28. Letter to Oldenburg, July 6, 1672, quoted by Cajori in the appendix to the *Principia*, p. 673. Note that this objection actually precedes the dispute over gravity.
29. See *Opticks*, pp. 404–405.
30. Letter to Boyle, February 28, 1678, in Thayer, pp. 112–116; Newton returns to those topics in Query 21 of the *Opticks*.
31. Second lettter to Bentley, January 17, 1693, in Thayer, p. 53; see also the advertisement to the second edition of the *Opticks* (p. cxxiii).
32. First letter to Bentley, December 10, 1693, in Thayer, p. 49.
33. F. Capra, *The Turning Point* (New York: Bantam Books, 1983).
34. This attitude first finds expression (I think) in Galileo's "Letter to the Grand Duchess Christina"; see Drake, esp. p. 183.
35. I use Grier's terminology; see his footnotes 7, 8, and 9.
36. For Reid's Newtonianism, see Laudan, chap. 7.
37. See R. I. G. Hughes, "Hume's Second Enquiry: Ethics as Natural Science," *History of Philosophy Quarterly* 2 (1985), 291–307.
38. First letter to Bentley, December 10, 1692, in Thayer, p. 46.
39. Capra, p. 66.
40. Ibid., p. 40.
41. *Opticks*, pp. 369–370.

Chapter 2

On Locke, "the Great Huygenius, and the incomparable Mr. Newton"

Howard Stein

The quoted phrase occurs in the Epistle to the Reader, prefatory to Locke's great *Essay*: imagining himself "censured" for "pretend[ing] to instruct this our knowing age," Locke acknowledges that such a purpose is indeed the only rational justification for publishing such a book, and proceeds to indicate the place he claims to fill in "the Commonwealth of Learning": "Everyone," he says, in a nicely gauged crescendo of admiration, "must not hope to be a Boyle, or a Sydenham; and in an Age that produces such Masters, as the Great Huygenius, and the incomparable Mr. Newton, . . . 'tis Ambition enough to be employed as an Under-labourer in clearing Ground a little, and removing some of the Rubbish, that lies in the way to Knowledge."

In recent years Locke's interest (and involvement) in the science of his time, and the influence of that science on his philosophy, have been the subject of much discussion. This seems to me a salutary historiographic development, and I should be pleased to have some of what I shall say here count as a contribution to that discussion; but my main concern is a bit different. I believe (and this, at least in our century, has not been a common opinion) that in the work of clearing the underbrush and removing obstacles to the advancement of knowledge—just as in the positive work of the advancement itself—Newton was nonpareil; and I wish to illustrate this claim, in part, by a comparison with Locke. But to prevent misunderstanding, let me emphasize at the outset that, notwithstanding Locke's characterization of this task of an "underlaborer," what we have to do with is nothing less than epistemology—or methodology—and metaphysics; my contention is that Newton attained, and deployed, in most intimate connection with his scientific work, conceptions both of method and metaphysics of a subtlety that has not been generally appreciated—conceptions from which there is still something to learn.

Besides the three mid-to-late seventeenth-century figures I have mentioned, two from the early part of the century are of great relevance to my subject: Descartes and Galileo. Huygens is sometimes described as a Cartesian; and, to be sure, he was—in his student years. In 1650, on hearing the news of Descartes's death in Sweden, the twenty-one-year-old Huygens wrote a verse elegy, whose concluding stanza is as follows:

> Nature, put on mourning—come and be first mourner
> For the Great Descartes, and show your despair;
> When he went into darkness, you lost your light—
> It is only by this torch that we have been able to see you.[1]

But his riper opinion, written some forty-odd years later, is very different, and deserves to be considered. In 1693, while making notes on Baillet's life of Descartes (chiefly to correct errors of fact concerning the Huygens family), Huygens comes to a passage referring to Descartes's poor opinion of the explanation offered by Gassendi of the phenomenon of "parhelia"; and is led to a disquisition several pages long on the development of modern natural philosophy. I quote from it:

> He rightly disdains the explanation of parhelia by M. Gassendi, which is mistaken; but the one he himself gives in his Meteors is ridiculous and very easy to refute . . .
>
> M. des Cartes had found the way to make his conjectures and fictions pass for truths. And those who read his Principles of Philosophy experienced somewhat the same as do the readers of Romances, which give pleasure and make the same impression as veritable histories . . . It seemed to me when I first read this book of the Principles that everything in it was as good as could be [que tout alloit le mieux du monde], and I believed, when I found some difficulty there, that it was my fault for not correctly understanding his thought. I was only 15 or 16 years old. But having since then discovered there from time to time things visibly false, and others highly improbable, I have retreated far from the predilection I then had, and at the present hour I find almost nothing that I can approve as true in all his physics, or metaphysics, or meteors.[2]

Descartes's physics is indeed bizarre. It is hard to understand how anyone who has actually read Descartes's *Principia* could believe (what is often asserted) that Descartes's program involved "the reduction of physics to geometry," or alternatively could claim, as a historian who really should know better has done, that "[the] form [of Descartes's physics] is precisely that of Newton's."[3] To be sure,

Descartes does say that he "neither accepts nor desires any other principles in Physics, than in Geometry or in abstract Mathematics";[4] and of course he maintains that the one "principal attribute" constituting the "nature of corporeal substance" is "extension in length, breadth, and depth";[5] but one will search long and hard to find anything like mathematical reasoning in the book.

What comes closest, there, to a principled foundation for mathematical arguments concerning the operations of nature is the set of rules Descartes gives "to determine by how much any body's motion is changed by coming into contact with other bodies."[6] In view of his insistence that contact with other bodies is the *only* circumstance occasioning changes of motion, such rules ought to form the basis for the theory of motion; and in view of his insistence that all the "diversity of forms" in matter is based upon motion,[7] these rules should in fact constitute the foundations of physics.

Now, the two Cartesian principles I have just mentioned (that all "diversity of forms" in matter fundamentally consists in arrangements of motions, and that the laws of motion are the laws of *change* of motion *through contact*) are the essential principles of what became known as the *mechanical*, or—under the designation introduced by Boyle—the *corpuscularian* philosophy. When this general view takes the more particular (and anti-Cartesian) form in which it is supposed that all matter is constituted out of ultimate, discrete, *indivisible* corpuscles—"atoms"—it is clear that there is entailed a definite program for fundamental explanation in physics: one must know the characteristics of the atoms; one must know their arrangement and motions in the particular systems of interest; and one must know the general laws of the communication of motion through contact—that is, the laws of impact.

But Descartes's seven rules of impact are thoroughly absurd;[8] and Descartes is driven to defend himself against the objection that these rules are in blatant conflict with experience, by maintaining that the rules hold only for two bodies colliding *in vacuo*, whereas all observed impacts occur within an ambient medium.[9] This is a remarkable defense: according to Descartes, the notion of two separated bodies *in vacuo* is a formal contradiction; and it is a rather odd thing to see particular predictions made of the various results to be expected under various self-contradictory conditions. In any case, Descartes himself not only offers an excuse for his rules—although at the same time maintaining their absolute certainty[10]—but also suggests that the reader may safely ignore them, since they are not needed to understand the rest of the work.[11] (Imagine telling a reader of Newton's *Principia* that the laws of motion are unnecessary for an understanding of the rest of the book!—so much for the "identity of form" of the two.)

It was clearly this farrago of incoherencies that first led Huygens to moderate his admiration of Descartes; indeed, within a year or two of his composition of the elegy from which I have quoted, Huygens was in full possession of the correct laws of the elastic central impact of nonrotating spheres. In the course of his critical remarks of 1693, he refers to Descartes's "laws of motion in colliding bodies, which"—Huygens says—"he thought to make pass for true by giving it out that all of his physics would be false if these laws were false." (It is well known that Descartes did make such a statement about his theorem that light is propagated instantaneously;[12] I know of no authority other than that of Huygens for a similar assertion about the rules of impact.) Huygens' comment on this mode of rhetoric is, "This is almost as if he wished to prove them by taking an oath."

I want now to describe briefly Huygens' two fundamental investigations bearing on the general laws of motion: that of impact (extending from 1652 to 1667), and that of centrifugal force (dating from 1659).

In the critical notes on Descartes already twice cited, Huygens gives a brief appreciation of anti-Aristotelian natural philosophy among both the ancients (Democritus, Epicurus, "and many others")—who, however, he says, "did not explain a single phenomenon in a satisfactory way"—and the moderns. Among the latter "Telesius, Campanella, Gilbert retained . . . many occult qualities, and had not sufficient inventiveness nor sufficient mathematics . . . ; no more did Gassendi . . . Verulamius [that is, Bacon] . . . taught very good methods for building a better [philosophy] by making experiments and putting them to good use . . . But . . . he did not understand Mathematics and lacked penetration for the things of physics." Only one figure comes in for unqualified praise: "Galileo had, in point of genius and of knowledge of Mathematics, all that is needed to make progress in Physics, and . . . he was the first to make beautiful discoveries concerning the nature of motion, although he left a great deal to be done. He did not have so much boldness or presumption as to pretend to explain all natural causes, nor the vanity to wish to be the head of a sect [as Descartes did]. He was modest and loved truth too much; moreover, he believed he had acquired sufficient reputation—and one that would endure forever—by his novel discoveries." Huygens, in effect, regards himself as the corrector of Descartes, and the continuator of Galileo. The results of Galileo are fundamental to both the investigations of Huygens that I have named.

