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(*b*. Woolsthorpe, England, 25 December 1642; *d*. London, England, 20 March 1727)

mathematics, dynamics, celestial mechanics, astronomy, optics, natural philosophy.

Isaac Newton was born a posthumous child, his father having been buried the preceding 6 October. Newton was descended from yeomen on both sides: there is no record of any notable ancestor. He was born prematurely, and there was considerable concern for his survival. He later said that he could have fitted into a quart mug at birth. He grew up in his father's house, which still stands in the hamlet of Woolsthorpe, near Grantham in Lincolnshire.

Newton's mother, Hannah (née Ayscough), remarried, and left her three-year-old son in the care of his aged maternal grandmother. His stepfather, the Reverend Barnabas Smith, died in 1653; and Newton's mother returned to Woolsthorpe with her three younger children, a son and two daughters. Their surviving children, Newton's four nephews and four nieces, were his heirs. One niece, Catherine, kept house for Newton in the London years and married John Conduitt, who succeeded Newton as master of the Mint.

Newton's personality was no doubt influenced by his never having known his father. That he was, moreover, resentful of his mother's second marriage and jealous of her second husband may be documented by at least one entry in a youthful catalogue of sins, written in shorthand in 1662, which records "Threatning my father and mother Smith to burne them and the house over them."[1]

In his youth Newton was interested in mechanical contrivances. He is reported to have constructed a model of a mill (powered by a mouse), clocks, "lanthorns," and fiery kites, which he sent aloft to the fright of his neighbors, being inspired by John Bate's *Mysteries of Nature and Art*.[2] He scratched diagrams and an architectural drawing (now revealed and preserved) on the walls and window edges of the Woolsthorpe house, and made many other drawings of birds, animals, men, ships, and plants. His early education was in the dame schools at Skillington and Stoke, beginning perhaps when he was five. He then attended the King's School in Grantham, but his mother withdrew him from school upon her return to Woolsthorpe, intending to make him a farmer. He was, however, uninterested in farm chores, and absent-minded and lackadaisical. With the encouragement of John Stokes, master of the Grantham school, and William Ayscough, Newton's uncle and rector of Burton Coggles, it was therefore decided to prepare the youth for the university. He was admitted a member of Trinity College, Cambridge, on 5 June 1661 as a subsizar, and became scholar in 1664 and Bachelor of Arts in 1665.

Among the books that Newton studied while an undergraduate was Kepler's "optics" (presumably the *Dioptrice*, reprinted in London in 1653). He also began Euclid, which he reportedly found "trifling," throwing it aside for Schooten's second Latin edition of Descartes's *Géométrie*.[3] Somewhat later, on the occasion of his election as scholar, Newton was reportedly found deficient in Euclid when examined by Barrow.[4] He read Descartes's *Géométrie* in a borrowed copy of the Latin version (Amsterdam, 1659–1661) with commentary by Frans van Schooten, in which there were also letters and tracts by de Beaune, Hudde, Heuraet, de Witt, and Schooten himself. Other books that he studied at this time included Oughtred's *Clavis*, Wallis' *Arithmetica infinitorum*, Walter Charleton's compendium of Epicurus and Gassendi, Digby's *Two Essays*, Descartes's *Principia philosophiae* (as well as the Latin edition of his letters), Galileo's *Dialogo* (in Salisbury's English version)—but not, apparently, the *Discorsi*—Magirus' compendium of Scholastic philosophy, Wing and Streete on astronomy, and some writings of Henry More (himself a native of Grantham), with whom Newton became acquainted in Cambridge. Somewhat later, Newton read and annotated Sprat's *History of the Royal Society*, the early *Philosophical Transactions*, and Hooke's *Micrographia*.

Notebooks that survive from Newton's years at Trinity include an early one[5] containing notes in Greek on Aristotle's *Organon* and *Ethics*, with a supplement based on the commentaries by Daniel Stahl, Eustachius, and Gerard Vossius. This, together with his reading of Magirus and others, gives evidence of Newton's grounding in Scholastic rhetoric and syllogistic logic. His own reading in the moderns was organized into a collection of "Questiones quaedam philosophicae,"[6] which further indicate that he had also read Charleton and Digby. He was familiar with the works of Glanville and Boyle, and no doubt studied Gassendi's epitome of Copernican astronomy, which was then published together with Galileo's *Sidereus nuncius* and Kepler's *Dioptrice*.[7]

Little is known of Newton's friends during his college days other than his roommate and onetime amanuensis Wickins. The rooms he occupied are not known for certain; and we have no knowledge as to the subject of his thesis for the B.A., or where he stood academically among the group who were graduated with him. He himself did record what were no doubt unusual events in his undergraduate career: "Lost at cards twice" and "At the Taverne twice."

For eighteen months, after June 1665, Newton is supposed to have been in Lincolnshire, while the University was closed because of the plague. During this time he laid the foundations of his work in mathematics, optics, and astronomy or celestial mechanics. It was formerly believed that all of these discoveries were made while Newton remained in seclusion at Woolsthorpe, with only an occasional excursion into nearby Boothby. During these "two plague years of 1665 & 1666," Newton later said, "I was in the prime of my age for invention & minded Mathematicks & Philosophy more then at any time since." In fact, however, Newton was back in Cambridge on at least one visit between March and June 1666.[8] He appears to have written out his mathematical discoveries at Trinity, where he had access to the college and University libraries, and then to have returned to Lincolnshire to revise and polish these results. It is possible that even the prism experiments on refraction and dispersion were made in his rooms at Trinity, rather than in the country, although while at Woolsthorpe he may have made pendulum experiments to determine the gravitational pull of the earth. The episode of the falling of the apple, which Newton himself said "occasioned" the "notion of gravitation," must have occurred at either Boothby or Woolsthorpe.[9]

Lucasian Professor. On 1 October 1667, some two years after his graduation, Newton was elected minor fellow of Trinity, and on 16 March 1668 he was admitted major fellow. He was created M.A. on 7 July 1668 and on 29 October 1669, at the age of twenty-six, he was appointed Lucasian professor. He succeeded Isaac Barrow, first incumbent of the chair, and it is generally believed that Barrow resigned his professorship so that Newton might have it.[10]

University statutes required that the Lucasian professor give at least one lecture a week in every term. He was then ordered to put in finished form his ten (or more) annual lectures for deposit in the University Library. During Newton's tenure of the professorship, he accordingly deposited manuscripts of his lectures on optics (1670–1672), arithmetic and algebra (1673–1683), most of book **I** of the *Principia* (1684–1685), and "The System of the World" (1687). There is, however, no record of what lectures, if any, he gave in 1686, or from 1688 until he removed to London early in 1696. In the 1670's Newton attempted unsuccessfully to publish his annotations on Kinckhuysen's algebra and his own treatise on fluxions. In 1672 he did succeed in publishing an improved or corrected edition of Varenius' *Geographia generalis*, apparently intended for the use of his students.

During the years in which Newton was writing the *Principia*, according to Humphrey Newton's recollection, [11] "he seldom left his chamber except at term time, when he read in the schools as being Lucasianus Professor, where so few went to hear him, and fewer that understood him, that ofttimes he did in a manner, for want of hearers, read to the walls." When he lectured he "usually staid about half an hour; when he had no auditors, he commonly returned in a 4th part of that time or less." He occasionally received foreigners "with a great deal of freedom, candour, and respect." He "ate sparingly," and often "forgot to eat at all," rarely dining "in the hall, except on some public days," when he was apt to appear "with shoes down at heels, stockings untied, surplice on, and his head scarcely combed." He "seldom went to the chapel," but very often "went to St Mary's church, especially in the forenoon." [12]

From time to time Newton went to London, where he attended meetings of the Royal Society (of which he had been a fellow since 1672). He contributed £40 toward the building of the new college library (1676), as well as giving it various books. He corresponded, both directly and indirectly (often through Henry Oldenburg as intermediary), with scientists in England and on the Continent, including Boyle, Collins, Flamsteed, David Gregory, Halley, Hooke, Huygens, Leibniz, and Wallis. He was often busy with chemical experiments, both before and after writing the *Principia*, and in the mid-1670's he contemplated a publication on optics.[13] During the 1690's Newton was further engaged in revising the *Principia* for a second edition; he then contemplated introducing into book **III** some selections from Lucretius and references to an ancient tradition of wisdom. A major research at this time was the effect of solar perturbations on the motions of the moon. He also worked on mathematical problems more or less continually throughout these years.

Among the students with whom Newton had friendly relations, the most significant for his life and career was Charles Montague, a fellow-commoner of Trinity and grandson of the Earl of Manchester; he "was one of the small band of students who assisted Newton in forming the Philosophical Society of Cambridge"[14] (the attempt to create this society was unsuccessful). Newton was also on familiar terms with Henry More, Edward Paget (whom he recommended for a post in mathematics at Christ' Hospital), Francis Aston, John Ellis (later master of Caius), and J. F. Vigani, first professor of chemistry at Cambridge, who is said to have eventually been banished from Newton's presence for having told him "a loose story about a nun." Newton was active in defending the rights of the university when the Catholic monarch James II tried to mandate the admission of the Benedictine monk Alban Francis. In 1689, he was elected by the university constituency to serve as Member of the Convention Parliament.

While in London as M.P., Newton renewed contact with Montague and with the Royal Society, and met Huygens and others, including Locke, with whom he thereafter corresponded on theological and biblical questions. Richard Bentley sought Newton's advice and assistance in preparing the inaugural Boyle Lectures (or sermons), entitled "The Confutation of Atheism" and based in part on the Newtonian system of the world.

Newton also came to know two other scientists, each of whom wanted to prepare a second edition of the *Principia*. One was David Gregory, a professor at Edinburgh, whom Newton helped to obtain a chair at Oxford, and who recorded his conversations with Newton while Newton was revising the *Principia* in the 1690's. The other was a refugee from Switzerland, Nicolas Fatio de Duillier, advocate of a mechanical explanation of gravitation which was at one time viewed kindly by Newton. Fatio soon became perhaps the most intimate of any of Newton's friends. In the early autumn of 1693, Newton apparently suffered a severe attack of depression and made fantastic accusations against Locke and Pepys and was said to have lost his reason.[15]

In the post-*Principia* years of the 1690's, Newton apparently became bored with Cambridge and his scientific professorship. He hoped to get a post that would take him elsewhere. An attempt to make him master of the Charterhouse "did not appeal to him"[16] but eventually Montague (whose star had risen with the Whigs' return to power in Parliament) was successful in obtaining for Newton (in March 1696) the post of warden of the mint. Newton appointed William Whiston as his deputy in the professorship. He did not resign officially until 10 December 1701, shortly after his second election as M.P. for the university.[17]

Mathematics. Any summary of Newton's contributions to mathematics must take account not only of his fundamental work in the calculus and other aspects of analysis—including infinite series (and most notably the general binomial expansion)—but also his activity in algebra and number theory, classical and analytic geometry, finite differences, the classification of curves, methods of computation and approximation, and even probability.

For three centuries, many of Newton's writings on mathematics have lain buried, chiefly in the Portsmouth Collection of his manuscripts. The major parts are now being published and scholars will shortly be able to trace the evolution of Newton's mathematics in detail.[18] It will be possible here only to indicate highlights, while maintaining a distinction among four levels of dissemination of his work: (1) writings printed in his lifetime, (2) writings circulated in manuscript, (3) writings hinted at or summarized in correspondence, and (4) writings that were published only much later. In his own day and afterward, Newton influenced mathematics "following his own wish," by "his creation of the fluxional calculus and the theory of infinite series," the "two strands of mathematical technique which he bound inseparably together in his 'analytick' method."[19] The following account therefore emphasizes these two topics.

Newton appears to have had no contact with higher mathematics until 1664 when—at the age of twenty-one—his dormant mathematical genius was awakened by Schooten's "Miscellanies" and his edition of Descartes's *Géométrie*, and by Wallis' *Arithmetica infinitorum* (and possibly others of his works). Schooten's edition introduced him to the mathematical contributions of Heuraet, de Witt, Hudde, De Beaune, and others; Newton also read in Viète, Oughtred, and Huygens. He had further compensated for his early neglect of Euclid by careful study of both the *Elements* and *Data* in Barrow's edition.

In recent years[20] scholars have come to recognize Descartes and Wallis as the two "great formative influences" on Newton in the two major areas of his mathematical achievement: the calculus, and analytic geometry and algebra. Newton's own copy of the *Géométrie* has lately turned up in the Trinity College Library; and his marginal comments are now seen to be something quite different from the general devaluation of Descartes's book previously supposed. Rather than the all-inclusive "Error. Error. Non est geom." reported by Conduitt and Brewster, Newton merely indicated an "Error" here and there, while the occasional marginal entry "non geom." was used to note such things as that the Cartesian classification of curves is not really geometry so much as it is algebra. Other of Newton's youthful annotations document what he learned from Wallis, chiefly the method of "indivisibles."[21].

In addition to studying the works cited, Newton encountered the concepts and methods of Fermat and James Gregory. Although Newton was apparently present when Barrow "read his Lectures about motion," and noted[22] that they "might put me upon taking these things into consideration," Barrow's influence on Newton's mathematical thought was probably not of such importance as is often supposed.

A major first step in Newton's creative mathematical life was his discovery of the general binomial theorem, or expansion of $(a + b)^n$, concerning which he wrote, "In the beginning of the year 1665 I found the Method of approximating series & the Rule for reducing any dignity [power] of any Binomical into such a series...."[23] He further stated that:

In the winter between the years 1664 & upon reading Dr Walls' Arithmetica Infinitorum & trying to interpole his progressions for squaring the circle [that is, finding the area or evaluating , I found out another infinite series for squaring the Hyperbola...[24]

On 13 June 1676, Newton sent Oldenburg the "Epistola prior" for transmission to Leibniz. In this communication he wrote that fractions "are reduced to infinite series by division; and radical quantities by extraction of roots" the latter

... much shortened by this theorem,

Where $P + PQ$ signifies the quantity whose root or even any power, or the root of a power is to be found; P signifies the first term of that quantity, Q the remaining terms divided by the first, and m/n the numerical index of the power of $P + PQ$, whether that power is integral or (so to speak) fractional, whether positive or negative.[25]

A sample given by Newton is the expansion

where

$P = c^2$, $Q = x^2/c^2$, $m = 1$, $n = 2$, and

B = (m/n) $AQ = x^2/2c$,

and so on.

Other examples include

What is perhaps the most important general statement made by Newton in this letter is that in dealing with infinite series all operations are carried out "in the symbols just as they are commonly carried out in decimal numbers"

Wallis had obtained the quadratures of certain curves (that is, the areas under the curves), by a technique of indivisibles yielding for certain positive integral values of *n* (0,1,2,3); in attempting to find the quadrature of a circle of unit radius, he had sought to evaluate the integral by interpolation. He showed that

Newton read Wallis and was stimulated to go considerably further, freeing the upper bound and then deriving the infinite series expressing the area of a quadrant of a circle of radius *x*:

In so freeing the upper bound, he was led to recongnize that the terms, identified by their powers of x, displayed the binomial coefficients. Thus, the factors … stand out plainly as, in the special case in the generalization

where

In this way, according to D. T. Whiteside, Newton ccould begin with the indefinite integral and, "by differentiation in a Wallisian manner," proceed to a straightforward derivation of the "series-expansion of the binomial $(1 - x^p)^q$ … virtually in its modern form," with "$|x^p|$ implicitly less than unity for convergence." As a check on the validity of this general series expansion, he "compared its particular expansions with the results of algebraic division and squareroot extraction ()." This work, which was done in the winter of 1664–1665, was later presented in modified form at the beginning of Newton' *De analysi*.

He correctly summarized the stages of development of his method in the "Epistola posterior" of 24 October 1676, which— as before— he wrote for Oldenburg to transmit to Leibniz:

At the beginning of my mathematical studies, when I had met with the works of our celebrated Wallis, on considering the series, by the intercalation of which he himself exhibits the area of the circle and the hyperbola, the fact that in the series of curves whose common base or axis is *x* and the ordinates.

etc., if the areas of every other of them, namely

could be interpolated, we would have the areas of the intermediate ones, of which the first is the circle.…[26]

The importance of changing Wallis' fixed upper boundary to a free variable *x* has been called "the crux of Newton's breakthrough," since the "various powers of *x* order the numerical coefficients and reveal for the first time the binomial character of the sequence."[27]

In about 1665, Newton found the power series (that is, actually determined the sequence of the coefficients) for

and—most important of all—the logarithmic series. He also squared the hyperbola $y(1 + x) = 1$, by tabulating

for $r = 0, 1, 2, \ldots$ in powers of x and then interpolating

From his table, he found the square of the hyperbola in the series

which is the series for the natural logarithm of $1 + x$. Newton wrote that having "found the method of infinite series," in the winter of 1664–1665, "in summer 1665 being forced from Cambridge by the Plague I computed the area of the Hyperbola at Boothby… to two & fifty figures by the same method."[29]

At about the same time Newton devised "a completely general differentiation procedure founded on the concept of an indefinitely small and ultimately vanishing element *0* of a variable, say, *x*." He first used the notation of a "little zero" in September 1664, in notes based on Descartes's *Géométrie*, then extended it to various kinds of mathematical investigations. From the derivative of an algebraic function *f* (x) conceived ("essentially") as

he developed general rules of differentiation.

The next year, in Lincolnshire and separated from books, Newton developed a new theoretical basis for his techniques of the calculus. Whiteside has summarized this stage as follows:

[Newton rejected] as his foundation the concept of the indefinitely small, discrete increment in favor of that of the "fluxion" of a variable, a finite instantaneous speed defined with respect to an independent, conventional dimension of time and on the geometrical model of the line-segment: in modern language, the fluxion of the variable x with regard to independent time-variable t is the "speed" dx/dt.[30]

Prior to 1691, when he introduced the more familiar dot notation ($\dot{x}$ for dx/dt, $\dot{y}$ for dy/dt, $\dot{z}$ for dz/dt; then $\ddot{x}$ for d^2x/dt^2, $\ddot{y}$ for d^2y/dt^2, $\ddot{z}$ for d^2z/dt^2), Newton generally used the letters p, q, r for the first derivatives (Leibnizian $dx/dt, dy/dt, dz/dt$) of variable quantities x, y, z, with respect to some independent variable t. In this scheme, the "little zero" *0* was "an arbitrary increment of time,"[31] and *op, oq, or* were the corresponding "moments," or increments of the variables, x, y, z (later these would, of course, become $o\dot{x}, 0\dot{y}, 0\dot{z}$).[32] Hence, in the limit (*0* → zero), in the modern Leibnizian terminology

$q/p = dy/dx \; r/p = dz/dx$,

where "we may think of the increment 0 as absorbed into the limit ratios," When, as was often done for the sake of simplicity, x itself was taken for the independent time variable, since $x = t$, then $p = \dot{} = dx/dx = 1$, $q = dy/dx$, and $r = dz/dx$.

In May 1665, Newton invented a "true partial derivative symbolism," and he "widely used the notation $\ddot{p}$ and $\ddot{p}$ for the respective homogenized derivatives $x(dp/dx)$ and $x^2(d^2p/dx^2)$," in particular to express the total derivative of the function

before "breaking through…to the first recorded use of a true partial-derivative symbolism." Armed with this tool, he constructed "the five first and second order partial derivatives of a two-valued function" and composed the fluxional tract of October 1666.[33]

Extracts were published by James Wilson in 1761, although the work as a whole remained in manuscript until recently.[34] Whiteside epitomizes Newton's work during this period as follows:

In two short years (summer 1664-October 1666) Newton the mathematician was born, and in a sense the rest of his creative life was largely the working out, in calculus as in his mathematical thought in general, of the mass of burgeoning ideas which sprouted in his mind on the threshold of intellectual maturity. There followed two mathematically dull years.[35]

From 1664 to 1669, Newton advanced to "more general considerations," namely that the derivatives and integrals of functions might themselves be expressed as expansions in infinite series, specifically power series. But he had no general method for determining the "limits of convergence of individual series," nor had he found any "valid tests for such convergence."[36] Then, in mid-1669, he came upon Nicolaus Mercator's *Logarithmotechnica*, published in September 1668, of which "Mr Collins a few months after sent a copy … to Dr Barrow," as Newton later recorded.[37] Barrow, according to Newton, "replied that the Method of Series was invented & made general by me about two years before the publication of" the *Logarithmotechnica* and "at the same time," July 1669, Barrow sent back to Collins Newton's tract *De analysi*.

We may easily imagine Newton's concern for his priority on reading Mercator's book, for here he found in print "for all the world to read … his [own] reduction of log(l + *a*) to an infinite series by continued division of 1 + *a* into 1 and successive integration of the quotient term by term."[38] Mercator had presented, among other numerical examples, that of log(1.1) calculated to forty-four decimal places, and he had no doubt calculated other logarithms over which Newton had spent untold hours. Newton might privately have been satisfied that Mercator's exposition was "cumbrous and inadequate" when compared to his own, but he must have been immeasurably anxious lest Mercator generalize a particular case (if indeed he had not already done so) and come upon Newton's discovery of "the extraction of roots in such series and indeed upon his cherished binomial expansion." [39] To make matters worse, Newton may have heard the depressing news (as Collins wrote to James Gregory, on 2 February 1668/1669) that "the Lord Brouncker asserts he can turne the square roote into an infinite Series."

To protect his priority, Newton hastily set to work to write up the results of his early researches into the properties of the binomial expansion and his methods for resolving "affected" equations, revising and amplifying his results in the course of composition. He submitted the tract, *De analysi per aequationes infinitas*, to Barrow, who sent it, as previously mentioned, to Collins.

Collins communicated Newton's results to James Gregory, Sluse, Bertet, Borelli, Vernon, and Strode, among others.[40] Newton was at that time unwilling to commit the tract to print; a year later, he incorporated its main parts into another manuscript, the *Methodus fluxionum et serierum infinitarum*. The original Latin text of the tract was not printed until long afterward.[41] Among those who saw the manuscript of *De analysi* was Leibniz, while on his second visit to London in October 1676; he read Collins' copy, and transcribed portions. Whiteside concurs with "the previously expressed opinions of the two eminent Leibniz scholars, Gerhardt and Hofmann," that Leibniz did not then "annex for his own purposes the fluxional method briefly exposed there," but "was interested only in Newton's series expansions."[42]

The *Methodus fluxionum* provides a better display of Newton's methods for the fluxional calculus in its generality than does the *De analysi*. In the preface to his English version of the *Methodus fluxionum*, John Colson wrote:

The chief Principle, upon which the Method of Fluxions is here built, is this very simple one, taken from the Rational Mechanicks; which is, That Mathematical Quantity, particularly Extension, may be conceived as generated by continued local Motion; and that all Quantities whatever, at least by analogy and accommodation, may be conceived as generated after a like manner. Consequently there must be comparative Velocities of increase and decrease, during such generations, whose Relations are fixt and determinable, and may therefore (problematically) be proposed to be found.[43]

Among the problems solved are the differentiation of any algebraic function *f(x)*; the "method of quadratures," or the integration of such a function by the inverse process; and, more generally, the "inverse method of tangents," or the solution of a first-order differential equation.

As an example, the "moments" $\dot{x}0$ and $\dot{y}0$ are "the infinitely little accessions of the flowing quantities [variables] x and y": that is, their increase in "infinitely small portions of time." Hence, after "any infinitely small interval of time" (designated by 0), x and y become $x + \dot{x}0$ and $y + \dot{y}0$. If one substitutes these for x and y in any given equation, for instance

$x^3 - ax^2 + axy - y^3=0$,

"there will arise"

The terms $x^3 - ax^2 + axy - y^3$ (of which "by supposition" the sum = 0) may be cast outl; the remaining terms are divided by 0, to get

"But whereas *o* is suppos'd to be infinitely little, that it may represent the moments of quantities, consequently the terms that are multiplied by it will be nothing in respect of the rest."[44] These terms are therefore "rejected," and there remains

$3x^2\dot{x} + 2ax\dot{x} - a\dot{x}y + a\dot{y}x + 3\dot{y}y^2 = 0$.

It is then easy to group by $\dot{x}$ and $\dot{y}$ to get

or

which is the same result as finding *dy*/*dx* after differentiating

$x^3 - ax^2 + axy - y^3 = 0$.[45]

Problem II then reverses the process, with

being given. Newton then intergrates term by term to get $x^3 - ax^2 + axy - y^3 = 0$, the validity of which he may then test by differentiation.

In an example given, 0 was an increment x (although again infinitely small). In the manuscript, as Whiteside points out, Newton canceled "the less precise equivalent 'indefinite' (indefinitely)" in favor of "infinitè.' (indefinitely)" in favor of "infinitely."[46] Certainly the most significant feature is Newton's general and detailed treatment of "fluxions' of given 'fluent' quantities, and vice versa)," and "the novelty of Newton' … reformulation of the calculus of continuous increase."[47]

Other illustrations given by Newton of his method are determining maxima and minima and drawing tangents to curves at any point. In dealing with maxima and minima, as applied to the foregoing equation, Newton invoked the rule (Problem III):

When a quantity is the greatest or the least that it can be, at that moment it neither flows backwards nor forwards: for if it flows forwards or increases it was less, and will presently be greater than it is; and on the contrary if it flows backwards or decreases, then it was greater, and will presently be less than it is.

In an example Newton sought the "greatest value of x" in the equation

$x^3 - ax^2 + axy - y^3 = 0$.

Having already found "the relation of the fluxions of x and y," he set $x = 0$. Thus, $y(ax - 3y^2) = 0$, or $3y^2 = ax$ gives the desired result since this relation may be used to "exterminate either x or y out of the primary equation; and by the resulting equation you may determine the other, and then both of them by $-3y^2 + ax = 0$." Newton showed how "that famous Rule of *Huddenius*" may be derived from his own general method, but he did not refer to Fermat's earlier method of maxima and minima. Newton also found the greatest value of y in the equation and then indicated that his method led to the solution of a number of specified maximum-minimum problems.

Newton's shift from a "loosely justified conceptual model of the 'fluent' (instantaneous 'speeds' of flow) of a set of dependent variables which continuously alter their magnitude" may have been due, in part, to Barrow.[48] This concept of a uniformly flowing time long remained a favorite of Newton's; it was to appear again in the *Principia*, in the scholium time" (which "of itself, and from its own nature, flows equably without relation to anything external"), and in lemma 2, book II (see below), in which he introduced quantities "variable and indetermined, and increasing or decreasing, as it were, by a continual motion or flux." He later explained his position in a draft review of the *Commercium epistolicum* (1712),

I consider time as flowing or increasing by continual flux & other quantities as increasing continually in time & from the fluxion of time I give the name of fluxions to the velocitys with which all other quantities increase. Also from the moments of time I give the name of moments to the parts of any other quantities generated in moments of time. I expose time by any quantity flowing uniformly & represent its fluxion by an unit, & the fluxions of other quantities I represent by any other fit symbols & the fluxions of their fluxions by other fit symbols & the fluxions of those fluxions by others, & their moments generated by those fluxions I represent by the symbols of the fluxions drawn into the letter *o* & its powers o^2, o^3&*c*: viz[t] their first moments by their first fluxions drawn into the letter *o*, their second moments by their second fluxions into o^2 & so on. And when I am investigating a truth or the solution of a Probleme I use all sorts of approximations & neglect to write down the letter *o*, but when I am demonstrating a Proposition I always write down the letter *o* & proceed exactly by the rules of Geometry without admitting any approximations. And I found the method not upon summs & differences, but upon the solution of this probleme: *By knowing the Quantities generated in time to find their fluxions*. And this is done by finding not prima momenta but primas momentorum nascentium rationes.

In an addendum (published only in 1969) to the 1671 *Methodus fluxionum,*[49] Newton developed an alternative geometrical theory of "first and last" ratios of lines and curves. This was later partially subsumed into the 1687 edition of the *Principia*, section 1, book I, and in the introduction to the *Tractatus de quadratura curvarum* (published by Newton in 1704 as one of the two mathematical appendixes to the *Opticks).* Newton had intended to issue a version of his *De quadratura* with the *Principia* on several occasions, both before and after the 1713 second edition, because, as he once wrote, "by the help of this method of Quadratures I found the Demonstration of Kepler's Propositions that the Planets revolve in Ellipses describing … areas proportional to the times," and again, "By the inverse Method of fluxions I found in the year 1677 the demonstration of Kepler's Astronomical Proposition."[50]

Newton began *De quadratura* with the statement that he did not use infinitesimals, "in this Place," considering "mathematical Quantities … not as consisting of very small Parts; but as described by a continued Motion."[51] Thus lines are generated "not by the Apposition of Parts, but by the continued Motion of Points," areas by the motion of lines, solids by the motion of surfaces, angles by the rotation of the sides, and "Portions of Time by a continual Flux. Recognizing that there are different rates of increase and decrease, he called the "Velocities of the Motions or Increments Fluxions, and the generated Quantities *Fluents*" adding that "Fluxions are very nearly as the Augments of the Fluents generated in equal but very small Particles of Time, and, to speak accurately, they are in the *first Ratio* of the nascent Augments; but they may be expounded in any Lines which are proportional to them."

As an example, consider that (as in Fig. 1) areas *ABC, ABDG* are described by the uniform motion of

the ordinates *BC, BD* moving along the base in the direction *AB*. Suppose *BC* to advance to any new position *bc* complete the parallelogram *BCEb*, draw the straight line *VTH* "touching the Curve in *C*, and meeting the two lines *bc* and *BA* [produced] in *T* and *V*." The "augments" generated will be: *Bb*, by *AB*: *Ec*, by *BC*; and *Cc*, by "the Curve Line *ACc*." Hence, "the Sides of the Triangle *CET* are in the *first Ratio* of these Augments considered as nascent." The "Fluxions of *AB, BC* and *AC*" are therefore "as the Sides *CE, ET* and *CT* of that Triangle *CET*" and "may be expounded" by those sides, or by the sides of the triangle *VBC*, which is similar to the triangle *CET*.

Contrariwise, one can "take the Fluxions in the *ultimate Ratio* of the evanescent Parts." Draw the straight line *Cc*; produce it to *K*. Now let *bc* return to its original position *BC*; when "*C* and *c* coalesce," the line *CK* will coincide with the tangent *CH* then, "the evanescent Triangle *CEc* in its ultimate Form will become similar to the Triangle *CET*, and its evanescent Sides *CE, Ec*, and *Cc* will be *ultimately* among themselves as the sides *CE, ET* and *CT* of the other Triangle *CET*, are, and therefore the Fluxions of the Lines *AB, BC* and *AC* are in this same Ratio."

Newton concluded with an admonition that for the line *CK* not to be "distant from the Tangent *CH* by a small Distance," it is necessary that the points *C*and *c* not be separated "by any small Distance." If the points *C* and *c* do not "coalesce and exactly coincide, the lines *CK* and *CII* will not coincide, and "the ultimate Ratios in the Lines *CE*, *Ec*and *Cc*" cannot be found. In short, "The very smallest Errors in mathematical Matters are not to be neglected."[52]

This same topic appears in the mathematical introduction (section I, book I) to the *Principia*, in which Newton stated a set of lemmas on limits of geometrical ratios, making a distinction between the limit of a ratio and the ratio of limits (for example, as $x \to 0$, lim. $x^n \to n$ but lim. x^n/lim. $x \to 0/0$, which is indeterminate).

The connection of fluxions with infinite series was first publicly stated in a scholium to proposition 11 of De *quadratura,* which Newton added for the 1704 printing, "We said formerly that there were first, second, third, fourth, &c. Fluxions of flowing Quantities. These Fluxions are as the Terms of an infinite converging series." As an example, he considered z^n to "be the flowing Quantity" and "by flowing" to become $(z + o)^n$; he then demonstrated that the successive terms of the expansion are the successive fluxions: "The first Term of this Series z^n will be that flowing Quantity; the second will be the first Increment or Difference, to which consider'd as nascent, its first Fluxion is proportional … and so on *in infinitum*" This clearly exemplifies the theorem formally stated by Brook Taylor in 1715; Newton himself explicitly derived it in an unpublished first version of *De quadratura* in 1691.[53] It should be noted that Newton here showed himself to be aware of the importance of convergence as a necessary condition for expansion in an infinite series.

