4000 wave-lengths of the interfering rays; and had concluded that there must be *coherence of the homogeneous constituents* of such light over distances of that magnitude—see [A. H. L.] Fizeau and [J. B.] Foucault, "Sur le phénomène des interférences entre deux rayons de lumière dans le cas de grandes différences de marche," *Annales de chimie et de physique,* 3rd ser., **26** (1849), pp. 138-148. Gouy, towards the end of his paper, had referred to this experiment, and argued on the basis of his own analysis that "the existence of interference fringes at great path differences, in the case of continuous spectra and of white light, by no means implies the regularity of the motion of the incident light." Poincaré challenged *this* claim; his own conclusion was that

Now, the defect in Gouy's analysis is not advantageous to Newton's case; quite the contrary, it is harmful to that case. For, we must recall, Newton claims that white light is "an aggregate of difform rays," and he says "by the rays of light I understand its least or indefinitely small parts, which are independent of each other"; but the only conception of homogeneous light that the wave theory admits is that of simple harmonic light-waves—and we have just seen that these cannot in strictness be acted upon independently in optical processes. If, however, I may be permitted to introduce an Aristotelian locution in such a context, it is pertinent to distinguish here between optical processes *qua* physical, and optical processes *qua* optical: viewed in their former—physically fundamental—guise, these processes are not governed by laws satisfied independently by the Fourier constituents of optical vibrations; but viewed *as optical*—that is, in so far as it is possible at all to speak of laws *of light itself*, as distinguished from the general laws of the electromagnetic field and its interactions with ordinary matter—the Fourier constituents do indeed behave independently. From a formal point of view, this may be taken as a part of the characterization of the distinctively "optical" realm, and the threat of tautology may appear to loom. That the matter, however, is not one of mere terminology is made clear by the existence of a well-defined body both of experimental techniques and of theoretical principles and methods constituting, within physics, the science of optics. Treatises on the subject take the "optical" point of view, in which restriction is made to "optically (nearly) steady" states with independent behavior of the Fourier components of waves, quite for granted, usually without even mentioning the point (the discussion in Sommerfeld's *Optics*—see n. 83—is a rare exception). I do not think Shapiro or Sabra would question the substantive legitimacy of this notion of the optical.[84]

"the experiment of Fizeau and Foucault teaches us that the luminous motion presents a certain sort of permanence," expressed by an equation Poincaré had just argued for. That equation (based by Poincaré upon a formula that he claims—incorrectly, as both Gouy and Schuster point out—to have been established experimentally by Fizeau and Foucault) has nothing whatever to do with the presence of *discrete, separated* (quasi-)homogeneous constituents in the white; it has to do only with a kind of *average similarity* of the wave motion in two short time-intervals separated by a time related to the path-difference. Thus, if Gouy's analysis is viewed as inimical to Newton's doctrine, Poincaré's stand cannot in any way be seen as having a bearing on the rehabilitation of that doctrine; to represent the discussion of 1895 as "a repetition of the seventeenth-century controversy" is thoroughly misleading. (Sabra, it may be observed, refers to that discussion only as it is reported in R. D. Wood's textbook, *Theoretical Optics;* it therefore appears that he has not consulted the original sources.)

[84]The assumption of "stationarity in the wide sense" is explicitly noted in the encyclopedic work of Born and Wolf—Max Born and Emil Wolf, *Principles of Optics*, 2nd. (rev.) ed. (New York: Macmillan, 1964), pp. 2 and 500. The analogy with thermodynamics as, from the point of view of

Assuming this point, then, it remains to reconsider the question of the adequacy both of Newton's conclusions (in the light of later knowledge), and of his evidence for those conclusions.