Huygens based his general theory of impact on three principles: that of inertia,[13] that of Galilean relativity,[14] and the principle that "by a motion of bodies that results from their gravity, their common center of gravity cannot be raised."[15] That this last follows from the impossibility of "perpetual motion" Huygens well knew.[16] As to the principle of relativity, he had of course encountered it, in rough but pregnant form, in Galileo; and for Huygens himself it was associated with deep philosophical convictions about the nature of motion (the fact that Descartes's rules of impact violate this principle—although Descartes professes a quasi-relativistic theory of the nature of motion—was surely one of the defects Huygens early discovered in those rules).

In addition to these three general principles, Huygens relies crucially on Galileo's laws of falling bodies: (1) that the speed acquired by a body, in frictionless fall *in vacuo* through a height h—whether vertically, or along an incline, or under any other frictionless constraint—is the same for all bodies, and depends only on h: is in fact proportional to the square root of h; and (2) that the speed resulting from a fall through the height h is able—along any frictionless upward path *in vacuo*—to lift the body to the same height h.

With the help of these assumptions, Huygens succeeded—apparently in 1667[17]—in proving a very general result about the "encounter" of arbitrarily many bodies, of any nature whatever—elastic, inelastic, or partially elastic (Huygens' terms are "hard," "soft," and "semiresilient")—provided, at least, that no rotations are involved. The gist of his beautiful argument, in the case of two bodies and direct central impact, is the following: Let the interacting bodies be called A and B, and let A and B likewise denote their "sizes." Let their velocities before the interaction—reckoned as positive toward one side, negative toward the other—be u_A and u_B; those after the interaction, u'_A and u'_B. Then if g is the acceleration in free fall, the initial velocities can have been acquired in falling through heights $h_A = (u_A)^2/2g$, $h_B = (u_B)^2/2g$ respectively; and in the initial resting configuration of the bodies, the height of their center of gravity (above the level on which they come to interact) will have been $(Ah_A + Bh_B)/(A + B)$. Again, if the resulting velocities are used to lift the bodies, the heights they can attain will be $h'_A = (u'_A)^2/2g$ and $h'_B = (u'_B)^2/2g$, and the height of the center of gravity $(Ah'_A + Bh'_B)/(A + B)$. By Huygens' assumption, therefore, we must have: $Ah'_A + Bh'_B \leq Ah_A + Bh_B$; in other words, $A(u'_A)^2 + B(u'_B)^2 \leq A(u_A)^2 + B(u_B)^2$.

Note that we have, so far, used only Galileo's laws and the impossibility of the center of gravity rising above its initial (resting) value. Note too that we have made no special reference to *impact* (except for the presumption that all "fundamental" interaction *is* impact): the argument so far applies to *any* "irrotational" encounter. On the other hand, the result, having the form of an inequality, is rather weak. Huygens' really beautiful discovery was that this inequality can be made to yield a fundamentally important equation—"a wonderful law of Nature," he calls it ("une loy admirable de la Nature")[18]—by applying to it the principle of relativity. For let the same interaction be viewed by an observer moving with velocity v; to this observer the initial and final velocities of the bodies will appear to be $u_A - v$, $u_B - v$, $u'_A - v$, $u'_B - v$, respectively; and since the same laws must hold for the moving observer[19] as for one at rest, we must have:

$$A(u'_A - v)^2 + B(u'_B - v)^2 \leq A(u_A - v)^2 + B(u_B - v)^2;$$

i.e., after expanding the squares, canceling the terms $Av^2 + Bv^2$, which occur on both sides, and moving all terms involving v to one side, all others to the other:

$$2v(Au_A + Bu_B - Au'_A - Bu'_B) \leq A(u_A)^2 + B(u_B)^2 - A(u'_A)^2 - B(u'_B)^2$$

Now, the term on the right does not depend on v; and v can be taken to be positive or negative, and as large as we wish. It follows that this inequality will surely be violated for some choice of v, unless the expression in parentheses on the left is equal to zero.[20] So we have established that, under the very general conditions specified, $Au'_A + Bu'_B = Au_A + Bu_B$. Moreover, there is no difficulty at all in extending the argument to the case of n bodies, $A_1, \ldots, A_n$, with initial (vectorial) velocities $\bar{u}_1, \ldots, \bar{u}_n$, and final velocities $\bar{u}'_1, \ldots, \bar{u}'_n$, obtaining the result:

$$\Sigma A_i\bar{u}'_i = \Sigma A_i\bar{u}_i$$

—that is, the general principle of the conservation of momentum. (To complete the theory of the direct impact of two bodies, Huygens gives a supplementary relation which, with the former, suffices to determine u'_A and u'_B, the other quantities being given. For "hard" bodies, this relation is: $u'_A - u'_B = u_B - u_A$—the bodies separate with the same relative speed with which they approached;[21] for "soft" bodies: $u'_A = u'_B$—the bodies remain in contact. The corresponding supplementary relation for partially elastic bodies was discovered experimentally by Newton, and first announced in the *Principia*.[22])

Huygens' investigation of centrifugal force has a rather different character, since here the essential difficulty lay in finding the "appropriate" *definition*—that is, *concept*—of the "quantity" of centrifugal force. As in the case of impact, he finds a strategy that leads with utmost clarity and simplicity to the desired end—particularly remarkable here, since the notion of an "appropriate" definition is not itself a well-defined one.

The idea of Huygens' argument is this:[23] First, we may take as the paradigm phenomenon the tug exerted upon a string by a body held by that string on a uniformly rotating disk; we want to know "how strong a tug." Next, we reflect that a weight, hung vertically by a string, also exerts a tug, whose "strength" or "intensity" we may reasonably estimate by the magnitude of that weight; so we are led to ask whether some kind of equivalence can be established between these two sorts of phenomena. Now, Huygens remarks, weight is a "tendency" to *fall*; and, as Galileo has shown, and Riccioli and Huygens himself have confirmed by careful experiments, freely falling bodies—and also bodies that descend along inclined planes—move with a definite and uniform acceleration, the same for all bodies (in the vertical direction, or along a given incline; Huygens expresses this result not in terms of acceleration, but by the equivalent proposition that the distances covered in successive equal time intervals starting from rest are in the proportion of the successive odd numbers starting from unity). To be sure, this statement requires some amendment for bodies falling in air, as Huygens notes at once; but he adds that he needs, for his argument, only the fact that the proposition holds *in the limit of "arbitrarily small spaces"*—which in effect means that the acceleration at the instant of release from rest is nonzero, and the same for all bodies.[24]

The difference of acceleration in vertical fall and along an incline plays a critical role in the argument; more especially, the fact that the acceleration when the body is released, and the tug it exerts on the string when it is restrained, are both diminished—and, Huygens says, in the same proportion—when the inclination of the path is reduced.[25] Finally, to show that only the *initial* acceleration matters, Huygens asks us to consider a weight suspended in contact with a *curved*, rather than a plane, guiding surface; and in particular, one whose direction at the point of contact is vertical. On the one hand, no diminution of "apparent weight"—of "tug"—is experienced in this case. On the other hand, when the body is released, both its direction of motion and the magnitude of its acceleration vary continuously; but its initial acceleration is vertically downwards, and equal to that in free fall.

We now turn to the body held on the uniformly rotating disk. If released, it will (we here ignore gravity as well as air resistance) move along the line tangent, at the point of release, to the circle in which it had been going, at a constant speed, equal to the circumferential velocity of the rotation. *How will this look to an observer on the disk?* The answer is simple and elegant: the body will move, relative to the disk, along the curve that would be described by the endpoint of a thread, held taut, being unrolled from the disk as from a spool—and will move just as if the thread were being unwound at a uniform rate.[26] This curve—the *involute* of the circle—is easily seen to be perpendicular to the circle at the initial point of unwinding; and the body's motion along that curve is, at the initial instant, an accelerated motion starting from rest. Huygens proceeds to evaluate this initial acceleration; without expressing the result explicitly in this form, he shows in effect that if the radius is r and the circumferential velocity v, the initial acceleration relative to the observer on the disk will be v^2/r. So for the rotating observer—or for the string—the *restrained* body is restrained from yielding to its tendency to accelerate radially outward to that degree. "But," he says (implicitly invoking what Clifford Truesdell and his associates have called "the principle of material frame-independence"), "this tendency is entirely similar to that by which heavy bodies suspended from a thread strain to go downwards";[27] and he concludes that the degree of this "straining"—that is, the centrifugal force—is, like the force of the weight, proportional both to the acceleration striven for and to the "size" of the body.

Thus Huygens' argument leads to a result that can be immediately generalized as follows: If, from the point of view of an observer (in no matter what state of motion), a body of "size" or "mass" m is held stationary by a string; and if, in the same conditions, but released from the string, the body would move (relative to that observer) with initial acceleration a in a given direction, then the tug on the string holding the body will be in that direction, and of a magnitude proportional to ma.[28]

I have said earlier that a consequence of Huygens' discussion of *interactions* of bodies is the general principle of the conservation of momentum. The result just formulated looks temptingly like Newton's second law of motion. Can we say that the essential contents of the introductory material of Newton's *Principia*—the "Definitions" and the "Laws of Motion"—were in Huygens' possession many years before Newton published them?

The answer, I think, is not entirely simple—not an unqualified yes or no. Newton himself, it should be emphasized, did *not* claim originality for the definitions and laws: he says, "Hitherto I have laid down such principles as have been receiv'd by Mathematicians, and are confirm'd by abundance of experiments"[29] (and refers in particular to Galileo, Sir Christopher Wren, John Wallis, and Huygens). But, as I shall explain presently, I believe that those Newtonian principles,

as Newton conceived them in the mid-1680s, involved a quite radical shift from the "receiv'd" view—and from what had previously been the view of Newton himself; yet I also believe that Huygens had come closer to them than has generally been recognized.