In describing his method of *quadrature* by "first and last ratios," Newton said:

Now to institute an Analysis after this manner in finite Quantities and investigate the *prime or ultimate* Ratios of these finite Quantities when in their nascent or evanescent State, is consonant to the Geometry of the Ancients: and I was willing [that is, desirous] to show that, in the Method of Fluxions, there is no necessity of introducing Figures infinitely small into Geometry.[54]

Newton's statement on the geometry of the ancients is typical of his lifelong philosophy. In mathematics and in mathematical physics, he believed that the results of analysis—the way in which things were discovered-should ideally be presented synthetically, in the form of a demonstration. Thus, in his review of the *Commercium epistolicum* (published anonymously), he wrote of the methods he had developed in *De quadratura* and other works as follows:

By the help of the new *Analysis* Mr. *Newton* found out most of the Propositions in his *Principia Philosophiae*: but because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically, that the Systeme of the Heavens might be founded upon good Geometry. And this makes it now difficult for unskilful Men to see the Analysis by which those Propositions were found out,[55]

As to analysis itself, David Gregory recorded that Newton once said "Algebra is the Analysis of the Bunglers in Mathematicks."[56] No doubt! Newton did, nevertheless, devote his main professorial lectures of 1673–1683 to algebra,[57] and these lectures were printed a number of times both during his lifetime and after.[58] This algebraical work includes, among other things, what H. W. Turnbull has described as a general method (given without proof) for discovering "the rational factors, if any, of a polynomial in one unknown and with integral coefficients"; he adds that the "most remarkable passage in the book" is Newton's rule for discovering the imaginary roots of such a polynomial.[59] (There is also developed a set of formulas for "the sums of the powers of the roots of a polynomial equation.")[60]

Newton's preference for geometric methods over purely analytical ones is further evident in his statement that "Equations are Expressions of Arithmetical Computation and properly have no place in Geometry." But such assertions must not be read out of context, as if they were pronouncements about algebra in general, since Newton was actually discussing various points of view or standards concerning what was proper to geometry. He included the positions of Pappus and Archimedes on whether to admit into geometry the conchoid for the problem of trisection and those of the "new generation of geometers" who "welcome" into geometry many curvcs, conies among them.[61]

Newton's concern was with the limits to be set in geometry, and in particular he took up the question of the legitimacy of the conic sections in solid geometry (that is, as solid constructions) as opposed to their illegitimacy in plane geometry (since they cannot be generated in a plane by a purely geometric construction). He wished to divorce synthetic geometric considerations from their "analytic" algebraic counterparts. Synthesis would make the ellipse the simplest of conic sections other than the circle; analysis would award this place to the parabola. "Simplicity in figures," he wrote, "is dependent on the simplicity of their genesis and conception, and it is not its equation but its description (whether geometrical or mechanical) by which a figure is generated and rendered easy to conceive."[62]

The "written record of [Newton's] first researches in the interlocking structures of Cartesian coordinate geometry and infinitesimal analysis"[63] shows him to have been establishing "the foundations of his mature work in mathematics" and reveals "for the first time the true magnitude of his genius."[64] And in fact Newton did contribute significantly to analytic geometry. In his 1671 *Methodis fluxionum* he devoted "Prob. 4: To draw tangents to corves" to a study of the different ways in which tangents may be drawn "according to the various relationships of curves to straight lines," that is, according to the "modes" or coordinate systems in which the curve is specified.[65]

Newton proceeded "by considering the ratios of limit-increments of the co-ordinate variables (which are those of their fluxions)."[66] His "Mode 3" consists of using what are now known as standard bipolar coordinates, which Newton applied to Cartesian ovals as follows: Let x, y be the distances from a pair of fixed points (two "poles"); the equation $a \pm (e/d)x - y = 0$ for Descartes's "second-order ovals" will then yield the fluxional relation $\pm(e/d)x - y - 0$ (in dot notation) or $\pm em/d - n = 0$ (in the notation of the original manuscript, in which m, n are used for the fluxions $x, y,$ *of* x, y). When $d = e$, "the curve turns out to be a conic." In "Mode 7," Newton introduced polar coordinates for the construction of spirals; "the equation of an Archimedean spiral" in these coordinates becomes $(a/b)\, x = y$, where y is the radius vector (now usually designated r or ϱ) and x the angle $(\nu$ *or* $\Phi)$.

Newton constructed equations for the transformation of coordinates (as, for example, from polar to Cartesian), and found formulas in both polar and rectangular coordinates for the curvature of a variety of curves, including conics and spirals. On the basis of these results Boyer has quite properly referred to Newton as "an originator of polar coordinates."[67]

Further geometrical results may be found in *Enumeratio linearum tertii ordinis*, first written in 1667 or 1668, and then redone and published, together with *De quadratura*, as an appendix to the *Opticks* (1704).[68] Newton devoted the bulk of the tract to classifying cubic curves into seventy-two *"Classes, Genders, or Orders*, according to the Number of the Dimensions of an Equation, expressing the relation between the *Ordinates* and the *Abscissae*; or which is much at one [that is, the same thing], according to the Number of Points in which they may be cut by a Right Line."

In a brief fifth section, Newton dealt with "The Generation of Curves by Shadows," or the theory of projections, by which he considered the shadows produced "by a luminous point" as projections "on an infinite plane." He showed that the "shadows" (or projections) of conic sections are themselves conic sections, while "those of curves of the second genus will always be curves of the second genus; those of the third genus will always be curves of the third genus; and so on ad infinitum" Furthermore, "in the same manner as the circle, projecting its shadow, generates all the conic sections, so the five divergent parabolae, by their shadows, generate all the other curves of the second genus." As C. R. M. Talbot observed, this presentation is "substantially the same as that which is discussed at greater length in the twenty-second lemma [book III, section 5] of the *Principia*, in which it is proposed to 'transmute' any rectilinear or curvilinear figure into another of the same analytical order by means of the method of projections."[69]

The work ends with a brief supplement on "The Organical Description of Curves," leading to the "Description of the Conick-Section by Five Given Points" and including the clear statement, *"The Use of Curves in Geometry is, that by their Intersections Problems may be solved"* (with an example of an equation of the ninth degree). Newton in this tract laid "the foundation for the study of Higher Plane Curves, bringing out the importance of asymptotes, nodes, cusps," according to Turnbull, while Boyer has asserted that it "is the earliest instance of a work devoted solely to graphs of higher plane curves in algebra," and has called attention to the systematic use of two axes and the lack of "hesitation about negative coordinates."[70]

Newton's major mathematical activity had come to a halt by 1696, when he left Cambridge for London. The *Principia*, composed in the 1680's, marked the last great exertion of his mathematical genius, although in the early 1690's he worked on porisms and began a "Liber geometriae," never completed, of which David Gregory gave a good description of the planned whole.[71] For the most part, Newton spent the rest of his mathematical life revising earlier works.

Newton's other chief mathematical activity during the London years lay in furthering his own position against Leibniz in the dispute over priority and originality in the invention of the calculus. But he did respond elegantly to a pair of challenge problems set by Johann [I] Bernoulli in June 1696. The first of these problems was "mechanico-geometrical," to find the curve of swiftest descent. Newton's answer was brief: the "brachistochrone" is a cycloid. The second problem was to find a curve with the following property, "that the two segments [of a right line drawn from a given point through the curve], being

raised to any given power, and taken together, may make everywhere the same sum."[72]

Newton's analytic solution of the curve of least descent is of particular interest as an early example of what became the calculus of variations. Newton had long been concerned with such problems, and in the *Principia* had included (without proof) his findings concerning the solid of least resistance. When David Gregory asked him how he had found such a solid, Newton sent him an analytic demonstration (using dotted fluxions), of which a version was published as an appendix to the second volume of Motte's English translation of the *Principia*.[73]

Optics. The study of Newton's work in optics has to date generally been limited to his published letters relating to light and color (in *Philosophical Transactions*, beginning in February 1672), his invention of a reflecting telescope and "sextant," and his published *Opticks* of 1704 and later editions (in Latin and English). There has never been an adequate edition or a full translation of the *Lectiones opticae*. Nor, indeed, have Newton's optical manuscripts as yet been thoroughly studied.[74]

Newton's optical work first came to the attention of the Royal Society when a telescope made by him was exhibited there. Newton was elected a fellow shortly thereafter, on 11 January 1672, and responded by offering the Society an account of the discovery that had led him to his invention. It was, he proudly alleged, "the oddest if not the most considerable detection yet made in the operations of nature": the analysis of dispersion and the composition of white light.

In the published account Newton related that in 1666 ("at which time I applyed myself to the grinding of Optick glasses of other figures than *Spherical*") he procured a triangular glass prism, "to try therewith the celebrated *Phaenomena of Colours*." Light from a tiny hole in a shutter passed through the prism; the multicolored image—to Newton's purported surprise —was of "an *oblong* form," whereas "according to the received laws of Refraction, I expected [it] should have been *circular*." To account for this unexpected appearance, Newton looked into a number of possibilities, among them that "the Rays, after their trajection through the Prisme did not move in curve lines," and was thereby led to the famous "experimentum crucis."[75] In this experiment Newton used two prisms: the first was employed to produce a spectrum on an opaque board *(BC)* into which a small hole had been drilled; a beam of light could thus pass through the hole to a second board *(DE)* with a similar aperture; in this way a narrow beam of light of a single color would be directed to a second prism, and the beam emerging from the second prism would project an image on another board (Fig. 2). Thus, all light reaching the final board had been twice subjected to prismatic dispersion. By rotating the first prism "to and fro slowly about its Axis," Newton allowed different portions of the dispersed light to reach the second prism.

Newton found that the second prism did not produce any further dispersion of the "homogeneal" light (that is, of light of about the same color); he therefore concluded that "Light it self is a *Heterogeneous mixture of differently refrangible Rays";* and asserted an exact correspondence between color and "degree of Refrangibility" (the least refrangible rays being "disposed to exhibit a *Red* colour," while those of greatest refrangibility are a deep violet). Hence, colors "are not *Qualifications* of Light, derived from Refractions, or Reflections of natural Bodies," as commonly believed, but *"Original* and *connate properties,"* differing in the different sorts of rays.[76]

The same experiment led Newton to two further conclusions, both of real consequence. First, he gave up any hope of "the perfection of Telescopes" based on combinations of lenses and turned to the principle of the reflector; second, he held it to be no longer a subject of dispute "whether Light be a Body." Observing, however, that it "is not so easie" to determine specifically "what Light is," he concluded, "I shall not mingle conjectures with certainties."[77]

Newton's letter was, as promised, read at the Royal Society on 6 February 1672. A week later Hooke delivered a report in which he criticized Newton for asserting a conclusion that did not seem to Hooke to follow necessarily from the experiments described, which—in any event—Hooke thought too few. Hooke had his own theory which, he claimed, could equally well explain Newton's experimental results.

In the controversy that followed with Hooke, Huygens, and others, Newton quickly discovered that he had not produced a convincing demonstration of the validity and significance of the conclusions he had drawn from his experiments. The objection was made that Newton had not explored the possibility that theories of color other than the one he had proposed might explain the phenomena. He was further criticized for having favored a corporeal hypothesis of light, and it was even said that his experimental results could not be reproduced.

In reply, Newton attacked the arguments about the "hypothesis" that he was said to have advanced about the nature of light, since he did not consider this issue to be fundamental to his interpretation of the "experimentum crucis." As he explained in reply to Pardies[78] he was not proposing "an hypothesis," but rather "properties of light" which could easily "be proved" and which, had he not held them to be true, he would "rather have … rejected as vain and empty speculation, than acknowledged even as an hypothesis." Hooke, however, persisted in the argument. Newton was led to state that he had deliberately declined all hypotheses so as "to speak of *Light in general* terms, considering it abstractly, as something or other propagated every way in straight lines from luminous bodies, without determining what that Thing is." But Newton's original communication did assert, "These things being so, it can be no longer disputed, whether there be colours in the dark, nor… perhaps, whether Light be a Body." In response to his critics, he emphasized his use of the word "perhaps" as evidence that he was not committed to one or another hypothesis on the nature of light itself.[79]

One consequence of the debate, which was carried on over a period of four years in the pages of the *Philosophical Transactions* and at meetings of the Royal Society, was that Newton wrote out a lengthy "Hypothesis Explaining the Properties of Light Discoursed of in my Several Papers,[80] in which he supposed that light "is something or other capable of exciting vibrations in the aether," assuming that "there is an aetherial medium much of the same constitution with air, but far rarer, subtler, and more strongly elastic." He suggested the possibility that "muscles are contracted and dilated to cause animal motion," by the action of an "aethereal animal spirit," then went on to offer ether vibration as an explanation of refraction and reflection, of transparency and opacity, of the production of colors, and of diffraction phenomena (including Newton's rings). Even "the gravitating attraction of the earth," he supposed, might "be caused by the continual condensation of some other such like aethereal spirit," which need not be "the main body of phlegmatic aether, but … something very thinly and subtilly diffused through it."[81]

The "Hypothesis" was one of two enclosures that Newton sent to Oldenburg, in his capacity of secretary of the Royal Society, together with a letter dated 7 December 1675. The other was a "Discourse of Observations," in which Newton set out "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." It also contained Newton's account of his discovery of the "rings" produced by light passing through a thin wedge or layer of air between two pieces of glass. He had based his experiments on earlier ones of a similar kind that had been recorded by Hooke in his *Micrographia* (observation 9). In particular Hooke had described the phenomena occurring when the "lamina," or space between the two glasses, was *"double concav*, that is, thinner in the middle then at the edge"; he had observed "various coloured rings or lines, with differing consecutions or orders of Colours."

When Newton's "Discourse" was read at the Royal Society on 20 January 1676, it contained a paragraph (proposition 3) in which Newton referred to Hooke and the *Micrographia*, "in which book he hath also largely discoursed of this … and delivered many other excellent things concerning the colours of thin plates, and other natural bodies, which I have not scrupled to make use of so far as they were for my purpose."[82] In recasting the "Discourse" as parts 1, 2, and 3 of book II of the *Opticks*, however, Newton omitted this statement. It may be assumed that he had carried these experiments so much further than Hooke, introducing careful measurements and quantitative analysis, that he believed them to be his own. Hooke, on the other hand, understandably thought that he deserved more credit for his own contributions —including hypothesis-based explanations—

than Newton was willing to allow him.[83] Newton ended the resulting correspondence on a conciliatory note when he wrote in a letter of 5 February 1676, "What Des-Cartes did was a good step. You have added much in several ways, and especially in taking the colours of thin plates into philosophical consideration. If I have seen further it is by standing on the shoulders of Giants."[84]

The opening of Newton's original letter on optics suggests that he began his prism experiments in 1666, presumably in his rooms in Trinity, but was interrupted by the plague at Cambridge, returning to this topic only two years later. Thus the famous eighteen months supposedly spent in Lincolnshire would mark a hiatus in his optical researches, rather than being the period in which he made his major discoveries concerning light and color. As noted earlier, the many pages of optical material in Newton's manuscripts[85] and notebooks have not yet been sufficiently analyzed to provide a precise record of the development of his experiments, concepts, and theories.

The lectures on optics that Newton gave on the assumption of the Lucasian chair likewise remain only incompletely studied. These exist as two complete, but very different, treatises, each with carefully drawn figures. One was deposited in the University Library, as required by the statutes of his professorship, and was almost certainly written out by his roommate, John Wickins,[86] while the other is in Newton's own hand and remained in his possession.[87] These two versions differ notably in their textual content, and also in their division into "lectures," allegedly given on specified dates. A Latin and an English version, both based on the deposited manuscript although differing in textual detail and completeness, were published after Newton's death. The English version, called *Optical Lectures*, was published in 1728, a year before the Latin. The second part of Newton's Latin text was not translated, since, according to the preface, it was "imperfect" and "has since been published in the *Opticks* by Sir Isaac himself with great improvements." The preface further states that the final two sections of this part are composed "in a manner purely Geometrical," and as such they differ markedly from the *Opticks*. The opening lecture (or section 1) pays tribute to Barrow and mentions telescopes, before getting down to the hard business of Newton's discovery "that … Rays [of light] in respect to the Quantity of Refraction differ from one another." To show the reader that he had not set forth "Fables instead of Truth," Newton at once gave "the Reasons and Experiments on which these things are founded." This account, unlike the later letter in the *Philosophical Transactions*, is not autobiographical; nor does it proceed by definitions, axioms, and propositions (proved "by Experiment"), as does the still later *Opticks*.[88]

R. S. Westfall has discussed the two versions of the later of the *Lectiones opticae*, which were first published in 1729;[89] he suggests that Newton eliminated from the *Lectiones* those "parts not immediately relevant to the central concern, the experimental demonstration of his theory of colors." Mathematical portions of the *Lectiones* have been analyzed by D. T. Whiteside, in Newton's *Mathematical Papers*, while J. A. Lohne and Zev Bechler have made major studies of Newton's manuscripts on optics. The formation of Newton's optical concepts and theories has been ably presented by A. I. Sabra; an edition of the *Opticks* is presently being prepared by Henry Guerlac.

Lohne finds great difficulty in repeating Newton's "experimentum crucis,"[90] but more important, he has traced the influence of Descartes, Hooke, and Boyle on Newton's work in optics.[91] He has further found that Newton used a prism in optical experiments much earlier than hitherto suspected—certainly before 1666, and probably before 1665—and has shown that very early in his optical research Newton was explaining his experiments by "the corpuscular hypothesis." In "Questiones philosophicae," Newton wrote: "Blue rays are reflected more than red rays, because they are slower. Each colour is caused by uniformly moving globuli. The uniform motion which gives the sensation of one colour is different from the motion which gives the sensation of any other colour."[92]

Accordingly, Lohne shows how difficult it is to accept the historical narrative proposed by Newton at the beginning of the letter read to the Royal Society on 8 February 1672 and published in the *Philosophical Transactions*. He asks why Newton should have been surprised to find the spectrum oblong, since his "note-books represent the sunbeam as a stream of slower and faster globules occasioning different refrangibility of the different colours?" Newton must, according to Lohne, have "found it opportune to let his theory of colours appear as a Baconian induction from experiments, although it primarily was deduced from speculations." Sabra, in his analysis of Newton's narrative, concludes that not even "the'fortunate Newton' could have been fortunate enough to have achieved this result in such a smooth manner." Thus one of the most famous examples of the scientific method in operation now seems to have been devised as a sort of scenario by which Newton attempted to convey the impression of a logical train of discovery based on deductions from experiment. The historical record, however, shows that Newton's great leap forward was actually a consequence of implications drawn from profound scientific speculation and insight.[93]

In any event, Newton himself did not publish the *Lectiones opticae*, nor did he produce his planned annotated edition of at least some (and maybe all) of his letters on light and color published in the *Philosophical Transactions*.[94] He completed his English *Opticks*, however, and after repeated requests that he do so, allowed it to be printed in 1704, although he withheld his name, save on the title page of one known copy. It has often been alleged that Newton released the *Opticks* for publication only after Hooke —the last of the original objectors to his theory of light and colors—had died. David Gregory, however, recorded another reason for the publication of the *Opticks* in 1704: Newton, Gregory wrote, had been "provoked" by the appearance, in 1703, of George Cheyne's *Fluxionum methoda inversa* "to publish his [own tract on] Quadratures, and with it, his Light & Colours, &c."[95]

In the *Opticks*, Newton presented his main discoveries and theories concerning light and color in logical order, beginning with eight definitions and eight axioms.[96] Definition 1 of book I reads: "By the Rays of Light I understand its least Parts, and those as well Successive in the same Lines, as Contemporary in several Lines." Eight propositions follow, the first stating that "Lights which differ in Colour, differ also in Degrees of Refrangibility." In appended experiments Newton discussed the appearance of a paper colored half red and half blue when viewed through a prism and showed that a given lens produces red and blue images, respectively, at different distances. The second proposition incorporates a variety of prism experiments as proof that "The Light of the Sun consists of Rays differently refrangible."

The figure given with experiment 10 of this series illustrates "two Prisms tied together in the form of a Parallelopiped" (Fig. 3). Under specified conditions, sunlight entering a darkened room through a small hole *F* in the shutter would not be refracted by the parallelopiped and would emerge parallel to the incident beam *FM*, from which it would pass by refraction through a third prism *1KH*, which would by refraction "cast the usual Colours of the Prism upon the opposite Wall." Turning the parallelopiped about its axis, Newton found that the rays producing the several colors were successively "taken out of the transmitted Light" by "total Reflexion"; first "the Rays which in the third Prism had suffered the greatest Refraction and painted [the wall] with violet and blew were … taken out of the transmitted Light, the rest remaining," then the rays producing green, yellow, orange, and red were "taken out" as the parallelopiped was rotated yet further. Newton thus experimentally confirmed the "experimentum crucis," showing that the light emerging from the two prisms "is compounded of Rays differently Refrangible, seeing [that] the more Refrangible Rays may be taken out while the less Refrangible remain." The arrangement of prisms is the basis of the important discovery reported in book II, part 1, observation 1.

In proposition 6 Newton showed that, contrary to the opinions of previous writers, the sine law actually holds for each single color. The first part of book I ends with Newton's remarks on the impossibility of improving telescopes by the use of colorcorrected lenses and his discussion of his consequent invention of the reflecting telescope (Fig. 4).

In the second part of book I, Newton dealt with colors produced by reflection and refraction (or transmission), and with the appearance of colored objects in relation to the color of the light illuminating them. He discussed colored pigments and their mixture and geometrically constructed a color wheel, drawing an analogy between the primary colors in a compound color and the "seven Musical Tones or Intervals of the eight Sounds, *Sol, la, fa, sol, la, mi, fa, sol….*"[97]

Proposition 9, "Prob. IV. By the discovered Properties of Light to explain the Colours of the Rain-bow," is devoted to the theory of the rainbow. Descartes had developed a geometrical theory, but had

used a single index of refraction (250:187) in his computation of the path of light through each raindrop.[98] Newton's discovery of the difference in refrangibility of the different colors composing white light, and their separation or dispersion as a consequence of refraction, on the other hand, permitted him to compute the radii of the bows for the separate colors. He used 108:81 as the index of refraction for red and 109:81 for violet, and further took into consideration that the light of the sun does not proceed from a single point. He determined the widths of the primary and secondary bows to be 2°15' and 3°40', respectively, and gave a formula for computing the radii of bows of any order *n* (and hence for orders of the rainbow greater than 2) for any given index of refraction.[99] Significant as Newton's achievement was, however, he gave only what can be considered a "first approximation to the solution of the problem," since a full explanation, particularly of the supernumerary or spurious bows, must require the general principle of interference and the "rigorous application of the wave theory."

Book II, which constitutes approximately one third of the *Opticks*, is devoted largely to what would later be called interference effects, growing out of the topics Newton first published in his 1675 letter to the Royal Society. Newton's discoveries in this regard would seem to have had their origin in the first experiment that he describes (book II, part 1, observation 1); he had, he reported, compressed "two Prisms hard together that their sides (which by chance were a very little convex) might somewhere touch one another" (as in the figure provided for experiment 10 of book I, part 1). He found "the place in which they touched" to be "absolutely transparent," as if there had been one "continued piece of Glass," even though there was total reflection from the rest of the surface; but "it appeared like a black or dark spot, by reason that little or no sensible light was reflected from thence, as from other places." When "looked through," it seemed like "a hole in that Air which was formed into a thin Plate, by being compress'd between the Glasses." Newton also found that this transparent spot "would become much broader than otherwise" when he pressed the two prisms "very hard together."

Rotating the two prisms around their common axis (observation 2) produced "many slender Arcs of Colours" which, the prisms being rotated further, "were compleated into Circles or Rings." In observation 4 Newton wrote that

To observe more nicely the order of the Colours … I took two Object-glasses, the one a Plano-convex for a fourteen Foot Telescope, and the other a large double Convex for one of about fifty Foot; and upon this, laying the other with its plane side downwards, I pressed them slowly together, to make the Colours successively emerge in the middle of the Circles, and then slowly lifted the upper Glass from the lower to make them successively vanish again in the same place.

It was thus evident that there was a direct correlation between particular colors of rings and the thickness of the layer of the entrapped air. In this way, as Mach observed, "Newton acquired a complete insight into the whole phenomenon, and at the same time the possibility of determining the thickness of the air gap from the known radius of curvature of the glass."[100]

Newton varied the experiment by using different lenses, and by wetting them, so that the gap or layer was composed of water rather than air. He also studied the rings that were produced by light of a single color, separated out of a prismatic spectrum; he found that in a darkened room the rings from a single color extended to the very edge of the lens. Furthermore, as he noted in observation 13, “the Circles which the red Light made” were “manifestly bigger than those which were made by the blue and violet”; he found it “very pleasant to see them gradually swell or contract accordingly as the Colour of the Light was changed.” He concluded that the rings visible in white light represented a superimposition of the rings of the several colors, and that the alternation of light and dark rings for each color must indicate a succession of regions of reflection and transmission of light, produced by the thin layer of air between the two glasses. He set down the latter conclusion in observation 15: “And from thence the origin of these Rings is manifest; namely that the Air between the Glasses, according to its various thickness, is disposed in some places to reflect, and in others to transmit the Light of any one Colour (as you may see represented …) and in the same place to reflect that of one Colour where it transmits that of another” (Fig. 5).

Book II, part 2, of the *Opticks* has a nomogram in which Newton summarized his measures and computations and demonstrated the agreement of his analysis of the ring phenomenon with his earlier conclusions drawn from his prism experiments—“that whiteness is a dissimilar mixture of all Colours, and that Light is a mixture of Rays endued with all those Colours.” The experiments of book II further confirmed Newton’s earlier findings “that every Ray have its proper and constant degree of Refrangibility connate with it, according to which its refraction is ever justly and regularly perform’d,” from which he argued that “it follows, that the colorifick Dispositions of Rays are also connate with them, and immutable.” The colors of the physical universe are thus derived “only from the various Mixtures or Separations of

Rays, by virtue of their different Refrangibility or Reflexibility”; the study of color thus becomes “a Speculation as truly mathematical as any other part of Opticks.”[101]

In part 3 of book II, Newton analyzed “the permanent Colours of natural Bodies, and the Analogy between them and the Colours of thin transparent Plates.” He concluded that the smallest possible subdivisions of matter must be transparent, and their dimensions optically determinable. A table accompanying proposition 10 gives the refractive powers of a variety of substances “in respect of … Densities.” Proposition 12 contains Newton’s conception of “fits”:

Every Ray of Light in its passage through any refracting Surface is put into a certain transient Constitution or State, which in the progress of the Ray returns at equal Intervals, and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it.

The succeeding definition is more specific: “The returns of the disposition of any Ray to be reflected I will call its *Fits of easy Reflection*, and those of its disposition to be transmitted its *Fits of easy Transmission*, and the space it passes between every return and the next return, the *Interval of its Fits*.”

The “fits” of easy reflection and of easy refraction could thus be described as a numerical sequence; if reflection occurs at distances 0, 2, 4, 6, 8, …, from some central point, then refraction (or transmission) must occur at distances 1, 3, 5, 7, 9, …. Newton did not attempt to explain this periodicity, stating that “I do not here enquire” into the question of “what kind of action or disposition this is.” He declined to speculate “whether it consists in a circulating or a vibrating motion of the Ray, or of the Medium, or something else,” contenting himself “with the bare Discovery, that the Rays of Light are by some cause or other alternately disposed to be reflected or refracted for many vicissitudes.”

Newton thus integrated the periodicity of light into his theoretical work (it had played only a marginal part in Hooke’s theory). His work was, moreover, based upon extraordinarily accurate measurements— so much so that when Thomas Young devised an explanation of Newton’s rings based on the revived wave theory of light and the new principle of interference, he used Newton’s own data to compute the wavelengths and wave numbers of the principal colors in the visible spectrum and attained results that are in close agreement with those generally accepted today.

In part 4 of book II, Newton addressed himself to “the Reflexions and Colours of thick transparent polish’d Plates.” This book ends with an analysis of halos around the sun and moon and the computation of their size, based on the assumption that they are produced by clouds of water or by hail. This led him to the series of eleven observations that begin the third and final book, “concerning the Inflexions of the Rays of Light, and the Colours made thereby,” in which Newton took up the class of optical phenomena previously studied by Grimaldi,[102] in which “fringes” are produced at the edges of the shadows of objects illuminated by light “let into a dark Room through a very small hole.” Newton discussed such fringes surrounding the projected shadows of a hair, the edge of a knife, and a narrow slit.

Newton concluded the first edition of the *Opticks* (1704) with a set of sixteen queries, introduced “in order to a further search to be made by others.” He had at one time hoped he might carry the investigations further, but was “interrupted,” and wrote that he could not “now think of taking these things into farther Consideration.” In the eighteenth century and after, these queries were considered the most important feature of the *Opticks*—particularly the later ones, which were added in two stages, in the Latin *Optice* of 1706 and in the second English edition of 1717–1718.

The original sixteen queries at once go beyond mere experiments on diffraction phenomena. In query 1, Newton suggested that bodies act on light at a distance to bend the rays; and in queries 2 and 3, he attempted to link differences in refrangibility with differences in “flexibility” and the bending that may produce color fringes. In query 4, he inquired into a single principle that, by “acting variously in various Circumstances,” may produce reflection, refraction, and inflection, suggesting that the bending (in reflection and refraction) begins before the rays "arrive at the Bodies." Query 5 concerns the mutual interaction of bodies and light, the heat of bodies being said to consist of having "their parts [put] into a vibrating motion"; while in query 6 Newton proposed a reason why black bodies "conceive heat more easily from Light than those of other Colours." He then discussed the action between light and "sulphureous" bodies, the causes of heat in friction, percussion, putrefaction, and so forth, and defined fire (in query 9) and flame (in query 10), discussing various chemical operations. In query 11, he extended his speculations on heat and vapors to sun and stars. The last four queries (12 to 16) of the original set deal with vision, associated with "Vibrations" (excited by "the Rays of Light") which cause sight by "being propagated along the solid Fibres of the optick Nerves into the Brain." In query 13 specific wavelengths are associated with each of several colors. In query 15 Newton discussed binocular vision, along with other aspects of seeing, while in query 16 he took up the phenomenon of persistence of vision.

Newton has been much criticized for believing dispersion to be independent of the material of the prism and for positing a constant relation between deviation and dispersion in all refractive substances. He thus dismissed the possibility of correcting for chromatic aberration in lenses, and directed attention from refraction to reflecting telescopes.[103]

Newton is often considered to be the chief advocate of the corpuscular or emission theory of light. Lohne has shown that Newton originally did believe in a simple corpuscular theory, an aspect of Newton's science also forcibly brought out by Sabra. Challenged by Hooke, Newton proposed a hypothesis of ether waves associated with (or caused by) these corpuscles, one of the strongest arguments for waves probably being his own discovery of periodicity in "Newton's rings." Unlike either Hooke or Huygens, who is usually held to be the founder of the wave theory but who denied periodicity to waves of light, Newton postulated periodicity as a fundamental property of waves of (or associated with) light, at the same time that he suggested that a particular wavelength characterizes the light producing each color. Indeed, in the queries, he even suggested that vision might be the result of the propagation of waves in the optic nerves. But despite this dual theory, Newton always preferred the corpuscle concept, whereby he might easily explain both rectilinear propagation and polarization, or "sides." The corpuscle concept lent itself further to an analysis by forces (as in section 14 of book I of the *Principia*), thus establishing a universal analogy between the action of gross bodies (of the atoms or corpuscles composing such bodies), and of light. These latter topics are discussed below in connection with the later queries of the *Opticks*.

Dynamics, Astronomy, and the Birth of the "Principia." Newton recorded his early thoughts on motion in various student notebooks and documents.[104] While still an undergraduate, he would certainly have studied the Aristotelian (or neo-Aristotelian) theory of motion and he is known to have read Magirus' *Physiologiae peripateticae libri sex;* his notes include a "Cap:4. De Motu" (wherein "Motus" is said to be the Aristotelian *ἐντελέχεια*). Extracts from Magirus occur in a notebook begun by Newton in 1661;[105] it is a repository of jottings from his student years on a variety of physical and nonphysical topics. In it Newton recorded, among other extracts, Kepler's third law, "that the mean distances of the primary Planets from the Sunne are in sesquialter proportion to the periods of their revolutions in time."[106] This and other astronomical material, including a method of finding planetary positions by approximation, comes from Thomas Streete's *Astronomia Carolina*.

Here, too, Newton set down a note on Horrox' observations, and an expression of concern about the vacuum and the gravity of bodies; he recorded, from "Galilaeus," that "an iron ball" falls freely through "100 braces Florentine or cubits [or 49.01 ells, perhaps 66 yards] in 5" of an hower." Notes of a later date—on matter, motion, gravity, and levity— give evidence of Newton's having read Charleton (on Gassendi), Digby (on Galileo), Descartes, and Henry More.

In addition to acquiring this miscellany of information, making tables of various kinds of observations, and supplementing his reading in Streete by Wing (and, probably, by Galileo's *Sidereus nuncius* and Gassendi's epitome of Copernican astronomy), Newton was developing his own revisions of the principles of motion. Here the major influence on his thought was Descartes (especially the *Principia philosophiae* and the Latin edition of the correspondence, both of which Newton cited in early writings), and Galileo (whose *Dialogue* he knew in the Salusbury version, and whose ideas he would have encountered in works by Henry More, by Charleton and Wallis, and in Digby's *Two Essays*).

An entry in Newton's Waste Book,[107] dated 20 January 1664, shows a quantitative approach to problems of inelastic collision. It was not long before Newton went beyond Descartes's law of conservation, correcting it by algebraically taking into account direction of motion rather than numerical products of size and speed of bodies. In a series of axioms he declared a principle of inertia (in "Axiomes" 1 and 2); he then asserted a relation between "force" and change of motion; and he gave a set of rules for elastic collision.[108] In "Axiome" 22, he had begun to approach the idea of centrifugal force by considering the pressure exerted by a sphere rolling around the inside surface of a cylinder. On the first page of the Waste Book, Newton had quantitated the centrifugal force by conceiving of a body moving along a square inscribed in a circle, and then adding up the shocks at each "reflection." As the number of sides were increased, the body in the limiting case would be "reflected by the sides of an equilateral circumscribed polygon of an infinite number of sides (i.e. by the circle it selfe)." Herivel has pointed out the near equivalence of such results to the early proof mentioned by Newton at the end of the scholium to proposition 4, book I, of the *Principia*. Evidently Newton learned the law of centrifugal force almost a decade before Huygens, who published a similar result in 1673. One early passage of the Waste Book also contains an entry on Newton's theory of conical pendulums.[109]

According to Newton himself, the "notion of gravitation" came to his mind "as he sat in a contemplative mood," and "was occasioned by the fall of an apple."[110] He postulated that, since the moon is sixty times as far away from the center of the earth as the apple, by an inverse-square relation it would accordingly have an acceleration of free fall $1/(60)^2 = 1/3600$ that of the apple. This "moon test" proved the inverse-square law of force which Newton said he "deduced" from combining "Kepler's

Rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the Centers of the Orbs"—that is, by Kepler's third law, that R^3/T^2 = constant, combined with the law of central (centrifugal) force. Clearly if $F \alpha V^2/R$ for a force F acting on a body moving with speed V in a circle of radius R (with period T), it follows simply and at once that

$F \alpha V^2/R = 4\pi^2R^2|T^2R = 4\pi^2|R^2 \times (R^3|T^2)$.