I have already suggested that Newton's doctrine of the *presence,* in the light he calls "hetero-geneous," of the homogeneous constituents out of which it has been made or into which it can be re-solved, can be accurately construed to mean that the *information* represented by that spectral decomposition exists—in some physical form or other—in that light. Specification of that physical form will be, then, what Newton calls "an Hypothesis to explain my Theory" (except that such specification will no longer be, in Newton;s sense, "an Hypothesis," if the evidence for it rises to the level he would deem adequate as "proof"). I have also remarked that this reading of Newton's intention does not exhaust the content of his theory—that there is a further crucial point to be made. The latter is, precisely, the principle of independence of the physical behavior of the homogeneous constituents (Newton's optical principle of the independence of the "rays").[85] I shall not repeat my arguments in favor of this reading; to me, in the light of both the textual passages I have cited from Newton and the reasoning I have presented, the case is compelling.[86] But perhaps one or two points of clarification are in order. I should

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fundamental physics, a derivative, but nevertheless in a significant sense an (approximately) autonomous realm, is again instructive—cf. above, n. 82.

[85]To avoid a possible confusion here, let me note that there is another sense of "independence of the rays" in which this concept breaks down when one passes from geometrical to physical (wave) optics: for in this theory—and in physical reality—light does not consist of *parts propagated independently in definite lines.* Thus one has to say the following about the Newtonian concept of a "ray," as the basic unit or element into which light is to be analyzed: (a) In the approximation appropriate to *geometrical* optics, there is such an element in the full sense envisaged by Newton—a spatially localized part, with definite refrangibility and color, propagated in definite paths. (b) In the deeper treatment of *wave* optics, analysis is still possible into elementary, i.e., "simplest," constituents (which, again, have definite refrangibility—i.e., according to the principles of the wave theory, definite phase velocity in any given transparent medium—and color, as well as definite frequency); but these are not localized constituents, propagated in definite paths, but "elementary" waves—waves characterized, as Gouy puts it, by "simple motion."

[86]The tradition of contemporary historians of science is extremely resistant to discussion of an au-thor of the past in language of our own time. There are honest reasons for such resistance: the fear of anachronism, and of what is called "Whig history"; but I myself do not believe that the preventatives usually adopted are either necessary or sufficient for the avoidance of the diseases feared—and I be-lieve that they bring with them serious disadvantages (as if, in fear of wandering off a trail that is difficult to follow, one chose to wear blinders and fetters). I have touched briefly on this point before—see my paper "Some Philosophical Prehistory of General Relativity," in John Earman, Clark

not wish to insist upon Newton's having had *explicitly in mind,* in his discussion of possibilities for a wave theory, the idea that the heterogeneity of white light might consist in complexity of wave-forms; after all, the mathematical representation of more or less general functions by trigonometric series was entirely unknown in the time of Newton. In citing evidence from Newton's remarks, and especially from his analogies with sound, I have only meant to point out (a) that he could not reasonably have supposed literal and discrete spatiotemporal separation of the intermixed homogeneous motions—that he must, therefore, even if somewhat vaguely, have entertained the idea of a superposition of motions (of the same part of the medium at the same time) as playing a role in the process of "mixing" (as he surely conceived it to in the process of "crossing") of "unlike motions"; and (b) that those remarks and analogies do in fact—if followed out systematically—imply a conception of white light of the kind associated with Gouy. Beyond this, and setting aside the particular notions of the wave theory, Newton's *general* statements on the subject of his doctrine and its relationship to objections on the one hand, explanatory hypotheses on the other, are (I think) entirely in accord with the interpretation I have offered.

But in terms of this interpretation of Newton's theory, and taking account of what I have said above about the character, in modern physics, of the "optical" realm, the adequacy of Newton's conclusions in the light of our own knowledge is clear. The presence of homogeneous constituents in heterogeneous light is exactly represented by the purely mathematical analysis of the latter with the help of the Fourier integral: the basic theorems about Fourier transforms assure us that when a wave-form is composed out of simple constituents, no information is lost.[87] The physical explanation for the heterogeneity of light in general is exactly that stated by Newton in a passage already given as quoted by Sabra: that "those motions or endeavours to motion caused in ye Æther by the severall parts of any lucid body wch differ in size figure & agitation, must necessarily be unequall."[88] The physically

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Glymour, and John Stachel, eds., *Foundations of Space-Time Theories* (Minnesota Studies in the Philosophy of Science, vol. VIII; Minneapolis: University of Minnesota Press, 1977), pp. 3-49; the relevant passage is on pp. 13-14. The historiographical issue deserves more extended discussion.