One fundamental point in respect to which this is so, in my opinion, is the notion of *mass*. No commentator I have encountered considers Huygens to have had a clear conception of mass (as distinguished from weight); and yet just this distinction—between the weight of a body on the one hand, which Huygens does *not* regard as an intrinsic or invariable property of the body, and the quantity that enters into the laws of impact (and of centrifugal force) on the other, which he does so regard—is explicitly affirmed, and argued for, by Huygens in several places, of which the earliest I know dates from 1668.[30] Thus Huygens' law of the conservation of momentum is indeed precisely that of Newton (Corollaries III and IV to the laws of motion).[31]

Just as Huygens succeeded in generalizing his earlier result about direct collision of "hard" bodies to (more or less) arbitrary interactions, so he also undertook to generalize the argument underlying his analysis of centrifugal force. There is a short manuscript of 1675 or 1676[32] in which Huygens considers "the force that acts upon a body to move it when it is at rest, or to augment or diminish its speed when it is in motion." For this kind of force he introduces the term *incitation*; and he argues that incitations deriving from diverse causes—, "weight, elasticity [or 'spring': *ressort*], wind, magnetic attraction"—can be compared quantitatively ("may be equal, the one to the other"). He makes quite clear how this quantitative comparison is to be effected: just as in the case of centrifugal force,[33] equality of incitations can be gauged by the fact that the one is just able to balance the other (and *identity* of "the one" and "the other" is identity, in a suitable senses, of the *state* of the causal agent: "same weight, suspended freely"; "same stretch of same spring"; and so on). The piece ends with a problem and a general principle:

(1) The principle (or "hypothesis"): Equal bodies, constrained to move in equal (that is to say, congruent—in general possibly curved) lines by incitations that are equal at corresponding points of the two paths, will describe their paths in equal times.

(2) The problem: Find the times required to traverse equal spaces, (a) for different incitations acting upon equal bodies, (b) for equal incitations acting upon unequal bodies.[35]

There can be no question but that Huygens was in a position to solve this problem with ease—indeed, the argument of his treatise on centrifugal force *does* solve it, *mutatis mutandis*; and I have no doubt that when Huygens wrote this interrupted manuscript note he knew the solution. But he did not bother to finish the note, just as he did not bother to incorporate the law of the conservation of momentum in the polished manuscript (published posthumously in 1703) of his treatise on impact. The implication seems clear: Huygens did not regard these results—even his "loy admirable de la Nature"—as of an importance comparable with the laws of impact (including that of the conservation of *vis viva*[36] in perfectly elastic impact) or the laws of centrifugal force. And the reason seems clear: the laws of impact *are* the fundamental laws of the communication of motion; the laws of centrifugal force are relevant to all processes that involve *vortex* motion—central, on the one hand, to Descartes's cosmology, and, on the other, to the theory of *gravitation* that Huygens developed (in opposition to Descartes) as early as 1668.[37] The results that come so near to the Newtonian concept of force and laws of motion are, by contrast, of subordinate interest to Huygens. For the task of physical investigation, according to him, is that of studying phenomena with a view to their "mechanical explanation": that is, the development of "hypotheses by motion"[38]—*models,* as we would say, in which all interaction is by pressure or impact—that account adequately for phenomena; which hypotheses are to be treated by testing their observable consequences.[39] In his *Treatise on Light,* Huygens refers to the principles of this program—that is to say, of the "corpuscularian" philosophy—as "the principles accepted in the Philosophy of the present day"; and, a little later, as "the true Philosophy, in which one conceives the causes of all natural effects in terms of mechanical motions"—adding "which in my opinion it is necessary to do, or else renounce all hope of ever understanding anything in Physics."[40]

Let us turn now to the incomparable Newton. At the beginning of his career, in the mid-1660s, Newton seems to have shared Huygens' view of the nature of fundamental explanation: "the lawes of motion" are the laws of impact,[41] and indeed impact of "absolutely hard" bodies;[42] as to *force,* it is "the pressure or crouding of one body upon another."[43] On the other hand, Newton held, already in this early period, a very different philosophy of physical inquiry from that of Huygens. This is exemplified in the optical investigations, in which—to the bafflement of many of his readers, most notably Hooke and Huygens—he claimed (and justly) to have proved with what may be called "experimental certainty,"[44] and quite independently of any

mechanical explanation of the nature of light, a set of propositions *about* that nature of far-reaching importance, and of a quite unprecedented character. The essential content of these propositions may be summarized as follows:

(1) "*Light* consists of Rays differently refrangible."[45] Here by "ray" is meant a "part" of light, propagated in a determinate *line*. A beam of light whose rays are of equal refrangibility Newton calls "homogeneal, similar or uniform."[46] The existence of (a continuous range of) such "kinds" of light, and their distinction from ordinary light (which is "heterogeneal"), was established by Newton in one crucial experiment.[47]

(2) "Homogeneal" light possesses a number of intrinsic—or, as Newton says, "*Original* and *connate*"—properties: properties, that is, which cannot be altered by any action exerted upon the light. Among these unmodifiable properties are the refrangibility itself, by which the homogeneous kinds were first distinguished; the "disposition to exhibit this or that particular colour";[48] and—although Newton did not fully discuss this point for some years after his first publication—a particular magnitude, of the nature of a *length* (what we know as the wavelength).[49]

(3) The colorific effect—the visual appearance, or "species" (as Newton calls it)—of light that comes to the eye, is determined by its physical constitution: that is, by the proportions in which it is constituted of the several kinds of rays; there is thus a *mapping* from the set of all possible physical constitutions to the set of perceptible species. But this mapping is by no means one-to-one: many different composite lights cause the same optical perception; and, indeed, Newton says, colors that are "the same in *Specie* with"—that is, indistinguishable by the eye from—those produced by homogeneous light, "may be also produced by composition."[50]

Now I want to call attention to one small point of terminology. Having asserted that although the colors associated with the kinds of homogeneous light are immutable, "seeming transmutations of Colours may be made," because "in . . . mixtures, the component colours appear not," but are "latent,"[51] Newton concludes: "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."[52]

The association of the terms *primary, original,* and *simple* had a clear connotation for Newton's audience: *primary* and *original* are approximate philosophical synonyms, denoting a fundamental cause or *principle* (Latin *principium,* rendering Greek ἀρχή, literally "beginning" or "origin"); *simple* denotes a *primary constituent* or "element." All three of these terms, of course, play a most prominent role in Locke's *Essay.* Here is a central passage:

> Qualities . . . in Bodies are, First such as are utterly inseparable from the Body, in what estate soever it be; such as in all the alterations and changes it suffers, all the force can be used upon it, it constantly keeps; and such as Sense constantly finds in every particle of Matter, which has bulk enough to be perceived, and the Mind finds inseparable from every particle of Matter, though less than to make it self singly be perceived by our Senses . . . These I call *original* or *primary Qualities* of Body, which I think we may observe to produce simple *Ideas* in us, viz. Solidity, Extension, Figure, Motion, or Rest, and Number.[53]

I do not at all mean to suggest that Locke has borrowed these terms from Newton. It is now generally supposed, on quite plausible evidence, that Locke's usage in distinguishing qualities into "primary" and "secondary" follows that of Boyle; but my point is that the words belonged to a well-understood philosophical vocabulary, common to the tradition—that is, to *all* the philosophical traditions—from before the time of Aristotle. That this core meaning (that of the "fundamental" and "fundamentally causal")—rather than the notorious distinction between ideas that are "resemblances" of the qualities in things and those that "have no resemblance" of the qualities or "powers" that produce them[54]—is what was central for Locke, appears not only from the passage I have quoted (in which the notion of primary qualities is first introduced), but still more plainly from one of the early drafts of the *Essay,*[55] where no issue of "resemblance" is mooted at all, and where Locke suggests that the only "primary ideas belonging *originally* to bodies"[56] are "extension and cohesion of parts"—giving as his reason that "all the other qualities we observe in, or ideas we receive from, body . . . are *probably* [my emphasis] but the results and modifications of these."

Having perhaps labored this point a bit, let me dwell yet a little further on Locke and primary qualities, to offer an interpretation of that perplexing claim about "resemblance," which provided Berkeley with one of the three major openings for his assault on Locke's philosophy (the other two being Locke's discussions of "abstract" or "general" ideas and of "material substance"). Berkeley's criticism can be paraphrased (or parodied) thus: Locke says that colors, for example, are not in bodies as we perceive them (the colors), but that shapes, for example, are; is he trying to tell us that in point of color bodies do not, but in point of shape they do, *really look* the way they look to us?—That of course would be arrant nonsense. To be sure, philosophers have all too often been guilty of just such nonsense, and I am far from certain that Locke is entirely innocent of it. But I want to suggest that at least a part of what Locke meant by his claim is this (which is not nonsense): that our "ideas" of the primary qualities fit together in a nexus of relations—a structural nexus—that represents "adequately," "faithfully," or "isomorphically" a corresponding nexus in things, at all levels of analysis—most particularly on the fundamental level as conceived in the corpuscularian philosophy, that of the ultimate constituent corpuscles. This, I think, is not a far-fetched, it is even a quite natural rendering (to be sure into a somewhat non-Lockean vocabulary), of his proposition: "That the *Ideas of primary Qualities* of Bodies, *are Resemblances* of them, and their Patterns do really exist in the bodies themselves."[57] On this reading, "resemblance of a pattern" will mean *correspondence to it, point for point* (and relation for relation).