Since $R^3|T^2$ is a constant, $F \alpha 1 | R^2$

An account by Whiston states that Newton took an incorrect value for the radius of the earth and so got a poor agreement between theory and observation, "which made Sir *Isaac* suspect that this Power was partly that of Gravity, and partly that of *Cartesius's* Vortices," whereupon "he threw aside the Paper of his Calculation, and went to other Studies." Pemberton's narration is in agreement as to the poor value taken for the radius of the earth, but omits the reference to Cartesian vortices. Newton himself said (later) only that he made the two calculations and "found them [to] answer pretty nearly."sup(111) In other words, he calculated the falling of the moon and the falling of a terrestrial object, and found the two to be (only) approximately equal.

A whole tradition has grown up (originated by Adams and Glaisher, and most fully expounded by Cajori)[112] that Newton was put off not so much by taking a poor value for the radius of the earth as by his inability then to prove that a sphere made up of uniform concentric shells acts gravitationally on an external point mass as if all its mass were concentrated at its center (proposition 71, book I, book III, of the *Principia*). No firm evidence has ever been found that would support Cajori's conclusion that the lack of this theorem was responsible for the supposed twenty-year delay in Newton's announcement of his "discovery" of the inverse-square law of gravitation. Nor is there evidence that Newton ever attempted to compute the attraction of a sphere until summer 1685, when he was actually writing the *Principia*.

An existing document docs suggest that Newton may have made just such calculations as Whiston and Pemberton described, calculations in which Newton appears to have used a figure for the radius of the Earth that he found in Salusbury's version of Galileo's *Dialogue*, 3,500 Italian miles (*milliaria*), in which one mile equals 5,000, rather than 5,280, feet.[113] Here, some time before 1669, Newton stated, to quote him in translation, "Finally, among the primary planets, since the cubes of their distances from the Sun are reciprocally as the squared numbers of their periods in a given time, their endeavours of recess from the Sun will be reciprocally as the squares of their distances from the Sun," and he then gave numerical examples from each of the six primary planets. A R. Hall has shown that this manuscript is the paper referred to by Newton in his letter to Halley of 20 June 1686, defending his claim to priority of discovery of the inverse-square law against Hooke's claims. It would have been this paper, too, that David Gregory saw and described in 1694, when Newton let him glance over a manuscript earlier than "the year 1669."

This document, however important it may be in enabling us to define Newton's values for the size of the earth, does not contain an actual calculation of the moon test, nor does it refer anywhere to other than centrifugal "endeavours" from the sun. But it does show that when Newton wrote it he had not found firm and convincing grounds on which to assert what Whiteside has called a perfect "balance between (apparent) planetary centrifugal force and that of solar gravity."[114]

By the end of the 1660's Newton had studied the Cartesian principles of motion and had taken a critical stand with regard to them. His comments occur in an essay of the 1670's or late 1660's, beginning "De gravitatione et aequipondio fluidorum,"[115] in which he discussed extensively Descartes's *Principia* and also referred to a letter that formed part of the correspondence with Mersenne. Newton further set up a series of definitions and axioms, then ventured "to dispose of his [Descartes's] fictions." A large part of the essay deals with space and extension; for example, Newton criticized Descartes's view "that extension is not infinite but rather indefinite." In this essay Newton also defined force ("the causal principle of motion and rest"), conatus (or "endeavour"), impetus, inertia, and gravity. Then, in the traditional manner, he reckoned "the quantity of these powers" in "a double way: that is, according to intension or extension." He defined bodies, in the later medieval language of the intension and remission of forms, as "denser when their inertia is more intense, and rarer when it is more remiss."

In a final set of "Propositions on Non-Elastic Fluids" (in which there are two axioms and two propositions), axiom 2, "Bodies in contact press each other equally," suggests that the eventual third law of motion (*Principia*, axiom 3: "To every action is always opposed an equal and opposite reaction") may have arisen in application to fluids as well as to the impact of bodies. The latter topic occurs in another early manuscript, "The Lawes of Motion," written about 1666 and almost certainly antedating the essay on Descartes and his *Principia*.[116] Mere Newton developed some rules for the impact of "bodyes which are absolutely hard," and then tempered them for application to "bodyes here amongst us," characterized by"a relenting softnesse & springynesse," which makes their contact be for some time in more points than one."

Newton's attention to the problems of elastic and inelastic impact is manifest throughout his early writings on dynamics. In the *Principia* it is demonstrated by the emphasis he there gave the concept of force as an "impulse," and by a second law of motion (Lex II, in all editions of the *Principia*) in which he set forth the proportionality of such an impulse (acting instantaneously) to the change in momentum it produces.[117] In the scholium to the laws of motion Newton further discussed elastic and inelastic impact, referring to papers of the late 1660's by Wallis, Wren, and Huygens. He meanwhile developed his concept of a continuously acting force as the limit of a series of impulses occurring at briefer and briefer intervals *in infinitum*.[118]

Indeed, it was not until 1679, or some time between 1680 and 1684, following an exchange with Hooke, that Newton achieved his mature grasp of dynamical principles, recognizing the significance of Kepler's area law, which he had apparently just encountered. Only during the years 1684–1686, when, stimulated by Halley, he wrote out the various versions of the tract *De motu* and its successors and went on to compose the *Principia*, did Newton achieve full command of his insight into mathematical dynamics and celestial mechanics. At that time he clarified the distinction between mass and weight, and saw how these two quantities were related under a variety of circumstances.

Newton's exchange with Hooke occurred when the latter, newly appointed secretary of the Royal Society, wrote to Newton to suggest a private philosophical correspondence. In particular, Hooke asked Newton for his "objections against any hypothesis or opinion of mine," particularly "that of compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body.…." Newton received the letter in November, some months after the death of his mother, and evidently did not wish to take up the problem. He introduced, instead, "a fancy of my own about discovering the Earth's diurnal motion, a spiral path that a freely falling body would follow as it supposedly fell to Earth, moved through the Earth's surface into the interior without material resistance, and eventually spiralled to (or very near to) the Earth's centre, after a few revolutions."[119]

Hooke responded that such a path would not be a spiral. He said that, according to "my theory of circular motion," in the absence of resistance, the body would not move in a spiral but in "a kind [of] Elleptueid," and its path would "resemble an Ellipse." This conclusion was based, said Hooke, on "my Theory of Circular Motions [being] compounded by a Direct [that is, tangential] motion and an attractive one to a Centre." Newton could not ignore this direct contradiction of his own expressed opinion. Accordingly, on 13 December 1679, he wrote Hooke that "I agree with you that … if its gravity be supposed uniform [the body would] not descend in a spiral to the very centre but circulate with an alternate descent & ascent." The cause was "its *vis centrifuga* & gravity alternately overballancing one another." This conception was very like Borelil's, and Newton imagined that "the body will not describe an Ellipsoeid," but a quite different figure. Newton here refused to accept the notion of an ellipse produced by gravitation decreasing as some power of the distance—although he had long before proved that for circular motion a combination of Kepler's third law and the rule for centrifugal force would yield a law of centrifugal force in the inverse square of the distance. There is no record of whether his reluctance was due to the poor agreement of the earlier moon test or to some other cause.

Fortunately for the advancement of science, Hooke kept pressing Newton. In a letter of 6 January 1680 he wrote "… But my supposition is that the Attraction always is in a duplicate proportion to the Distance from the Centre Reciprocall, and Consequently that the Velocity will be in a subduplicate proportion to the Attraction, and Consequently as Kepler Supposes Reciprocall to the Distance." We shall see below that this statement, often cited to support Hooke's claim to priority over Newton in the discovery of the inverse-square law, actually shows that Hooke was not a very good mathematician. As Newton proved, the force law here proposed contradicts the alleged velocity relation.

Hooke also claimed that this conception "doth very Intelligibly and truly make out all the Appearances of the Heavens," and that "the finding out the proprietys of a Curve made by two principles will be of great Concerne to Mankind, because the Invention of the Longitude by the Heavens is a necessary Consequence of it." After a few days, Hooke went on to challenge Newton directly:

… It now remaines to know the proprietys of a curve Line (not circular nor concentricall) made by a centrall attractive power which makes the velocitys of Descent from the tangent Line or equall straight motion at all Distances in a Duplicate proportion to the Distances Reciprocally taken. I doubt not but that by your excellent method you will easily find out what that Curve must be, and its proprietys, and suggest a physicall Reason of this proportion.[120]

Newton did not reply, but he later recorded his next steps:

I found now that whatsoever was the law of the forces which kept the Planets in their Orbs, the areas described by a Radius drawn from them to the Sun would be proportional to the times in which they were described. And … that their Orbs would be such Ellipses as Kepler had described [when] the forces which kept them in their Orbs about the Sun were as the squares of their … distances from the Suit reciprocally.[121]

Newton's account seems to be reliable; the proof he devised must have been that written out by him later in his "De motu corporum in gyrum.[122]

Newton's solution is based on his method of limits, and on the use of infinitesimals.[123] He considered the motion along an ellipse from one point to another during an indefinitely small interval of time, and evaluated the deflection from the tangent during that interval, assuming the deflection to be proportional to the inverse square of the distance from a focus. As one of the two points on the ellipse approaches the other, Newton found that the area law supplies the essential condition in the limit.[124] In short, Newton showed that if the area law holds, then the elliptical shape of an orbit implies that any force directed to a focus must vary inversely as the square of the distance.

But it was also incumbent upon Newton to show the significance of the area law itself; he therefore proved that the area law is a necessary and sufficient condition that the force on a moving body be directed to a center. Thus, for the first time, the true significance of Kepler's first two laws of planetary motion was revealed: that the area condition was equivalent to the action of a central force, and that the occurrence of the ellipse under this condition demonstrates that the force is as the inverse square of the distance. Newton further showed the law of areas to be only another aspect of the law of inertia, since in linear inertial motion, in the absence of external forces, equal areas are swept out in equal times by a line from the moving body directed toward any point not on the line of motion.[125]

Newton was thus quite correct in comparing Hooke's claim and Kepler's, as he wrote to Halley on 20 June 1686:

But grant I received it [the hypothesis of the inverse-square relation] afterwards [that is, after he had come upon it by himself, and independently of Hooke] from Mr Hook, yet have I as great a right to it as to the Ellipsis. For as Kepler knew the Orb to be not circular but oval & guest it to be Elliptical, so Mr Hook without knowing what I have found out since his letters to me, can know no more but that the proportion was duplicate *quam proximė* at great distances from the center, & only guest it to be so accurately & guest amiss in extending that proportion down to the very center, whereas Kepler guest right at the Ellipsis. And so Mr Hook found less of the Proportion than Kepler of the Ellipsis.[126]

What Newton "found out" after his correspondence with Hooke in 1679 was the proof that a homogeneous sphere (or a sphere composed of homogeneous spherical shells) will gravitate as if all its mass were concentrated at its geometric center.

Newton refrained from pointing out that Hooke's lack of mathematical ability prevented him (and many of those who have supported his claim) from seeing that the "approximate" law of speed (1 α 1/r) is

inconsistent with the true area law and does not accord with a force law of the form $f \alpha 1/r^2$. Newton proved (Fig. 6: *Principia*, book I, proposition 16), that the speed at any point in an elliptical orbit is inversely proportional to the perpendicular dropped from the sun (focus) to the tangent drawn to the ellipse at that point, rather than being inversely proportional to the simple distance as Hooke and others had supposed; these two quantities being, of course, the same at the apsides. In the second edition of the *Principia* (1713) Newton shifted the corollaries to propositions 1 and 2, introducing a new set of corollaries to proposition 1, with the result that a prominent place was given to the true speed law.

Newton therefore deserves sole credit for recognizing the significance of the area law, a matter of some importance between 1679 and 1684. Following the exchange with Hooke in the earlier year, however, Newton did not at once go on to complete his work in celestial mechanics, although he did become interested in comets, corresponding with Flamsteed about their motion. He was converted from a belief in the straight-line motion of comets to a belief in parabolic paths, and thereafter attributed the motions of comets (in conic sections) to the action of the inverse-square law of the gravitation of the sun. He was particularly concerned with the comet of 1680, and in book III of the *Principia* devoted much space to its path.

In 1684, Halley visited Newton to ask about the path a planet would follow under the action of an inverse-square force: Wren, Hooke, and he had all been unsuccessful in satisfactorily resolving the matter, although Hooke had asserted (vainly) that he could do it. When Newton said to Hooke that he himself had "calculated" the result and that it was "an Ellipsis," Halley pressed him "for his calculation," but Newton could not find it among his papers and had to send it to Halley at a later date, in November. Halley then went back to Cambridge, where he saw "a curious treatise, *De Motu*." He obtained Newton's promise to send it "to the [Royal] Society to be entered upon their Register,"[127] and Newton, thus encouraged, wrote out a *De motu corporum*, of which the first section largely corresponds to book I of the *Principia* (together with an earnest of what was to become book II), while the second represents a popular account of what was later presented in book III.

Texts of both parts were deposited in the University Library, as if they were Newton's professorial lectures for 1684, 1685, and 1687; the second was published posthumously in both Latin and English, with the introduction of a new and misleading title of *De mundi systemate, or The System of the World*. (This misnomer has ever since caused the second part of *De motu* to be confused with book III of the *Principia*, which is subtitled "De mundi systemate.")

Newton composed the *Principia* in a surprisingly short time.[128] The manuscript of book I was presented on 28 April 1686 to the Royal Society, which ordered it to be printed, although in the event Halley paid the costs and saw the work through the press. Halley's job was not an easy one; when Hooke demanded credit in print for his share in the inverse-square law, Newton demurred and even threatened to suppress book III. Halley fortunately dissuaded Newton from so mutilating his great treatise.

On 1 March 1687 Newton wrote to Halley that book II had been sent to him "by the Coach." The following 5 April Halley reported to Newton that he had received book III, "the last part of your divine Treatise." The printing was completed on 5 July 1687. The first edition included a short preface by Newton and an introductory ode to Newton by Halley—but book III ended abruptly, in the midst of a discussion of comets. Newton had originally drafted a "Conclusio" dealing with general aspects of natural philosophy and the theory of matter,[129] but he suppressed it. The famous conclusion, the "Scholium Generale," was first published some twenty-six years later, in 1713, in the second edition.

The development of Newton's views on comets may be traced through his correspondence with Flamsteed[130] and with Halley, and by comparing the first and second editions of the *Principia*. From Flamsteed he obtained information not only on comets, but also on the distances and periods of the satellites of Jupiter (which data appear in the beginning of book III of the *Principia* as a primary instance of Kepler's third law), and on the possible influence of Jupiter on the motion of Saturn. When Newton at first believed the great comet observed November 1680–March 1681 to be a pair of comets moving (as Kepler proposed) in straight lines, although in opposite directions, it was Flamsteed who convinced him that there was only one, observed coming and going, and that it must have turned about the sun.[131] Newton worked out a parabolic path for the comet of 1680 that was consistent with the observations of Flamsteed and others, the details of which occupy a great part of book III of the *Principia*. Such a parabolic path had been shown in book I to result from the inverse-square law under certain initial conditions, differing from those producing ellipses and hyperbolas.

In 1695, Halley postulated that the path of the comet of 1680 was an elongated ellipse—a path not very distinguishable from a parabola in the region of the sun, but significantly different in that the ellipse implies periodic returns of the cornet—and worked out the details with Newton. In the second and third editions of the *Principia*, Newton gave tables for both the parabolic and elliptical orbits; he asserted unequivocally that Halley had found "a remarkable comet" appearing every seventy-five years or so, and added that Halley had "computed the motions of the comet in this elliptic orbit." Nevertheless, Newton himself remained primarily concerned with parabolic orbits. In the conclusion to the example following proposition 41 (an the comet of 1680), Newton said that "comets are a sort of planets revolved in very eccentric orbits about the sun," Even so, the proposition itself states (in all editions): "From three given observations to determine the orbit of a comet moving in a parabola."

Mathematics in the "Principia." The *Philosophiae naturalis principia mathematica is*, as its title suggests, an exposition of a natural philosophy conceived in terms of new principles based on Newton's own innovations in mathematics. It is too often described as a treatise in the style of Greek geometry, since on superficial examination it appears to have been written in a synthetic geometrical style.[132] But a close examination shows that this external Euclidean form masks the true and novel mathematical character of Newton's treatise, which was recognized even in his own day. (L'Hospital, for example—to Newton's delight—observed in the preface to his 1696 *Analyse des infiniment petits*, the first textbook on the infinitesimal calculus, that Newton's "excellent Livre intitulé *Philosophiae Naturalis principia Mathematica* … est presque tout de ce calcul.") Indeed, the most superficial reading of the *Principia* must show that, proposition by proposition and lemma by lemma, Newton usually proceeded by establishing geometrical conditions and their corresponding ratios and then at once introducing some carefully defined limiting process. This manner of proof or "invention," in marked distinction to the style of the classical Greek geometers, is based on a set of general principles of limits, or of prime and ultimate ratios., posited by Newton so as to deal with nascent or evanescent quantities or ratios of such quantities.

The doctrine of limits occurs in the *Principia* in a set of eleven lemmas that constitute section 1 of book I. These lemmas justify Newton in dealing with areas as limits of sums of inscribed or circumscribed rectangles (whose breadth → 0, or whose number → ∞), and in assuming the equality, in the limit, of arc, chord, and tangent (lemma 7), based on the proportionality of "homologous sides of similar figures, whether curvilinear or rectilinear" (lemma 5), whose "areas are as the squares of the homologous sides." Newton's mathematical principles are founded on a concept of limit disclosed at the very beginning of lemma I, "Quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer to each other than by any given difference, become ultimately equal."

Newton further devoted the concluding scholium of section 1 to his concept of limit, and his method of taking limits, stating the guiding principle thus: "These lemmas are premised to avoid the tediousness of deducing involved demonstrations *ad absurdum*, according to the method of the ancient geometers." While he could have produced shorter ("more contracted") demonstrations by the "method of indivisibles," he judged the "hypothesis of indivisibles "to be "sornewhat harsh" and not geometrical:

I chose rather to reduce the demonstrations of the following propositions to the first and last sums and ratios of nascent and evanescent quantities, that is, to the limits of those sums and ratios; and so to premise, as short as I could, the demonstrations of those limits. For hereby the same thing is performed as by the method of indivisibles; and now those principles being demonstrated, we may use them with greater safety. Therefore if hereafter I should happen to consider quantities as made up of particles, or should use little curved lines for right ones, I would not be understood to mean indivisibles, but evanescent divisible quantities; not the sums and ratios of determinate parts, but always the limits of sums and ratios; and that the force of such demonstrations always depends on the method laid down in the foregoing Lemmas.

Newton was aware that his principles were open to criticism on the ground "that there is no ultimate proportion of evanescent quantities; because the proportion, before the quantities have vanished, is not the ultimate, and when they are vanished, is none"; and he anticipated any possible unfavorable reaction by insisting that "the ultimate ratio of evanescent quantities" is to be understood to mean "the ratio of the quantities not before they vanish, nor afterwards, but [that] with which they vanish." In a "like manner, the first ratio of nascent quantities is that with which they begin to be," and "the first or last sum is that with which they begin and cease to be (or to be augmented or diminished)," Comparing such ratios and sums to velocities (for "it may be alleged, that a body arriving at a certain place, and there stopping, has no ultimate velocity; because the velocity, before the body comes to the place, is not its ultimate velocity; when it has arrived, there is none"), he imagined the existence of "a limit which the velocity at the end of the motion may attain, but not exceed," which limit is "the ultimate velocity," or "that velocity with which the body arrives at its last place, and with which the motion ceases." By analogy, he argued, "there is the like limit in all quantities and proportions that begin and cease to be," and "such limits are certain and definite." Hence, "to determine the same is a problem strictly geometrical," and thus may be used legitimately "in determining and demonstrating any other thing that is also geometrical".

In short, Newton wished to make a clear distinction between the ratios of ultimate quantities and "those ultimate ratios with which quantities vanish," the latter being "limits towards which the ratios of quantities decreasing without limit do always converge …," He pointed out that this distinction may be seen most clearly in the case in which two quantities become infinitely great; then their "ultimate ratio" may be "given, namely, the ratio of equality," even though "it does not from thence follow, that the ultimate or greatest quantities themselves, whose ratio that is, will be given."

Section 1 of book I is unambiguous in its statement that the treatise to follow is based on theorems of which the truth and demonstration almost always depend on the taking of limits. Of course, the occasional analytical intrusions in book I and the explicit use of the fluxional method in book II (notably in section 2) show the mathematical character of the book as a whole, as does the occasional but characteristic introduction of the methods of expansion in infinite series. A careful reading of almost any proof in book I will, moreover, demonstrate the truly limital or infinitesimal character of the work as a whole. But nowhere in the *Principia* (or in any other generally accessible manuscript) did Newton write any of the equations of dynamics as fluxions, as Maciaurin did later on. This continuous form is effectively that published by Varignon in the *Mémoires* of the Paris Academy in 1700; Newton's second law was written as a differential equation in J. Hermann's *Phoronomia* (1716).

The similarity of section 1, book I, to the introductory portion of the later De quadratura should not be taken to mean that in the Principia Newton developed his principles of natural philosophy on the basis of first and last ratios exclusively, since in the Principia Newton presented not one, but rather three modes of presentation of his fluxional or infinitesimal calculus. A second approach to the calculus occurs in section 2, book 11, notably in lemma 2, in which Newton introduced the concept and method of moments. This represents the first printed statement (in the first edition of 1687) by Newton himself of his new mathematics, apart from its application to physics (with which the opening discussion of limits in section 1, book I is concerned). In a scholium to lemma 2, Newton wrote that this lemma contains the "foundation" of "a general method," one

… which extends itself, without any troublesome calculation, not only to the drawing of tangents to any curve lines …, but also to the resolving other abstruser kinds of problems about the crookedness, areas, lengths, centres of gravity of curves, &c.; nor is it … limited to equations which are free from surd quantities. This method I have interwoven with that other of working in equations, by reducing them to infinite series.

He added that the "last words relate to a treatise I composed on that subject in the year 1671"[133] and that the paragraph quoted above came from a letter he had written to Collins on 10 December 1672, describing "a method of tangents.'

The lemma itself reads: "The moment of any *gentium* is equal to the moments of each of the generating sides multiplied by the indices of the powers of those sides, and by their coefficients continually."[134] It may be illustrated by Newton's first example: Let *AB* be a rectangle with sides *A, B,* diminished by , , respectively. The diminished area is . Now, by a "continual flux," let the sides be augmented by , , respectively; the area ("rectangle") will then become (Fig. 7). Subtract one from the other, "and there will

remain the excess $aB + bA$." Newton concluded, "Therefore with the whole increments a and b of the sides, the increment $aB + bA$ of the rectangle is generated." Here a and b are the moments of A and B, respectively, and Newton has shown that the moment of *AB*, corresponding to the moments *a* and *b* of *A* and *B*, respectivly, is $aB + bA$. And, for the special case of $A = B$, the moment of A^2 is determined as $2aA$.

In order to extend the result from "area" to "content" or ("bulk"), from *AB* to *ABC*, Newton set $AB = G$ and then used the prior result for *AB* twice, once for *AB*, and again for *GC*, so as to get the moment of *ABC* to be $cAB + bCA + aBC$; whence, by setting $A = B = C$, the moment of *A*sup(3) is determined as $3aA^2$. And, in general, the moment of A^n is shown to be naA^{n-1} for n as a positive integer.

The result is readily extended to negative integral powers and even to all products A^mB^n, "whether the indices *m* and *n* of the powers be whole numbers or fractions, affirmative or negative." Whiteside has pointed out that by using the decrements , and the increments , , rather than the increments *a, b*, "Newton … deluded himself into believe" he had "contrived an approach which avoids the comparatively messy appeal to the limit-value of $(A + a)/(B + b) - AB$ as the increments *a, b* vanish." The result is what is now seen as a "celebrated *nonsequitur*."[135]

In discussing lemma 2, Newton defined moments as the "momentary increments or decrements" of "variable and indetermined" quantities, which might be "products, quotients, roots, rectangles, squares, cubes, square and cubic sides, and the like." He called these "quantities" *genitae*, because he conceived them to be "generated or produced in arithmetic by the multiplication, division, or extraction of the root of any terms whatsoever; in geometry by the finding of contents and sides, or of the extremes and means of proportionals." So much is clear. But Newton warned his readers not "to look upon finite particles as such [moments]," for finite particles "are not moments, but the very quantities generated by the moments. We are to conceive them as the just nascent principles of finite magnitudes." And, in fact, it is not "the magnitude of the moments, but their first proportion [which is to be regarded] as nascent."

Boyer has called attention to the difficulty of conceiving "the limit of a ratio in determining the moment of *AB*."[136] The moment of *AB* is not really a product of two independent variables *A* and *B*, implying a problem in partial differentiation, but rather a product of two functions of the single independent variable time. Newton himself said, "It will be the same thing, if, instead of moments, we use either the velocities of the increments and decrements (which may also be called the motions, mutations, and fluxions of quantities), or any finite quantities proportional to those velocities."

Newton thus shifted the conceptual base of his procedure from infinitely small quantities or moments—which are not finite, and clearly not zero—to the "first proporation," or ratio of moments (rather than "the magnitude of the moments" "as nascent." This nascent ratio is generally not infinitesimal but finite, and Newton thus suggested that the ratio of finite quantities may be substituted for the ratio of infinitesimals, with the same result, using in fact the velocities of the increments or decrements instead of the moments, or "any finite quantities proportional to those velocities," which are also the "fluxions of the quantities." Boyer summarized this succinctly:

Newton thus offered in the *Principia* three modes of interpretation of the new analysis: that in terms of infinitesimals (used in his *De analysi* …); that in terms of prime and ultimate ratios or limits (given particularly in De quadratura, and the view which he seems to have considered most rigorous); and that in terms of fluxions (given in his *Methodus fluxionum*, and one which appears to have appealed most strongly to his imagination).[137]

From the point of view of mathematics, proposition 10, book II, may particularly attract our attention. Here Newton boldly displayed his methods of using the terms of a converging series to solve problems and his method of second differences. Expansions are given with respect to "the indefinite quantity *0,*" but there are no references to (nor uses of) moments, as in the preceding lemma 2, and, of course, there is no use made of dotted or "pricked" letters.

The proposition is of particular interest for at least two reasons. First, its proof and exposition (or exemplification) are highly analytic and not geometric (or synthetic), as are most proofs in the Principia. Second, an error in the first edition and in the original printed pages of the second edition was discovered by Johann [I] Bernoulli and called to Newton's attention by Nikolaus [I] Bernoulli, who visited England in September or October 1712. As a result, Newton had Cotes reprint a whole signature and an additional leaf of the already printed text of the second edition; these pages thus appear as cancels in every copy of this edition of the *Principia* that has been recorded. The corrected proposition, analyzed by Whiteside, illustrates "the power of Newton's infinitesimal techniques in the *Principia*," and may thus confute the opinion that "Newton did not (at least in principle, and in his own algorithm) know how 'to formulate and resolve problems through the integration of differential equations.'"[138]

From at least 1712 onward, Newton attempted to impose upon the *Principia* a mode of composition that could lend support to his position in the priority dispute with Leibniz: he wished to demonstrate that he had actually composed the *Principia* by analysis and had rewritten the work synthetically. He affirmed this claim, in and after 1713, in several manuscript versions of prefaces to planned new editions of the *Principia* (both with or without *De quadratura* as a supplement). It is indeed plausible to argue that much of the *Principia* was based upon an infinitesimal analysis, veiled by the traditional form of Greek synthetic geometry, but the question remains whether Newton drew upon working papers in which (in extreme form) he gave solutions in dotted fluxions to problems that he later presented geometrically. But, additionally, there is no evidence that Newton used an analytic method of ordinary fluxional form to discover the propositions he presented synthetically.

All evidence indicates that Newton had actually found the propositions in the *Principia* in essentially the way in which he there presented them to his readers. He did, however, use algebraic methods to determine the solid of least resistance. But in this case, he did not make the discovery by analysis and then recast it as an example of synthesis; he simply stated his result without proof.[139]

It has already been mentioned that Newton did make explicit use of the infinitesimal calculus in section 2, book II, of the *Principia*, and that in that work he often employed his favored method of infinite series.[140] But this claim is very different indeed from such a statement of Newton's as: "… At length in 1685 and part of 1686 by the aid of this method and the help of the book on Quadratures I wrote the first two books of the mathematical Principles of Philosophy. And therefore I have subjoined a Book on Quadratures to the Book of Principles."[141] This "method" refers to fluxions, or the method of differential calculus. But it is true, as mentioned earlier, that Newton stated in the *Principia* that certain theorems depended upon the "quadrature" (or integration) of "certain curves"; he did need, for this purpose, the inverse method of fluxions, or the integral calculus. And proposition 41 of book I is, moreover, an obvious exercise in the calculus.

Newton himself never did bring out an edition of the *Principia* together with a version of *De quadratura*.[142] In the review that he published of the *Commercium epistolicum*,[143] Newton did announce in print, although anonymously, that he had "found out most of the Propositions in his *Principia*" by using "the new *Analysis*." and had then reworked the material and had "demonstrated the Propositions synthetically." (This claim cannot, however, be substantiated by documentary evidence.)

Apart from questions of the priority of Newton's method, the *Principia* contains some problems of notable mathematical interest. Sections 4 and 5 of book I deal with conic sections, and section 6 with Kepler's problem; Newton here introduced the method of solution by successive iteration. Lemma 5 of book III treats of a locus through a given number of points, an example of Newton's widely used method of interpolating a function. Proposition 71, book I, contains Newton's important solution to a major problem of integration, the attraction of a sphere, called by Turnbull "the crown of all." Newton's proof that two spheres will mutually attract each other as if the whole of their masses were concentrated at their respective centers is posited on the condition that, however the mass or density may vary within each sphere as a function of that radius, the density at any given radius is everywhere the same (or is constant throughout any concentric shell).

The "Principia": General Plan. Newton's master-work was worked up and put into its final form in an incredibly short time. His strategy was to develop the subject of general dynamics from a mathematical point of view in book I, then to apply his most important results to solving astronomical and physical problems in book III. Book II, introduced at some point between Newton's first conception of the treatise and the completion of the printer's manuscript, is almost independent, and appears extraneous.

Book I opens with a series of definitions and axioms, followed by a set of mathematical principles and procedural rules for the use of limits; book III begins with general precepts concerning empirical science and a presentation of the phenomenological bases of celestial mechanics, based on observation.

It is clear to any careful reader that Newton was, in book I, developing mathematical principles of motion chiefly so that he might apply them to the physical conditions of experiment and observation in book III, on the system of the world. Newton maintained that even though he had, in book I, used such apparently physical concepts as "force" and "attraction," he did so in a purely mathematical sense. In fact, in book I (as in book II), he tended to follow his inspiration to whatever aspect of any topic might prove of mathematical interest, often going far beyond any possible physical application. Only in an occasional scholium in books I and II did he raise the question of whether the mathematical propositions might indeed be properly applied to the physical circumstances that the use of such words as "force" and "attraction" would seem to imply.

Newton's method of composition led to a certain amount of repetition, since many topics are discussed twice—in book I, with mathematical proofs, to illustrate the general principles of the motions of bodies, then again in book III, in application to the motions of planets and their satellites or of comets. While this mode of presentation makes the *Principia* more difficult for the reader, it does have the decided advantage of separating the Newtonian principles as they apply to the physical universe from the details of the mathematics from which they derive.

As an example of this separation, proposition I of book III states that the satellites of Jupiter are "continually drawn off from rectilinear motions, and are retained in their proper orbits" by forces that "tend to Jupiter's centre" and that these forces vary inversely as the square of their distances from that center. The proof given in this proposition is short and direct; the centripetal force itself follows from "Phen. I [of book III], and Prop. II or III, Book I." The phenomenon cited is a statement, based upon "astronomical observations," that a radius drawn from the center of Jupiter to any satellite sweeps out areas "proportional to the times of descriptions"; propositions 2 and 3 of book I prove by mathematics that under these circumstances the force about which such areas are described must be centripetal and proportional to the times. The inverse-square property of this force is derived from the second part of the phenomenon, which states that the distances from Jupiter's center are as the 3/2th power of their periods of revolution, and from corollary 6 to proposition 4 of book I, in which it is proved that centripetal force in uniform circular motion must be as the inverse square of the distance from the center.

Newton's practice of introducing a particular instance repeatedly, with what may seem to be only minor variations, may render the *Principia* difficult for the modern reader. But the main hurdle for any would-be student of the treatise lies elsewhere, in the essential mathematical difficulty of the main subject matter, celestial mechanics, however presented. A further obstacle is that Newton's mathematical vocabulary became archaic soon after the *Principia* was published, as dynamics in general and celestial mechanics in particular came to be written in the language of differentials and integrals still used today. The reader is thus required almost to translate for himself Newton's geometrical-limit mode of proof and statement into the characters of the analytic algorithms of the calculus. Even so, dynamics was taught directly from the *Principia* at Cambridge until well into the twentieth century.