[87]The development of the purely mathematical theory to a point at which this statement can be made in suitable generality and with full rigour is strikingly recent. If we wish to allow for the possibility of oscillations with continuous spectra, with discrete spectra, and with a superposition of the two, we must have recourse to the theory of Fourier-Stieltjes transforms. A satisfactory "information-recoverability" theorem (i.e., a suitable *uniqueness* theorem), whether for the continuous case or the more general case just alluded to, was only established as late as 1937, by A. C. Offord. Cf. Antoni Zygmund, *Trigonometric Series* (2nd ed.; Cambridge University Press, 1959), vol. II, pp. 291-293 and p. 335, n. to ch. XVI, §10.

[88]See n. 68 above, and the passage to which it is attached.

independent behavior of the homogeneous constituents is part of what characterizes distinctively optical processes. The only substantive qualification I am aware of that needs to be made of Newton's doctrine—besides that contained in the limitation to processes *qua* optical, and the distinction (see n. 85) between "rays" in geometrical optics and wave optics—is that the principle of immutability (of the character of a homogeneous constituent fails in the Doppler effect: a monochromatic wave reflected from a moving mirror has its frequency altered.

As to Newton's evidence for his conclusions, surely not much more needs to be said. His view that the properties of heterogeneous light are the result of the independent behavior of immutable homogeneous constituents was based upon a large array of experiments in which he was unable to produce changes in those properties of the latter that he describes as "connate" to them (this Shapiro allows as satisfactory evidence for the "weak principle of immutability"); and in which he was able to account for, and to predict, the vicissitudes of the former in terms of the independent behavior of the latter (this I have discussed at some length above). If one is to allow the possibility of empirical (inductive) evidence for general conclusions at all—and in my view, in spite of the contrary position of some recent philosophers of science (notably Popper), one must do so: "it is," as Newton says, "the best way of arguing which the Nature of Things admits of"—then this is precisely the kind of evidence suited to support the conclusions here at issue.

§7. Two further criticisms offered at the symposium deserve comment.

The first of these has to do with historical methodology. It was objected—and claimed to be characteristic of a difference between the practice of philosophers and that of historians—that in **MMN** I cite passages from quite different periods of Newton's career indiscriminately, without attention to the possibility that his views may have changed over time. (This was characterized—with an allusion to a phrase of mine, to be discussed presently—as "Newton- mashing.")

I can only reply that I believe this charge to be wrong. It is quite true that I cite later works to reinforce evidence, or to explicate a principle, drawn from earlier ones—as, for example, when I quote the Definition from Book II of the *Opticks* to emphasize Newton's care in distinguishing the various senses of the word 'color' and of color-words. But I have done so only when there seemed to me good evidence that the views expressed in the later passages agree with those in the earlier ones—as is clear in the case cited, in my opinion, from the language of *dispositions* already used by Newton in his first publication.

That I have at least not ignored the question of changes of view should be plain from the emphasis I have placed on certain fundamental changes which, as I believe, did in fact occur at the time of the writing of the *Principia;* and also from the fact that, far from taking such things for granted, I express some amazement at the maturity of the metaphysical conceptions of the presumably early manuscript "De gravitatione [etc.]," and at the ease with which those conceptions accommodate Newton's later thought.[89] Of course, I may be wrong in concluding that Newton's views on a given matter remained constant; but not because I assume this as a matter of course. As to the issue considered in detail, I have tried to deal with it in earlier sections of the present paper.

The second criticism concerned a particular expression I have used in my paper (the phrase alluded to above). Referring to a statement of Alan Shapiro's, to the effect that Newton "in his typically sly manner, rather than admitting to his own confusion," claimed in his answer to Hooke merely to be restating a distinction he had already made, I have said that this seems to me "an all too typical instance of the strange fashion for Newton-bashing." This was seen as an attack upon historical work in general, and in particular as impugning the *motives* of historians.