But Locke's claims for primary qualities extend farther than this: not only does he, on the positive side, assert that the ideas of those qualities correctly represent a corresponding metaphysical reality; he also maintains—although (oddly enough, since it is a central tenet of his epistemology) merely as "probable"—that the *only* representations we can form that correspond to a metaphysical reality are those constructed out of ideas of primary qualities. Or rather, Locke throughout the *Essay* in general maintains this; but there are two minute exceptional passages—the results of revisions made, one in the second, one in the fourth edition. I shall have something to say about these passages presently.

There are three points of contrast between Newton and Locke that I want here to call attention to (with two more to come later): First, the qualities Locke calls *primary* and *original* constitute a fixed list; it is clear that Newton's usage is more flexible, since the qualities he attributes to rays of light as primary and original are ones that have been discovered by his experimental investigation itself (and therefore it is reasonable to suppose that further investigations may discover new "primary qualities"). Second, the primary qualities of Locke correspond, in principle, to a subset of "the simple *Ideas* we receive from Sensation and Reflection"—themselves "the Boundaries of our Thoughts; beyond which, the Mind, whatever efforts it would make, is not able to advance one jot; nor can it make any discoveries, when it would prie into the Nature and hidden causes of those *Ideas*."[58] The properties called "primary" by Newton are, on the contrary, of the class that Locke calls "secondary Qualities, mediately perceivable"[59]—namely, "powers" that manifest themselves only in the perceptible consequences of *physical interaction*—which are not apprehended in any purely passive perception at all.

It might have been suspected that the first of these contrasts was merely terminological, and showed no more than that Newton and Locke differed in their use of the word *primary*. The second contrast, however, makes it clear that a great deal more is at issue. Further insight into what this "more" is is provided by the third contrast, which will require a little more exposition.

In Newton's letter informing the Royal Society of his first optical investigations there occurs, at the point of transition from the account of his discovery of the differences of refrangibility to that of the theory of "the *Origin of Colours*," the cryptic remark that although "a naturalist would scearce expect to see ye science of those [namely, colors] become mathematicall," Newton yet "dare[s] affirm that there is as much certainty in it"—that is, in his mathematical theory of the origin of colors—"as in any other part of Opticks."[60] To this passage Hooke demurred, replying that he could not agree that Newton's theory was "soe certain as mathematicall Demonstrations."[61] In reply, under the heading "That the Science of Colours is most properly a Mathematicall Science," Newton offered a fundamental clarification.[62] His assertion, he says, contained two parts: that his science of colors is mathematical; and that it is as certain as any other part of optics. But this is not to say that this science—any more than the rest of optics—possesses *mathematical certainty*. A science is mathematical, he says, if *from its principles* "a Mathematician may determin all the Phaenomena" it is concerned with; but the "absolute certainty" of any science "cannot exceed the certainty of its Principles"; and "who knows not that Optiques and many other Mathematicall Sciences depend . . . on Physical Principles"—that is, principles "the evidence [for] which . . . is from *Experiments*." (It will be recalled that in the preface to the *Principia* Newton says the same about geometry itself: that its principles are "fetched from without"; that "Geometry is founded in mechanical practice." I am not aware of any other mathematician or philosopher of the seventeenth century who expressed such a view.[63] Locke quite certainly did not; and neither, more than half a century later, did that arch-empiricist Hume.)

Now, Locke's doctrine about *knowledge*—his "official" doctrine, let me say—is this, that there are "three degrees of Knowledge, *viz.*, Intuitive, Demonstrative, and Sensitive";[64] of which the first two alone, which "we must search and find only in our Minds," can be "universal," or "general",[65] whereas sensitive knowledge reaches "no farther than the Existence of Things *actually present to our Senses*."[66] In accordance with this theory of knowledge in the strict sense of the word, Locke repeatedly—but, as it happens, not quite uniformly—denies the very possibility of a *scientific natural philosophy*.[67] The exceptions are all passages in which Locke refers explicitly to Newton. In the *Essay* we find, perhaps ambiguously: "Mr. *Newton*, in his never enough to be admired Book, has demonstrated several Propositions, which are so many new Truths, before unknown to the World, and are farther Advances in Mathematical Knowledge."[68] The possible ambiguity is suggested by the last clause: Locke may refer only to what in the *Principia* Newton calls "mathematical," as contrasted with "philosophical," principles. I am inclined to doubt that he does intend that distinction, because Locke's own interest in the *Principia* certainly did not derive from its purely mathematical content. In any case, the other passages in question admit of no such doubt. In his "Thoughts concerning Education," published in the same year as the *Essay*, one does indeed find the statement: "Natural philosphy, as a speculative science, I imagine, we have none; and perhaps I may think I have reason to say, we never shall be able to make a science of it."[69] But some four pages later Locke qualifies this:

> Though the systems of physics that I have met with afford little encouragement to look for certainty, or science, in any treatise, which shall pretend to give us a body of natural philosophy from the first principles of bodies in general; yet the incomparable Mr. Newton has shown, how far mathematics, applied to some parts of nature, may, upon principles that matter of fact justify, carry us in the knowledge of some, as I may so call them, particular provinces of the incomprehensible universe. And if others could give us so good and clear an account of other parts of nature, as he has of this our planetary world, and the most considerable phaenomena observable in it, in his admirable book "Philosophiae naturalis Principia Mathematica," we might in time hope to be furnished with more *true and certain knowledge* in several parts of this stupendous machine, than hitherto we could have expected.[70]

A second quite definite statement occurs in the public correspondence with Stillingfleet, and bears upon one of the two revisions to Locke's *Essay* that I mentioned earlier. The change is to Book II, chapter 8, §11, which in the first three editions asserted it as "manifest" that "*Bodies operate* one upon another . . . *by impulse*, and nothing else. It being impossible to conceive, that Body should operate on what it does not touch, . . . or when it does touch, operate any other way than by Motion"[71]—thus succinctly expressing the basic tenet of corpuscularianism—and to the first sentence of §12, which began, "If then Bodies cannot operate at a distance . . ." In Locke's letter to Stillingfleet he says, referring to this passage:

> It is true, I say, "that bodies operate by impulse, and nothing else." And so I thought when I writ it, and can yet conceive no other way of their operation. But I am since convinced by the judicious Mr. Newton's incomparable book, that it is too bold a presumption to limit God's power, in this point, by my narrow conceptions. The gravitation of matter towards matter, by ways inconceivable to me, is not only a demonstration that God can, if he pleases, put into bodies powers and ways of operation, above what can be derived from our idea of body, or can be explained by what we know of matter, *but also an unquestionable and every where visible instance, that he has done so.* And therefore in the next edition I shall take care to have that passage rectified.[72]

The emendation Locke actually introduced in his fourth edition is characteristically cautious—to the point, indeed, of *hiding* the issue. In §11 he stills says that "the only way which we can conceive bodies operate in" is "by impulse"; he concludes that this is, manifestly, "how *Bodies* produce *Ideas* in us"; only he makes no statement, now, about the operation of bodies *upon one another*—and, similarly, he drops from §12 the clause about bodies not operating at a distance. I think there is no explicit reference to gravitational attraction in the *Essay*.

There is, however, such a reference in a work originally intended to form a new chapter of the *Essay*: the posthumously published treatise *The Conduct of the Understanding*; and this passage may, I think, fairly be characterized as astonishing. It occurs in section 43, whose title is "Fundamental Verities"; and one might think that a very famous piece of eloquence in Kant's *Critique of Practical Reason* was derived from it. "There are," Locke tells us, "fundamental truths that lie at the bottom, the basis upon which a great many others rest, and in which they have their consistency. These are teeming truths, rich in store, with which they furnish the mind, and, like the lights of heaven, are not only beautiful and entertaining in themselves, but give light and evidence to other things that without them could not be seen or known." He gives just two examples of such "fundamental," "teeming" (that is, pregnant) truths. The first is "that admirable discovery of Mr. Newton, that all bodies gravitate to one another, which may be counted as the basis of natural philosophy; which of what use it is to the understanding of the great frame of our solar system, he has to the astonishment of the learned world shown, and how much further it would guide us in other things, if rightly pursued, is not yet known." And what is the second fundamental truth, deemed worthy by Locke of comparison with Newton's admirable discovery? It is "our Saviours great rule, that *we should love our neighbor as ourselves*": —this "is such a fundamental truth for the regulating of human society, that I think by that alone one might without difficulty determine all the cases and doubts in social morality." (It should be remembered that in the *Essay* Locke has affirmed his belief that morals—like mathematics, and *unlike* natural philosophy—could be made a demonstrative science.)

The third contrast I have drawn, then, is between two different conceptions of what is required for a systematic science. Locke's curious ambivalence on the point—why does he not, although admiring Newton's discoveries, refuse to allow them the name of "science" in its strict sense, on the grounds that they do not rest on principles evident to intuition?—suggests that he is less firmly committed to his "official" epistemology (and metaphysics) than most of his commentators take him to be. For instance, Michael Ayers, in a very acute and stimulating article on Locke,[73] has declared that "if he let in the possibility that powers or phenomenal properties should belong to things as a matter of brute or miraculous fact not naturally intelligible, Locke's whole carefully constructed philosophy of science and his support for the corpuscularian case against the Aristotelians would collapse."[74] But this possibility—indeed the certainty that it is true, in the case of the power of gravitational attraction—is exactly what, in the passage quoted above, Locke tells Stillingfleet that Newton has convinced him of.