In his "Mathematical Principles" Whiteside describes the *Principia* as''slipshod, its level of verbal fluency none too high, its arguments unnecessarily diffuse and repetitive, and its content on occasion markedly irrelevant to its professed theme: the theory of bodies moving under impressed forces." This view is somewhat extreme. Nevertheless, the work might have been easier to read today had Newton chosen to rely to a greater extent on general algorithms.

The *Principia* is often described as if it were a "synthesis," notably of Kepler's three laws of planetary motion and Galileo's laws of falling bodies and projectile motion; but in fact it denies the validity of both these sets of basic laws unless they be modified. For instance, Newton showed for the first time the dynamical significance of Kepler's so-called laws of planetary motion; but in so doing he proved that in the form originally stated by Kepler they apply exactly only to the highly artificial condition of a point mass moving about a mathematical center of force, unaffected by any other stationary or moving masses. In the real universe, these laws or planetary "hypotheses" are true only to the limits of ordinary observation, which may very well have been the reason that Newton called them "Hypotheses" in the first edition. Later, in the second and third editions, he referred to these relations as "Phaenomena," by which it may be assumed that he now meant that they were not simply true as stated (that is, not strictly deducible from the definitions and axioms), but were rather valid only to the limit of (or within the limits of) observation, or were phenomenologically true. In other words, these statements were to be regarded as not necessarily true, but only contingently (phenomenologically) so.

In the *Principia*, Newton proved that Kepler's planetary hypotheses must be modified by at least two factors: (1) the mutual attraction of each of any pair of bodies, and (2) the perturbation of a moving body by any and all neighboring bodies. He also showed that the rate of free fall of bodies is not constant, as Galileo had supposed, but varies with distance from the center of the earth and with latitude along the surface of the earth.[144] in a scholium at the end of section 2, book I, Newton further pointed out that it is only in a limiting case, not really achieved on earth, that projectiles (even *in vacuo*) move in Galilean parabolic trajectories, as Galileo himself knew full well. Thus, as Karl Popper has pointed out, although "Newton's dynamics achieved a unification of Galileo's terrestrial and Kepler's celestial physics," it appears that "from a logical point of view, Newton's theory, strictly speaking, contradicts both Galileo's and Kepler's."[145]

The "Principia": Definitions and Axioms. The *Principia* opens with two preliminary presentations: the "Definitions" and the "Axioms, or Laws of Motion." The first two entities defined are "quantity of matter," or "mass," and "quantity of motion." The former is said to be the measure of matter proportional to bulk and density conjunctively. "Mass" is, in addition, given as being generally known by its weight, to which it is proportional at any given place, as shown by Newton's experiments with pendulums, of which the results are more exact than Galileo's for freely falling bodies. Newton's "quantity of motion" is the entity now known as momentum; it is said to be measured by the velocity and mass of a body, conjunctively.

Definition 3 introduces *vis insita* (probably best translated as "inherent force"), a concept of which the actual definition and explanation are both so difficult to understand that much scholarly debate has been expended on them.[146] Newton wrote that the *vis insita* may be known by "a most significant name, *vis inertiae*." But this "force" is not like the "impressed forces" of definition 4, which change the state of rest or uniform rectilinear motion of a body; the *vis inertiae* merely maintains any new state acquired by a body, and it may cause a body to "resist" any change in state.[147]

Newton then defined "centripetal force" (*vis centripeta*), a concept he had invented and named to complement the *vis centrifuga* of Christiaan Huygens.[148] In definitions 6 through 8, Newton gave three "measures" of centripetal force, of which the most important for the purposes of the *Principia* is that one "proportional to the velocity which it generates in a given time" (for point masses, unit masses, or for comparing equal masses). There follows the famous scholium on space and time, in which Newton opted for concepts of absolute space and absolute time, although recognizing that both are usually reckoned by "sensible measures"; time, especially, is usually "relative, apparent, and common." Newton's belief in absolute space led him to hold that absolute motion is sensible or detectable, notably in rotation, although contemporaries as different in their outlooks as Huygens and Berkeley demurred from this view.

The "Axioms" or "Laws of Motion" are three in number: the law of inertia, a form of what is today known as the second law, and finally the law that "To every action there is always opposed an equal and opposite reaction." There is much puzzlement over the second law, which Newton stated as a proportionality between "change in motion" (in momentum) and "the motive force impressed" (a change "made in the direction …, in which that force is impressed"); he did not specify "per unit time" or "in some given time." The second law thus seems clearly to be stated for an impulse, but throughout the *Principia* (and, in a special case, in the antecedent definition 8), Newton used the law for continuous forces, including gravitation, taking account of time. For Newton, in fact, the concepts of impulse and continuous force were infinitesimally equivalent, and represented conditions of action "altogether and at once" or "by degrees and successively."[149] There are thus two conditions of "force" in the second law; accordingly, this Newtonian law may be written in the two forms $f \alpha\, d(mv)$ and $f \alpha\, d(mv)/dt$, in which both concepts of force are taken account of by means of two different constants of proportionality. The two forms of the law can be considered equivalent through Newton's concept of a uniformly flowing time, which makes dt a kind of secondary constant, which can arbitrarily be absorbed in the constant of proportionality.

There may be some doubt as to whether or not Newton himself was unclear in his own mind about these matters. His use of such expressions as "vis impressa" shows an abiding influence of older physics, while his continued reference to a "vis" or a "force" needed to maintain bodies in a state of motion raises the question of whether such usage is one of a number of possibly misleading "artifacts left behind in the historical development of his [Newton's] dynamics."[150] It must be remembered, of course, that throughout the seventeenth and much of the eighteenth century the word "force" could be used in a number of ways. Most notably, it served to indicate the concept now called "momentum," although it could also even mean energy. In Newton's time there were no categories of strict formalistic logic that required a unitary one-to-one correspondence between names and concepts, and neither Newton nor his contemporaries (or, for that matter, his successors) were always precise in making such distinctions.

The careful reader of books I—III should not be confused by such language, however, nor by the preliminary intrusion of such concepts. Even the idea of force as a measure of motion or of change of motion (or of change *per se,* or rate of change) is not troublesome in practice, once Newton's own formulation is accepted and the infinitesimal level of his discourse (which is not always explicitly stated) understood. In short, Newton's dynamical and mathematical elaboration of the three books of the *Principia* is tree of the errors and ambiguities implicit in his less successful attempt to give a logically simple and coherent set of definitions and axioms for dynamics. (It is even possible that the definitions and axioms may represent an independent later exercise, since there are, for example, varying sets of definitions and axioms for the same system of dynamics.) One of the most important consequences of Newton's analysis is that it must be one and the same law of force that operates in the centrally directed acceleration of the planetary bodies (toward the sun) and of satellites (toward planets), and that controls the linear downward acceleration of freely falling bodies. This force of universal gravitation is also shown to be the cause of the tides, through the action of the sun and the moon on the seas.

Book I of the "Principia." Book I of the *Principia* contains the first of the two parts of De motu *corporum*. It is a mathematical treatment of motion under the action of impressed forces in free spaces—that is, spaces devoid of resistance. (Although Newton discussed elastic and inelastic impact in the scholium to the laws, he did not reintroduce this topic in book I.) For the most part, the subject of Newton's inquiries is the motion of unit or point masses, usually having some initial inertial motion and being acted upon by a centripetal force. Newton thus tended to use the change in velocity produced in a given time (the "accelerative measure") of such forces, rather than the change in momentum produced in a given time (their "motive" measure).[151] He generally compared the effects of different forces or conditions of force on one and the same body, rather than on different bodies, preferring to consider a mass point or unit mass to computing actual magnitudes. Eventually, however, when the properties and actions of force had been displayed by an investigation of their "accelerative" and "motive" measures, Newton was able to approach the problem of their "absolute" measure. Later in the book he considered the attraction of spherical shells and spheres and of nonsymmetrical bodies.

Sections 2 and 3 are devoted to aspects of motion according to Kepler's laws. In proposition 1 Newton proceeded by four stages. He first showed that in a purely uniform linear (or purely inertial) motion, a radius vector drawn from the moving body to any point not in the line of motion sweeps out equal areas in equal times. The reason for this is clearly shown in

Figure 8, in which in equal times the body will move through the equal distances *AB, BC, CD, DE*, …, If a radius vector is drawn from a point *PS*, then triangles *ABS, BCS, CDS, DES*, … have equal bases and a common altitude *h*, and their areas are equal. In the second stage, Newton assumed the moving object to receive an impulsive force when it reaches point *B*. *A* component of motion toward *S* is thereby added to its motion toward *C*; its actual path is thus along the diagonal *Bc* of a parallelogram (Figure 9).

Newton then showed by simple geometry that the area of the triangle *SBc* is the same as the area of the triangle *SBC*, so that area is still conserved. He repeated the procedure in the third stage, with the body receiving a new impetus toward *S* at point *C*, and so on. In this way, the path is converted from a straight line into a series of joined line segments, traversed in equal intervals of time, which determine triangles of equal areas, with *S* as a common vertex.

In Newton's final development of the problem, the number of triangles is increased "and their breadth diminished in *infinitum*"; in the limit the "ultimate perimeter" will be a curve, the centripetal force "will act continually," and "any described areas" will be proportional to the times. Newton thus showed that inertial motion of and by itself implies an area-conservation law, and that if a centripetal force is directed to "an immovable centre" when a body has such inertial motion initially, area is still conserved as determined by a radius vector drawn from the moving body to the immovable center of force. (A critical examination of Newton's proof reveals the use of second-order infinitesimals.)[152] The most significant aspect of this proposition (and its converse, proposition 2) may be its demonstration of the hitherto wholly unsuspected logical connection, in the case of planetary motion, between Descartes's law of inertia and Kepler's law of areas (generalized to hold for an arbitrary central orbit).

Combining proposition 1 and proposition 2, Newton showed the physical significance of the law of areas as a necessary and sufficient condition for a central force (supposing that such forces exist; the "reality" of accelerative and motive forces of attraction is discussed in book III). In proposition 3, Newton dealt with the case of a body moving around a moving, rather than a stationary, center. Proposition 4 is concerned with uniform circular motion, in which the forces (F, f) are shown not only to be directed to the centers of the circles, but also to be to each other "as the squares of the ares [S, s] described in equal times divided respectively by the radii [R, r] of the circles" ($F: f = S/R^2 : s/r^2$). A series of corollaries demonstrate that $F:f = V^2/R : v^2\ r = R/T^2 : r/t^2$, where V, v are the tangential velocities, and so on; and that, universally, T being the period of revolution, if $T \alpha R^n$, $V \alpha 1/R^{n-1}$, then $F \alpha 1/R^{2n-1}$, and conversely. A special case of the last condition (corollary 6) is $T \alpha R^{3/2}$, yielding $F \alpha 1/R^2$, a condition (according to a scholium) obtaining "in the celestial bodies," as Wren, Hooke, and Halley "have severally observed," Newton further referred to Huygens' derivation, in *De horologio oscillatorio*, of the magnitude of "the centrifugal force of revolving bodies" and introduced his own independent method for determining the centrifugal force in uniform circular motion. In proposition 6 he went on to a general concept of instantaneous measure of a force, for a body revolving in any curve about a fixed center of force. He then applied this measure, developed as a limit in several forms, in a number of major examples, among them proposition 11.

The last propositions of section 2 were altered in successive editions. In them Newton discussed the laws of force related to motion in a given circle and equiangular (logarithmic) spiral. In proposition 10 Newton took up elliptical motion in which the force tends toward the center of the ellipse. A necessary and sufficient cause of this motion is that "the force is as the distance." Hence if the center is "removed to an infinite distance," the ellips "degenerates into a parabola," and the force will be constant, yielding "Galileo's theorem" concerning projectile motion.

Section 3 of book I opens with proposition 11, "If a body revolves in an ellipse; it is required to find the law of the centripetal force tending to the focus of the ellipse." The law is: "the centripetal force is inversely … as the square of the distance." Propositions 12 and 13 show that a hyperbolic and a parabolic orbit imply the same law of force to a focus. It is obvious that the converse condition, that the centripetal force varies inversely as the square of the distance, does not by itself specify which conic section will constitute the orbit. Proposition 15 demonstrates that in ellipses "the periodic times are as the 3/2th power of their greater axes" (Kepler's third law). Hence the periodic times in all ellipses with equal major axes are equal to one another, and equal to the periodic time in a circle of which the diameter is equal to the greater axis of each ellipse. In proposition 17, Newton supposed a centripetal force "inversely proportional to the squares of the distances" and exhibited the conditions for an orbit in the shape of an ellipse, parabola, or hyperbola. Sections 4 and 5, on conic sections, are purely mathematical.

In section 6, Newton discussed Kepler's problem, introducing methods of approximation to find the future position of a body on an ellipse, according to the law of areas; it is here that one finds the method of successive iteration. In section 7, Newton found the rectilinear distance through which a body falls freely in any given time under the action of a "centripetal force …

inversely proportional to the square of the distance … from the centre." Having found the times of descent of such a body, he then applied his results to the problem of parabolic motion and the motion of "a body projected upwards or downwards," under conditions in which "the centripetal force is proportional to the … distance." Eventually, in proposition 39, Newton postulated "a centripetal force of any kind" and found both the velocity at any point to which any body may ascend or descent in a straight line and the time it would take the body to get there. In this proposition, as in many in section 8, he added the condition of "granting the quadratures of curvilinear figures," referring to his then unpublished methods of integration (printed for the first time in the *De quadratura* of 1704).

In section 8, Newton often assumed such quadrature. In proposition 41 he postulated "a centripetal force of any kind" that is, as he added in proposition 42, he supposed "the centripetal force to vary in its recess from the center according to some law, which anyone may imagine at pleasure, but [which] at equal distances from the centre [is taken] to be everywhere the same." Under these general conditions, Newton determined both "the curves in which bodies will move" and "the times of their motions in the curves found." In other words, Newton presented to his readers a truly general resolution of the inverse problem of finding the orbit from a given law of force. He extended this problem into a dynamics far beyond that commonly associated with the *Principia*. In the ancillary proposition 40, for example, Newton (again under the most general conditions of force) had sought the velocity at a point on an orbit, finding a result that is the equivalent of an integral, which (in E. J. Aiton's words) in "modern terms … expresses the invariance of the sum of the kinetic and gravitational potential energies in an orbit."[153]

In section 11, Newton reached a level of mathematical analysis of celestial motions that fully distinguishes the *Principia* from any of its predecessors. Until this point, he there explained, he had been "treating of the attractions of bodies towards an immovable centre; though very probably there is no such thing existent in nature." He then outlined a plan to deal with nature herself, although in a "purely mathematical" way, "laying aside all physical considerations"—such as the nature of the gravitating force. "Attractions" are to be treated here as originating in bodies and acting toward other bodies; in a two-body system, therefore, "neither the attracted nor the attracting body is truly at rest, but both … being as it were mutually attracted, revolve about a common centre of gravity." In general, for any system of bodies that mutually attract one another, "their common centre of gravity will either be at rest, or move uniformly" in a straight line. Under these conditions, both members of a pair of mutually attractive bodies will describe "similar figures about their common centre of gravity, and about each other mutually" (proposition 57).

By studying such systems, rather than a single body attracted toward a point-center of force, Newton proved that Kepler's laws (or "planetary hypotheses") cannot be true within this context, and hence need modification when applied to the real system of the world. Thus, in proposition 59, Newton stated that Kepler's third law should not be written $T_1^2 : T_2^2 = a_1^3 : a_2^3$, as Kepler, Hooke, and everybody else had supposed, but must be modified.

A corollary that may be drawn from the proposition is that the law might be written as $(M + m_1)T_1^2 : (M + m_2)T_2^2 = a_1^3 : a_2^3$, where m_1, m_2 are any two planetary masses and M is the mass of the sun. (Newton's expression of this new relation may be reduced at once to the more familiar form in which we use this law today.) Clearly, it follows from Newton's analysis and formulation that Kepler's own third law may safely be used as an approximation in most astronomical calculations only because m^1Mg are very small in relation to M. Newton's modification of Kepler's third law fails to take account of any possible interplanetary perturbations. The chief function of proposition 59 thus appears to be not to reach the utmost generalization of that law, but rather to reach a result that will be useful in the problems that follow, most notably proposition 60 (on the orbits described when each of two bodies attracts the other with a force proportional to the square of the distance, each body "revolving about the common centre of gravity").

From proposition 59 onward, Newton almost at once advanced to various motions of mutually attractive bodies "let fall from given places" (in proposition 62), "going off from given places in given directions with given velocities" (proposition 63), or even when the attractive forces "increase in a simple ratio of their [that is, the bodies'] distances from the centres" (proposition 64). This led him to examine Kepler's first two laws for real "bodies," those "whose forces decrease as the square of their distances from their centres." Newton demonstrated in proposition 65 that in general it is not "possible that bodies attracting each other according to the law supposed in this proposition should move exactly in ellipses," because of interplanetary perturbations, and discussed cases in astronomy in which "the orbits will not much differ from ellipses." He added that the areas described will be only "very nearly proportional to the times."

Proposition 66 presents the restricted three-body problem, developed in a series of twenty-two corollaries. Here Newton attempted to apply the law of mutual gravitational attraction to a body like the sun to determine how it might perturb the motion of a moonlike body around an earthlike body. Newton examined the motion in longitude and in latitude, the annual equation, the evection, the change of the inclination of the orbit of the body resembling the moon, and the motion on the line of apsides. He considered the tides and explained, in corollary 22, that the internal "constitution of the globe" (of the earth) can be known "from the motion of the nodes." He further demonstrated that the shape of the globe can be derived from the precession constant (precession being caused, in the case of the earth, by the pull of the moon on the equatorial bulge of the spinning earth). He thus established, for the first time, a physical theory, elaborated in mathematical expression, from which some of the "inequalities" of the motion of the moon could be deduced; and he added some hitherto unknown ""inequalities" that he had found. Previous to Newton's work, the study of the irregularities in the motion of the moon had been posited on the elaboration of geometric models, in an attempt to make predicted positions agree with actual observations.[154]

Section 12 of book I contains Newton's results on the attractions of spheres, or of spherical shells. He dealt first with homogeneous, then nonogeneous spheres, the latter being composed of uniform and concentric spherical shells so that the density is the same at any single given distance from the center. In proposition 71 he proved that a "corpuscle" situated outside such a nonogeneous sphere is "attracted towards the centre of the sphere with a force inversely proportional to the square of its distance from the centre." In proposition 75, he reached the general conclusion that any two such spheres will gravitationally attract one another as if their masses were concentrated at their respective centers—or, in other words, that the distance required for the inverse-square law is measured from their centers. A series of elegant and purely mathematical theorems follow, including one designed to find the force with which a corpuscle placed inside a sphere may be "attracted toward any segment of that sphere whatsoever." In section 13, Newton, with a brilliant display of mathematics (which he did not fully reveal for the benefit of the reader) discussed the "attractive forces" of nonspherical solids of revolution, concluding with a solution in the form of an infinite series for the attraction of a body "towards a given plane."[155]

Book I concludes with section 14, on the "motion of very small bodies" acted on by "centripetal forces tending to the several parts of any very great body." Here Newton used the concept of "centripetal forces" that act under very special conditions to produce motions of corpuscles that simulate the phenomena of light—including reflection and refraction (according to the laws of Snell and Descartes), the inflection of light (as discovered by Grimaldi), and even the action of lenses. In a scholium, Newton noted that these "attractions bear a great resemblance to the reflections and refractions of light," and so

… because of the analogy there is between the propagation of the rays of light and the motion of bodies, I thought it not amiss to add the following Propositions for optical uses; not at all considering the nature of the rays of light, or inquiring whether they are bodies or not; but only determining the curves of [the paths of] bodies which are extremely like the curves of the rays.

A similar viewpoint with respect to mathematical analyses (or models and analogies) and physical phenomena is generally sustained throughout books I and II of the *Principia*.

Newton's general plan in book I may thus be seen as one in which he began with the simplest conditions and added complexities step by step. In sections 2 and 3, for example, he dealt with a mass-point moving under the action of a centripetal force directed toward a stationary or moving point, by which the dynamical significance of each of Kepler's three laws of planetary motion is demonstrated. In section 6, Newton developed methods to compute Keplerian motion (along an ellipse, according to the law of areas), which leads to "regular ascent and descent" of bodies when the force is not uniform (as in Galilean free fall) but varies, primarily as the inverse square of the distance, as in Keplerian orbital motion. In section 8 Newton considered the general case of "orbits in which bodies will revolve, being acted upon by any sort of centripetal force." From stationary orbits he went on, in section 9, to "movable orbits; and the motion of the apsides" and to a mathematical treatment of two (and then three) mutually attractive bodies. In section 10 he dealt with motion along surfaces of bodies acted upon by centripetal force; in section 12, the problems of bodies that are not mere points or point-masses and the question of the "attractive forces of spherical bodies"; and in section 13, "the attractive forces of bodies that are not spherical."

Book II of the "Principia." Book II, on the motion of bodies in resisting mediums, is very different from book 1. It was an afterthought to the original treatise, which was conceived as consisting of only two books, of which one underwent more or less serious modifications to become book I as it exists today, while the other, a more popular version of the "system of the world," was wholly transformed so as to become what is now book **III** . At first the question of motion in resisting mediums had been relegated to some theorems at the end of the original book I; Newton had also dealt with this topic in a somewhat similar manner at the end of his earlier tract *De motu*. The latter parts of the published book *II* were added only at the final redaction of the *Principia*.

Book II is perhaps of greater mathematical than physical interest. To the extent that Newton proceeded by setting up a sequence of mathematical conditions and then exploring their consequences, book II resembles book I. But there is a world of difference between the style of the two books. In book I Newton made it plain that the gravitational force exists in the universe, varying inversely as the square of the distance, and that this force accordingly merits our particular attention. In book II, however, the reader is never certain as to which of the many conditions of resistance that Newton considers may actually occur in nature.[156]

Book II enabled Newton to display his mathematical ingenuity and some of his new discoveries. Occasionally, as in the static model that he proposed to explain the elasticity and compressibility of gases according to Boyle's law, he could explore what he believed might be actual physical reality. But he nonetheless reminded his readers (as in the scholium at the end of section 1) that the condition of resistance that he was discussing was "more a mathematical hypothesis than a physical one." Even in his final argument against Cartesian vortices (section 9), he admitted the implausibility of the proposed hypothesis that "the resistance … is, other things being equal, proportional to the velocity." Although a scholium to proposition 52 states that "it is in truth probable that the resistance is in a less ratio than that of the velocity," Newton in fact never explored the consequences of this probable assumption in detail. Such a procedure is in marked contrast to book I, in which Newton examined a variety of conditions of attractive and centripetal forces, but so concentrated on the inverse-square force as to leave the reader in no doubt that this is the chief force acting (insofar as weight is concerned) on the sun, the planets, the satellites, the seas, and all terrestrial objects.

Book II differs further from book I in having a separate section devoted to each of the imagined conditions of resistance. In section 1, resistance to the motions of bodies is said to be as "the ratio of the velocity"; in section 2, it is as "the square of their velocities"; and in section 3, it is given as "partly in the ratio of the velocities and partly as the square of the same ratio." Then, in section 4, Newton introduced the orbital "motion of bodies in resisting mediums," under the mathematical condition that "the density of a medium" may vary inversely as the distance from "an immovable centre"; the "centripetal force" is said in proposition 15 to be as the square of the said density, but is thereafter arbitrary. In a very short scholium, Newton added that these conditions of varying density apply only to the motions of very small bodies. He supposed the resistance of a medium, "other things being equal," to be proportional to its "density."

In section 5, Newton went on to discuss some general principles of hydrostatics, including properties of the density and compression of fluids. Historically, the most significant proposition of section 5 is proposition 23, in which Newton supposed "a fluid [to] be composed of particles fleeing from each other," and then showed that Boyle's law ("the density" of a gas varying directly as "the compression") is a necessary and a sufficient condition for the centrifugal forces to "be inversely proportional to distances of their [that is, the particles'] centers."

Then, in the scholium to this proposition, Newton generalized the results, showing that for the compressing forces to "be as the cube roots of the power E^{n+2}," where *E* is "the density of the compressed fluid," it is both a necessary and sufficient condition that the centrifugal forces be "inversely as any power $D^{(n)}$ of the distance [between particles]." He made it explicit that the "centrifugal forces" of particles must "terminate in those particles that are next [to] them, or are diffused not much farther," and drew upon the example of magnetic bodies. Having set such a model, however, Newton concluded that it would be "a physical question" as to "whether elastic fluids [gases] do really consist of particles so repelling each other," and stated that he had limited himself to demonstrating "mathematically the property of fluids consisting of particles of this kind, that hence philosophers may take occasion to discuss that question."[157]

Section 6 introduces the "motion and resistance of pendulous bodies." The opening proposition (24) relates the quantity of matter in the bob to its weight, the length of the pendulum, and the time of oscillation in a vacuum. Because, as corollary 5 states, "in general, the quantity of matter in the pendulous body is directly as the weight and the square of the time, and inversely as the length of the pendulum," a method is at hand for using pendulum experiments to compare directly "the quantity of matter" in bodies, and to prove that the mass of bodies is proportional "to their weight." Newton added that he had tested this proposition experimentally, then further stated, in corollary 7, that the same experiment may be used for "comparing the weights of the same body in different places, to know the variation of its gravity."[158] This is the first clear recognition that "mass" determines both weight (the amount of gravitational action) and inertia (the measure of resistance to acceleration)—the two properties of which the "equivalence" can, in classical physics, be determined only by experiment.

In section 6 Newton also considered the motion of pendulums in resisting mediums, especially oscillations in a cycloid, and gave methods for finding "the resistance of mediums by pendulums oscillating therein." An account of such experiments makes up the "General Scholium" with which section 6 concludes.[159] Among them is an experiment Newton described from memory, designed to confute "the opinion of some that there is a certain aethereal medium, extremely rare and subtile, which freely pervades the pores of all bodies."

Section 7 introduces the "motion of fluids," and "the resistance made to projected bodies," and section 8 deals with wave motion. Proposition 42 asserts that "All motion propagated through a fluid diverges from a rectilinear progress into the unmoved spaces"; while proposition 50 gives a method of finding "the distances of the pulses," or the wavelength. In a scholium, Newton stated that the previous propositions "respect the motions of light and sound" and asserted that "since light is propagated in right lines, it is certain that it cannot consist in action alone (by Prop. XLI and XLII)"; there can be no doubt that sounds are "nothing else but pulses of the air" which "arise from tremulous bodies" This section concludes with various mathematical theorems concerning the velocity of waves or pulses, and their relation to the "density and elastic force of a medium."

In section 9, Newton showed that in wave motion a disturbance moves forward, but the parts (particles) of the medium in which the disturbance occurs only vibrate about a fixed position; he thereby established the relation between wavelength, frequency, and velocity of undulations. Proposition 47 (proposition 48 in the first edition) analyzes undulatory motion in a fluid; Newton disclosed that the parts (or particles) of an undulating fluid have the same oscillation as the bob of a simple pendulum. Proposition 48 (proposition 47 in the first edition) exhibits the proportionality of the velocity of waves to the square root of the elastic force divided by the density of an elastic fluid (one whose pressure is proportional to the density). The final scholium (much rewritten for the second edition) shows that Newton's propositions yield a velocity of sound in air of 979 feet per second, whereas experiment gives a value of 1,142 feet per second under the same conditions. Newton offered an ingenious explanation (including the supposition, in the interest of simplicity, that air particles might be rigid spheres separated from one another by a distance of some nine times their diameter), but it remained for Laplace to resolve the problem in 1816.[160]

Section 9, the last of book 11, is on vortices, or "the circular motion of fluids." In all editions of the *Principia*, this section begins with a clearly labeled "hypothesis" concerning the "resistance arising from the want of lubricity in the parts of a fluid … other things being equal, [being] proportional to the velocity with which the parts of the fluid are separated from one another." Newton used this hypothesis as the basis for investigating the physics of vortices and their mathematical properties, culminating in a lengthy proposition 52 and eleven corollaries, followed by a scholium in which he said that he has attempted "to investigate the properties of vortices" so that he might find out "whether the celestial phenomena can be explained by them." The chief "phenomenon" with which Newton was here concerned is Kepler's third (or harmonic) law for the motion of the satellites of Jupiter about that planet, and for the primary "planets that revolve about the Sun"—although Newton did not refer to Kepler by name. He found "the periodic times of the parts of the vortex" to be "as the squares of their distances." Hence, he concluded, "Let philosophers then see how that phenomenon of the 3/2th power can be accounted for by vortices."

Newton ended book II with proposition 53, also on vortices, and a scholium, in which he showed that "it is manifest that the planets are not carried round in corporeal vortices." He was there dealing with Kepler's second or area law (although again without naming Kepler), in application to elliptic orbits. He concluded "that the hypothesis of vortices is utterly irreconcilable with astronomical phenomena, and rather serves to perplex than to explain the heavenly motions." Newton himself noted that his demonstration was based on "an hypothesis," proposed "for the sake of demonstration … at the beginning of this Section," but went on to add that "it is in truth probable that the resistance is in a less ratio than that of the velocity." Hence "the periodic times of the parts of the vortex will be in a greater ratio than the square of the distances from its centre." But it must be noted that it is in fact probable that the resistance would be in a greater "ratio than that of the velocity," not a lesser, since almost all fluids give rise to a resistance proportional to the square (or higher powers) of the velocity.[161]

Book III, "The System of the World." In the Newtonian system of the world, the motions of planets and their satellites, the motions of comets, and the phenomena of tides are all comprehended under a single mode of explanation. Newton stated that the force that causes the observed celestial motions and the tides and the force that causes weight are one and the same; for this reason he gave the name "gravity" to the centripetal force of universal attraction. In book III he showed that the earth must be an oblate spheroid, and he computed the magnitude of the equatorial bulge in relation to the pull of the moon so as to produce the long-known constant of precession; he also gave an explanation of variation in weight (as shown by the change in the period of a seconds pendulum) as a function of latitude on such a rotating non-spherical earth. But above all, in book III Newton stated the law of universal gravitation. He showed that planetary motion must be subject to interplanetary perturbation—most apparent in the most massive planets, Jupiter and Saturn, when they are in near conjunction—and he explored the perturbing action of the sun on the motion of the moon.

Book III opens with a preface in which Newton stated that in books I and II he had set forth principles of mathematical philosophy, which he would now apply to the system of the world. The preface refers to an earlier, more popular version,[162] of which Newton had recast the substance "into the form of Propositions (in the mathematical way)."

A set of four "rules of reasoning in [natural] philosophy follows the preface. Rule I is to admit no more causes than are "true and sufficient to explain" phenomena, while rule 2 is to "assign the same causes" insofar as possible to "the same natural effects." In the first edition, rules 1 and 2 were called "hypotheses," and they were followed by hypothesis 3, on the possibility of the transformation of every body "into a body of any other kind," in the course of which it "can take on successively all the intermediate grades of qualities." This "hypothesis" was deleted by the time of the second edition.[163]

A second group of the original "hypotheses" (5 through 9) were transformed into "phenomena" 1 and 3 through 6. The first states (with phenomenological evidence) the area law and Kepler's third law for the system of Jupiter's satellites (again Kepler is not named as the discoverer of the law). Phenomenon 2, which was introduced in the second edition, does the same for the satellites of Saturn (just discovered as the *Principia* was being written, and not mentioned in the first edition, where reference is made only to the first [Huygenian] satellite discovered). Phenomena 3 through 6 (originally hypotheses 6 through 9) assert, within the limits of observation: the validity of the Copernican system (phenomenon 3); the third law of Kepler for the five primary planets and the earth—here for the first time in the *Principia* mentioning Kepler by name and thus providing the only reference to him in relation to the laws or hypotheses of planetary motion (phenomenon 4); the area law for the "primary planets," although without significant evidence (phenomenon 5); and the area law for the moon, again with only weak evidence and coupled with the statement that the law does not apply exactly since "the motion of the moon is a little disturbed by the action of the sun" (phenomenon 6).

It has been mentioned that Newton probably called these statements "phenomena" because he knew that they are valid only to the limits of observation. In this sense, Newton had originally conceived Kepler's laws as planetary "hypotheses," as he had also done for the phenomena and laws of planetary satellites.[164]

The first six propositions given in book III display deductions from these "phenomena," using the mathematical results that Newton had set out in book 1. Thus, in proposition 1, the forces "by which the circumjovial planets are continually drawn off from rectilinear motions, and retained in their proper orbits" are shown (on the basis of the area law discussed in propositions 2 and 3, book I, and in phenomenon 1) to be directed toward Jupiter's center. On the basis of Kepler's third law (and corollary 6, proposition 4, book 1) these forces must vary inversely as the square of the distance; propositions 2 and 3 deal similarly with the primary planets and our moon.

By proposition 5, Newton was able to conclude (in corollary 1) that there "is … a power of gravity tending to all the planets" and that the planets "gravitate" toward their satellites, and the sun "towards all the primary planets." This "force of gravity" varies (corollary 2) as the inverse square of the distance; corollary 3 states that "all the planets do mutually gravitate towards one another." Hence, "near their conjunction," Jupiter and Saturn, since their masses are so great, "sensibly disturb each other's motions," while the sun "disturbs" the motion of the moon and together both sun and moon "disturb our sea, as we shall hereafter explain."