Despite the objection, I have decided to allow my remark to stand unaltered in **MMN**; but I think that an explanation—a clarification, and to some degree a defense—is called for.

First, I wish to make plain that in writing what I did I had no thought at all of impugning the motives of any historian. Motives, I believe, are always more or less complex; I also believe, on the whole, that people who do scholarly work have a serious interest in finding and clarifying what at the risk of appearing naïve I shall call "the truth." Fashions can, however, be very powerful; and they can also be (again to put it naïvely) good or bad. I do think there is a fashion for Newton-bashing; and I think it has had a very serious effect, not upon the motives, but upon the judgment of historians, for several decades now.

Second, note that the example I cited involves the ascription to Newton not merely of error, but of—by coincidence—bad motives (and opprobrious behavior). Now, this is certainly not the place to embark upon an analysis of Newton's character. Let me just say that in my own view, it is clear beyond a doubt that Locke's remark about Newton: "I have several reasons to think him truly my friend, but he is a nice man to deal with, and a little apt to raise in himself suspicions where there is no ground"[90] is nicely understated. On the other hand, I think that Newton's defects have, in the recent literature, been (in contrast) overstated; and that in some degree his character has not been correctly understood. All that, however, is beside the point. The nub of my complaint against those historians I criticize in this respect is that, having formed an opinion of Newton's extreme dishonesty, they are rather easily

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[89]I may point out further that I suggested long ago, on the basis of the general maturity of thought of "De gravitatione," of the striking similarity of some of its argumentation about space and motion to the famous scholium on that subject in the *Principia,* and of a bit of testimony (although in terms that are certainly far from conclusive) by Pierre Coste, that the assignment of that manuscript to Newton's early years might be wrong—see H. Stein, "On the Notion of Field" (referred to above, n. 29), pp. 274-275, n. 11. I mention this not to question the date agreed upon by historians, but only as a further indication that I am not insensitive to such questions.

[90]Quoted by Westfall, *Never at Rest,* pp. 590-591.

inclined by that opinion to dismiss as misrepresentations statements of his that in my own view are demonstrably true; and the evaluation of these statements is used in turn to reinforce the charge of dishonesty. In short, a simple case of "Give a dog a bad name and hang him!"—with the unfortunate consequence for the history of *science* (not of character) that serious distortions enter the historical literature.

I have already given a number of examples of this, in which I have defended my own interpretations of Newton in detail. I shall here give two more.

I have quoted (**MMN**, n. 36) a passage in which Thomas Kuhn suggests that Newton is "convicted of an irrationally motivated lie" in his second reply to Huygens. This is a very damaging accusation; let us consider on what it is based.

"In his first paper," Kuhn writes, "Newton had said, in discussing colors"—and then quotes the following passage:

> But the most . . . wonderful composition is that of *Whiteness* . . . 'Tis ever compounded, and to its composition are requisite all the aforesaid primary Colours, mixed in a due proportion . . . Hence therefore it comes to pass, that *Whiteness* is the usual colour of *Light;* for Light is a confused aggregate of Rays indued with all sorts of *Colours* . . . if any one predominate, the Light must incline to that colour.

"Yet," Kuhn continues:

> when Huygens suggested that the combination of yellow and blue might generate white, Newton admitted the possibility but claimed he had never meant anything else. The apparent contradiction he reconciled by saying that Huygens's white would be different from his own by virtue of its composition. Newton's position was correct in the reply, but surely he had changed his mind in reaching it.[91]

Kuhn has here simply misread the exchange between Newton and Huygens. I have earlier quoted from Newton's reply to Huygens's first letter the sarcastic remark, "If therefore M. Hugens would conclude any thing, he must show 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."[92] Huygens very understandably expressed reluctance to continue a dispute that had engendered more heat than he had anticipated, but curiosity to know what that strange remark of Newton's could have meant:

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[91]Kuhn, "Newton's Optical Papers" (in Newton, *Papers and Letters*), p. 40.