The two issues—whether "primary" qualities, or at least our *knowledge* of primary qualities, need be directly and simply related to our modes of perception; and whether science need be grounded in what is immediately perceived ("intuited") by the mind in contemplating its "ideas"—are very closely connected. It has been characteristic of an influential branch of modern empiricism to adopt the affirmative position on each of these issues; and thus to deny a central distinction of that great ancient empiricist Aristotle: the distinction between what is "first, and better known, in nature" and what is first, and better known, "to us." It is possible that a recognition of such a distinction is implied by one cryptic (and to me puzzling) remark by Locke in the *Essay*: "'Tis fit to observe," he says, "that Certainty is twofold; *Cer-*

tainty of Truth, and *Certainty of Knowledge*. *Certainty of Truth* is, when Words are so put together in Propositions, as exactly to express the agreement or disagreement of the *Ideas* they stand for, as really it is."[75] What is the difference Locke implies here between what he calls "certainty of truth"—which does *not* imply certain *knowledge*—and just "truth"? His emphasis seems to be on the phrase "as it really is"; and this may, although it surely is obscure, be a remote echo of Aristotle's "better known *in nature*."[76] However this may be, it is certainly characteristic of what I have called Locke's "official" doctrine to conflate—so far as humanly possible science is concerned—epistemological priority and metaphysical priority, and to find in the evident inadequacy of this correspondence an impassable boundary to human knowledge.

On this issue, then, Newton may be said to stand with Aristotle. Not, however, on another—(and closely related)—one; for Aristotle and Locke—and Descartes, and, to a degree, Huygens—agree on this point: that genuine science is possible only on the basis of genuinely, "absolutely," first principles (of which Aristotle says—and indeed of "causes" in general, whether "first" or "intermediate"—that there must be *in toto* a *finite* and *known system* of causes if there is to be science at all).[77] Descartes thought he had definitively established such a system through an analysis of qualitative experience in the light of principles innate to the mind. Huygens came to reject most of Descartes's analysis, and to regard fundamental physical knowledge as something to seek on probable, rather than certain, evidence; but, as we have seen, he continued to hold firmly to the view that the whole possibility of progress in physics depended upon the reduction of all phenomena to "mechanical" interaction "by motion." Huygens offers us no argument on this point; Locke, the mere assertion that this is the only mode of action that the "idea of body" makes intelligible. Newton, however, no more believes in the intrinsic and unique "intelligibility" of this mode of interaction than does Hume.[78] Newton writes:

> We no otherwise know the extension of bodies than by our senses, nor do these reach it in all bodies; but because we perceive extension in all that are sensible, therefore we ascribe it to all others also. That abundance of bodies are hard we learn from experience. And because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles not only of the bodies we feel but of all others. That all bodies are impenetrable, we gather not from reason, but from sensation . . . That all bodies are moveable, and endow'd with certain powers (which we call the *vires inertiae*) of persevering in their motion or in their rest, we only infer from the like properties observ'd in the bodies which we have seen. The extension, hardness, impenetrability, mobility, and *vires inertiae* of the whole, result from the extension, hardness, impenetrability, mobility, and *vires inertiae* of the parts: and thence we conclude the least particles of all bodies to be also extended, and hard, and impenetrable, and moveable, and endow'd with their proper *vires inertiae*. And this is the foundation of all philosophy.[79]

The "foundation of all philosophy" (by which he means "of all physics")—that part, we may say, of metaphysics, that is required for physics—is itself founded on, or derived from, not rational insight, but empirical evidence. The fourth of the contrasts I wish to draw between Locke and Newton concerns this metaphysics itself (rather than its epistemological grounds). It is related to the second revision to Locke's *Essay* known to have been made under the influence of Newton.

In the first edition of the *Essay*, in attacking the argument that matter must be eternal because its creation *ex nihilo* is inconceivable, Locke had countered that the creation of a mind or "spirit" is as much beyond our comprehension as that of a body.[80] In the second edition this was revised to say that in fact, "if we would emancipate ourselves from vulgar Notions," we might be able to form "some dim and seeming conception how Matter might at first be made . . . : But to give beginning and being to a Spirit, would be found a more inconceivable effect of omnipotent Power." He declines, however, to particularize, saying that to do so "would perhaps lead us too far from the Notions, on which the Philosophy now in the World is built."

We know from Locke's French translator, Pierre Coste, that the intimated radical departure from the received philosophy was sketched to Locke, in conversation, by Newton;[81] and we now have, in the fragmentary manuscript "De gravitatione et aequipondio fluidorum et solidorum in fluidis," published by the Halls[82] (together with, unfortunately, a most defective translation), Newton's own exposition of this heterodox metaphysics. I have discussed this elsewhere in more detail;[83] here let me only remark, first, that Newton's explicit aim, in his discussion of "corporeal nature" in that text, is to free the conception of body from two scholastic notions he regards as unintelligible: that of a *formless substrate*, and that of a "substantial form" inhering in such a substrate (or "unintelligible substance") as subject; second, that what he offers instead of the "unintelligible substance" and its unintelligible "substantial form" is the conception of *spatial distributions of clearly conceived attributes*—space or extension, of which (Newton says) we have "an exceptionally clear Idea," playing the role of "subject," and the distributed attributes that of "form"; third, that it is a crucial part of this view of bodies as constituted by "spatially distributed attributes"—what we should now call "fields" on space—that these have, as part of their formal constitution, definite "laws of motion," or of propagation from one region of space to another; fourth, corresponding to Locke's remark that the creation of a mind is *more inconceivable* than that of a body, that Newton, although he suggests the possibility of an analogous conception of God himself, free of any unintelligible notion of a "substantial subject" in which his attributes inhere, quickly adds that the defect of our ideas of God's attributes and even of our own mental powers makes it "rash to say what may be the substantial basis of minds."

From the "official" point of view of Locke's *Essay*, the creation of a substance exceeds human comprehension precisely because, in that view, the "idea" of such a substance inescapably includes what Locke himself characterizes as the to us necessarily obscure and confused "idea of substance in general."[84] The "Philosophy now in the World"—the corpuscularian philosophy—while rejecting "prime matter" and "substantial forms" (or "occult qualities"), still required that obscure idea of substance. And this Newton's more radical metaphysics contrives to do without.

It is of some importance to note that the metaphysics of "De gravitatione" attributes to bodies, alongside Locke's "primary and original" attributes of extension, solidity (or impenetrability), and mobility,[85] two others, not on Locke's list: namely, mass or *vis inertiae*, and the power of stimulating perceptions in a mind. To be sure, mass is not there mentioned explicitly. It is, however, implied by the requirement that there be definite laws of motion of the impenetrable regions of space; and this may serve to remind us that Locke's own corpuscularian theory of the "intelligible" interaction "by impulse" presupposes this attribute of mass—which cannot be construed to correspond to a simple idea, but can only be understood as a power "mediately perceivable." In this sense, even apart from the questionable "idea of substance," the Lockean view defended by Ayers has to be regarded as not fully coherent.

The account of the creation of matter in "De gravitatione" is parallel to the much better known passage on the same subject in Newton's *Opticks*.[86] The latter, with its reference to "solid, massy, hard, impenetrable, moveable Particles," is reticent about the deeper metaphysical foundations; but the correspondence is made evident by a cryptic phrase which, I suggest, becomes clear when the two accounts are juxtaposed. Newton says that the "principles" he has spoken of are to be regarded, "not as occult Qualities, supposed to result from the specific Forms of Things, but as general Laws of Nature, *by which the Things themselves are form'd*" (my emphasis). What does this last phrase mean? It means precisely that Newton proposes to replace, as constituting the essence of corporeal things, the "occult qualities" and "substantial forms" of the scholastics with *clear* forms, specifiable as "laws of nature" whose truth is evinced for us by phenomena. (There is actually a little more that needs to be said for a careful analysis of Newton's statement, but I have not time for that here; the main qualification will be implied by my concluding remarks—and last contrast with Locke.)

In one positive point the passage in the *Opticks* differs significantly from that in "De gravitatione." The "principles," or constitutive laws of nature, that Newton most especially means to defend against the charge that they are "occult" are, not those of solidity, impenetrability, rigidity, mobility, and inertia, with the "passive Laws of Motion" that characterize the *vis inertiae*, but a new set, not at all appearing in "De gravitatione": "certain active Principles, such as is that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies." This very crucial amendment is the result of the great investigation that gave us the *Principia*.

That investigation may be succinctly described as having three phases. In the first phase—already partially accomplished in the famous plague years of 1665–66—Newton found, on the basis of Kepler's so-called third law and the approximation of planetary motion as uniform and circular, that (to use Huygens' word) the "incitations" of the planets toward the sun are inversely as the squares of the distances; and found also, transferring this law to the moon, that the latter's "incitation" toward the earth can be identified with its *weight* toward the earth. With this result, and with the further inference that (a) the "incitation" of the planets also is to be identified with their *weight toward the sun*, and that (b) weight in general obeys this law of the inverse square, Huygens declared himself in full agreement; intimated, not unjustly, that he himself could have made this discovery if he had had the boldness to consider the possibility of weight acting at such vast distances; and expressed his great admiration of Newton for having done so.[87] But Newton's move in what I am calling the second phase is far bolder, and is the turning point of the investigation: Quite setting aside the standard view that the fundamental laws of motion apply to the fundamental interactions "by impulse," Newton treats the gravitation of a body *A* toward a body *B* as a case of *interaction* between *A* and *B*, and applies directly to it his own version of Huygens' "wonderful law of Nature," the conservation of momentum—namely, Newton's third law of motion. From this, with some qualitative considerations regarding weight, he infers with breathtaking swiftness the existence of a *universal* law of gravitation between all pairs of corporeal particles in the universe.[88]

The third and final phase of the investigation takes up the major part of Book III of the *Principia*—and the major part of astronomy for the next two-hundred-odd years. It consists in the deduction of the detailed consequences of Newton's new law, and their detailed comparison with increasingly precise data. It is, as I have remarked elsewhere,[89] on the success of this third phase that Newton rests his case for the correctness of his result: the critical second phase of the investigation must be regarded as *heuristic*, not as *demonstrative*.