In a scholium, Newton said that the force keeping celestial bodies in their orbits "has been hitherto called centripetal force since it is now "plain" that it is "a gravitating force" he will "hereafter call it gravity." In proposition 6 he asserted that "all bodies gravitate towards every planet"; while at equal distances from the center of any planet "the weight" of any body toward that planet is proportional to its "quantity of matter." He provided experimental proof, using a pair of eleven-foot pendulums, each weighted with a round wooden box (for equal air resistance), into the center of which he placed seriatim equal weights of wood and gold, having experimented as well with silver, lead, glass, sand, common salt, water, and wheat. According to proposition 24, corollaries I and 6, book II, any variation in the ratio of mass to weight would have appeared as a variation in the period; Newton reported that through these experiments he could have discovered a difference as small as less than one part in a thousand in this ratio, had there been any.[165]

Newton was thus led to the law of universal gravitation, proposition 7: "That there is a power of gravity tending to all bodies, proportional to the several quantities of matter which they contain." He had shown this power to vary inversely as the square of the distance; it is by this law that bodies (according to the third law of motion) act mutually upon one another.

From these general results, Newton turned to practical problems of astronomy. Proposition 8 deals with gravitating spheres and the relative masses and densities of the planets (the numerical calculations in this proposition were much altered for the second edition). In proposition 9, Newton estimated the force of gravity within a planet and, in proposition 10, demonstrated the long-term stability of the solar system. A general "Hypothesis I" (in the second and third editions; "Hypothesis IV" in the first) holds the "centre of the system of the world" to be "immovable," which center is given as the center of gravity of the solar system in proposition 11; the sun is in constant motion, but never "recedes" far from that center of gravity (proposition 12).

It is often asserted that Newton attained his results by neglecting the interplanetary attractions, and dealing exclusively with the mutual gravitational attractions of the planets and our sun. But this is not the case, since the most fully explored example of perturbation in the *Principia* is indeed that of the sun-earth-moon system. Thus Newton determined (proposition 25) the "forces with which the sun disturbs the motions of the moon," and (proposition 26) the action of those forces in producing an inequality ("horary increment") of the area described by the moon (although "in a circular orbit").

The stated intention of proposition 29 is to "find the variation of the moon," the inequality thus being sought being due "partly to the elliptic figure of the Moon's orbit, partly to the inequality of the moments of the area which the Moon by a radius drawn to the Earth describes." (Newton dealt with this topic more fully in the second edition.) Then Newton studied the "horary motion of the nodes of the moon," first (proposition 30) "in a circular orbit," and then (proposition 31) "in an elliptic orbit." In proposition 32, he found "the mean motion of the nodes," and, in proposition 33, their "true motion." (In the third edition, following proposition 33, Newton inserted two propositions and a scholium on the motion of the nodes, written by John Machin.) Propositions 34 and 35, on the inclination of the orbit of the moon to the ecliptic plane, are followed by a scholium, considerably expanded and rewritten for the second edition, in which Newton discussed yet other "inequalities" in the motion of the moon and developed the practical aspects of computing the elements of that body's motion and position.

Propositions 36 and 37 deal at length and in a quantitative fashion with the tide-producing forces of the sun and of the moon, yielding, in proposition 38, an explanation of the spheroidal shape of the moon and the reason that (librations apart) the same face of it is always visible. A series of three lemmas introduces the subject of precession and a fourth lemma (transformed into hypothesis 2 in the second and third editions) treats the precession of a ring. Proposition 39 represents an outstanding example of the high level of mathematical natural science that Newton reached in the *Principia*. In it he showed the manner in which the shape of the earth, in relation to the pull of the moon, acts on its axis of rotation so as to produce the observed precession, a presentation that he augmented and improved for the second edition. Newton here employed the result he had previously obtained (in propositions 20 and 21, book III) concerning the shape of the earth, and joined it to both the facts and theory of precession and yet another aspect of the perturbing force of the moon on the motion of the earth. He thus inaugurated a major aspect of celestial mechanics, the study of a three-body system.

Lemma 4, book III initiates a section on comets, proving that comets are "higher" than the moon, move through the solar system, and (corollary 1) shine by reflecting sunlight; their motion shows (corollary 3) that "the celestial spaces are void of resistance." Comets move in conic sections (proposition 40) having the sun as a focus, according to the law of areas. Those comets that return move in elliptic orbits (corollary 1) and follow Kepler's third law, but (corollary 2) "their orbits will be so near to parabolas, that parabolas may be used for them without sensible error."

Almost immediately following publication of the *Principia*, Halley, in a letter of 5 July 1687, urged Newton to go on with his work on lunar theory.[166] Newton later remarked that his head so ached from studying this problem that it often "kept him awake" and "he would think of it no more." But he also said that if he lived long enough for Halley to complete enough additional observations, he "would have another stroke at the moon." In the 1690's Newton had depended on Flamsteed for observations of the moon, promising Flamsteed (in a letter of 16 February 1695) not to communicate any of his observations, "much less publish them, without your consent." But Newton and Flamsteed disagreed on the value of theory, which Newton held to be useful as "a demonstration" of the "exactness" of observations, while Flamsteed believed that "theories do not command observations; but are to be tried by them," since "theories are … only probable" (even "when they agree with exact and indubitable observations"). At about this same time Newton was drawing up a set of propositions on the motion of the moon for a proposed new edition of the *Principia*, for which he requested from Flamsteed such planetary observations "as tend to [be useful for] perfecting the theory of the planets," to serve Newton in the preparation of a second edition of his book.

Revision of the "Opticks" (the Later Queries); Chemistry and Theory of Matter. Newton's *Opticks*, published in 1704, concluded with a Third Book, consisting of eleven "Observations" and sixteen queries, occupying a bare five pages of print. A Latin translation, undertaken at Newton's behest by Samuel Clarke, appeared in 1706, and included as its most notable feature the expansion of the original sixteen queries into twenty-three. The new queries 17 through 23 correspond to the final queries 25—31 of the later editions. In a series of "Errata, Corrigenda, & Addenda," at the beginning of the Latin volume, lengthy additions are provided to be inserted at the end of query 8 and of query 11; there is also a short insertion for query 14.

In a second English edition (London, 1717) the number of queries was increased to thirty-one. The queries appearing for the first time are numbered 17 to 24, and they have no counterparts in the 1706 Latin version. Newton's own copy of the 1717 English edition, in the Babson Institute Library, contains a number of emendations and corrections in Newton's hand, some of which were incorporated into the third edition (London 1721), as was a postscript to the end of the last sentence, referring to Noah and his sons.

The queries new to the 1717 edition cover a wide range of topics. Query 17 introduces the possibility that waves or vibrations may be excited in the eye by light and that vibrations of this sort may occur in the medium in which light travels. Query 18 suggests that radiant heat may be transmitted by vibrations of a medium subtler than air that pervades all bodies and expands by its elastic force throughout the heavenly spaces—the same medium by which light is put into "fits" of "easy" reflection and refraction, thus producing "Newton's rings." In queries 19 and 20, variations in the density of this medium are given as the possible cause of refraction and of the "inflection" (diffraction) of light rays. Query 21 would have the medium be rarer within celestial bodies than in empty celestial spaces, which may "impel Bodies from the denser parts of the Medium towards the rarer"; its elasticity may be estimated by the ratio of the speed of light to the speed of sound. Although he referred in this query to the mutually repulsive "particles" of ether as being "exceedingly smaller than those of Air, or even those of Light," Newton confessed that he does "not know what this *Aether* is."

In query 22, the resistance of the ether is said to be inconsiderable; the exhalations emitted by "electrick" bodies and magnetic "effluvia" are offered as other instances of such rareness. The subject of vision is introduced in query 23. Here vision is again said to be chiefly the effect of vibrations of the medium, propagated through the "optick Nerves"; an analogy is made to hearing and the other senses. Animal motion (query 24) is considered as a result of vibrations in the medium propagated from the brain through the nerves to the muscles.

Queries 25 to 31 are the English recasting of queries 17 to 23 of the Latin edition. Query 25 contains a discussion of double refraction in calcite (Iceland spar) and a geometrical construction of both the ordinary ray and (fallaciously) the extraordinary ray; query 26 concludes that double refraction may be caused by the two "sides" of rays of light. Then, in query 27, Newton attacked as erroneous all hypotheses explaining optical phenomena by new modifications of rays, since such phenomena depend upon original unalterable properties.

Query 28 questions "all Hypotheses" in which light is supposed to be a "Pression or Motion, propagated through a fluid Medium." Newton showed that Huygens' wave theory of double refraction would fail to account for the heating of bodies and the rectilinear propagation of light. Those who would fill "the Heavens with fluid Mediums" come under attack, while Newton praised the ancient philosophers who "made a *Vacuum*, and Atoms, and the Gravity of Atoms, the first Principles of their Philosophy." He added that "the main Business of natural Philosophy is to argue from Phaenomena without feigning Hypotheses"; we are to "deduce Causes from Effects, till we come to the very first Cause, which certainly is not mechanical," since nature exhibits design and purpose.

In query 29, Newton suggested that rays of light are composed of "very small Bodies emitted from shining Substances," since rays could not have a permanent virtue in two of their sides (as demonstrated by the double refraction of Iceland spar) unless they be bodies. This query also contains Newton's famous theory that rays of light could be put into "Fits of easy Reflexion and easy Transmission" if they were "small Bodies which by their attractive Powers, or some other Force, stir up Vibrations in what they act upon." These vibrations would move more swiftly than the rays themselves, would "overtake them successively," and by agitating them "so as by turns to increase and decrease their Velocities" would put them into those "fits."[167] Newton further argued that if light were to consist of waves in an ethereal medium, then in order to have the fits of easy reflection and easy transmission, a second ether would be required, in which there would be waves (of higher velocity) to put the waves of the first ether into the necessary fits. He had, however, already argued in query 28 that it would be inconceivable for two ethers to be "diffused through all Space, one of which acts upon the other, and by consequence is re-acted upon, without retarding, shattering, dispersing and compounding one another's Motions."

In query 30, Newton discussed the convertibility of gross bodies and light, with examples showing that nature delights in transmutations. In illustration, he cited Boyle's assertion that frequent distillations had turned water into earth. In query 31, he discussed questions ranging from the forces that hold particles of matter together to the impact of bodies on one another; also causes of motion, fermentation, the circulation of the blood and animal heat, putrefaction, the force of inertia, and occult qualities. He stated a general philosophy and concluded with the pious hope that the perfection of natural philosophy will enlarge the "Bounds of Moral Philosophy."

Newton's queries, particularly the later ones, thus go far beyond any simple questions of physical or geometrical optics. In them he even proposed tentative explanations of phenomena, although explanations that are perhaps not as fully worked out, or as fully supported by experimental evidence, as he might have wished. (Some queries even propose what is, by Newton's own definition, a hypothesis.) In each case, Newton's own position is made clear; and especially in the queries added in the Latin version of 1706 (and presented again in the English version of 1717/1718), his supporting evidence is apt to be a short essay.

One notable development of the later queries is the emphasis on an "Aethereal Medium" as an explanation for phenomena. In his first papers on optics, in the 1670's, Newton had combined his cherished conception of corpuscular or globular light with the possibly Cartesian notion of a space-filling ether, elastic and varying in density. Although Newton had introduced this ether to permit wave phenomena to exist as concomitants of the rays of light, he also suggested other possible functions for it—including causing sensation and animal motion, transmitting radiant heat, and even causing gravitation. His speculations on the ether were incorporated in the "Hypothesis" that he sent to the Royal Society (read at their meetings in 1675 and 1676) and in a letter to Boyle of 28 February 1679.[168]

In the second English edition of the *Opticks* (1717/1718) Newton made additions which "embodied arguments for the existence of an elastic, tenuous, aetherial medium." The new queries in the Latin version of 1706 did not deal with an ether, however, and by the time of the *Principia*, Newton may have "rejected the Cartesian dense aether" as well as "his own youthful aetherial speculations."[169]

Newton thus did not propose a new version of the ether until possibly the 1710's; he then suggested, in the general scholium at the conclusion of the second edition of the *Principia* (1713), that a most subtle "spiritus" ("which pervades and lies hid in all gross bodies") might produce just such effects as his earlier ether (or the later ethereal medium of queries 18 through 24). In the general scholium of the *Principia*, however, Newton omitted gravitation from the list of effects that the "spiritus" may produce. There is evidence that Newton conceived of this "spiritus" as electrical, and may well have been a precursor of the ether or ethereal medium of the 1717/1718 queries.[170] In a manuscript intended for the revised second English edition of the *Opticks*,[171] Newton wrote the heading, "The Third Book of Opticks. Part II. Observations concerning the Medium through which Light passes, & the Agent which emits it," a title that would thus seem to link the ethereal medium with the emission of electrical effluvia. It would further appear that Newton used both the earlier and later concepts of the ether to explain, however hypothetically, results he had already obtained; and that the concept of the ether was never the basis for significant new experiments or theoretical results. In a general scholium to book II, Newton described from memory an experiment that he had performed which seemed to him to prove the nonexistence of an ether; since Newton's original notes have never been found, this experiment, which was presumably an important element in the decline of his belief in an ether, cannot be dated.

The later queries also develop a concept of matter, further expounded by Newton in his often reprinted *De natura acidorum* (of which there appear to have been several versions in circulation).[172] Newton here, as a true disciple of Boyle, began with the traditional "mechanical philosophy" but added "the assumption that particles move mainly under the influence of what he at first called sociability and later called attraction."[173] Although Newton also considered a principle of repulsion, especially in gases, in discussing chemical reactions he seems to have preferred to use a concept of "sociability" (as, for example, to explain how substances dissolve).

He was equally concerned with the "aggregation" of particles (in queries 28 and 31 as well as at the end of *De natura acidorum*) and even suggested a means of "differentiating between reaction and transmutation."[174] Another major concern was the way in which aqua regia dissolves gold but not silver, while aqua fortis dissolves silver but not gold,[175] a phenomenon Newton explained by a combination of the attraction of particles and the relation between the size of the acid particles and the "pores" between the particles of metal. He did not, however, have a sound operational definition of acid, but referred to acids theoretically, in *De natura acidorum*, as those substances "endued with a great Attractive Force; in which Force their Activity consists." He maintained this definition in query 31, in which he further called attention to the way in which metals may replace one another in acid solutions and even "went so far as to list the six common metals in the order in which they would displace one another from a solution of aqua fortis (strong nitric acid)."[176]

Alchemy, Prophecy, and Theology. Chronology and History . Newton is often alleged to have been a mystic. That he was highly interested in alchemy has been embarrassing to many students of his life and work, while others delight in finding traces of hermeticism in the father of the "age of reason." The entries in the *catalogue of the Portsmouth Collection* give no idea of the extent of the documents in Newton's hand dealing with alchemy; these were listed in the catalogue, but not then presented to Cambridge University. Such information became generally available only when the alchemical writings were dispersed in 1936, in the Sotheby sale. The catalogue of that sale gives the only full printed guide to these materials, and estimates their bulk at some 650,000 words, almost all in Newton's hand.

A major problem in assessing Newton's alchemical "writings" is that they are not, for the most part, original compositions, nor even critical essays on his readings (in the sense that the early "De gravitatione et aequipondio fluidorum" is an essay based on his reading in Descartes's *Principia*). It would be necessary to know the whole corpus of the alchemical literature to be able to declare that any paper in Newton's hand is an original composition, rather than a series of extracts or summaries.[177]

In a famous letter to Oldenburg (26 April 1676), Newton offered an explanation of Boyle's presentations of the "incalescence" of gold and mercury (*Philosophical Transactions*, **9** , no. 122 [1675], 515–533), and presented an explanation based on the size of the particles of matter and their mechanical action. Newton particularly commended Boyle for having concealed some major steps, since here was possibly "an inlet into something more noble, and not to be communicated without immense dammage to the world if there be any verity in the Hermetick writers." He also gave some cautionary advice about alchemists, even referring to a "true Hermetic Philosopher, whose judgment (if there be any such)" might be of interest and highly regarded, “there being other things beside the transmutation of metalls (if those pretenders bragg not) which none but they understand.” The apparently positive declarations in Newton’s letter thus conflict with the doubts expressed in the two parenthetical expressions.

Newton’s studies of prophecy may possibly provide a key to the method of his alchemical studies. His major work on the subject is *Observations upon the Prophecies of Daniel, and the Apocalypse of St. John* (London, 1733). Here Newton was concerned with “a figurative language” used by the prophets, which he sought to decipher. Newton’s text is a historical exegesis, unmarked by any mystical short-circuiting of the rational process or direct communication from the godhead. He assumed an “analogy between the world natural, and an empire or kingdom considered as a world politic,” and concluded, for example, that Daniel’s prophecy of an “image composed of four metals” and a stone that broke “the four metals into pieces” referred to the four nations successively ruling the earth (“*viz*, the peoples of Babylonia, the Persians, the Greeks, and the Romans”). The four nations are represented again in the “four beasts.”

“The folly of interpreters,” Newton wrote, has been “to foretell times and things by this Prophecy, as if God designed to make them Prophets” This is, however, far from God’s intent, for God meant the prophecies “not to gratify men’s curiosities by enabling them to foreknow things” but rather to stand as witnesses to His providence when “after they were fulfilled, they might be interpreted by events.” Surely, Newton added, “the event of things predicted many ages before, will then be a convincing argument that the world is governed by providence.” (It may be noted that this book also provided Newton with occasion to refer to his favorite themes of “the corruption of scripture” and the “corruption of Christianity.”)

The catalogue of the Sotheby sale states that Newton’s manuscript remains include some 1,300,000 words on biblical and theological subjects. These are not particularly relevant to his scientific work and—for the most part—might have been written by any ordinary divinity student of that period, save for the extent to which they show Newton’s convinced anti-Trinitarian monotheism or Unitarian Arianism. (His tract *Two Notable corruptions of Scripture*, for example, uses historical analysis to attack Trinitarian doctrine.) “It is the temper of the hot and superstitious part of mankind in matters of religion,” Newton wrote, “ever to be fond of mysteries, and for that reason to like best what they understand least.”[178]

Typical of Newton’s theological exercises is his “Queries regarding the word *homoousios*” The first query asks “Whether Christ sent his apostles to preach metaphysics to the unlearned common people, and to their wives and children?” Other queries in this set are also historical; in the seventh Newton marshaled his historico-philological acumen in the matter of the Latin rendering *unius substantiae*, which he considered to have been imposed on the Western churches instead of *consubstantials* by “Hosius (or whoever translated that [Nicene] Creed into Latin).” Another manuscript entitled “Paradoxical Questions” turns out to he less a theological inquiry than a carefully reasoned proof of what Lord Keynes called “the dishonesty and falsification of records for which St Athanasius [and his followers] were responsible.” In it Newton cited, as an example, the spreading of the story that Arius died in a house of prostitution.

In a Keynes manuscript (in King’s College, Cambridge), “The First Book Concerning the Language of the Prophets,” Newton explained his method:

He that would understand a book written in a strange language must first learn the language … Such a language was that wherein the Prophets wrote, and the want of sufficient skill in that language is the reason why they are so little understood. John …, Daniel Isaiah … all write in one and the same mystical language … [which] so far as I can find, was as certain and definite in its signification as is the vulgar language of any nation. …

Having established this basic premise, Newton went on: “it is only through want of skill therein that Interpreters so frequently turn the Prophetic types and phrases to signify whatever their fancies and hypotheses lead them to” Then, in a manner reminiscent of the rules at the beginning of book III of the *Principia*, he added:

The rule I have followed has been to compare the several mystical places of scripture where the same prophetic phrase or type is used, and to fix such a signification to that phrase as agrees best with all the places: … and when I had found the necessary significations, to reject all others as the offspring of luxuriant fancy, for no more significations are to be admitted for true ones than can be proved.

Newton’s alchemical manuscripts show that he sometimes used a similar method, drawing up comparative tables of symbols and of symbolic names used by alchemists, no doubt in the conviction that a key to their common language might be found thereby. His careful discrimination among the alchemical writers may be seen in two manuscripts in the Keynes Collection, one a three-page classified list of alchemical writers and the other a two-page selection of “authores optimi,” by whom Newton perhaps meant authorities who described processes that might be repeated and verified. The Babson Collection of Newtoniana contains a two-page autograph manuscript listing 113 writers on alchemy arranged by nationalities and another seven-page manuscript of “chemical authors and their writings” in which Newton commented on the more important ones. At least two other such bibliographical works by Newton are known. An “Index Chemicus,” an elaborate subject index to the literature of alchemy with page references to a number of different works (described as containing more than 20,000 words on 113 pages), is one of at least five such indexes, all in autograph manuscripts.[179]

It must be emphasized that Newton's study of alchemy was not a wholly rational pursuit, guided by a strict code of linguistic and historical investigative procedures. To so consider it would be to put it on the same plane as his chronological inquiries.[180] The chronological studies are, to a considerable degree, the result of the application of sound principles of astronomical dating to poor historical evidence—for which his *chronology of Ancient Kingdoms Amended* was quite properly criticized by the French antiquarians of his day—while his alchemical works show that he drew upon esoterical and even mystical authors, far beyond the confines of an ordinary rational science.

It is difficult to determine whether to consider Newton's alchemy as an irrational vagary of an otherwise rational mind, or whether to give his hermeticism a significant role as a developmental force in his rational science. It is tempting, furthermore, to link his concern for alchemy with his belief in a secret tradition of ancient learning. He believed that he had traced this *prisca sapientia* to the ancient Greeks (notably Pythagoras) and to the Chaldean philosophers or magicians; he concluded that these ancients had known even the inverse-square law of gravitation. Cohen, MeGuire, and Rattansi have shown that in the 1690's, when Newton was preparing a revised edition of the Principia, he thought of including references to such an ancient tradition in a series of new scholia for the propositions at the beginning of book III of the *Principia*, along with a considerable selection of verses from Lucretius' *De natura rerum*. All of this was to be an addendum to an already created *Principia*, which Newton was revising for a new edition.

There is not a shred of real evidence, however, that Newton ever had such concerns primarily in mind in those earlier years when he was writing the *Principia* or initially developing the principles of dynamics and of mathematics on which the *Principia* was ultimately to be based. In Newton's record of alchemical experiments (University Library, Cambridge, MS Add. 3975), the experiments dated 23 May [1684] are immediately followed by an entry dated 26 April 1686. The former ends in the middle of a page, and the latter starts on the very next line; there is no lacuna, and no possibility that a page—which chronologically might concern experiments made while the *Principia* was being written—might be missing from the notebook.[181]

The overtones of alchemy are on occasion discernible in Newton's purely scientific writings. In query 30 of the *Opticks* (first published in the Latin version, then in the second English edition), Newton said that "Nature … seems delighted with Transmutations," although he was not referring specifically to changing metals from one to another. (It must be remembered in fact that "transmutation" would not necessarily hold an exclusively chemical or alchemical meaning for Newton; it might, rather, signify not only transformations in general, but also particular transformations of a purely mathematical sort, as in lemma 22 of book I of the *Principia*.) This is a far cry, indeed, from Newton's extracts from the mystical Count Michael Maier and kindred authors. P. M. Rattansi particularly calls attention to the alchemist's "universal spirit," and observes: "It is difficult to understand how, without a conviction of deep and hidden truths concealed in alchemy, Newton should have attached much significance to such ideas."[182]

Notable instances of the conflation of alchemical inspiration and science occur in Newton's letter to Boyle (1679) and in the hypothesis he presented to explain those properties of light of which he wrote in his papers in the *Philosophical Transactions*. While it is not difficult to discover alchemical images in Newton's presentation, and to find even specific alchemical doctrines in undisguised form and language, the problem of evaluating the influence of alchemy on Newton's true science is only thereby compounded, since there is no firm indication of the role of such speculations in the development of Newton's physical science. The result is, at best, one mystery explained by another, like the alchemist's confusing doctrine of *ignotum per ignotius*. Rattansi further suggests that alchemy may have served as a guiding principle in the formulation of Newton's views on fermentation and the nourishment of the vegetation of the earth by fluids attracted from the tails of comets. He would even have us believe that alchemical influences may have influenced "the revival of aetherical notions in the last period of Newton's life."[183] This may be so; but what, if any, creative effect such "aetherical notions" then had on Newton's thought would seem to be a matter of pure hypothesis.

Scholars do not agree whether Newton's association with some "Hermetic tradition" may have been a creative force in his science, or whether it is legitimate to separate his alleged hermeticism from his positive science. Apart from the level of general inspiration, it must be concluded that, excluding some aspects of the theory of matter and chemistry, notably fermentation, and possibly the ether hypotheses, the real creative influence of alchemy or hermeticism on Newton's mathematics and his work in optics, dynamics, and astronomy (save for the role of the tails of comets in the economy of nature) must today be evaluated in terms of the Scottish verdict, "not proven." Investigations of this topic may provide valuable insights into the whole man, Newton, and into the complexities of his scientific inspiration. His concern for alchemy and theology should not be cast aside as irrelevant aberrations of senility or the product of a mental breakdown. Yet it remains a fact beyond dispute that such early manuscripts as the Waste Book—in which Newton worked out and recorded his purely scientific discoveries and innovations—are free from the tinges of alchemy and hermeticism.

The London Years: the Mint, the Royal Society, Quarrels with Flamsteed and with Leibniz. On 19 March 1696, Newton received a letter from Charles Montagu informing him that he had been appointed warden of the mint. He set up William Whiston as his deputy in the Lucasian professorship, to receive "the full profits of the place." On 10 December 1701 he resigned his professorship, and soon afterward his fellowship. He was designated an *associé étranger* of the Paris Académic des Sciences in February 1699, chosen a member of the Council of the Royal Society on the following 30 November, and on 30 November 1703 was made president of the Royal Society, an office he held until his death. He was elected M.P. for Cambridge University, for the second time, on 26 November 1701, Parliament being prorogued on 25 May 1702. Queen Anne knighted Newton at Trinity College on 16 April 1705; on the following 17 May he was defeated in his third contest for the university's seat in Parliament.

At the mint, Newton applied his knowledge of chemistry and of laboratory technique to assaying, but he apparently did not introduce any innovations in the art of coinage. His role was administrative and his duties were largely the supervision of the recoinage and (curious to contemplate) the capture, interrogation, and prosecution of counterfeiters. Newton used the patronage of the mint to benefit fellow scientists. Halley entered the service in 1696 as comptroller of the Chester mint, and in 1707 David Gregory was appointed (at a fee of £250) as general supervisor of the conversion of the Scottish coinage to British.

Newton ruled over the Royal Society with an iron hand. When Whiston was proposed as a fellow in 1720, Newton said that if Whiston were chosen, he "would not be president." At Newton's urging, the council brought the society from the verge of bankruptcy to solvency by obtaining regular contributions from fellows. When a dispute arose between Woodward and Sloane, Newton had Woodward ejected from the council. Of Newton's chairmanship of meetings, Stukeley reported, "Everything was transacted with great attention and solemnity and dignity," for "his presence created a natural awe in the assembly"; there was never a sign of "levity or indecorum." As England's foremost scientist, president of the Royal Society, and civil servant, Newton appeared before Parliament in Spring 1714, to give advice about a prize for a method of finding longitude.

When Newton moved from Cambridge to London in the 1690's to take up the wardenship of the mint, he continued to work on the motion of the moon. He became impatient for Flamsteed's latest observations and they soon had a falling-out, no doubt aggravated by the strong enmity which had grown up between Halley and Flamsteed. Newton fanned the flames by the growing arrogance of his letters: "I want not your calculations but your observations only." And when in 1699 Flamsteed let it be known that Newton was working to perfect lunar theory, Newton sent Flamsteed a letter insisting that on this occasion he not "be brought upon the stage," since "I do not love to be printed upon every occasion much less to be dunned & teezed by foreigners about Mathematical things or to be thought by our own people to be trifling away my time about them when I should be about the King's business" Newton and Halley published Flamsteed's observations in an unauthorized printing in 1712, probably in the conviction that his work had been supported by the government and was therefore public property. Flamsteed had the bitter joy of burning most of the spurious edition; and he then started printing his own *Historia coelestis Brittanica*.

A more intense quarrel arose with Leibniz. This took two forms: a disagreement over philosophy or theology in relation to science (carried out through Samuel Clarke as intermediary), and an attempt on Newton's part to prove that Leibniz had no claim to originality in the calculus. The initial charge of plagiarism against Leibniz came from Fatio de Duiilier, but before long Keill and other Newtonians were involved and Leibniz began to rally his own supporters. Newton held that not only had Leibniz stolen the calculus from him, but that he had also composed three tracts for publication in the *Acta eruditorum* claiming some of the main truths of the *Principia* as independent discoveries, with the sole original addition of some mistakes. Today it appears that Newton was wrong; no doubt Leibniz had (as he said) seen the "epitome" or lengthy review of the Principia in the *Acta eruditorum* of June 1688, and not the book, when (to use his own words) "Newton's work stimulated me" to write out some earlier thoughts on "the causes of the motions of the heavenly bodies" as well as on the "resistance of a medium" and motion in a medium.[184] Newton stated, however, that even if Leibniz "had not seen the book itself, he ought nevertheless to have seen it before he published his own thoughts concerning these matters."[185]

That Newton should have connived at declaring Leibniz a plagiarist gives witness to his intense possessiveness concerning his discoveries or inventions; hence his consequent feeling of violation or robbery when Leibniz seemed to be publishing them. Newton was also aware that Leibniz must have seen one or more of his manuscript tracts then in circulation; and Leibniz had actually done so on one of his visits, when, however, he copied out some material on series expansions, not on fluxions.[186]

No one today seriously questions Leibniz' originality and true mathematical genius, nor his independence— to the degree that any two creative mathematicians living in the same world of mathematical thought can be independent—in the formulation of the calculus. Moreover, the algorithm in general use nowadays is the Leibnizian rather than the Newtonian. But by any normal standards, the behavior of both men was astonishing. When Leibniz appealed to the Royal Society for a fair hearing, Newton appointed a committee of good Newtonians. It has only recently become known that Newton himself wrote the committee's report, the famous *Cornmercium epistolicum*,[187] which he presented as if it were a set of impartial findings in his own favor.

Newton was not, however, content to stop there; following publication of the report there appeared an anonymous review, or summary, of it in the *Philosophical Transactions*. This, too, was Newton's work. When the *Cornmercium epistoticum* was reprinted, this review was included, in Latin translation, as a kind of introduction, together with an anonymous new preface "To the Reader," which was also written by Newton. This episode must be an incomparable display of thoroughness in destroying an enemy, and Whiston reported that he had heard directly that Newton had "once pleasantly" said to Samuel Clarke that "He had broke Leibnitz's Heart with his Reply to him."

Newton's later London years were marked by creative scientific efforts. During this time he published the *Opticks*, with the two mathematical tracts, and added new queries for its later editions. He also produced, with Roger Cotes's aid, a second edition of the *Principia*, including the noteworthy general scholium, and, with assistance from Henry Pemberton, a third edition. In the last, however, Newton altered the scholium to lemma 2, book II, to prevent its being read as if Leibniz were entitled to a share of credit for the calculus—although Leibniz had been dead for nearly twelve years.

Newton died on Monday, 20 March 1727,[188] at the age of eighty-five, having been ill with gout and inflamed lungs for some time. He was buried in Westminster Abbey.

Newton's Philosophy: The Rules of Philosophizing, the General Scholium, the Queries of the "Opticks." Like others of his day, Newton believed that the study of natural philosophy would provide evidence for the existence of God the Creator in the regularities of the solar system. In the general scholium at the end of book III of the *Principia*, he said "it is not to be conceived that mere mechanical causes could give birth to so many, regular motions," then concluded his discussion with observations about God, "to discourse of whom from phenomena does certainly belong to Natural Philosophy" ("Experimental Philosophy" in the second edition). He then went on to point out that he had "explained the phenomena of the heavens and of our sea, by the power of Gravity" but had not yet "assigned the cause of this power," alleging that "it is enough that Gravity does really exist, and act according to the laws which we have explained" and that its action "abundantly serves to account for all the motions of the celestial bodies, and of our sea." The reader was thus to accept the facts of the *Principia*, even though Newton had not "been able to discover the cause of those properties of gravity from phenomena." Newton here stated his philosophy, "Hypotheses non fingo."[189]

Clearly, Newton was referring here only to "feigning" a hypothesis about the cause of gravitation, and never intended that his statement should be applied on all levels of scientific discourse, or to all meanings of the word "hypothesis." Indeed, in each of the three editions of the *Principia*, there is a "hypothesis" stated in book II. In the second and third editions there are a "Hypothesis I" and a "Hypothesis II" in book III. Thc "phaenomena" at the beginning of book III, in the second and third editions, were largely the "hypotheses" of the first edition. It may be that Newton used these two designations to imply that these particular statements concerning planetary motions are not mathematically true (as he proved), but could be only approximately "true," on the level of (or to the limits of) phenomena.

Newton believed that his science was based upon a philosophy of induction, in the third edition of the *Principia*, he introduced rule 4, so that "the argument of induction may not be evaded by hypotheses." Here he said that one may look upon the results of "general induction from phenomena as accurately or very nearly true," even though many contrary hypotheses might be imagined, until such time as the inductive result may "either be made more accurate or liable to exceptions" by new phenomena, in rule 3, in the second and third editions, he stated his philosophical basis for establishing general properties of matter by means of phenomena.

Newton's philosophical ideas are even more fully developed in query 31, the final query of the later editions of the *Opticks*, in which he argued for both the philosophy of induction and the method of analysis and composition (or synthesis). In both mathematics and natural philosophy, he said, the "Investigation of difficult Things by the method of Analysis, ought ever to precede the Method of Composition." Such "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."