[92]See n. 19 above, and the text to which it is attached.

> Touching the Solutions, given by M. *Newton* to the scruples by me propos'd about his Theory of Colors, there were matter to answer them, and to form new difficulties; but seeing that he maintains his opinion with so much concern [Huygens actually wrote *avec quelque chaleur*—"with some heat"], I list not to dispute. But what means it, I pray, that he saith; *Though I should shew him, that the White could be produced of only two Uncompounded colors, yet I could conclude nothing from that.* And yet he hath affirm'd in *p.* 3083. of the *Transactions,* that to compose the White, all primitive colors are necessary.[93]

Newton's second reply finds him very much cooled off from the "heat" Huygens deprecates. He offers an explanation for his initial irritation: "As for M. Hugens expression that I maintain my doctrine wth some concern, I confess it was a little ungrateful to me to meet wth objections wch had been answered before, without having the least reason given me why those answers were insufficient" (citing pages of the *Philosophical Transactions* on which those previous answers are to be found); and he adds: "However, since there seems to have happened some misunderstanding between us, I shall endeavour to explain my self a little further in these things"—and gives, in the sequel, a very lucid amplified treatment of his doctrine of light and colors[94] (my characterization, of course; Kuhn and Shapiro consider this amplified treatment to embody fundamental revisions of the original doctrine).

As to the question about his puzzling taunt, this is what Newton says in response:

> Concerning the bussiness of colours in my saying that when M. Hugens hath shown how *White* may be produced out of two uncompounded colours, I will tell him why he can conclude nothing from that; my meaning was, that such a White, (were there any such,) would have different properties from the White, wch I had respect to, when I described my Theory, that is, from ye white of ye Sun's immediate light, of ye ordinary objects of our senses, & of all white Phænomena that have hitherto falln under my observation.[95]

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[93]Newton, *Correspondence,* I, p. 285 (quoted to Newton by Oldenburg in Huygens's original French); *Papers and Letters,* p. 147 (in the English version published by Oldenburg, which I follow).

[94]*Correspondence,* I, pp. 292ff.; *Papers and Letters,* pp. 139ff. It is worth noting that the irritation Newton had expressed to Huygens (thirteen years his senior, and a very eminent man, at a time when Newton was only first becoming known) had no damaging effect upon their future relations, which were those of the greatest respect and admiration on both sides.

[95]*Correspondence,* I, p. 291; *Papers and Letters,* p. 137.

It is in this passage that Kuhn finds "an irrationally motivated lie"; namely in the claim (as Kuhn puts it) that Newton *had never meant anything else.* (The only use of the word *meaning,* or any other word derived from the verb "to mean," in Newton's entire letter, is that in the phrase "my meaning was" just quoted.) But what Newton is explaining here is *what he had meant* by the remark *about whose meaning Huygens had asked;* he was not at all referring here to "his first paper." And the explanation is patently true—when Newton made the sarcastic remark "I will [then] further tell him why he can conclude nothing from that," he obviously *did* mean that any light compounded out of only two homogeneous components, even should it appear white, would have different properties from the white light of the sun, etc.; what else could one suppose him to have meant? (Later in this letter Newton goes into considerable detail in regard to these differences). Kuhn, therefore, has clearly misconstrued Newton's statement, and has based his charge of lying on this misconstruction.

There is something else deeply misleading in Kuhn's remark. He says that "when Huygens suggested that the combination of yellow and blue might generate white, Newton admitted the possibility but claimed that he had never meant anything else." Now, even aside from the erroneous interpretation of the reference of the phrase "my meaning was," this is quite false. In his first reply to Huygens (which I have discussed earlier), Newton *avows* that he does not believe an apparent white can be produced from two uncompounded colors, but recommends that his own old attempt to do that be repeated. This follows directly after his sarcastic remark that if Huygens succeeds in producing white out of two uncompounded colors, he (Newton) will tell him why he can conclude nothing from that. It is therefore quite plain both that Newton has not claimed to have "meant" that it might be possible to compose white out of uncompounded yellow and blue (the mendacious claim Kuhn's statement attributes to him); and that his sarcastic remark itself was not intended to be taken in its literal sense—*nothing* (or "nothing of any interest") can be concluded—but in the sense: nothing *fundamental to the "New Theory"* can be concluded from success in that attempt. (If Newton had literally meant that nothing of scientific interest would follow, he would not have recommended—and not just to Huygens, but to other investigators generally—that the trial be made again.)