But the third phase—or its success—had in turn a heuristic consequence of profound importance: it led Newton to the amendment of his metaphysics that I have already cited, and therewith to a new program for physics. This is adumbrated in the introductory sections of the *Principia*, preceding Book I: the "Definitions" and "Laws of Motion"; and is summarized in Newton's preface to the work. Its central notion is that of a *vis naturae* or *potentia naturalis*—a "force of nature" or "natural power." The three laws of motion (as Newton makes explicit in the *Opticks*) constitute, together, the characterization of one of these forces: the "*Vis inertiae*," which is "a passive Principle."[90] The remaining forces, the "active Principles," are what, according to this program, it is the chief task of natural philosophy to seek to discover. And it is within this scheme that the deep importance of Huygens' "incitation," which Newton calls "motive force," really emerges: for the *action upon a body* of a force of nature—what Newton calls an *impressed* force—has this "incitation," or "motive quantity," as its appropriate measure; the latter therefore enters into the expression of every fundamental law of nature.

The account I have just sketched illustrates the last of the contrasts I wish to make with Locke, and with the received corpuscularian view generally. Perhaps it is already implied by the other four contrasts. Artistotle, as I have said, thought that the very possibility of science depends upon the possession of first principles; and on this point, as I have also said, Locke and Huygens agree with him; but Newton does not. In Aristotle's view, first principles themselves are discovered in a process that *precedes* science—a process he describes as

"dialectical" rather than "scientific." In these terms (which are foreign to Newton's own usage), Newton may be said to agree rather with Plato, for whom science itself was "dialectical." The double-facedness of the result in the *Principia*—"forward" to the greater mastery of phenomena, "backward" to new principles—is a perfect illustration. But that it is a genuine illustration of a philosophy of inquiry that reigned over Newton's entire creative career is apparent from his writings, both early and late. In his inaugural lectures as Lucasian Professor at Cambridge in 1670 (when he was not yet twenty-eight), Newton declares, concerning his "mathematical science of colors": "I hope to show—as it were, by my example—how valuable Mathematics is in natural Philosophy. I therefore urge Geometers to investigate Nature more rigorously, and those devoted to natural science to learn Geometry first. Hence . . . truly with the help of philosophizing Geometers and geometrizing Philosophers, instead of the conjectures and probabilities that are hawked on all sides, we shall at last achieve a natural science supported by the highest evidence."[91] In the final query of the *Opticks*, to which I have already referred, and which first appeared in the Latin edition of 1706 (when Newton was approaching sixty-four), after proposing the scheme of passive and active forces (or laws), Newton conspicuously refrains from claiming for these any *final*, any *ultimately "foundational"* status: "To tell us," he says,

> that every species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phaenomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd. And therefore I scruple not to propose the Principles of Motion above-mention'd, they being of very general Extent, and leave their Causes to be found out.[92]

Perhaps the simplest of all Newton's statements of what I have called his dialectical conception of science is that in the preface to the *Principia*. As in the other passages I have cited, here too what Newton offers is not a proposed *foundation* for physics, but a *framework within which physical investigation may be possible*. But the subtlest dialectical turn is Newton's intimation—which has been dramatically confirmed in our own century—that such investigation may lead, not only to new laws and deeper causes, but to a revision of the framework itself. After setting forth his new program for natural philosophy, Newton concludes—in words that I have quoted on more than one previous occasion, and which I have always found moving—"But I hope the principles here laid down will afford some light either to that, or some truer, method of Philosophy."

Notes

1. Christiaan Huygens, *Oeuvres complètes de Christiaan Huygens,* Société hollandaise des Sciences, vol. I (La Haye: Martinus Nijhoff, 1888), p. 125; the quoted stanza also appears ibid., vol. XVI (1929), p. 4.
2. Ibid., vol. X (1905), pp. 402–406.
3. J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley: University of California Press, 1979), p. 31.
4. René Descartes, *Principia Philosophiae,* pt. II, ¶64.
5. Ibid., pt. I, ¶53; pt. II, ¶4.
6. Ibid., pt. II, ¶45.
7. Ibid., ¶23.
8. Ibid., ¶¶46–52. The first rule is correct for the elastic impact of equal bodies colliding "symmetrically"; the third, the fourth, and the first part of the seventh are correct for perfectly inelastic impacts of the three sorts they treat; the remaining ones are never true. And the relationships among these rules for the several cases are quite inconsistent with the theory of the nature of motion itself professed by Descartes in ¶¶25–30; for instance, the case in which a smaller body in motion strikes a larger one at rest (the fourth rule, ¶49), and that in which a larger body in motion strikes a smaller one at rest (the fifth rule, ¶50), are treated as fundamentally different, although ¶29 has told us that "we cannot understand a body AB to be transferred from the vicinity of a body CD, without at the same time also understanding the body CD to be transferred from the vicinity of the body AB: and that exactly the same force and action is required for the one as for the other"—in other words, that to which of two bodies motion is ascribed, to which rest, is entirely conventional, and corresponds to no physical difference. (Yet again, this stands in contradiction to ¶44, which asserts that a relation of "contrariety" holds, not between one motion and another, but between motion and rest.) It may incidentally be noted that Descartes's seven rules do not cover all possible cases of impact: he fails to consider bodies of unequal size moving toward one another with unequal speeds.
9. Ibid., ¶53. Descartes does not say explicitly that his rules apply *in vacuo;* but he does imply that they hold only for colliding bodies that are "separated from all others" (cf. n. 10 below). This cannot mean merely that the bodies do not *cohere* with those that surround them; for according to Descartes's theory of cohesion (ibid.), this in turn would mean that the bodies surrounding the given ones were moved *diversely* from the latter (and necessarily then—if the plenum is to be maintained—diversely from one another); which (again by Descartes's theory) is to say that the ambient medium is *fluid;* and it is to the effect of just such an ambient medium that Descartes attributes the *failure* of his rules.
10. Ibid., end of ¶52 in the French version: "And the demonstrations . . . are so certain that even if experience were to appear to show us the opposite, we should nevertheless be obliged to place more trust in our reason than in our senses." (This passage, added in the French translation by the Abbé Picot, is ascribed to Descartes himself, on the basis of a letter to Mersenne of April 20, 1646, indicating that he was engaged in "clarifying my laws of motion" for Picot's translation.) A reason for the divergence of the actual behavior of colliding bodies from his rules of impact quite different from that given in the *Principia,* and perhaps even more astounding, is given by Descartes in a letter of February 17, 1645, to Claude Clerselier. Whereas in the passage cited in n. 9 above Descartes attributes this divergence to the circumstance that "there cannot be any bodies in the world that are separated from all others," in the letter to Clerselier he says almost the opposite: "In those rules, by a body devoid of motion, I intend a body that is not at all in the act of separating its surface from those of the other bodies that surround it, and, in consequence, that forms a part of another, larger, solid body." How this picture of the body at rest as embedded in a *solid ambient medium* is to be reconciled, e.g., with the discussion in the fifth rule of a larger body striking a *smaller* one at rest—or with the stipulation in the fourth rule that the body at rest may be taken to be just *slightly* larger than the one striking it—is not further explained.
11. Descartes, letter of February 26, 1649, to Pierre Chanut.
12. Letter of August 22, 1634. (The addressee—not indicated in the extant source—was tentatively identified by Adam and Tannery as Isaac Beeckman; but in their supplement to the correspondence—volume 10 of their edition of the *Oeuvres* of Descartes—they express substantial doubt on this point.)
13. Huygens, *Oeuvres complètes,* XVI, 30/31 (French translation and Latin original, respectively), Hypothesis I.
14. Ibid., pp. 32/33, Hypothesis III. Huygens' careful formulation is worth quoting: "The motion of bodies, and equal or unequal speeds, are to be understood as relative, having relation to other bodies that are considered as resting—although both the latter and the former may be involved in some other common motion. And therefore when two bodies collide, although both together may be subject to some further equable motion, they do not drive one another any differently, in relation to one who is carried by the same common motion, than if that additional motion were entirely absent." It is especially noteworthy that although the first sentence seems to ground this principle in the philosophical view—which Huygens certainly held—that the only *intelligible concept* of motion is that of one body relative to another, his insight—and conscience—as a physicist compelled him to restrict the principle to "additional common motions" that are *equable:* in the manuscript from which the treatise on impact was printed, which was written out by an amanuensis, the word *aequabili* is inserted in Huygens' own hand (see ibid., p. 33, n. 3).
15. Ibid., pp. 56, ll. 3ff./57, ll. 14ff.
16. Cf. ibid., pp. 164–165, n. 2. For the converse, see ibid. XVIII (1934), 250, ll. 9ff./ 251, ll. 8ff. (with reference back to Hypothesis I, pp. 246/247).
17. See the two manuscript pieces, ibid., XVI, 161–167; and cf. p. 181, first paragraph.
18. Ibid., p. 181.
19. That is, for one moving "equably" (see n. 14).
20. Huygens, in his argument, specifies a particular choice of v that will violate the inequality when the quantity in question is nonzero.
21. This relation appears in the major (posthumously published) treatise—which deals only with "hard" bodies—as Proposition IV. It is there deduced with the help of another assumption (Hypothesis V). But Huygens had earlier taken Proposition IV as a hypothesis (see ibid., p. 40 n. 2 and p. 42 n. 1).
22. Isaac Newton, *Philosophiae Naturalis Principia Mathematica,* Scholium to the laws of motion and their Corollaries.
23. Huygens, *Oeuvres complètes,* XVI, 254/255ff. My exposition does not follow the same order as that of Huygens: he is not explicit about the train of thought that motivates his argument.