In both the Principia and the *Opticks*, Newton tried to maintain a distinction among his speculations. his experimental results (and the inductions based upon them), and his mathematical derivations from certain assumed conditions. In the *Principia* in particular, he was always careful to separate any mathematical hypotheses or assumed conditions from those results that were "derived" in some way from experiments and observations. Often, too, when he suggested, as in various scholiums, the applicability of mathematical or hypothetical conditions to physical nature, he stated that he had not proved whether his result really so applies. His treatment of the motion of small corpuscles, in book I, section 14, and his static model of a gas composed of mutually repulsive particles, in book II, proposition 23, exemplify Newton's use of mathematical models of physical reality for which he lacked experimental evidence sufficient for an unequivocal statement.

Perhaps the best expression of Newton's general philosophy of nature occurs in a letter to Cotes (28 March 1713), written during the preparation of the second edition of the *Principia*, in which he referred to the laws of motion as "the first Principles or Axiomes" and said that they "are deduced from Phaenomena & made general by Induction"; this "is the highest evidence that a Proposition can have in this philosophy." Declaring that "the mutual & mutually equal attraction of bodies is a branch of the third Law of motion." Newton pointed out to Cotes "how this branch is deduced from Phaenomena," referring him to the "end of the Corollaries of the Laws of Motion." Shortly thereafter, in a manuscript bearing upon the Leibniz controversy, he wrote. "To make an exception upon a mere Hypothesis is to feign an exception. It is to reject the argument from Induction, & turn Philosophy into a heap of Hypotheses, which are no other than a chimerical Romance."[190] That is a statement with which few would disagree.

NOTES

1. See R. S. Westfall. "Short-writing and the State or Newton's Conscience, 1662," in *Notes and Records. Royal Society of London*, **18** (1963), 10–16. L. T. More, in *Isaac Newton* (New York, 1934), p. 16, drew attention to the necessary "menial suffering" of a boy of Newton's physical weakness, living in a lonely "farmhouse situated in a countryside only slowly recovering from the terrors of a protracted ami bitter civil war," with "no proteetion from the frights of his imagination except that of his grandmother and such unreliable labourers as could be hired."

F. E. Manuel, in *A Portrait of Isaac Newton* (Cambridge, Mass., 1968), has subjected Newton's life to a kind of psychoanalytic scrutiny. He draws the conclusion (pp. 54–59) that the "scrupulosity, punitiveness. austerity, discipline, industriousness, and fear associated with a repressive morality" were apparent in Newton's character at an early age. and finds that notebooks bear witness to "the fear, anxiety, distrust, sadness, withdrawal, self-belittlement, and generally depressive state of the young Newton."

For an examination of Manuel's portrait of Newton, see J. E. McGuire, "Newton and the Demonic Furies: Some Current Problems and Approaches in the History of Science," in *History of Science*, **11** (1973), 36–46; see also the review in *Times Literary Supplement* (I June 1973), 615–616, With letters by Manuel (8 June 1973), 644–645; D. T. Whiteside 115 June 1973), 692, and (6 July 1973), 779; and G. S. Rousseau (29 June 1973), 749.

2. See E. N. da C. Andrade, "Newton's Early Notebook," in *Nature***135** (1935), 360; and G. L. Huxley, "Two Newtonian Studies: **1** . Newton's Boyhood Interests," in *Harvard Library Bulletin*, **13** (1959), 348–354. in which Andrade has first called attention to the importance of Bate's collection, an argument amplified by Huxley.

3. Newton apparently came to realize that he had been hasty in discarding Euclid, since Pemberton later heard him "even censure himself for not following them [that is, 'the ancients' in their 'taste, and form of demonstration'] yet more closely than he did; and speak with regret of his mistake at the beginning of his mathematical studies, in applying himself to the works of Des Cartes and other algebraic writers, before he had considered the elements of Euclide with that attention, which so excellent a writer deserves" (*View of Sir Isaac Newton's Philosophy* [London, 1728], preface).

4. Newton's college tutor was not (and indeed by statute could not have been) the Lucasian professor, Barrow, but was Benjamin Pulleyn.

5. University Library, Cambridge. MS Add. 3996, discussed by A. R. Hall in "Sir Isaac Newton's Notebook, 1661–1665," in *Cambridge HistaricalJotirnal*, **9** (1948), 239–250.

6.*Ibid.;* also partially analyzed by R. S. Westfall, in "The Foundations of Newton's Philosophy of Nature," in *British Journal for the History of Science*, **1** (1962), 171–182. Westfall; has attempted a reconstruction of Newton's philosophy of nature, and his growing allegiance to the "mechanical philosophy," in ch. 7 of his Force in *Newton's Physics* (London, 1971).

7. On Newton's entrance into the domains of mathematics higher than arithmetic, sec the account by A. De Moivre (in the Newton MSS presented by the late J. H. Schaffner to the University of Chicago) and the recollections of Newton assembled by John Conduitt, now mainly in the Keynes Collection, King's College, Cambridge.

8. See D. T. Whiteside, "Newton's Marvellous Year. 1666 and All That," in *Notes and Records. Royal Society of London*.**21** (1966), 37–38.

9. See A. H. White, ed., William Stukeley, *Memoirs of Sir Isaac Newton's Life* (London, 1936). Written in 1752, this records a conversation with Newton about his discovery of universal gravitation (the apple story), pp. 19–20.

10. In November 1669 John Collins wrote to James Gregory that "Mr Barrow hath resigned his Lecturers place to one Mr Newton of Cambridge" (in the Royal Society ed. of Newton's *Correspondence*, I, 15). Newton himself may have been referring to Barrow in an autobiographical note (ca. 1716) that stated, "Upon account of my progress in these matters he procured for me a fellowship … in the year 1667 & the Mathematick Professorship two years later"—see University Library, Cambridge, MS Add. 3968, Ş41, fol. 117, and I. B Cohen, *Introduction to New ton's Prineipia, supp*. III, p. 303, n. 14.

11. Among the biographical memoirs assembled by Conduitt (Keynes Collection, King's College, Cambridge). Humphrey Newton's memoir is in L. T. More, *Isaac Newton*, pp. 246, 381, and 389.

12. According to J. Edleston (p.xlv in his ed. of *Correspondence of Sir Isaac Newton and Professor Cotes* …; see also pp. xlix–I), in 1675 (or March 1674, OS), "Newton obtained a Royal Patent allowing the Professor to remain Fellow of a College without being obliged to go into orders." See also L. T. More, *Isaac Newton*, p. 169.

13. This work might have been an early version of the *Lectiones opticae*, his professorial lectures of 1670–1672; or perhaps an annotated version of his letters and communications to Oldenburg, which were read at the Royal Society and published in major part in its *Philosophical Transactions* from 1672 onward.

14. Quoted in L. T. More, *Isaac Newton*, p. 217.

15. It has been erroneously thought that Newton's "breakdown" may in part have been caused by the death of his mother. But her death occurred in 1679, and she was buried on 4 June. "Her will was proved 11 June 1679 by Isaac Newton, the executor, who was the residuary legatee": see *Correspondence*, II, 303. n. 2. David Brewster, in *Memoirs* … **II, 123,** suggested that Newton's "ailment may have arisen from the disappointment he experienced in the application of his friends for a permanent

situation for him." On these events and on contemporaneous discussion and gossip about Newton's state of mind, see L. T. More, *Isaac Newton*, pp. 387–388, and F. I. Manuel, *A Portrait of Isaac Newton*, pp. 220–223. Newton himself, in a letter to Locke of 5 October 1693, blamed his "distemper" and insomnia on "sleeping too often by my fire"

16. L. T. More, *Isaac Newton*, p. 368.

17. See J. Edleston, ed., *Correspondence … Newton and … Cotes*, pp. xxxvi, esp. n. 142.

18.*Mathematical Papers of Isaac Newton*, D. T. Whiteside, ed., in progress, to be completed in 8 vols. (Cambridge, 1967-); these will contain edited versions of Newton's mathematical writings with translations and explanatory notes, as well as introductions and commentaries that constitute a guide to Newton's mathematics and scientific life, and to the main currents in the mathematics of the seventeenth century, live volumes have been published (1973).

19. See D. T. Whiteside, "Newton's Discovery of the General Binomial Theorem," in *Mathematical Gazette*, **45** (1961), 175.

20. Especially because of Whiteside's researches.

21. Whiteside, ed., *Mathematical Papers*, **I** , 1–142. Whiteside concludes: "By and large Newton took his arithmetical symbolisms from Oughtred and his algebraical from Descartes, and onto them … he grafted new modifications of his own" (I, 11).

22. Ca, 1714; see University Library, Cambridge, MS Add. 3968, fol. 21. On this often debated point, see D. T. Whiteside, "Isaac Newton: Birth of a Mathematician," in *Notes* and *Records, Royal Society of London*, **19** (1964), n. 25; but compare n. 48, below.

23. University Library, Cambridge, MS Add. 3968. 41, fol. 85. This sentence occurs in a passage canceled by Newton.

24.*Ibid*., fol. 72. This accords with De Moivre's later statement (in the Newton manuscripts recently bequeathed the University of Chicago by J. H. Schalfner) that after reading Wallis' book, Newton "on the occasion of a certain interpolation for the quadrature of the circle, found that admirable theorem for raising a Binomial to a power given."

25. Translated from the Latin in the Royal Society ed. of the *Correspondence*, II, 20 ff. and 32 ff.; see the comments by Whiteside in *Mathematical Papers*, IV, 666 ff. In the second term, *A* stands for $P^{m/n}$(the first term), while in the third term *B* stands for (m/n) AQ (the second term), and so on. This letter and its sequel came into Wallis' hands and he twice published summaries of them, the second time with Newton's own emendations and grudging approval. Newton listed some results of series expansion—coupled with quadratures as needed—for $z = r \sin^{-1} [x/r]$ and the inverse $x = r \sin[z/r]$; the versed sine $r(1 - \cos[z/r])$; and $x = e^{z/b}$ the inverse of $z = b \log(1 + x)$ the Mercator series (see Whiteside, ed., *Mathematical Papers,* 1V, 668).

26. Translated from the Latin in the Royal Society ed. of the *Correspondence,* II, 110 II., 110 ff., see the comments by Whiteside in *Mathematical Papers*, IV, 672 ff.

27. See Whiteside, *Mathematical Papers* I, 106.

28. *Ibid*., 112 and n. 81.

29. The Boothby referred to may be presumed to be Boothby Pagnell (about three miles northeast of Woolsthorpe), whose rector, H. Babington, was senior fellow of Trinity and had a good library. See further Whiteside, *Mathematical Papers*, I, 8, n. 21; and n. 8, above.

30.*The Mathematical Works of Isaac Newton*, I, x.

31.*Ibid*., I, xi.

32. Here the "little zero" 0 is not, as formerly, the "indefinitely small" increment in the variable *t*, which "ultimately vanishes." In the *Principia*, bk. II, sec. 2, Newton used an alternative system of notation in which a, b, c, … are the "moments of any quantities A, B, C, &c," increasing by a continual flux or "the velocities of the mutations which are proportional" to those moments, that is, their fluxions.

33. Sec Whiteside, *Mathematical Works*, I, x.

34. See A. R. and M. B. Hall, eds., *Unpublished Scientific Papers of Isaac Newton* (Cambridge, 1962)

35.*Mathematical Works*, 1, xi.

36.*Ibid*., xii.

37. University Library, Cambridge, MS Add. 3968.41, fol. 86, v.

38. Whiteside, *Mathematical Papers*, II, 166.

39.*Ibid*., 166–167.

40.*Ibid*., I, II, n. 27. where Whiteside lists those "known to have seen substantial portions of Newton's mathematical papers during his lifetime" as including Collins, John Craig, Fatio de Duillier, Raphson, Halley, De Moivre, David Gregory, and William Jones, "but not, significantly, John Wallis," who did, however, see the "Epistola prior" and "Epistola posterior" (sec n. 25, above); and **II** , 168. Isaac Barrow "probably saw only the De analyst"

41.*The Methodus fluxiomim* also contained an amplified version of the tract of October 1666; it was published in English in 1736, translated by John Colson, but was not properly printed in its original Latin until 1779, when Horsley brought out *Analysis per quantitatum series, fluxiones, ac differentias*, incorporating William Jones's transcript, which he collated with an autograph manuscript by Newton. Various MS copies of the *Methodus fluxionum* had, however, been in circulation many years before 1693, when David Gregory wrote out an abridged version. Buffon translated it into French (1740) and Castillon used Colson's English version as the basis of a retranslation into Latin (*Opnsctda mathematica*, I, 295 ff.). In all these versions, Newton's equivalent notation was transcribed into dotted letters. Horsley (*Opera*, I) entitled his version *Artis analytical1 specimina vel geometm analytica*. The full text was first printed by Whiteside in *Mathematical Papers*, vol. **III.**

42.*Mathematical Papers*, **II** , 170.

43. P. xi; and see n. 41, above.

44. The reader may observe the confusion inherent in using both "indefinitely small portions of time" and "infinitely little" in relation to o; the use of index notation for powers (x^3, x^2, 0^2) together with the doubling of letters (*00*) in the same equation occurs in the original. These quotations are from the anonymous English version of 1737, reproduced in facsimile in Whiteside, ed., *Mathematical Works*. See n. 46.

45. In this example, I have (following the tradition of more than two centuries) introduced ẋ and ẏ where Newton in in his MS used m and n. In his notation, too, *r* stood for the later *z*.

46.*Mathematical Papers*, **III** , 80, n. 96. In the anonymous English version of 1737, as in Colson's translation of 1736, the word "indefinitely" appears; Castillon followed these (see n. 41). Horsley first introduced "*infinité*"

47.*Ibid*., pp. 16–17.

48. See Whiteside, *ibid*., p. 17; on Barrow's influence, see further pp. 71–74, notes 81, 82, 84.

49.*Ibid*., pp. 328–352. On p. 329, n. I, Whiteside agrees with a brief note by Alexander Witting (1911), in which the "source of the celebrated 'fluxiona' Lemma II of the second Book of Newton's Principia" was accurately found in the first theorem of this addendum; see also p. 331, n. 11, and p. 334, n. 16.

50. On this topic, see the collection of statements by Newton assembled in supp. I to I. B. Cohen, *Introduction to Newton's Principia*.

51. This and the following quotations of the *De quadratura* are from John Stewart's translation of 1745.

52. As C. B. Boyer points out, in *Concepts of the Calculus*, p. 201, Newton was thus showing that one should not reach the conclusion "by simply neglecting infinitely small terms, but by finding the ultimate ratio as these terms become evanescent." Newton unfortunately compounded the confusion, however, by not wholly abjuring infinitesimals thereafter; in bk. II, lemma 2, of the *Principia* he warned the reader that his "moments" were not finite quantities. In the eighteenth century, many English mathematicians, according to Boyer, "began to associate fluxions with the infinitely small differentials of Leibniz."

53. University Library, Cambridge, MS Add. 3960, fol. 177. Newton, however, was not the first mathematician to anticipate the Taylor series.

54. Inoduction to De quadrature in John Stewart, trans., Two Treatises of the Quadrature of Curves, and Analysis by Equations of an Infinite Number of Terms … (London, 1745), p. 4.

55. Philosophical Transactions, no. 342 (1715), 206.

56. Attributed to Newton, May 1708, in W. G. Hiscock, ed., *David Gregory, Isaac Newton and Their Circle* (Oxford, 1937), p. 42.

57. Henry Pemberton recorded, in his preface to his *View of … Newton's Philosophy* (London, 1728), that "I have often heard him censure the handling [of] geometrical subjects by-algebraic calculations; and his book of Algebra he called by the name of Universal Arithmetic, in opposition to the injudicious title of Geometry, which Des Cartes had given to the treatise wherein he shews, how the geometer may assist his invention by such kind of computations."

58. There were live Latin eds. between 1707 and 1761, of which one was supervised by Newton, and three English eds. between 1720 and 1769.

59. For details, see *Turnbull, The Mathematical Discoveries of Newton*, pp. 49–50.

60. See C B. Boyer, *History of Mathematics* p. 450.

61.*Arithmetica universalis*, English ed. (London, 1728), p. 247; see Whiteside, *Mathematical Papers,* V, 428–429, 470–471.

62.*Arithmetica universalis*, in Whiteside' translation. Mathematical Papers, V, 477.

63. Published by Whiteside, *Mathematical Papers*, I, pp. 145 ff.

64.*See especially ibid., pp.*298 ff., pt. 2, sec. 5, "The Calculus Becomes an Algorithm."

65.*ibid,*, in, pp. 120 tr.

66.*Ibid*.

67. In "Newton as an Originator of Polar Coördinates,"in *American Mathematical Monthly*. 56 (1949), 73–78.

68. Made available in English translation (perhaps supervised by Newton himself) in John Harris, *Lexicon technician*, vol. II (London, 1710); reprinted in facsimile (New-York, 1966). The essay entitled 'Curves" is reprinted in Whiteside, *Mathematical Papers II*,.

69. C. R. M. Talbot, ed. and trans., *Enumeration of Lines of the Third Order* (London, I860), p. 72.

70. On other aspects of Newton' mathematics see Whiteside. *Mathematical Papers*, specifically III, 50–52, on the development of infinite series; If, 218–232, on an iterative procedure for finding approximate solutions to equations; and I, 519, and V, 360, on "Newton' identities" for finding the sums of the powers of the roots in any polynomial equation. See, additionally, for Newton' contributions in porisms. solid loci, number theory, trigonometry, and interpolation, among other topics, Whiteside, *Mathematical Papers, passim, and Turnbuli, Mathematical Discoveries*.

71. See Whiteside. *Mathematical Works, I. XV, and Boyer, History of Mathematics*, p. 448. Drafts of the "Liber gcometria" are University Library, Cambridge, MS Add. 3963 *passim* and MS Add. 4004, fols. 129–159. Gregory' comprehensive statement of Newton' plans as of summer 1694 is in Edinburgh University Library, David Gregory MS (42; an English version in Newton' *Correspondence*, III, 384–386, is not entirely satisfactory.

72. Newton' laconic statement of his solution, published anonymously in *Philosophy al Transactions*, no. 224 (1697), p. 384, elicited from Bernoulli the reply "Ex ungue, Leonem" (the claw was sufficient to reveal the lion); see *Histoirc des outrages des suvans* (1697), 454–455.

73. See I. B. Cohen. "Isaac Newton, John Craig, and the Design of" in *Boston Studies for the Philsophy of Science* (in Press).

74. Even the variants in the eds. of the *Optieks* have never been fully documented in print (although Horsley's ed. gives such information for the Queries), nor have the differences between the Latin and English versions been fully analyzed. Zev Bechler is in the process of publishing four studies based on a perceptive and extensive examination of Newton's optical MSS. Henry Guerlac is presently engaged in preparing a new ed. of the *Optieks* itself.

75. The expression "experimentum cruces" is often attributed to Bacon, but Newton in fact encountered it in Hookes account of his optical experiments as given in *Micrographia* (observation 9), where Hooke referred to an experiment that "will prove such a one as our *thrice excellent Verulam* [that is, Francis Bacon] calls *Experimentum cruces*. While many investigators before Newton— Dietrich von Freiberg, Marci, Descartes, and Grimaldi among them—had observed the oval dispersion of a circular beam of light passing through a prism, they ail tended to assign the cause of the phenomenon to the consideration that the light source was not a point, but a physical object, so that light from opposite limbs of the sun would differ in angle of

inclination by as much as half a degree, Newton' measurements led him from this initial supposition to the conclusion that the effect—a spectrum some live times longer than its width—was too great for the given cause, and therefore the prism must refract some rays to a considerable degree more than others.

76. This account of the experiment is greatly simplified, as was Newton' own account, presented in his letter to Oldenburg and published in *Philosophical Transactions*, See J. A. Lohne, "Experimentum Crucis," in *Notes and Records. Royal Society of London*, **23** (1968), 169–199; Lohne has traced the variations introduced into both the later diagrams and descriptions of the experiment. Newton' doctrine of the separation of white light into its component colors, each corresponding to a unique and fixed index of refraction, had been anticipated by Johannes Marcus Marci de Kronland in his *Thaumantias, liber de arcu coelesti* (Prague, 1648). An important analysis of Newton' experiment is in A. I. Sabra, *Theories of Light*.

77. See R. S. Westfall, "The Development of Newton' Theory of Color," in Isis, **53** (1962), 339–358; and A. R. (1962), 339–358; and A. R. Hall, "Newton' Notebook," pp. 245–250.

78. Dated 13 April 1672, in *Philosophical Transactions*, no. 84.

79. See R. S. Westfall, "Newton' Reply to Hooke and the Theory of Colors," in his, 54 (1963), 82–96; an edited text of the "Hypothesis" is in *Correspondence* I, 362–386.

80. Published in Birch' *History of the Royal Society* and in I. B. Cohen, ed., *Newton' Papers* and Letters.

81. R. S. Westfall has further sketched Newton' changing views in relation to corpuscles and the ether, and, in "Isaac Newton' Coloured Circles Twixt Two Contiguous Glasses," in *Archive for History of Exact Sciences*, **2** (1965), 190, has concluded that "When Newton composed the Opticks, he had ceased to believe in an aether; the pulses of earlier years became 'fits of easy reflection and transmission,' offered as observed phenomena without explanation." Westfall discusses Newton' abandonment of the ether in "Uneasily Fitful Reflections on Fits of Easy Transmission [and of Easy Reflection]," in Robert Palter, ed., *The Annus Mirabilis of Sir Isaac Newton 16661966, pp. 88–104; he*emphasizes the pendulum experiment that Newton reported from memory in the Primipia (bk. II, scholium at the end of sec. 7, in the first edu or of sec. 6, in the 2nd and 3rd eds.). Henry Guerlac has discussed Newton's return to a modified concept of the ether in a series of studies (see Bibliography, sec. 8).

82. Birch, *History of the Royal Society*, III, 299; the early text of the "Discourse" is HI, 247–305, but Newton had already published it, with major revisions, as hook 11 of the *Optieks*. Both the "Hypothesis" and the "Discourse"are reprinted in Newton's *Papers and Letters*. 77 235. Newton's original notes on Hooke's *Micrographia* have been published by A. R. and M. B. Hall, *Unpublished Scientific Papers of Isaac Newton*, 400 ff., especially sec. 48, in which he refers to "coloured rings" of "8 or 9 such circuits" in this "order (white perhaps in the midst bless, purple, scarlet, yellow, greene, blew … "

83. Newton's notes on Hooke were first published by Geoffrey Keynes in *Bibliography of Robert Hooke* (Oxford, 1960), pp. 97–108. Hooke claimed in particular that Newton's "Hypothesis" was largely taken from the *Micrographia;* see Newton's letters to Oldenburg, 21 December 1675 and 10 January 1676, in *Correspondence*, 1, 404 ff. Hooke then wrote to Newton in a more kindly vein on 20 January 1676, provoking Newton's famous reply.

84. In this presentation, attention has been directed only to certain gross differences that exist between the texts of Newton's "Discourse of Observations" of 1675 and bk, II of the *Qpticks*. The elaboration of Newton's view may be traced through certain notebooks and an early essay "On Colours" to his optical lectures and communications to the Royal Society. In particular, R. S. Westfall has explored certain relations between the essay and the later *Opticks*. See also his discussion on Newton's experiments cited in a 81, above.

85. Chielly in University Library, Cambridge, MS Add. 3970; but sec n. 76.

86. University library, Cambridge, MS Dd. 9.67.

87. Now part of the Portsmouth Collection, University Library. Cambridge, MS Add. 4002. This MS has been reproduced in facsimile, with an introduction by Whiteside, as *The Unpublished First Version of Isaac Newton's Cambridge Lectures on Optics* (Cambridge, 1973).

88. The development of the *Optieks* can be traced to some degree through a study of Newton's correspondence, notebooks, and optical MSS, chiefly University Library, Cambridge, MS Add. 3970, of which the first 233 pages contain the autograph MS used for printing the 1704 ed., although the final query 16 is lacking. An early draft, without the preliminary definitions and axioms. begins on fol. 304; the first version of prop. 1, book I, here reads, "The light of one natural body is more refrangible than that of another." There are many drafts and versions of the later queries, and a number of miscellaneous items, including the explanation of animal motion and sensation by the action of an "electric" and "elastic" spirit and the attribution of an "electric force" to all living bodies. A draft of a proposed "fourth Book" contains, on fol. 336, a "Conclusion" altered to "Hypoth. I. The particles of bodies have certain spheres of activity with in which they attract or shun one another …"; in a subsequent version, a form of this is inserted between props. 16 and 17, white fl later prop. 18 is converted into "Hypoth. 2," which is followed shortly by hypotheses 3 to 5. It may thus be seen that Newton did not, in the 1690's, fully disdain

speculative hypotheses. On fol. 409 there begins a tract, written before the *Opticks*, entitled "Fundamentum Opticks," which is similar to the *Opticks* in form and content. The three major notebooks in which Newton entered notes on his optical reading and his early thoughts and experiments on light, color, vision, the rainbow, and astronomical refraction are MSS Add. 3975, 3996. and 4000.

89. In "Newton's Reply to Hooke and the Theory of Colors," in *Isis*, 54 (1963), 82–96; an analysis of the two versions of Newton's lectures on optics is given in I. B. Cohen, *Introduction to Newtons* 'Prineipia' supp. III.

90. See "Experimentum Crucis,' in *Sons and Records. Royal Society of London*, **23** (1968), 169–199.

91. See, notably, "Isaac Newton: The Rise of a Scientist 1661–1671," in *Notes and Records. Royal Society of London*, 20(1965), 125 139.

92. University Library, Cambridge, MS Add. 3996.

93. See Sabra, *Theories of Light;* also Westfall, "The Development of Newton's Theory of Color." in *Isis*, **53** (1962),339–358. A major source fox the development of Newton's optical concepts is, of course, the series of articles by Lohnc, esp. those cited in nn. 90 and 91.

94. The surviving pages of this abortive ed. are reproduced in I. B. Cohen, "Versions of Isaac Newton's First Published Paper, With Remarks on the Question of Whether Newton Planned to Publish an Edition of His Larly Papers on Light and Color," in Archives *internationales d'histoire des sciences*, *11* (1958). 357–375, 8 plates. See also A. R. Hall, "Newton's First Book," in *Archives internationales dl'histoire des sciences*, **13** (1960), 39–61.

95. In W. C Hiscock, ed., *David Gregory*, p. 15. The preface to the first ed. of the *Opticks* is signed "I.N."

96. See the "Analytical Table of Contents" prepared by Duane H, D. Roller for the Dover ed. of the *Opticks* (New York, 1952) for the contents of the entire work.

97.*Opticks*, book 1, pan 2, proposition 6. Newton's first statement of a musical analogy to color occurs in his "Hypothesis" of 1675; for an analysis of Newton's musical theory, see Correspondence, 1, 388, n. 14. which includes a significant contribution by J. E. Bullard.

98. As Boyer has pointed out, "In the Cartesian geometrical theory [of the rainbow] it matters little what light is, or how it is transmitted, so long as propagation is rectilinear and the laws of reflection and refraction are satisfied"; see *The Rainbow from Myth to Mathematics* (New York, 1959), ch. 9.

99. Although Newton had worked out the formula at the time of his optical lectures of 1669–1671, he published no statement of it until the *Opticks*. In the meantime Hallcy and Johann [I] Bernoulli had reached this formula independently and had published it; see Boyer, *The Rainbow*, pp. 247 If. In the *Opticks*, Newton offered the formula without proof, observing merely that "The Truth of all this Mathematicians will easily examine." His analysis is, however, given in detail in the *Lectiones opticae*, part I, section 4, propositions 35 and 36, as a note informs the reader of the 1730 ed. of the *Optieks*.
For a detailed analysis of the topic, see Whiteside. *Mathematical Papers*. III. 500–509.

100. Ernst Much. *The Principles of Physical Optics*, John S. Anderson and A. F. A. Young, trans. (London, 1926), 139.

101. This final sentence of book II, part 2, is a variant of a sentiment expressed a few paragraphs earlier: Now as all these things follow from properties of Light by a mathematical way of reasoning, so the truth of them may be manifested by Experiments."

102. The word "diffraction" appears to have been introduced into optical discourse by Grimaldi, in his Phwuo-mathesis de lumine eo riidse, et iride (Bologna, 1665), in which the opening proposition reads: "Lumen propagatur seu ditfunditur non solum Direct, Rcfracte ae Reflexe, sed ctiam alio quodam Quarto modo, DIFFRACTÈ." Although Newton mentioned Grimaldi by name (calling him "Grimaldo") and referred to his experiments, he did not use the term "dilfraction," but rather "inflexion," a usage the more curious in that it had been introduced into optics by none other than Hooke *Micrographia*, "Obs. LVIII. Of a new Property in the Air and several other transparent Mediums nam'd In/lection …"). Newton may thus have been making a public acknowledgment of his debt to Hooke; sec n. 83.

103. Newton's alleged denial of the possibility of correcting chromatic aberration has been greatly misunderstood. See the analysis of Newton's essay "Of Refractions" in Whiteside, *Mathematical Papers*, I, 549–550 and 559–576, esp. the notes on the theory or compound lenses, pp. 575–576, and notes 60 and 61. This topic has also been studied by Zev Bechler; see " 'A Less Agreeable Matter'—Newton and Achromatic Retraction" (in press).

104. Many of these are available in two collections: A. R. and M. B. Hall, eds., *Unpublished Scientific Papers;* and John Herivel, *The Background to New-tail's Principia*, See also the Royal Society's ed. of the *Correspondence*.

105. University Library, Cambridge, MS Add. 3996. first analyzed by A. R.Hall in 1948.

106. Ibid., fol. 29. See also R. S. Westfall, *Force in Newton's Physics*. Newton's entry concerning the third law was first published by Whiteside in 1964;see n. 114.

107. University Library, Cambridge, MS Add. 4004; Herivel also gives the dynamical portions, with commentaries.

108. Def. 4; see Herivel, *Background*, p.137.

109. Ibid., p.141.

110. See William Stukeley, *Memoirs of Sir Isaac Sew ton's Life*, p. 20; see also Douglas McKie and G. R. de Beer, "Newton's Apple," in *Notes and Records. Royal Society of London*, **9** (1952), 46–54, 333–335.

111. Various nearly contemporary accounts are given by W. W. Rouse Ball, An Essay on Newton's "Principal," ch. 1.

112. See F. Cajori, "Newton's Twenty Years Delay in Announcing the Law of Gravitation," in F. I Brasch, ed., *Sir Isaac Newton*. pp. 127–188.

113. This document, a tract on "circular motion," University Library, Cambridge, MS Add. 3958.5, fol. 87, was in major pan published for the first time by A. R. Hall in 1957. It has since been republished, with translation, in *Correspondence*. I. 297–300, and by Herivel in *Backgrounds* pp. 192 ff.

114. In "Newton's Early Thoughts on Planetary Motion: A fresh Look," in *British Journal for the History of Science*, 2(1964), 120, n. 13.

115. In A. R. and M. B. Hall, *Unpublished Papers*, pp. 89 ff.

116. University Library, Cambridge, MS Add. 3958, fols. 81–83; also in Turnbull, *Correspondence*, 111, 60–64.

117. Newton's concept of force has been traced, in its historical context, by Westfall, *Force in Newton s Physics;* see also Herivel, *Background*, and sec I. B, Cohen, "Newton's Second Law and the Concept of Force in the Principia" in R. Palter, ed., *Annus Mirabilis*, pp. 143–185.

118. In the scholium to the Laws of Motion, Newton mentioned that Wren. Wallis, and Huygens at "about the same time" communicated their "discoveries to the Royal Society"; they agreed "exactly among themselves" as to "the rules of the congress and reflexion of hard bodies."

119. Almost all discussions of Newton's spiral are based on a poor version of Newton's diagram; see J. A. Lohne, "The Increasing Corruption of Newton's Diagrams," in *History of Science*,**6** (1967), 69–89, esp. pp. 72–76.

120. Whiteside, "Newton's Early Thoughts; p. 135, has paraphrased Hooke's challenge as "Does the central force which, directed to a focus, deflects a body uniformly travelling in a straight line into an elliptical path vary as the inverse-square of its instantaneous distance from that focus ?"

121. University Library, Cambridge, MS Add. 3968.41, fol. 85r, first printed in *Catalogue of the Portsmouth Collection*, p. xviii; it is in fact part of a draft of a letter to Des Maizeaux, written in summer 1718, when Des Maizeaux was composing his ReeueiL In a famous MS memorandum (University Library, Cambridge, MS Add. 3968, fol. 101), Newton recalled the occasion of his correspondence with Hooke concerning his use of Kepler's area law in relation to elliptic orbits; see I. B. Cohen, *Introduction to Newton s Principia, supp*. L sec. 2.

122. University Library, Cambridge, MS Add. 3965.7, fols. 55r-62(bis)r; primed versions appear in A. R. and M. B. Hall, *Unpublished Papers;* J. Hcrivel, *Background;* and W, W. Rouse Ball, Essay.

123. See Whiteside, "Newton's Early Thoughts,"4 pp. 135–136; and see I. B. Cohen, "Newton's Second Law and the Concept of Force in the *Principia*" in R. Palter, ed., *Anmus Mirabilis*, pp. 143–185.