I must say, also, that this way of describing the exchange seems to me to show a most unfortunate perspective upon scientific discussion. Newton goes on to explain that the white he "had respect to" when he described his theory was that of direct sunlight, of ordinary bodies, etc. Now, Huygens was the last man in the world to have dismissed this explanation as a mere quibble, or a weaseling attempt to evade a criticism (much less as anything so base as "an irrationally motivated lie"). Huygens—like Newton (or should I perhaps say, in view of their relative seniority, "Newton—like Huygens?)—was a serious natural philosopher. Huygens's interest in this discussion was in learning what he could about the nature of light. For Huygens, as for Newton (compare my earlier discussion of this point), the "immediate light of the sun," the light in which ordinary vision of ordinary objects occurs, was what one normally "had respect to" in speaking of white light. Huygens had failed to appreciate the power of Newton's evidence and arguments in the "New Theory." Newton's initial response to this failure was, to be sure, an unpleasantly irascible one. That passed, Newton explained the position to Huygens more fully, and Huygens certainly came to appreciate the difficulties that Newton's discovery presented for his own view that there are just two basic colors.[96] Thus, in the exchange with Huygens, Newton's thin-skinnedness caused no more than a transient disturbance; it had no lasting effect upon either their personal relations or their scientific commerce.

The second example I want to present does not involve an accusation of moral fault against Newton; but it does show rather spectacularly how a certain anti-Newtonian animus has come to infect the atmosphere of intellectual history.[97]

In a book dealing with "the intersections of science and government" in enlightenment France,[98] Charles Gillispie devotes an extended section to the scientific work of Marat, who attacked the optical theories of Newton. In the course of this account, Gillispie makes a rather sweeping reference to "the recent intensive and often very critical scholarship bearing on Newton's optical work," citing three papers of J. A. Lohne and two of Zev Bechler as "the most important items" in this scholarship (and refer-

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[96]In his *Traité de la lumière,* Huygens abstains from any discussion whatever of color (in particular, does not suggest that a mechanical account of two colors would suffice as the basis for a complete theory). In the preface to that work he remarks that he hopes others will penetrate further into the nature of light on the basis of the beginnings he has made, since the subject is far from exhausted; and mentions expressly, as something he has left entirely untouched, "everything that concerns the Colors; in which no one up to now can boast of having succeeded" (meaning, of course, "succeeded in explaining their nature"). See Huygens, *Œuvres complètes,* vol. XIX (1937), p. 455; *Treatise on Light,* tr. Silvanus P. Thompson (reprinted Chicago: University of Chicago Press, 1945), p. vii (the version here quoted). In a letter of August 24 1690 Huygens wrote to Leibniz: "I have said nothing about colors in my treatise of Light, finding this matter very difficult; above all because of so many different ways by which colors are produced. Mr. Newton, whom I saw this past summer in England, promised something about this, and communicated to me some very beautiful experiments he had made." See Huygens, *Œuvres complètes,* vol. IX (1901), p. 471.

[97]This example was called to my attention by Robert Palter.