24. Huygens does not mention the *buoyant* effect of the air, which really demands a little more argument.—The editors of this volume of the *Oeuvres complètes* of Huygens annotate Huygens' reference to the experiments of Riccioli by citing the latter's *Almagestum Novum* (1651 ed.), I, 381–397; they give no reference to any published or manuscript record of Huygens' own experiments testing Galileo's law of falling bodies. The nearest approach to such an account that I have found is a series of records published in volume 17 (1932) of the *Oeuvres complètes*, pp. 278–284. Three of these, dating from October and November 1659, describe experiments designed to determine the distance a body falls in a given time; but the only times in fact used are 1/2 second and 3/4 second—the aim being to determine (in effect) the acceleration of free fall (in the form: the distance covered in one second by a body falling freely from rest)—and in the latest of these pieces Huygens expresses his distrust of the method used, and his reliance instead upon the value obtained from the relation of length to period of a conical pendulum. A fourth piece in this series, dated simply to 1659, deals with a qualitative experiment whose aim is to show that the initial velocity of a body falling from rest is zero. The two remaining pieces in the series date from August or September 1664, and describe an apparatus for measuring distances fallen in a given time; but no numerical data are given, and the editors of the volume state (p. 247) that Huygens apparently did not in fact build apparatus of the sort he describes or have such built. In short, none of this material directly illuminates Huygens' reference to his own experimental confirmation of Galileo's law.
25. The string is supposed parallel to the inclined plane. Huygens asserts (loc. cit., pp. 256/257), but does not argue for, the proportionality of "tug" to acceleration. He could have given such an argument, by referring to the magnitude of the weight required for equilibrium if the string passes over a pulley and an equilibrating weight is hung vertically from its other end. Experiments, then, on the relation of acceleration to the inclination of the plane, compared with the relation of equilibrating weight to that inclination, would support the assertion. (There remains the further consideration of the relationship of the acceleration of a body, presumably *rolling* on an incline, to that of a *freely falling* body—or to that "ideally" expected of a body *sliding* without friction on an incline. I am not aware that this is a matter ever addressed by Huygens.)
26. This may have been the occasion of Huygens' first reflection upon the involutes of curves—a notion he introduced publicly, and put to ingenious use, in his book on the pendulum clock.
27. Ibid., pp. 266/267.
28. Huygens, it should be made clear, does *not* enunciate a principle as general as this (but see below, on "incitation"). He does, however, prove (although not in explicit algebraic form) that a body of weight W, moving in a circle of radius r with velocity v, will exert a centrifugal force F such that $F:W = v^2/r:g$, where g is the acceleration of free fall (at the location where the weight of the body amounts to W: for Huygens does not think that weight is independent of location). So for bodies in general in whirling motion, F is proportional to $(W/g)\cdot(v^2/r)$. Furthermore (see discussion below), Huygens *does* believe that W/g is constant—i.e., is an intrinsic property of a body.
29. Newton, *Principia*, Scholium to the laws of motion and their corollaries.
30. Huygens, *Oeuvres complètes*, XIX (1937), 627: "Moy je dis que chasque corps a de la pesanteur suivant la quantité de la matiere qui le compose . . . Cela paroit de l'effet de l'impulsion qui suit exactement la raison de la pesanteur des corps." Here, to be sure, the distinction is *presupposed* rather than argued for. But one finds an argument for it in a passage of 1669, ibid., pp. 637–638. (Cf. also vol. XXI [1944], pp. 382 and 458.)

31. The conclusion explicitly stated by Huygens in the passage cited in n. 18 above corresponds to Newton's Corollary IV: conservation of motion of the center of gravity.
32. Huygens, *Oeuvres complètes*, XVIII, 496–498.
33. To prevent misunderstanding: the *strategy* is the same as in the case of centrifugal force; Huygens himself does not mention centrifugal force in this manuscript.
34. This is clearly implied by Huygens' examples, although he does not quite make it explicit.
35. My interpretation of the logical connections among Huygens' three fragmentary clauses:

    incitations differentes uniformes et corps egaux.
    quel temps par des espaces egaux.
    incitations egales sur des corps inegaux.

    —loc. cit., p. 498.
36. Leibniz's term, of course; not Huygens'.
37. Huygens, *Oeuvres complètes*, XIX, 625ff.
38. See *Philosophical Transactions* [scil., *of the Royal Society of London*] no. 96 (July 21, 1673), 6086; in I. Bernard Cohen, ed., *Isaac Newton's Papers and Letters on Natural Philosophy* (Cambridge, Mass.: Harvard University Press, 1958), p. 136; in H. W. Turnbull, ed., *The Correspondence of Isaac Newton*, vol. I (Cambridge: Cambridge University Press, 1959), 255 (in this last place, quoted by Oldenburg to Newton in Huygens' original French).
39. Huygens, *Treatise on Light*, trans. Silvanus P. Thompson (reprint: Chicago: University of Chicago Press, 1945), pp. vi–vii; *Oeuvres complètes*, XIX, 454–455.
40. Ibid. (Thompson trans.), pp. 2, 3; *Oeuvres complètes*, pp. 459, 461.
41. *Unpublished Scientific Papers of Isaac Newton*, ed. A. Rupert Hall and Marie Boas Hall (Cambridge: Cambridge University Press, 1962), pp. 157ff.; also in John W. Herivel, *The Background to Newton's Principia* (Oxford: Clarendon Press, 1965), pp. 208ff.
42. Loc. cit., Hall and Hall, p. 162; Herivel, p. 213.
43. Herivel, *Background to Newton's Principia*, p. 138.
44. Cf. the concluding paragraph of Newton's reply to Hooke's comments, in Newton, *Correspondence*, I, 187. (This passage is not to be found in the Cohen edition of Newton's papers and letters, cited above, n. 38, which reproduces Newton's reply as published by Oldenburg in the *Philosophical Transactions:* Oldenburg omitted the paragraph in question.)
45. Newton, *Correspondence*, I, 95; *Papers and Letters*, p. 51.
46. *Correspondence*, I, 96, 292; *Papers and Letters*, pp. 53, 140.
47. Newton's claim, which I am prepared to defend.
48. *Correspondence*, I, 97; *Papers and Letters*, p. 53.
49. That Newton was in possession of this result in 1672 (although he did not expound it in detail until December 1675—see *Papers and Letters*, pp. 193–198, 204–206 (Observations 5–7), 207–208 (Observations 14–15), 210 (Observation 16); less completely in *Correspondence*, I, 377–383—is clear from the first paragraph of §3 of his reply to Hooke (*Correspondence*, I, 174–175; in *Papers and Letters*, this is §4, pp. 120–121).
50. *Correspondence*, I, 98 (¶6); *Papers and Letters*, pp. 54–55.
51. *Correspondence*, I, 97–98; *Papers and Letters*, pp. 53–54.

52. *Correspondence*, I, 98; *Papers and Letters*, p. 54.
53. John Locke, *An Essay Concerning Human Understanding*, II, viii, §9; quoted from the edition of Peter H. Nidditch (Oxford: Clarendon Press, 1975, 1979), pp. 134–135.
54. Ibid., II, viii, §15, p. 137.
55. Locke, *An Essay Concerning the Understanding, Knowledge, Opinion, and Assent*, ed. Benjamin Rand (Cambridge, Mass.: Harvard University Press, 1931), now generally known as Draft B of the *Essay*; see pp. 198–199.
56. This is an example of Locke's habit—which he explicitly notes—of using the word *idea* for what, when he is more precise, he calls *quality*.
57. Locke, *Essay*, II, viii, §15, p. 137. The word *pattern*, by the way (see the *Oxford English Dictionary*) is originally the same word as *patron*, hence with the root meaning "father": "original"; the two spellings were not fully differentiated until c. 1700.
58. Locke, *Essay*, II, xxiii, §29, p. 312.
59. Ibid., viii. §26, pp. 142–143.
60. Newton, *Correspondence*, I, 96; the passage was omitted by Oldenburg from the version published in the *Philosophical Transactions* and is therefore not to be found in *Papers and Letters*.
61. Ibid., p. 113.
62. Ibid., pp. 187–188.
63. In particular connection with the theory of parallels, a similar conviction was expressed in a Göttingen dissertation of 1763 by G. S. Klügel. See Roberto Bonola, *Non-Euclidean Geometry: A Critical Study of its Development*, trans. H. S. Carslaw (reprint: New York: Dover Publications, 1955), pp. 50–51 and p. 44, n. 3. Gauss appears to have been the first mathematician of stature (after Newton) to have come—and only after a struggle—to hold seriously the view that the grounds of geometry are empirical.
64. Locke, *Essay*, IV, ii, §14, p. 538.
65. Ibid., iii, §31, p. 562.
66. Ibid., §5, p. 539 (emphasis added).
67. Cf., e.g., ibid., iii, §26, pp. 556–557, and xii, §§9–10, pp. 644–645.
68. Ibid., IV, vii, §11, p. 599.
69. Locke, *Some Thoughts Concerning Education*, §190, in *The Works of John Locke*, new ed., corrected, in 10 vols. (London: Thomas Tegg et al., 1823), IX, 182.
70. Ibid., §194, p. 186 (emphasis added). Note, too, the sentence that follows, which evidently alludes to the assurance given to Locke by a Dutch mathematician—reportedly Huygens—that Newton's mathematical demonstrations were correct. In his biography of Huygens in volume 22 of the *Oeuvres complètes*, J. A. Vollgraff expresses doubt that Huygens and Locke had met during the latter's sojourn in Holland (see p. 744, n. 26); but what other Dutch mathematician would have been competent to offer that assurance?
71. This is the wording of 1st edition; in editions 2–3 there is a slight change, which seems to make the statement a little less clear (see Nidditch edition, pp. 135–136, and critical apparatus to 135, l. 31–136, l. 2).
72. In *The Works of John Locke*, 9 vols., 12th ed. (London: C. and J. Rivington et al., 1824), vol. III, "Mr. Locke's Reply to the Bishop of Worcester's Answer to his second Letter," pp. 467–468 (emphasis added); cf. the edition of Locke's *Essay* by Alexander Campbell Fraser (reprint: New York: Dover Publications, 1959), I, 171, n. 1.
73. M. R. Ayers, "The Ideas of Power and Substance in Locke's Philosophy," *The Philosophical Quarterly* 25 (1975), 1–27.