124. Analysis shows that great care is necessary in dealing with the limit process in even the simplest of Newton's examples, as in his early derivation of the Huygenian rule for centrifugal force (in the Waste Book, and referred to in the scholium to prop. 4, bk. I, in the *Principia*), or in the proof (props. 1–2, bk. I) that the law of areas is a necessary and sufficient condition

for a central force. Whiteside has analyzed these and other propositions in “Newtonian Dynamics.” pp. 109–111, and “Mathematical Principles,” pp. 11 ff., and has shown the logical pitfalls that await the credulous reader, most notably the implied use by Newton of infinitesimals of an order higher than one (chiefly those of the second, and occasionally those of the third, order).

125. See the *Principia*, props. 1–3, bk. 1, and the various versions of De motu printed by A. R. and M. B. Hall, J. Herivel, and W. W. Rouse Ball.

126. In *Correspondence*, 11, 436–437. This letter unambiguously shows that Newton did not have the solution to the problem of the attraction of a sphere until considerably later than 1679, and declaredly not “until last summer [1685].”

127. There is considerable uncertainty about what “curious treatise, *De Motu*” Halley saw; see I. B. Cohen, *Introduction*, ch. 3, sec. 2.

128. Ibid., sec.6.

129. First published by A. R. and M. B. Hall, *Unpublished Papers*.

130. Newton at first corresponded with Flamsteed indirectly, beginning in December 1680, through the agency of James Crompton.

131. In 1681, Newton still thought that the “comets” seen in November and December 1680 were “two different ones” (Newton to Crompton for Flamsteed, 28 February 1681, in *Correspondence* II, 342); in a letter to Flamsteed of 16 April 1681 (ibid., p. 364), Newton restated his doubts that “the Comets of November & December [were] but one.” In a letter of 5 January 1685 (ibid., p. 408), Flamsteed hazarded a “guess” at Newton’s “designe”: to define the curve that the comet of 1680 “described in the aether” from a general “Theory of motion,” while on 19 September 1685 (ibid., p. 419), Newton at last admitted to Flamsteed that “it seems very probable that those of November & December were the same comet,” Flamsteed noted in the margin of the last letter that Newton “would not grant it before,” adding, “see his letter of 1681,” In the *Arithmetica universalis* of 1707, Newton, in problem 52. explored the “uniform rectilinear motion” of a comet, “supposing the Copernican hypothesis” see Whiteside, *Mathematical Papers*, V, 299, n. 400, and esp. pp. 524 ff.

132. As far as actual Greek geometry goes, Newton barely makes use of Archimedes, Apollonius, or even Pappus (mentioned in passing in the preface to the 1st ed. of the Prineipia); see Whiteside, “Mathematical Principles,” p.7.

133. This is the tract “De methodis seriecrum et fluxionum,” printed with translation in Whiteside, ed., *Mathematical Papers*, III. 32 ff.

134. Motte has standardized the use of the neuter *genitum* in his English translation, although Newton actually wrote: “Momentum Genitae aequatur …,” and then said “Genitam voco quantitatem omnem quae…,” where *quantitas genita* (or “generated quantity”) is, of course, feminine.

135.*Whiteside, Mathematical Papers*, IV, 523, note 6.

136.*Concepts*, p. 200.

137. Ibid.; on Newton’s use of infinitesimals in the *Principia*, see also A. De Morgan, “On the Early History of Infinitesimals in England,” in *Philosophical Magazine*, **4** (1852), 321–330, in which he notes especially some changes in Newton’s usage from the 1687 to the 1713 eds. See further F. Cajori, *A History of the Conceptions of Limits*, pp. 2–32.

138. Whiteside, “Mathematical Principles,” pp. 20 ff.

139. Newton’s method, contained in University Library, Cambridge, MS Add. 3965.10, fols. 107v and 134v, will be published for the first time in Whiteside, *Mathematical Papers*, VI.

140. Halley refers to this specifically in the first paragraph of his review of the *Principia*, in *Philosophical Transactions of the Royal Society*, no. 186 (1687), p. 291.

141. Translated from University Library, Cambridge, MS Add. 3968, fol. 112.

142.*De quadratura* was printed, together with the other tracts in the collection published by W. Jones in 1711, as a supp. to the second reprint of the 2nd ed. of the *Principia* (1723).

143. In *Philosophical Transactions of the Royal Society* (1715), p. 206.

144. Newton was aware that a shift in latitude causes a variation in rotational speed, since $v = 2r/T \times \cos \phi$, where v is the linear tangential speed at latitude ϕ; r, T being the average values of the radius of the earth and the period of rotation. The distance from the center of the earth is also affected by latitude, since the earth is an oblate spheroid. These two factors appear in the variation with latitude in the length of a seconds pendulum.

145. "The Aim of Science," in *Ratio*, **1** (1957), 24–35; repr. in Karl Popper, *Objective Knowledge* (Oxford, 1972), 191–205.

146. See, for example, R. S. Westfall, *Force in Newton's Physics*. See also Alan Gabbey, "Force and Inertia in 17th-century Dynamics," in *Studies in History and Philosophy of Science*, **2** (1971), 1–67; Gabbey contests Westfall's point of view concerning the *vis insita*, in *Science*, **176** (1972), 157–159.

147. This would no longer even be called a force; some present translations, among them F. Cajori's version of Motte, anachronistically render Newton's *vis inertiae* as simple "inertia."

148. University Library, Cambridge, MS Add. 3968, fol. 415; published in A. Koyré and I. B. Cohen, "Newton and the Leibniz-Clarke Correspondence," in *Archives internationales d'histoire des sciences*, **15** (1962), 122–123.

149. See I.B. Cohen, "Newton's Second Law and the Concept of Force in the *Principia*," in R. Palter, ed., *Annus Mirabilis*, pp. 143–185.

150. R. S. Westfall, *Force*, p. 490. It is with this point of view in particular that Gabbey takes issue; see n. 146. See further E. J. Aiton, "The Concept of Force," in A. C. Crombie and M. A. Hoskin, eds., *History of Science*, X (Cambridge, 1971), 88–102.

151. In prop. 7, bk. III (referring to prop. 69, bk. I, and its corollaries), Newton argued from "accelerative" measures of forces to "absolute" forces, in specific cases of attraction.

152. See D. T. Whiteside, in *History of Science*, V (Cambridge, 1966), 110.

153. E. J. Aiton, "The Inverse Problem of Central Forces," in *Annals of Science*, **20** (1964), 82.

154. This position of the *Principia* was greatly altered between the 1st and 2nd eds.; Newton's intermediate results were summarized in a set of procedural rules for making up lunar tables and were published in a Latin version in David Gregory's treatise on astronomy (1702). Several separate English versions were later published; these are reprinted in facsimile in I.B. Cohen, *Newton's Theory of the Moon* (London, 1974).

155. W. W. Rouse Ball gives a useful paraphrase in *Essay*, p. 92.

156. See the analyses by Clifford Truesdell, listed in the bibliography to this article.

157. In his review of the *Principia*, in *Philosophical Transactions* (1687), p. 295, Halley referred specifically to this proposition, "which being rather a Physical than Mathematical Inquiry, our Author forbears to discuss."

158. This problem had gained prominence through the independent discovery by Halley and Richer that the length of a pendulum clock must be adjusted for changes in latitude.

159. This "General Scholium" should not be confused with the general scholium that ends the *Principia*. It was revised and expanded for the 2nd ed., where it appears at the end of sec. 6; in the 1st ed. it appears at the end of sec. 7.

160. In *Mécanique céleste*, V, bk. XII, ch. 3, sec. 7. Newton failed to take into account the changes in elasticity due to the "heat of compression and cold of rarefaction"; Laplace corrected Newton's formula , replacing it with his own where p is the air pressure and d the density of the air). Laplace, who had first published his own results in 1816, later said that Newton's studies on the velocity of sound in the atmosphere were the most important application yet made of the equations of motion in elastic fluids: "sa théorie, quoique imparfaite, est un monument de son genie" (*Méchanique céleste*, V, bk. XII, ch. 1, pp. 95–96). Lord Rayleigh pointed out that Newton's investigations "established that the velocity of sound should be independent of the amplitude of the vibration, and also of the pitch."

161. The confutation of Descartes's vortex theory was thought by men of Newton's century to be one of the major aims of bk. II. Huygens, for one, accepted Newton's conclusion that the Cartesian vortices must be cast out of physics, and wrote to Leibniz to find out whether he would be able to continue to believe in them after reading the *Principia*. In "my view," Huygens wrote, "these vortices are superfluous if one accepts the system of Mr. Newton."

162. On the earlier tract in relation to bk. III of the *Principia*, see the preface to the repr. (London, 1969) and I. B. Cohen, *Introduction*, supp. VI.

163. AT one time, according to a manuscript note, Newton was unequivocal that hypothesis 3 expressed the belief of Aristotle, Descartes, and unspecified "others." It was originally followed by a hypothesis **4** , which in the 2nd and 3rd eds. was moved to a later part of bk. III. For details, see I. B. Cohen, "Hypotheses in Newton's Philosophy," in *Physis*, **8** (1966), 163–184.

164. See *De motu* in A.R. and M.B. Hall, *Unpublished Papers*, and J. Herivel, *Background*.

165. Newton apparently never made the experiment of comparing mass and weight of different quantities of the same material.

166. There has been little research on the general subject of Newton's lunar theory; even the methods he used to obtain the results given in a short scholium to prop. 35, bk. I, in the 1st ed., are not known. W. W. Rouse Ball, in *Essay*, p. 109, discusses Newton's formula for "the mean hourly motion of the moon's apogee," and says, "The investigation on this point is not entirely satisfactory, and from the alterations made in the MS. Newton evidently felt doubts about the correctness of the coefficient 11/2 which occurs in this formula. From this, however, he deduces quite correctly that the mean annual motion of the apogee resulting would amount to 38° 51'51," whereas the annual motion" is known to be 40° 41' 30" His discussion is based upon the statement, presumably by J. C. Adams, in the preface to the *Catalogue of the Portsmouth Collection* (Cambridge, 1888), pp. xii-xiii. Newton's MSS on the motion of the moon—chiefly University Library, Cambridge, MS Add. 3966—are one of the major unanalyzed collections of his work. For further documents concerning this topic, and a scholarly analysis by A. R. Hall of some aspects of Newton's researches on the motion of the moon, see *Correspondence*, V (in press), and I. B. Cohen, intro. to a facsimile repr. of Newton's pamphlet on the motion of the moon (London, in press).

167. Although Newton had suspected the association of color with wavelength of vibration as early as his "Hypothesis" of 1675, he did not go on from his experiments on rings, which suggested a periodicity in optical phenomena, to a true wave theory—no doubt because, as A. I. Sabra has suggested, his a priori "conception of the rays as discrete entities or corpuscles" effectively "prevented him from envisaging the possibility of an undulatory interpretation in which the ray, as something distinguished from the waves, would be redundant" (*Theories of Light*, p. 341).

168. Both printed in facsimile in 1. B. Cohen, ed., *Isaac Newton's Papers and Letters on Natural Philosophy*. They were published and studied in the eighteenth century and had a significant influence on the development of the concept of electric fluid (or fluids) and caloric. This topic is explored in some detail in I. B. Cohen, *Franklin and Newton* (Philadelphia, 1956; Cambridge, 1966; rev. ed. in press). esp. chs. 6 and 7.

169. Henry Guerlac has studied the development of the queries themselves, and in particular the decline of Newton's use of the ether until its reappearance in a new form in the queries of the 2nd English ed. He has also noted that the concept of the ether is conspicuously absent from the Latin ed. of 1706, See especially his "Newton's Optical Aether" in *Notes and Records. Royal Society of London*, **22** (1967), 43–57. See, further, Joan L. Hawes, "Newton's Revival of the Aether Hypothesis ...," *ibid*., **23** (1968), 200–212.

170. A. R. and M. B. Hall have found evidence that Newton thought of this "spiritus" as electrical in nature; see *Unpublished Papers*, pp. 231 ff., 348 ff. Guerlac has shown that Newton was fascinated by Hauksbee's electrical experiments and by certain experiments of Desaguliers; see bibliography for this series of articles.

171. University Library, Cambridge, MS Add. 3970, sec. 9, fols. 623 IT.

172. These works, especially queries 28 and 31, have been studied in conjunction with Newton's MSS (particularly his notebooks) by A. R. and M. B. Hall, D. McKie, J. R. Partington, R. Kargon, J. E. McGuire, A. Thackray, and others, in their elucidations of a Newtonian doctrine of chemistry or theory of matter. *De natura acidorum* has been printed from an autograph MS, with notes by Pitcairne and transcripts by David Gregory, in *Correspondence*, III, 205–214. The first printing, in both Latin and English, is reproduced in I. B. Cohen, ed., *Newton's Papers and Letters*, pp. 255–258.

173. According to M. B. Hall, "Newton's Chemical Papers," in *Newtons Papers and Letters*, p. 244.

174.*Ibid*., p. 245.

175. Discussed by T. S. Kuhn, "Newton' '31st Query'and the Degradation of Gold," in *Isis*.**42** (1951), 296–298.

176. M. B. Hall, "Newton's Chemical Papers," p. 245; she continues that there we may find a "forerunner of the tables of affinity" developed in the eighteenth century, by means of which "chemists tried to predict the course of a reaction."

177. In "Newton's Chemical Experiments," in *Archives internationals d'histoire des sciences*, **11** (1958), 113–152— a study of Newton's chemical notes and papers—A. R. and M. B. Hall have tried to show that Newton's primary concern in these matters was the chemistry of metals, and that the writings of alchemists were a major source of information on every aspect of metals. Humphrey Newton wrote up a confusing account of Newton's alchemical experiments, in which he said that Newton's guide was the *De re metallica* of Agricola; this work, however, is largely free of alchemical overtones and concentrates on mining and metallurgy.

178. R. S. Westfall, in *Science and Religion in Seventeenth-Century England*, ch. 8, draws upon such expressions by Newton to prove that "Newton was a religious rationalist who remained blind to the mystic's spiritual communion with the divine."

179. These MSS are described in the Sotheby sale catalog and by F. Sherwood Taylor, in "An Alchemical Work of Sir Isaac Newton," in *Ambix*, **5** (1956), 59–84.

180. These have been the subject of a considerable study by Frank E. Manuel, *Isaac Newton, Historian* (Cambridge, Mass.. 1964).

181. Newton's interest in alchemy mirrors all the bewildering aspects of that subject, ranging from the manipulative chemistry of metals, mineral acids, and salts, to esoteric and symbolic (often sexual) illustrations and mysticism of a religious or philosophical kind. His interest in alchemy persisted through his days at the mint, although there is no indication that he at that time still seriously believed that pure metallic gold might be produced from baser metals— if, indeed, he had ever so believed. The extent of his notes on his reading indicate the seriousness of Newton's interest in the general subject, but it is impossible to ascertain to what degree, if any, his alchemical concerns may have influenced his science, beyond his vague and general commitment to "transmutations" as a mode for the operations of nature. But even this belief would not imply a commitment to the entire hermetic tradition, and it is not necessary to seek a unity of the diverse interests and intellectual concerns in a mind as complex as Newton's.

182. P. M. Rattansi, "Newton's Alchemical Studies," in Allen Debus, ed., *Science, Medicine and Society in the Renaissance*, **II** (New York, 1972), 174.

183. The first suggestion that Newton's concept of the ether might be linked to his alchemical concerns was made by Taylor; see n. 179, above.

184. Leibniz, *Tentamen* … ("An Essay on the Cause of the Motions of the Heavenly Bodies"), in *Acta eruditorum* (Feb. 1689), 82–96, English trans, by E. J. Collins. Leibniz' marked copy of the 1st ed. of the *Principia*, presumably the one sent to him by Fatio de Duillier at Newton's direction, is now in the possession of E. A. Fellmann of Basel, who has discussed Leibniz' annotations in "Die Marginalnoten von Leibniz in Newtons Principia Mathematica 1687," in *Humanismus und Technik*, **2** (1972), 110–129; Fellmann's critical ed., G. W. Leibniz, *Marginalia in Newtoni Principia Mathematica* 1687 (Paris, 1973), includes facsimiles of the annotated pages.

185. Translated from some MS comments on Leibniz' essay, first printed in Edleston, *Correspondence*, pp. 307–314.

186. Leibniz' excepts from Newton's *De attalysi*, made in 1676 from a transcript by John Collins, have been published from the Hannover MS by Whiteside, in *Mathematical Papers*, II, 248–258. Whiteside thus demonstrates that Leibniz was "clearly interested only in its algebraic portions: fluxional sections are ignored."

187. Several MS versions in his hand survive in University Library, Cambridge, MS Add. 3968.

188. At this period the year in England officially began on Lady Day. 25 March. Hence Newton died on 20 March 1726 old style, or in 1726/7 (to use the form then current for dates in January, February, and the first part of March).

189. In the 2-vol, ed. of the *Principia* with variant readings edited by A. Koyré, I. B. Cohen, and Anne Whitman; Koyre has shown that in the English *Opticks* Newton used the word "feign" in relation to hypotheses, in the sense of "lingo" in the slogan, a usage confirmed by example in Newton's MSS. Motte renders the phrase as "I frame no hypotheses." Newton himself in MSS used both "feign" and "frame" in relation to hypotheses in this regard; see I. B. Cohen, "The First English Version of Newton's *Hypotheses non fingo,"* in *Isis*, **53** (1962), 379–388.

190. University Library, Cambridge, MS Add. 3968, fol. 437.

BIBLIOGRAPHY

This bibliography is divided into four major sections. The last, by A. P. Youschkeviteh, is concerned with Soviet studies on Newton and is independent of the text.

Original Works (numbered I-IV): Newton's major writings, together with collected works and editions, bibliographies, manuscript collections, and catalogues.

Secondary Literature (numbered V-VI): including general works and specific writings about Newton and his life.

SOURCES (numbered 1–11): the chief works used in the preparation of this biography; the subdivisions of this section are correlated to the subdivisions of the biography itself.

Soviet Literature: a special section devoted to Newtonian scholarship in the Soviet Union.

The first three sections of the bibliography contain a number of cross-references; a parenthetical number refers the reader to the section of the bibliography in which a complete citation may be found.

ORIGINAL WORKS

I. Major Works. Newton's first publications were on optics and appeared in the *Philosophical Transactions of the Royal Society* (1672–1676); rcpr. in facs., with intro. by T. S. Kuhn, in I. B. Cohen, ed., *Isaac Newton's Papers & Letters on Natural Philosophy*(Cambridge, Mass., 1958; 2nd ed., in press). His *Opticks* (London, 1704; enl. versions in Latin [London, 1706], and in English [London, 1717 or 1718]) contained two supps.: his *Enumeratio linearum tertii ordinis and Tractatus de quadratura curvarum*, his first published works in pure mathematics. The 1704 ed. has been repr. in facs. (Brussels, 1966) and (optical part only) in type (London, 1931); also repr. with an analytical table of contents prepared by D. H. D. Roller (New York, 1952). French trans, are by P. Coste (Amsterdam, 1720; rev. ed. 1722; facs. repr., with intro. by M. Solovine, Paris, 1955); a German ed. is W. Abendroth, 2 vols. (Leipzig, 1898); and a Rumanian trans, is Victor Marian (Bucharest, 1970). A new ed. is currently being prepared by Henry Guerlac.

The *Philosophiae naturalis principia mathematica* (London, 1687; rev. eds., Cambridge, 1713 [repr. Amsterdam, 1714, 1723], and London, 1726) is available in an ed. with variant readings (based on the three printed eds., the MS for the 1st ed. and Newton's annotations in his own copies of the 1st and 2nd eds.) prepared by A. Koyré, I. B. Cohen, and Anne Whitman: *Isaac Newton's Philosophiae naturalis principia mathematica, the Third Edition (1726) With Variant Readings*, 2 vols. (Cambridge, Mass.-Cambridge, England, 1972). Translations and excerpts have appeared in Dutch, English, French, German, Italian, Japanese, Rumanian, Russian, and Swedish, and are listed in app. VIII, vol. II, of the Koyre, Cohen, and Whitman ed., together with an account of reprs. of the whole treatise. The 1st ed. has been printed twice in facs. (London, 1954[?]; Brussels, 1965).

William Jones published Newton's *De analysi* in his ed. of *Analysis per quantitatum series, fluxiones, ac differentias* …(London, 1711), repr. in the Royal Society's *Commercium epistolicum D. Johannis Collins, et aliorum de analyst promota* … (London, 1712–1713; enl. version, 1722; "variorum" ed. by J.-B. Biot and F. Lefort, Paris, 1856), and as an appendix to the 1723 Amsterdam printing of the *Principia*. Newton's *Arithmetica universalis* was published from the MS of Newton's lectures by W. Whiston (Cambridge, 1707); an amended ed. followed, supervised by Newton himself (London, 1722). For bibliographical notes on these and some other mathematical writings (and indications of other eds. and translations), see the introductions by D. T. Whiteside to the facs. repr. of *The Mathematical Works of Isaac Newton* 2 vols. (New York-London, 1964–1967). Newton's *Arithmetica universalis* was translated into Russian with notes and commentaries by A. P. Youschkevitch (Moscow, 1948); English eds. were published in London in 1720, 1728, and 1769.

After Newton's death the early version of what became bk. III of the Principia was published in English as *A Treatise of the System of the World* (London, 1728; rev. London, 1731, facs. repr., with intro. by I. B. Cohen, London, 1969) and in Latin as *De mundi systemate liber* (London, 1728). An Italian trans, is by Marcella Renzoni (Turin, 1959; 1969). The first part of the *Lectiones opticae* was translated and published as *Optical Lectures* (London, 1728) before the full Latin ed. was printed (1729); both are imperfect and incomplete. The only modern ed. is in Russian, *Lektsii po optike* (Leningrad, 1946), with commentary by S. I. Vavilov.

For Newton's nonscientific works (theology, biblical studies, chronology), and for other scientific writings, see the various sections below.

II. Collected Works OR Editions. The only attempt ever made to produce a general ed. of Newton was S. Horsley, *Isaaci Newtoni opera quae exstant omnia*, 5 vols. (London, 1779–1785; photo repr. Stuttgart-Bad Cannstatt, 1964), which barely takes account of Newton's available MS writings but has the virtue of including (vol. I) the published mathematical tracts; (vols. II-III) the Principia and *De mundi systemate, Theoria lunae, and Lectiones opticae;* (vol. IV) letters from the *Philosophical Transactions* on light and color, the letter to Boyle on the ether, *De problematis Bernoullianis*, the letters to Bentley, and the *Commercium epistolicum*; (vol. V) the *Chronology* the *Prophecies*, and the *Corruptions of Scripture*. An earlier and more modest collection was the 3-vol. *Opuscula mathematica, philosophica, et philologica*, Giovanni Francesco Salvemini (known as Johann Castillon), ed. (Lausanne-Geneva, 1744); it contains only works then in print.

A major collection of letters and documents, edited in the most exemplary manner, is Edleston (1); Rigaud's *Essay* (5) is also valuable. S. P. Rigaud's *Correspondence of Scientific Men of the Seventeenth Century … in the collection of… the Earl of Macclesfield*, 2 vols. (Oxford, 1841; rev., with table of contents and index, 1862) is of special importance because the Macclesfield collection is not at present open to scholars.

Four vols. of the Royal Society's ed. of Newton's *Correspondence* (Cambridge, 1959-) have (as of 1974) been published, vols. I—III edited by II. W. Turnbull, vol. IV by J. F. Scott; A. R. Hall has been appointed editor of the succeeding volumes. The *Correspondence* is not limited to letters but contains scientific documents of primary importance. A recent major collection is A. R. and M. B. Hall, eds., *Unpublished Scientific Papers of Isaac Newton, a Selection From the Portsmouth Collection in the University Library, Cambridge*(Cambridge, 1964). Other presentations of MSS are given in the ed. of the

Principia with variant readings (1972, cited above), Herivel's *Background* (5), and in D. T. Whiteside's ed. of Newton's *Mathematical Papers* (3).

III.Bibliographies. There are three bibliographies of Newton's writings, none complete or free of major error. One is George J. Gray, *A Bibliography of the Works of Sir Isaac Newton, Together With a List of Books Illustrating His Works*, 2nd ed., rev. and enl. (Cambridge, 1907; repr. London, 1966); H. Zeitlinger, "A Newton Bibliography' pp. 148–170 of the volume ed. by W. J. Greenstreet (VI); and *A Descriptive Catalogue of the Grace K. Babson Collection of the Works of Sir Isaac Newton…* (New York, 1950), plus A *Supplement* … compiled by Henry P. Macomber (Babson Park, Mass., 1955), which lists some secondary materials from journals as well as books.

IV. Manuscript Collections AND Catalogues. The Portsmouth Collection (University Library, Cambridge) was roughly catalogued by a syndicate consisting of H. R. Luard, G. G. Stokes, J. C.Adams, and G. D. Liveing, who produced *A Catalogue of the Portsmouth Collection of Books and Papers Written by or Belonging to Sir Isaac Newton …*(Cambridge, 1888); the bare descriptions do not always identify the major MSS or give the catalogue numbers (*e.g*., the Waste Book, U.L.C. MS Add. 4004, the major repository of Newton's early work in dynamics and in mathematics, appears as "A common-place book, written originally by B. Smith, D.D., with calculations by Newton written in the blank spaces. This contains Newton's first idea of Fluxions"). There is no adequate catalogue or printed guide to the Newton MSS in the libraries of Trinity College (Cambridge), the Royal Society of London, or the British Museum. The Keynes Collection (in the library of King's College, Cambridge) is almost entirely based on the Sotheby sale and is inventoried in the form of a marked copy of the sale catalogue, available in the library; see A. N. L. Munby, "The Keynes Collection of the Works of Sir Isaac Newton at King's College, Cambridge," in Notes and Records. Royal Society of London, 10 (1952), 40–50. The "scientific portion" of the Portsmouth Collection was given to Cambridge University in the 1870's; the remainder was dispersed at public auction in 1936. See Sotheby's *Catalogue of the Newton Papers, Sold by Order of the Viscount Lymington, to Whom They Have Descended From Catherine Conduitt, Viscountess Lymington, Great-niece of Sir Isaac Newton* (London, 1936). No catalogue has ever been made available of the Macclesfield Collection (rich in Newton MSS), based originally on the papers of John Collins and William Jones, for which see S. P. Rigaud's 2-vol. *Correspondence* … (I). Further information concerning MS sources is given in Whiteside, *Mathematical Papers*, I, xxiv-xxxiii (3).

Many books from Newton's library are in the Trinity College Library (Cambridge); others are in public and private collections all over the world. R. de Villamil, *Newton: The Man* (London, 1931 [?]; repr., with intro. by 1. B. Cohen, New York, 1972), contains a catalogue (imperfect and incomplete) of books in Newton's library at the time of his death; an inventory with present locations of Newton's books is greatly to be desired. See P. E. Spargo, "Newton's Library," in *Endeavour*, **31** (1972), 29–33, with short but valuable list of references. See also *Library of Sir Isaac Newton. Presentation by the Pilgrim Trust to Trinity College Cambridge 30 October 1943 (Cambridge, 1944), described on pp. 5–7 of Thirteenth Annual Report of the Pilgrim Trust* (Harlech, 1943).

SECONDARY LITERATURE

V. Guides TO THE Secondary Literature. For guides to the literature concerning Newton, see … *Catalogue* … *Babson Collection* … (III); and scholarly eds., such as *Mathematical Papers* (3), *Principia* (I), and *Correspondence* (II). A most valuable year-by-year list of articles and books has been prepared and published by Clelia Pighetti:"Cinquant'anni di studi newtoniani (1908–1959)," in *Rivista critica di storia delta filosofia*, **20** (1960), 181–203,295–318. See also Magda Whitrow, ed., *ISIS Cumulative Bibliography…* 1913–65, II (London, 1971), 221–232. Two fairly recent surveys of the literature are I. B. Cohen, "Newton in the Light of Recent Scholarship," in *Isis*, **51** (1960), 489–514; and D. T. Whiteside, "The Expanding World of Newtonian Research," in *History of Science*, 1 (1962), 16 29.

VI. General Works. Biographies (*e.g*., by Stukeley, Brewster, More, Manuel) are listed below (1). Some major interpretative works and collections of studies on Newton are Ferd. Rosenberger, *Isaac Newton and seine physikalischen Principien* (Leipzig, 1895); Leon Bloch, *La philosophic de Newton* (Paris, 1908); S. I. Vavilov, *Isaak Nyuton; nauchnaya biografia i stati,* 3rd ed. (Moscow, 1961), German trans, by Josef Grön as *Isaac Newton* (Vienna, 1948), 2nd ed., rev., German trans, by Franz Boncourt (Berlin, 1951); Alexandre Koyré, *Newtonian Studies* (London-Cambridge, Mass., 1965) which, posthumously published, contains a number of errors—a more correct version is the French trans., *Études newtoniennes* (Paris, 1968), with an *avertissement* by Yvon Belaval; and Alberto Pala, *Isaac Newton, scienza e fihsofia* (Turin, 1969).

Major collections of Newtonian studies include W. J. Greenstreet, ed., *Isaac Newton* 1642–1727 (London, 1927); F. E. Brasch, ed., *Sir Isaac Newton* 1727–1927 (Baltimore, 1928); S. I. Vavilov, ed., Isaak Nyuton 1643[n.s.]–1727, a symposium in Russian (Moscow-Leningrad, 1943); Royal Society, *Newton Tercentenary Celebrations, 15–19 July 1946*(Cambridge, 1947); and Robert Palter, ed.. *The Annus Mirabilis of Sir Isaac Newton 1666–1966* (Cambridge, Mass., 1970), based on an earlier version in *The Texas Quarterly*, **10** , no. 3 (autumn 1967).

On Newton's reputation and influence (notably in the eighteenth century), see Hélène Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris, 1930), and *Attraction universelle et religion naturelle chez quelques commentateurs anglais de Newton* (Paris, 1938); Pierre Brunt, *L'introduction des théories de Newton en France au XVIIIε siècle, I, Avant 1738* (Paris, 1931); Marjorie Hope Nicolson, *Newton Demands the Muse, Newton's Opticks and the Eighteenth Century Poets* (Princeton, 1946); I. B. Cohen, *Franklin and Newton, an Inquiry Into Speculative Newtonian Experimental Science…* (Philadelphia, 1956;

Cambridge, Mass., 1966; rev. repr. 1974); Henry Guerlac, "Where the Statue Stood: Divergent Loyalties to Newton in the Eighteenth Century," in Earl R. Wasserman, ed., *Aspects of the Eighteenth Century* (Baltimore, 1965), pp. 317–334; R. E. Schofield, *Mechanism and Materialism, British Natural Philosophy in an Age of Reason* (Princeton, 1970); Paolo Casini, *L'universo-macchina, origini della filosofia newtoniana* (Bari, 1969); and Arnold Thackray, *Atoms and Powers, an Essay in Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass., 1970). Still of value today are three major eighteenth-century expositions of the Newtonian natural philosophy, by Henry Pemberton, Voltaire, and Colin Maclaurin.

Whoever studies any of Newton's mathematical or scientific writings would be well advised to consult J. A. Lohne, "The Increasing Corruption of Newton's Diagrams," in *History of Science*, **6** (1967), 69–89.

Newton's MSS comprise some 20–25 million words; most of them have never been studied fully, and some are currently "lost," having been dispersed at the Sotheby sale in 1936. Among the areas in which there is a great need for editing of MSS and research are Newton's studies of lunar motions (chiefly U.L.C. MS Add. 3966); his work in optics (chiefly U.L.C. MS Add. 3970; plus other MSS such as notebooks, etc.); and the technical innovations he proposed for the *Principia* in the 1690's (chiefly U.L.C. MS Add. 3965); see (4), (7). It would be further valuable to have full annotated editions of his early notebooks and of some major alchemical notes and writings.

Some recent Newtonian publications include Valentin Boss, *Newton and Russia, the Early Influence 1698–1796* (Cambridge, Mass., 1972); Klaus-Dietwardt Buchholtz, *Isaac Newton als Theologe* (Wittenburg, 1965); Mary S. Churchill, "The Seven Chapters With Explanatory Notes," in *Chymia*, **12** (1967), 27–57, the first publication of one of Newton's complete alchemical MS; J. E. Hofmann, "Neue Newtoniana," in *Studia Leibnitiana*, **2** (1970), 140–145, a review of recent literature; D. Kubrin, "Newton and the Cyclical Cosmos," in *Journal of the History of Ideas*, **28** (1967), 325–346; J. E. McGuire, "The Origin of Newton's Doctrine of Essential Qualities," in *Centaurus*, **12** (1968), 233–260; and L. Trengrove, "Newton's Theological Views," in *Annals of Science*, **22** (1966), 277–294.

SOURCES

1. *Early Life and Education*. The major biographies of Newton are David Brewster, *Memoirs of the Life, Writings, and Discoveries of Isaac Newton*, 2 vols. (Edinburgh, 1855; 2nd ed., 1860; repr. New York, 1965), the best biography of Newton, despite its stuffiness; for a corrective, see Augustus De Morgan, *Essays on the Life and Work of Newton* (Chicago-London, 1914); Louis Trenchard More, *Isaac Newton* (New York-London, 1934; repr. New York, 1962); and Frank E. Manuel, *A Portrait of Isaac Newton* (Cambridge, Mass., 1968). Of the greatest value is the "synoptical view" of Newton's life, pp. xxi–lxxxi, with supplementary documents, in J. Edleston, ed., *Correspondence of Sir Isaac Newton and Professor Cotes…* (London, 1850; repr. London, 1969). Supplementary information concerning Newton's youthful studies is given in D. T. Whiteside, "Isaac Newton: Birth of a Mathematician," in *Notes and Records. Royal Society of London*, **19** (1964), 53–62, and "Newton's Marvellous Year: 1666 and All That," *ibid.*, **21** (1966), 32–41.