[98]Charles Coulston Gillispie, *Science and Polity in France at the End of the Old Regime* (Princeton: Princeton University Press, 1980). The characterizing phrase I place in quotes is not taken verbatim from Gillispie; it is based upon the following statement in his preface (ibid., p. ix): "My book is written in awareness that much of science has in general little or nothing to do with government, and that much of government has little or nothing to do with science, but that there are intersections. This is a history of the intersections, when they began to assume a form characteristic of the modern state and of modern science."

ring also to the paper of Alan Shapiro, part of which I have discussed above).[99] Gillispie describes as follows one of Marat's attacks on Newton:

> In Newton's most famous experiment, a beam of sunlight was admitted through a hole in the shutter, passed through a prism, and projected onto the opposite wall where the colors of the spectrum appeared along a band. Newton takes its elongation for evidence that the heterogeneous rays of white light had been refracted each a specific amount according to its color, and thus spread into the oblong shape of the image. Very persuasive, replies Marat, but look: even allowing Newton his arrangement of the apparatus, there is a difficulty. He supposes that the rays of the original beam are parallel when they impinge on the incident face of the prism. In fact they are not, for they have already been inflected in their passage through the aperture of the shutter. The elongation of the spectrum results from that initial decomposition, and owes nothing to refraction in the glass.[100]

At this point Gillispie appends a footnote, in which he tells us: "Lohne (1961) objects to the same assumption"—that is, the assumption that the rays of the incident beam are parallel—"though on different grounds—namely that the rays come from all points of the disk of the sun and are not parallel."

The grotesqueness of this is staggering. Newton's argument that the image of the sun should, according to the received law of refraction, have been *circular,* was of course based upon the fact that the beam after passing through a hole that could be regarded as essentially punctiform had the form of a *circular cone* (with a vertex angle equal to the angular diameter of the sun, namely a half degree); it was most definitely *not* based upon the assumption that the incident rays were parallel. Marat's indefensible hypothesis that the oblong shape was the result of diffraction ("inflection") at the hole may be set aside—Gillispie does not attempt to defend it. But Gillispie *does* suggest, on the authority of Lohne, that Newton had made a faulty assumption of parallelism of the rays, and that to this extent Marat's criticism was justified.

Lohne is not responsible for this absurdity: what one finds in the paper Gillispie cites is something very different. Lohne, in the first place, after describing a very important mistake that Newton really did make in his optical investigations,[101] adds this remark: "I do not wish to throw suspicion on the

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[99]Ibid., p. 308, n. 165.

[100]Ibid., p. 324.

[101]The mistake concerns the quantitative discussion of the result of successive refractions of a sunbeam through a pair of prisms oriented at right angles to one another (as well as to the beam). This experiment of the crossed prisms is appealed to by Newton in the *Opticks* (Book I, Part I, Proposition VI), for experimental support of the very important conclusion that Snell's law holds strictly for each sort of

rest of Newton's prism experiments. He was ordinarily very painstaking and accurate, and among his few 'trips' we have here selected the worst."[102] As to the point referred to by Gillispie, here is the state of affairs: In his *Optical Lectures,* Newton discusses in some detail the image to be expected, on the received law of refraction, when a sunbeam is refracted by a prism in the position of minimum deviation. Having shown that this image should to a close approximation be circular, Newton sketches an argument to show that on a stricter analysis, taking account of the difference between the refractions in a principal plane of the prism and oblique to such a plane, the expected image will be slightly foreshortened in the direction in which the beam is deflected. The point is such a minute one that Newton excuses himself from a full geometric discussion, saying: "I am omitting the demonstration of this, as it is rather long and not completely necessary for my purpose," and: "the difference [between the two diameters of the image] will be so small that they may be considered as sensibly equal"; and, finally: "to include everything in a few words, it is manifest that generally the sun's image must be *sensibly nearly circular.*"[103] Lohne says nothing whatever against this; and there is indeed nothing to be said against it. But Lohne does object to Newton's conclusion that (strictly considered) the image should be foreshortened. In arguing geometrically for that conclusion, Newton takes the prism to be placed between the sun and the hole that determines the beam (that is, outside his window-shutter); but in the actual experiment, the prism was behind the hole (inside Newton's chamber). Newton also takes the position of the prism to be such that the rays from the sun's upper and lower extremities deviate equally from symmetry in traversing the prism. Lohne tells us: "I have investigated for myself the arrangement I consider natural, namely prism on the inside and exact symmetry for the ray from the sun's center. I found a slightly *oblong* or rather egg-shaped image of the sun. But if the prism is supposed to be on the outside, the solar image will be flattened, as Newton contends."[104]