74. Ibid., p. 22.
75. Locke, *Essay*, IV, vi, §3, p. 579.
76. The suggestion I have made here seems to me at best marginally convincing; nevertheless, I wish to offer a further argument in its favor. Locke's account of truth (ibid., v, §§2, 5–8, pp. 574, 575–578) is essentially contained in these words: "When *Ideas* are so put together, or separated in the Mind, as they, or the Things they stand for do agree, or not, that is, as I may call it, *mental Truth*. But *Truth of Words* is something more, and that is the affirming or denying of Words of one another, as the *Ideas* they stand for agree or disagree: And this again is twofold. Either *purely Verbal*, and trifling, . . . *or Real* and instructive . . . Truth, as well as Knowledge, may well come under the distinction of *Verbal* and *Real*; that being only *verbal Truth*, wherein Terms are joined according to the agreement or disagreement of the *Ideas* they stand for, without regarding whether our *Ideas* are such, as really have, or are capable of having an Existence in Nature. But then it is they contain *real Truth*, when these signs are joined, as our *Ideas* agree; and when our *Ideas* are such, as we know are capable of having an Existence in Nature." This seems clearly enough to warrant the reading of the phrase quoted in the text—"the agreement or disagreement of the *Ideas* [the words stand for] *as it really is*"—as demanding a correspondence of the "Ideas" (which agree or disagree) with some correlate *in rerum natura*. But the trouble is that it fails to suggest why the satisfaction of this demand is appropriately called a kind of *certainty*; especially in view of the fact that the condition Locke states for what he calls *"Certainty of Truth"* is precisely the same as the one he has stated just previously for what he calls *"real Truth."* Perhaps, after all, no weight should be attached to this expression, and it should simply be taken as one instance among many of Locke's almost studied imprecision of usage.
77. Aristotle, *Metaphysics*, II, 2.
78. Cf., e.g., David Hume, *An Enquiry concerning Human Understanding*, §4, pt. 2, in the edition of L. A. Selby-Bigge (3d ed., rev. by P. H. Nidditch) (Oxford: Clarendon Press, 1975), §25, pp. 29–30.
79. Newton, *Principia*, Book III, Rule III (of the Rules of Philosophizing). Quoted from the original (unrevised) translation of 1729 by Andrew Motte (reprint: London: Dawsons of Pall Mall, 1968).
80. Locke, *Essay*, IV, x, §18, p. 628 (with critical apparatus to ll. 30ff.).
81. See Locke, *Essay*, ed. Fraser, II, 321, n. 2.
82. Newton, *Unpublished Scientific Papers*, pp. 90–121 (Latin text): pp. 121–156 (translation).
83. Howard Stein, "On the Notion of Field in Newton, Maxwell, and Beyond," in *Historical and Philosophical Perspectives of Science*, ed. Roger H. Stuewer, Minnesota Studies in the Philosophy of Science, vol. V (Minneapolis: University of Minnesota Press, 1970), pp. 273ff.; "On Space-Time and Ontology," in *Foundations of Space-Time Theories*, ed. John S. Earman, Clark N. Glymour, and John J. Stachel, Minnesota Studies in the Philosophy of Science, vol. VIII (Minneapolis: University of Minnesota Press, 1977), pp. 395ff.; "On Metaphysics and Method in Newton" (unpublished).
84. See, e.g., Locke, *Essay*, II, xii, §6, p. 165: "The *Ideas* of *Substances* are such combinations of simple *Ideas*, as are taken to represent distinct particular things subsisting by themselves; in which the supposed, or confused *Idea* of Substance, such as it is, is always the first and chief"; and cf., in Locke's own index to the work, the following entry under the head "SUBSTANCE" (p. 745): "The confused Idea of S. in general makes always a part of the Essence of the Species of Ss."

85. Ibid., II, xxi, §73, p. 286.
86. Newton, *Opticks* (reprint: New York: Dover Publications, 1952), pp. 401–403. It should be noted that by *solid* Newton does not mean (as Locke does) "impenetrable": in Newton's usage, *solid* means what it ordinarily means to us: "cohesive" (and when, further, Newton wishes to characterize a body as what we call *rigid*, he describes it as [both solid and] *hard*).
87. See Huygens, *Discours de la Cause de la Pesanteur*, in his *Oeuvres complètes*, XXI (1944), p. 472: "Je n'avois point etendu l'action de la pesanteur à de si grandes distances, comme du Soleil aux planetes, ni de la Terre à la Lune; parce que les Tourbillons de Mr. Des Cartes, qui m'avoient autrefois paru fort vraisemblable, & que j'avois encore dans l'esprit, venoient à la traverse. Je n'avois pas pensé non plus à cette diminution reglée de la pesanteur, sçavoir qu'elle estoit en raison reciproque des quarrez des distances du centre: qui est une nouvelle & fort remarquable proprieté de la pesanteur, dont il vaut bien la peine de chercher la raison." And, after having discussed the objections raised by Newton in his *Principia* against the wave theory of light, he continues (pp. 475–476): "J'ay crû devoir aller au devant de ces objections que pouvoit suggerer le Livre de Mr. Newton, sçachant la grande estime qu'on fait de cet ouvrage, & avec raison; puis qu'on ne sçauroit rien voir de plus sçavant en ces matieres, ni qui temoigne une plus grande penetration d'esprit."
88. The premises of the argument can be stated as follows: (1) "By the third law," gravitation is mutual. (I put the initial phrase in quotation marks because this is by no means a straightforward application of the third law: the latter states that the actions of bodies upon one another are equal and opposite; but it does not imply that if, for example, the moon is urged by a force *toward* the earth, that force is exerted on the moon *by* the earth—an implication that Newton explicitly disavows for his use of the term *attraction* (see *Principia*, I, Section XI]; and if, e.g., as Newton considered possible, the force of gravitation were in fact exerted by some sort of "ether" in contact with the gravitating body, the reaction required by the third law would be exerted *upon that ether*, not upon the body toward which the force is directed.) (2) Gravity—that is, weight—is a force exerted, not simply "upon a body" *as a whole*, but upon *each particle* of that body, in proportion to the mass of the particle (or: throughout the volume of the body, with a force-density proportional, at each point, to the mass-density of the body at that point). It follows from these premises that the gravitation is mutual not simply between earth and moon, or sun and planet, but between *each particle* of the one and *each particle* of the other. (3) The force of gravity toward any center of that force is characterized by a centrally directed *acceleration-field*—in Newton's terminology by a distribution throughout space of a "centripetal force" whose "accelerative measure" is independent of the body acted upon, depending only on the position of that body in relation to the center of force; more precisely, the accelerative measure is inversely proportional to the square of the distance of the body from the center. (This premise is based, so far as the law of the inverse square is concerned, upon the conclusions of the first phase of Newton's argument; and so far as the *universal* application to *all* bodies is concerned, on the known laws of terrestrial weight—and the further consideration that any particle in the universe can in principle become part of a terrestrial body (see *Principia*, III, Prop. VI, Cor. 2), so that if there were kinds of matter not equally subject to gravitational acceleration, one would expect to find terrestrial bodies that accelerate unequally in free fall.) It now follows directly that one must attribute not only "gravitational susceptibility," but also "gravitational power," to all bodies; and from the "accelerative" nature of the gravitational field, that the ("motive") force of gravitation between two bodies is proportional to the mass of each. Since this force is also inversely proportional to the square of the distance between the two, and since no other factor influences its value, the law of universal gravitation in its complete form has been attained.

89. Howard Stein, "Newtonian Space-Time," *Texas Quarterly* (1967), 180; also in Robert Palter, ed., *The* Annus Mirabilis *of Sir Isaac Newton* (Cambridge, Mass.: MIT Press, 1970), p. 264.
90. Newton, *Opticks*, p. 397: "The *Vis inertiae* is a passive Principle by which Bodies persist in their Motion or Rest, receive Motion in proportion to the Force impressing it, and resist as much as they are resisted."
91. Alan E. Shapiro, ed., *The Optical Papers of Isaac Newton*, vol. I (Cambridge: Cambridge University Press, 1984), 86/87–88/89 (Latin and English, respectively; I have slightly revised the translation in my text).
92. Newton, *Opticks*, pp. 401–402.