The issue, in short, is minute in the extreme. It is odd that Lohne does not inform us of the exact method by which he arrived at the result he states. I assume he found this result by calculation, not by experiment. Indeed, the issue concerns the exact prediction of geometrical optics for strictly homoge-

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homogeneous ray taken separately. The mistake in question, however, occurs already in the *Optical Lectures* (although there nothing of moment hinges upon it).—Perhaps I should remark here that Lohne's analysis of the problem posed by this error of Newton's itself suffers from some shortcomings; I expect to discuss the point in detail in a more technical article than the present one.

[102]Johannes A. Lohne, "Newton's 'Proof' of the Sine Law," (see reference in n. 41 above), p. 391. (The word "trips" is clearly a mistake in English idiom; Lohne means "slips" or "blunders.")

[103]See Shapiro, *The Optical Papers of Newton,* I, pp. 59/59-60-61 (Latin/English; emphasis added), and cf. Fig. 3, p. 52; correspondingly (in the later version of the lectures) pp.290/291-292/293, and Fig. I,3, p. 286.

[104]Lohne, "Newton's 'Proof' of the Sine Law," p. 397.

neous light; and the deviation from circularity is so small that in real circumstances it is certain to be obscured by diffraction, by the unavoidable heterogeneity of the light, and by the limitations of exact determination of the edge of an illuminated area. As to the calculation itself, it is not hard to see that, when the prism is inside the chamber, the result must depend upon the relative distances from the hole of the prism and the screen on which the image falls. Thus we have a claim that the result "really" to be expected differs from what Newton says; but without explicit demonstration, without specification of conditions essential to the issue—and, in any case, in a matter that is entirely without "real" significance. And *this* is what Gillispie cites as casting doubt upon the cogency of Newton's statement that the shape of his spectrum reveals an inadequacy in the received laws of refraction![105]

I emphasize once more that Gillispie's misunderstanding is not chargeable to Lohne. If it stood as an isolated instance, it could be dismissed as a simple aberration. But it is far from an isolated instance. It is clear that Gillispie himself believes Newton's argumentation to have been (of course not totally, but) widely discredited; and that this belief has led him to *seek out* instances to cite against Newton. He has been encouraged to do so by a tone very widespread, and (I have argued) very misleading, in the historical literature of recent decades. It seems to me not inappropriate to refer to such a phenomenon as a "fashion," and a regrettable one; and believing what I have said to be true, it has seemed to me important to make the case. I should hope that this would no more be considered an attack upon historical work in general, or upon the character or motives of historians, than my criticisms of what I see as mistakes made about Newton by philosophers are regarded as an attack upon philosophical work in general or upon the character and motives of philosophers. I care very much about both these disciplines, and think it essential to their health—as to that of any field of scholarly work—that criticism be offered frankly and openly.

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[105]It might be asked what, in Lohne's discussion, could possibly have been interpreted by Gillispie as an objection to "the same assumption" that Marat objects to (the assumption, namely, that the incident beam of light is a parallel beam), or could have been read by Gillispie as attributing such an assumption to Newton at all. Lohne explicitly notes (ibid., p. 396) that Newton supposed the rays from the sun's upper and lower rims to deviate equally from "the ideal symmetry" with the prism (i.e., from the direction of minimum total deflection of a ray). Now, of course, this means equal deviation *in opposite senses.* I can only conclude that Gillispie thought it meant equal deviation in the *same* sense—which would make the rays in question parallel. Of course, Newton's only reason for specifying any assumption about a deviation is the fact that those rays are *not* parallel; he wants the beam as a whole to approximate as well as possible to the position of minimum deflection, and to secure that aim makes the extreme rays deviate equally, in opposite directions, from that position. Lohne objects to this as a less "natural" assumption than his own (namely, miimum deflection for the ray from the sun's center); and apparently Gillispie takes this as objecting to an assumption of parallelism.