John Conduitt assembled recollections of Newton by Humphrey Newton, William Stukeley, William Derham, A. De Moivre, and others, which are now mainly in the Keynes Collection, King's College, Cambridge. Many of these documents have been printed in Edmund Turnor, *Collections for the History of the Town and Soke of Grantham* (London, 1806). William Stukeley's *Memoirs of Sir Isaac Newton's Life* (1752) was edited by A. Hastings White (London, 1936).

On Newton's family and origins, see C. W. Foster, "Sir Isaac Newton's Family," in *Reports and Papers of the Architectural Societies of the Country of Lincoln, Country of York, Archdeaconries of Northampton and Oakham, and Country of Leicester*, **39** (1928–1929), 1–62. Newton's early notebooks are in Cambridge in the University Library, the Fitzwilliam Museum, and Trinity College Library; and in New York City in the Morgan Library. For the latter, see David Eugene Smith, "Two Unpublished Documents of Sir Isaac Newton," in W. J. Greenstreet, ed., *Isaac Newton 1642–1727* (London, 1927), pp. 16 ff. Also, E. N. da C. Andrade, "Newton's Early Notebook," in *Nature*, **135** (1935), 360; George L. Huxley: "Two Newtonian Studies: I. Newton's Boyhood Interests," in *Harvard Library Bulletin*, **13** (1959), 348–354; and A. R. Hall, "Sir Isaac Newton's Notebook, 1661–1665," in *Cambridge Historical Journal*, **9** (1948), 239–250. Elsewhere, Andrade has shown that Newton did not write the poem, attributed to him, concerning Charles II, a conclusion supported by William Stukeley's 1752 *Memoirs of Sir Isaac Newton's Life*, A. Hastings White, ed. (London, 1936).

On Newton's early diagrams and his sundial, see Charles Turnor, "An Account of the Newtonian Dial Presented to the Royal Society," in *Proceedings of the Royal Society*, **5** (1851), 513 (13 June 1844); and H. W. Robinson, "Note on Some Recently Discovered Geometrical Drawings in the Stonework of Woolsthorpe Manor House," in *Notes and Records. Royal Society of London*, **5** (1947), 35–36. For Newton's catalogue of "sins," see R. S. Westfall, "Short-writing and the State of Newton's Conscience, 1662," in *Notes and Records. Royal Society of London*, **18** (1963), 10–16.

On Newton's early reading, see R. S. Westfall, "The Foundations of Newton's Philosophy of Nature," *British Journal for the History of Science*, **1** (1962), 171–182, which is repr. in somewhat amplified form in his *Force in Newton's Physics*. On Newton's reading, see further I. B. Cohen, *Introduction to Newton's Principia* (7) and vol. I of Whiteside's ed. of Newton's *Mathematical Papers* (3). And, of course, a major source of biographical information is the Royal Society's edition of Newton's *Correspondence* (II).

2. *Lucasian Professor*. For the major sources concerning this period of Newton's life, see (1) above, notably Brewster, Cohen (*Introduction*), Edleston, Manuel, More, Whiteside (*Mathematical Papers*) and *Correspondence*.

Edleston (pp. xci-xcviii) gives a "Table of Newton's Lectures as Lucasian Professor," with the dates and corresponding pages of the deposited MSS and the published ed. for the lectures on optics (U.L.C. MS Dd. 9.67, deposited 1674; printed London, 1729); lectures on arithmetic and algebra (U.L.C. MS Dd. 9.68; first published by Whiston, Cambridge, 1707); lectures *De motu corporum* (U.L.C. MS Dd. 9.46), corresponding *grosso modo* to bk. I of the *Principia* through prop. 54; and finally *De motu corporum liber secundus* (U.L.C. MS Dd. 9.67); of which a more complete version was printed as *De mundi systemate liber* (London, 1728)—see below.

Except for the last two, the deposited lectures are final copies, complete with numbered illustrations, as if ready for the press or for any reader who might have access to these MSS. The *Lectiones opticae* exist in two MS versions, an earlier one, which Newton kept (U.L.C. MS Add. 4002, in Newton's hand), having a division by dates quite different from that of the deposited lectures; this has been printed in facs., with an intro. by D. T. Whiteside as *The Unpublished First Version of Isaac Newton's Cambridge Lectures on Optics 1670–1672* (Cambridge, 1973). See I. B. Cohen, *Introduction*, supp. III, "Newton's Professorial Lectures," esp. pp. 303–306.

The deposited MS *De motu corporum* consists of leaves corresponding to different states of composition of bk. I of the *Principia;* the second state (in the hand of Humphrey Newton, with additions and emendations by Isaac Newton) is all but the equivalent of the corresponding part of the MS of the *Principia* sent to the printer, but the earlier state is notably different and more primitive. See I. B. Cohen, *Introduction*, supp. IV, pp. 310–321.

Edleston did not list the deposited copy of the lectures for 1687, a fair copy of only the first portion of *De motu corporum liber secundus* (corresponding to the first 27 sections, roughly half of Newton's own copy of the whole work, U.L.C. MS Add. 3990); he referred to a copy of the deposited lectures made by Cotes (Trinity College Library, MS R. 19.39), in which the remainder of the text was added from a copy of the whole MS belonging to Charles Morgan. See I. B. Cohen, *Introduction*, supp. III, pp. 306–308, and supp. VI, pp. 327–335. This MS, an early version of what was to be rewritten as *Liber tertius: De mundi systemate of the Principia*, was published in English (London, 1728) and in Latin (London, 1728); see I. B. Cohen, "Newton's *System of the World*," in *Physis*, **11** (1969), 152–166; and intro. to repr. of the English *System of the World* (London, 1969).

The statutes of the Lucasian professorship (dated 19 Dec. 1663) are printed in the appendix to William Whiston's *An Account of…* [His] Prosecution at, and Banishment *From, the University of Cambridge* (London, 1718) and are printed again by D. T. Whiteside in Newton's *Mathematical Papers*, **III,** xx-xxvii.

It is often supposed, probably mistakenly, that Newton actually read the lectures that he deposited, or that the deposited lectures are evidence of the state of his knowledge or his formulation of a given subject at the time of giving a particular lecture, because the deposited MSS may be divided into dated lectures; but the statutes required that the lectures be rewritten after they had been read.

The MSS of Humphrey Newton's memoranda are in the Keynes Collection, King's College, Cambridge (K. MS 135) and are printed in David Brewster, *Memoirs*, **II,** 91–98, and again in L. T. More, *Isaac Newton*, pp. 246–251.

The evidence for Newton's plan to publish an ed. of his early optical papers, including the letters in the *Philosophical Transactions*, is in a set of printed pages (possibly printed proofs) forming part of such an annotated printing of these letters, discovered by D. J. de S. Price. See I. B. Cohen, "Versions of Isaac Newton's First Published Paper With Remarks on … an Edition of His Early Papers on Light and Color," in *Archives internationales d'histoire des sciences*, **11** (1958), 357–375; D. J. de S. Price, "Newton in a Church Tower: The Discovery of an Unknown Book by Isaac Newton," in *Yale University Library Gazette*, **34** (1960), 124–126; A. R. Hall, "Newton's First Book," in *Archives internationales d'histoire des sciences*, **13** (1960), 39–61. On 5 Mar. 1677, Collins wrote to Newton that David Loggan "informs me that he hath drawn your effigies in order to [produce] a sculpture thereof to be prefixed to a book of Light [&] Colours [&] Dioptricks which you intend to publish."

The most recent and detailed analysis of the Newton-Fatio relationship is given in Frank E. Manuel, *A Portrait of Isaac Newton*, ch. 9, "The Ape of Newton: Fatio de Duillier," and ch. 10, "The Black Year 1693." For factual details, see Newton, *Correspondence*, **III** . The late Charles A. Domson completed a doctoral dissertation, "Nicolas Fatio de Duillier and the Prophets of London: An Essay in the Historical Interaction of Natural Philosophy and Millennial Belief in the Age of Newton" (Yale, 1972).

Newton's gifts to the Trinity College Library are listed in an old MS catalogue of the library; see I. B. Cohen: "Newton's Attribution of the First Two Laws of Motion to Galileo," in *Atti del Symposium internazionale di storia, metodologia, logica e filosofia delta scienza: "Galileo nella storia e nella filosofia della scienza"* (Florence, 1967), pp. xxii-xlii, esp. pp. xxvii-xxviii and n. 22.

3. *Mathematics*. The primary work for the study of Newton's mathematics is the ed. (to be completed in 8 vols.) by D. T. Whiteside: *Mathematical Papers of Isaac Newton* (Cambridge, 1967-). Whiteside has also provided a valuable pair of introductions to a facs. repr. of early translations of a number of Newton's tracts, *The MathematicalWorks of Isaac Newton*, 2 vols. (New York-London, 1964–1967); these introductions give an admirable and concise summary of the development of Newton's mathematical thought and contain bibliographical notes on the printings and translations of the tracts reprinted, embracing *De analyst; De quadratura; Methodus fluxionum et serierum infinitarum; Arithmetica universalis* (based on his professorial lectures, deposited in the University Library); *Enumeratio lineanun tertii ordinis;* and Methodus differentialis ("Newton's Interpolation Formulas"). Attention may also be directed to several other of Whiteside's publications: "Isaac Newton: Birth of a Mathematician," in *Notes and Records. Royal Society of London*, **19** (1964), 53–62; "Newton's Marvellous Year: 1666 and All That," *ibid*., **21** (1966), 32–41; "Newton's Discovery of the General Binomial Theorem," in *Mathematical Gazette*, **45** (1961), 175–180. (See other articles of his cited in (6), (7), (8) below.)

Further information concerning the eds. and translations of Newton's mathematical writings may be gleaned from the bibliographies (Gray, Zeitlinger, Babson) cited above (III). Various Newtonian tracts appeared in Johann Castillon's *Opuscula* … (II), I, supplemented by a two-volume ed. (Amsterdam, 1761) of *Arithmetica universalis*. The naturalist Buffon translated the *Methodus fluxionum*…(Paris, 1740), and James Wilso n replied to Buffon's preface in an appendix to vol. II (1761) of his own ed. of Benjamin Robins' *Mathematical Tracts;* these two works give a real insight into "what an interested student could then know of Newton's private thoughts," See also Pierre Brunet, "La notion d'infini mathématique chez Buffon," in *Archeion*, **13** (1931), 24–39; and Lesley Hanks, *Buffon avant l'"Histoire naturelle"*; (Paris, 1966), pt. 2, ch. 4 and app. 4. Horsley's ed. of Newton' Opera (II) contains some of Newton's mathematical tracts. A modern version of the *Arithmetica universalis*, with extended notes and commentary, has been published by A. P. Youschkevitch (Moscow, 1948). A. Rupert Hall and Marie Boas Hall have published Newton's October 1666 tract, "to resolve problems by motion" (U.L.C. MS Add. 3458, fols. 49–63) in their *Unpublished Scientific Papers* (II); *see also* H. W. Turnbull, "The Discovery of the Infinitesimal Calculus," in *Nature*, 167 (1951), 1048–1050.

Newton's *Correspondence* (II) contains letters and other documents relating to mathematics, with valuable annotations by H. W. Turnbull and J. F. Scott. See, further, Turnbull's *The Mathematical Discoveries of Newton* (London-Glasgow, 1945), produced before he started to edit the *Correspondence* and thus presenting a view not wholly borne out by later research. Carl B. Boyer has dealt with Newton in *Concepts of the Calculus* (New York, 1939; repr. 1949, 1959), ch. 5; "Newton as an Originator of Polar Coordinates," in *American Mathematical Monthly* 56 (1949), 73–78; *History of Analytic Geometry*(New York, 1956), ch. 7; and *A History of Mathematics* (New York, 1968), ch. 19.

Other secondary works are W. W. Rouse Ball, *A Short Account of the History of Mathematics*, 4th ed. (London, 1908), ch. 16—even more useful is his A *History of the Study of Mathematics at Cambridge* (Cambridge, 1889), chs. 4–6; J. F. Scott, A *History of Mathematics* (London, 1958), chs. 10, 11; and Margaret E. Baron, *The Origins of the Infinitesimal Calculus* (Oxford-London-New York, 1969).

Some specialized studies of value are D. T. Whiteside, "Patterns of Mathematical Thought in the Later Seventeenth Century," in *Archive for History of Exact Sciences*, 1 (1961), 179–388; W. W. Rouse Bali, "On Newton's Classification of Cubic Curves," in *Proceedings of the London Mathematical Society*, 22 (1891), 104–143, summarized in *Bibliotheca mathematica*, n.s. **5** (1891), 35–40; Florian Cajori, "Fourier's Improvement of the Newton-Raphson Method of Approximation Anticipated by Mourraile," in *Bibliotheca mathematica*, 11 (1910–1911), 132–137; "Historical Note on the Newton-Raphson Method of Approximation," in *American Mathematical Monthly*, 18 (1911), 29–32; and A *History of the Conceptions of Limits and Fluxions in Great Britain From Newton to Woodhouse* (Chicago-London, 1919); W. J. Greenstreet, ed., *Isaac Newton 1642–1727* (London, 1927), including D. C. Fraser, "Newton and Interpolation"; A. R. Forsyth, "Newton's Problem of the Solid of Least Resistance"; J. J. Milne, "Newton' Contribution to the Geometry of Conies"; H. Hilton, "Newton on Plane Cubic Curves"; and J. M. Child, "Newton and the Art of Discovery" Duncan C. Fraser, *Newton' Interpolation Formulas* (London, 1927), repr. from *Journal of the Institute of Actuaries*, **51** (1918–1919), 77–106, 211–232, and **58** (1927), 53–95; C. R. M. Talbot, *Sir Isaac Newton's Enumeration of Lines of the Third Order, Generation of Curves by Shadows, Organic Description of Curves, and Construction of Equations by Curves*, trans, from the Latin, with notes and examples (London, 1860); Florence N. David, "Mr. Newton, Mr. Pepys and Dyse," in *Annals of Science*, **13** (1957), 137–147, on dice-throwing and probability; Jean Pelseneer, "Une lettre inédite de Newton à Pepys (23 décembre 1693)," in *Osiris*, 1 (1936), 497–499, on probabilities; J. M. Keynes, "A Mathematical Analysis by Newton of a Problem in College Administration," in *Isis***49** (1958), 174–176; Maximilian Miller, "Newton, Aufzahlung der Linien dritter Ordnung," in *Wissenschaftliche Zeitschrift der Hochschule für Verkehrswesen* Dresden, 1, no. I (1953), 5–32; "Newtons Differenzmethode," *ibid*., 2, no. I (1954), 1–13; and Über die Analysis mit Hilfe unendlicher Reihen," *ibid*., no. 2 (1954), 1–16; Oskar Bolza, "Bemerkungen zu Newtons Beweis seines Satzes über den Rotationskörper kleinsten Widerstandes," in *Bibliotheca mathematical* 3rd ser., **13** (1912–1913), 146–149.

Other works relating to Newton's mathematics are cited in (6) and (for the quarrel with Leibniz over priority in the calculus) (10).

4. *Optics*. The eds. of the *Opticks and Lectiones opticae* are mentioned above (I); the two MS versions of the latter are U.L.C. MS Add. 4002, MS Dd.9.67. An annotated copy of the 1st ed. of the *Opticks*, used by the printer for the composition of the 2nd ed. still exists (U.L.C. MS Adv.b.39.3—formerly MS Add. 4001). For information Cohen, *Introduction to Newton's Principia* (7), p. 34; and R. S. Westfall, "Newton's Reply," pp. 83–84—extracts are printed with commentary in D. T. Whiteside's ed. of Newton's *Mathematical Papers* (3). At one time Newton began to write a *Fundamentum opticae*, the text of

which is readily reconstructive from the MSS and which is a necessary tool for a complete analysis of bk. I of the *Opticks* into which its contents were later incorporated; for pagination, see *Mathematical Papers* (3), III, 552. This work is barely known to Newton scholars. Most of Newton's optical MSS are assembled in the University Library, Cambridge, as MS Add. 3970, but other MS writings appear in the Waste Book, correspondence, and various notebooks.

Among the older literature, F. Rosenberger's book (VI) may still be studied with profit, and there is much to be learned from Joseph Priestley's 18th-century presentation of the development and current state of concepts and theories of light and vision. See also Ernst Mach, *The Principles of Physical Optics: An Historical and Philosophical Treatment*, trans, by John S. Anderson and A. F. A. Young (London, 1926; repr. New York, 1953); and Vasco Ronchi, *The Nature of Light: An Historical Survey*, trans, by V. Barocas (Cambridge, Mass., 1970)— also 2 eds. in Italian and a French translation by Juliette Taton.

Newton's MSS have been used in A. R. Hall, "Newton's Notebook" (1), pp. 239–250; and in J. A. Lohne, "Newton's 'Proof' of* the Sine Law," in *Archive for History of Exact Sciences*, **1** (1961), 389–405; "Isaac Newton: The Rise of a Scientist 1661–1671," in *Notes and Records. Royal Society of London*, **20** (1965), 125–139; and "Experimentum crucis" *ibid*., **23** (1968), 169–199. See also J. A. Lohne and Bernhard Sticker, *Newtons Theorie der Prismenfarben, mit Überscizung and Erläutcrung der Abhandlung von 1672* (Munich, 1969); and R. S. Westfall, "The Development of Newton's Theory of Color," in *Isis*, **53** (1962), 339–358; "Newton and his Critics on the Nature of Colors," in *Archives internationales d'histoire des sciences*, **15** (1962), 47–58; "Newton's Reply to Hooke and the Theory of Colors," in *Isis*, **54** (1963), 82–96; "Isaac Newton's Coloured Circles Twixt Two Contiguous Glasses," in *Archive for History of Exact Sciences*, **2** (1965), 181–196; and "Uneasily Fitful Reflections on Fits of Easy Transmission [and of easy reflection]," in Robert Palter, ed., *The Annus Mirabilis* (VI), pp. 88–104.

Newton's optical papers (from the *Philosophical Transactions* and T, Birch's *History of the Royal Society*) are repr. in facs. in *Newton's Papers and Letters* (1), with an intro. by T. S, Kuhn. See also I. B. Cohen, "I prismi del Newton e i prismi deli'Algarotti," in *Atti delta Fondazione "Giorgio Ronchi* (Florence), **12** (1957), 1–11; Vasco Ronehi, "I prismi del Newton' del Museo Civico di Treviso," *ibid*, 12–28; and N. R. Hanson, "Waves, Particles, and Newton's 'Fits,'" in *Journal of the History of Ideas*, **21** (1960), 370–391. On Newton's work on color, see George Biernson, "Why did Newton see Indigo in the Spectrum?," in *American Journal of Physics***40** (1972), 526–533; and Torger Holtzmark, "Newton's Experimentum Crucis Reconsidered," *ibid*., **38** (1970), 1229–1235.

An able account of Newton's work in optics, set against the background of his century, is A. I. Sabra, *Theories of Light From Descartes to Newton* (London, 1967), ch. 9–13. An important series of studies, based on extensive examination of the MSS, are Zev Bechler, "Newton's 1672 Optical Controversies: A Study in the Grammar of Scientific Dissent," in Y. Elkana, ed., *Some Aspects of the Interaction Between Science and Philosophy* (New York, in press); "Newton's Search for a Mechanistic Model of Color Dispersion: A Suggested Interpretation," in *Archive for History of Exact Sciences*, **11** (1973), 1–37; and an analysis of Newton's work on chromatic aberration in lenses (in press). On the last topic, see also D. T. Whiteside, *Mathematical Papers*, III, pt. 3, esp. pp. 442–443, 512 513 (n. 61), 533 (n. 13), and 555–556 (nn. 5–6).

5. *Dynamics, Astronomy, and the Birth of the "Principia."* The primary documents for the study of Newton's dynamics have been assembled by A. R. and M. B. Hall (II) and by J. Herivel, *The Background to Newton's Principia* (Oxford, 1965); other major documents are printed (with historical and critical essays) in the Royal Society's ed. of Newton's *Correspondence* (II); S. P. Rigaud, *Historical Essay on the First Publication of Sir Isaac Newton's Principia* (Oxford, 1838; repr., with intro. by I. B. Cohen, New York, 1972); W. W. Rouse Ball, *An Essay on Newton's Principia* (London, 1893; repr. with intro. by I. B. Cohen, New York, 1972); and I. B. Cohen, *Introduction* (7).

The development of Newton's concepts of dynamics is discussed by Herivel (in *Background*, and in a series of articles summarized in that work), in Rouse Ball's *Essay*, I. B. Cohen's *Introduction*, and in R. S. Westfall's *Force in Newton's Physics* (London-New York, 1971). On the concept of inertia and the laws of motion, see I. B, Cohen, *Transformations of Scientific Ideas: Variations on Newtonian Themes in the History of Science*, the Wiles Lectures (Cambridge, in press), ch. 2; and "Newton's Second Law and the Concept of Force in the *Principia,"* in R. Palter cd.. *Annus mirabilis* (VI), pp. 143–185; Alan Gabbey, "Force and Inertia in Seventeenth-Century Dynamics," in *Studies in History and Philosophy of Science*, **2** (1971), l–68; E. J. Aiton, The *Vortex Theory of Planetary Motions* (London-New York, 1972); and A. R. Hall, "Newton on the Calculation of Central Forces," in *Annals of Science*, **13** (1957), 62–71. Newton's encounter with Hooke in 1679 and his progress from the Ward-Bullialdus approximation to the area law are studied in J. A. Lohne, "Hooke Versus Newton, an Analysis of the Documents in the Case of Free Fall and Planetary Motion," in *Centaurus*, **7** (1960), 6–52; D. T. Whiteside, "Newton's Early Thoughts on Planetary Motion: A Fresh Look," in *British Journal for the History of Science*, **2** (1964), 117–137, "Newtonian Dynamics," in *History of Science*, **5** (1966), 104–117, and "Before the Principia: The Maturing of Newton's Thoughts on Dynamical Astronomy, 1664–84," in *Journal for the History of Astronomy*, **1** (1970), 5–19; A. Koyre, "An Unpublished Letter of Robert Hooke to Isaac *Newton*," in *Isis*, **43** (1952), 312–337, repr, in Koyre's Newtonian Studies (VI); and R. S. Westfall, "Hooke and the Law of Universal Gravitation," in *British Journal for the Historyof Science*, **3** (1967), 245–261. "The Background and Early Development of Newton's Theory of Comets" is the title of a Ph.D. thesis by James Alan Ruffner (Indiana Univ., May 1966).

6. *Mathematics in the Principia*. The references for this section will be few, since works dealing with Newton's preparation for the *Principia* are listed under (5), and additional sources for the *Principia* itself are given under (7). See, further, Yasukatsu Maeyama, *Hypothesen zur Planetentheorie des 17. Jahrhunderts* (Frankfurt, 1971), and Curtis A. Wilson, "From Kepler's Laws, So-called, to Universal Gravitation: Empirical Factors," in *Archive for History of Exact Sciences*, **6** (1970), 89–170.

Two scholarly studies may especially commend our attention: H. W. Turnbull, *Mathematical Discoveries* (3), of which chs. 7 and 12 deal specifically with the *Principia;* D. T. Whiteside, "The Mathematical Principles Underlying Newton's *Principia Mathematica"* in *Journal for the History of Astronomy*, **1** (1970), 116–138, of which a version with less annotation was published in pamphlet form by the University of Glasgow (1970). See also C. B. Boyer, *Concepts of Calculus* and *History* (3), and J. F. Scott, *History* (3), ch. 11. Valuable documents and commentaries also appear in the Royal Society's ed. of Newton's *Correspondence*, J. Herivel's *Background* (5) and various articles, and D. T. Whiteside, *Mathematical Papers* (3). Especially valuable are three commentaries: J. M. F. Wright, *A Commentary on Newton's Principia*, 2 vols. (London, 1833; repr., with intro. by I. B. Cohen, New York, 1972); Henry Lord Brougham and E. J. Routh, *Analytical View of Sir Isaac Newton's Principia* (London, 1855; repr., with intro. by I. B. Cohen, New York, 1972); and Percival Frost, *Newton's Principia, First Book, Sections I., II., III., With Notes and Illustrations* (Cambridge, 1854; 5th ed., London-New York, 1900). On a post-*Principia* MS on dynamics, using fluxions, see W. W. Rouse Ball, "A Newtonian Fragment Relating to Centripetal Forces" in *Proceedings of the London Mathematical Society*, **23** (1892), 226–231; A. R. and M. B. Hall, *Unpublished Papers* (II), pp. 65–68; and commentary by D. T. Whiteside, in *History of Science*, **2** (1963), 129, n. 4.

7. *The Principia*. Many of the major sources for studying the *Principia* have already been given, in (5), (6), including works by A. R. Hall and M. B. Hall, J. Herivel, R. S. Westfall, and D. T. Whiteside. Information on the writing of the *Principia* and the evolution of the text is given in I. B. Cohen, *Introduction to Newton's Principia* (Cambridge, 1971) and the 2-vol. ed. of the *Principia* with variant readings, ed. by A. Koyré, I. B. Cohen, and Anne Whitman (I). Some additional works are R. S. Westfall, "Newton and Absolute Space," in *Archives internationales d'histoire des sciences*, **17** (1964), 121–132; Clifford Truesdell, "A Program Toward Rediscovering the Rational Mechanics of the Age of Reason," in *Archive for History of Exact Sciences*, **1** (1960), 3–36, and "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*," in Robert Palter, ed., *The Annus Mirabilis* (VI), pp. 192–232—both articles by Truesdeli are repr. in his *Essays in the History of Mechanics* (New York-Berlin, 1968); E. J. Aiton, "The Inverse Problem of Central Forces," in *Annals of Science*, **20** (1964), 81–99; J. A. Lohne, "The Increasing Corruption" (VI), esp. "5. The Planetary Ellipse of the *Principia*"; and Thomas L. Hankins, "The Reception of Newton's Second Law of Motion in the Eighteenth Century," in *Archives internationales d'histoire des sciences*, **20** (1967), 43–65. Highly recommended is L. Rosenfeld, "Newton and the Law of Gravitation," in *Archive for History of Exact Sciences*, **2** (1965), 365–386: see also E. J. Aiton, "Newton's Aether-Stream Hypothesis and the Inverse-Square Law of Gravitation," in *Annals of Science*, **25** (1969), 255–260; and L. Rosenfeld, "Newton's Views on Aether and Gravitation," in *Archive for History of Exact Sciences*, **6** (1969), 29–37.

I. B. Cohen has discussed some further aspects of *Principia* questions in the Wiles Lectures (5) and a study of "Newton's Second Law" (5); and in "Isaac Newton's *Principia*, the Scriptures and the Divine Providence", in S. Morgenbesser, P. Suppes, and M. White, eds., *Essays in Honor of Ernest Nagel* (New York, 1969), pp. 523–548, esp. pp. 537 ff.; and "New Light on the Form of Definitions I-II-VI-VII," where Newton's concept of "measure" is explored. On the incompatibility of Newton's dynamics and Galileo's and Kepler's laws, see Karl R. Popper, "The Aim of Science," in *Ratio*, **1** (1957), 24–35; and 1. B. Cohen, "Newton's Theories vs. Kepler's Theory," in Y. Elkana, ed., *Some Aspects of the Interaction Between Science and Philosophy* (New York, in press).

8. *Revision of the Opticks (The Later Queries); Chemistry, and Theory of Matter*.The doctrine of the later queries has been studied by F. Rosenberger, *Newton und seine physikalischen Principien* (VI), and by Philip E. B. Jourdain, in a series of articles entitled "Newton's Hypothesis of Ether and of Gravitation…," in *The Monist*, **25** (1915), 79–106, 233–254, 418–440; and by I. B. Cohen in *Franklin and Newton* (VI).

In addition to his studies of the queries, Henry Guerlac has analyzed Newton's philosophy of matter, suggesting an influence of Hauksbee's electrical experiments on the formation of Newton's later concept of ether. See his *Newton et Epicure* (Paris, 1963); "Francis Hauksbee: Experimentateur au profit de Newton," in *Archives intemationales d'histoire des sciences*, **17** (1963), 113–128; "Sir Isaac and the Ingenious Mr. Hauksbee," in *Mélanges Alexandre Koyré: L'aventure de la science* (Paris, 1964), pp. 228–253; and "Newton's Optical Aether," in *Notes and Records. Royal Society of London*, **22** (1967), 45–57. See also Joan L. Hawes, "Newton and the 'Electrical Attraction Unexcited'" in *Annals of Science*, **24** (1968), 121–130; "Newton's Revival of the Aether Hypothesis and the Explanation of Gravitational Attraction," in *Notes* and *Records. Royal Society of London*, **23** (1968), 200–212; and the studies by Bechler listed above (4).

The electrical character of Newton's concept of "spiritus" in the final paragraph of the General Scholium has been disclosed by A. R. and M. B. Hall, in *Unpublished Papers* (II). On Newton's theory of matter, see Marie Boas [Hall], "Newton's Chemical Papers," in *Newton's Papers and Letters* (I), pp. 241–248; and A. R. Hall and M. B. Hall, "Newton's Chemical Experiments," *in Archives Internationales d'histoire des science*, **11** (1958), 113–152; "Newton's Mechanical Principles," in *Journal of the History of Ideas*, **20** (1959), 167–178; "Newton's Theory of Matter," in Isis, 51 (1960), 131–144; and "Newton and the Theory of Matter," in Robert Palter, ed., *The Annus Mirabilis* (VI), pp. 54–68.

On Newton's chemistry and theory of matter, see additionally R. Kargon, *Atomism in England From Hariot to Newton* (Oxford, 1966); A. Koyré, "Les Queries dc I'Optique," in *Archives internationales d'histoire des sciences*, **13** (1960), 15–29; T. S. Kuhn, "Newton's 31st Query and the Degradation of Gold," in *Isis*, **42** (1951), 296–298, with discussion *ibid*., **43** (1952), 123–124; J. E. McGuire, "Body and Void…" in *Archive for History of Exact Sciences*, **3** (1966), 206–248; "Transmutation and Immutability," in *Ambix*, **14** (1967), 69–95; and other papers; D. McKie, "Some Notes on Newton's Chemical Philosophy," in *Philosophical Magazine*, **33** (1942), 847–870; and J. R. Partington, *A History of Chemistry*, II (London, 1961), 468–477, 482–485.

For Newton's theories of chemistry and matter, and their influence, see the books by Hélène Metzger (VI), R. E. Schofield (VI), and A. Thackray (VI).

Geoffroy's summary ("extrait") of the *Opticks*, presented at meetings of the Paris Academy of Sciences, is discussed in I. B. Cohen, "Isaac Newton, Hans Sloane, and the Académic Royale des Sciences," in *Mélanges Alexandre Koyré, I, L'aventure de la science* (Paris, 1964), 61–116; on the general agreement by Newtonians that the queries were not so much asking questions as stating answers to such questions (and on the rhetorical form of the queries), see I. B. Cohen, *Franklin and Newton*(VI), ch. 6.

9. *Alchemy, Theology, and Prophecy. Chronology and History*. Newton published no essays or books on alchemy. His *Chronology of Ancient Kingdoms Amended* (London, 1728) also appeared in an abridged version (London, 1728). His major study of prophecy is *Observations Upon the Prophecies of Daniel, and the Apocalypse of St. John* (London, 1733). A selection of *Theological Manuscripts* was edited by H. McLachlan (Liverpool, 1950).

For details concerning Newton's theological MSS, and MSS relating to chronology, see sees. **VII-VIII** of the catalogue of the Sotheby sale of the Newton papers (IV); for other eds. of the *Chronology* and the *Observations* see the Gray bibliography and the catalogue of the Babson Collection **(III)** . There is no analysis of Newton's theological writings based on a thorough analysis of the MSS; see R. S. Westfall, *Science and Religion in Seventeenth-Century England* (New Haven, 1958), ch. 8; F. E. Manuel, *The Eighteenth Century Confronts the Gods* (Cambridge, 1959), ch. 3; and George S. Brett, "Newton's Place in the History of Religious Thought," in F. E. Brasch, ed., *Sir Isaac Newton* (VI), pp. 259–273. For Newton's chronological and allied studies, see F. E. Manuel, Isaac Newton, Historian (Cambridge, 1963).

On alchemy, the catalogue of the Sotheby sale is most illuminating. Important MSS and annotated alchemical books are to be found in the Keynes Collection (King's College, Cambridge) and in the Burndy Library and the University of Wisconsin, M.I.T., and the Babson Institute. A major scholarly study of Newton's alchemy and hermeticism, based on an extensive study of Newton's **MSS,** is P. M. Rattansi, "Newton's Alchemical Studies," in Allen G. Debus, ed., *Science, Medicine and Society in the Renaissance: Essays to Honor Walter Pagel*, II (New York, 1972), 167–182; see also R. S. Westfall, "Newton and the Hermetic Tradition," *ibid*., pp. 183–198.

On Newton and the tradition of the ancients, and the intended inclusion in the *Principia* of references to an ancient tradition of wisdom, see I. B. Cohen, "'Quantum in se est': Newton's Concept of Inertia in Relation to Descartes and Lucretius," in *Notes and Records. Royal Society of London***19** (1964), 131–155; and esp. J. E. McGuire and P. M. Rattansi, "Newton and the 'Pipes of Pan'," *ibid*., 21 (1966), 108–143; also J. E. McGuire, "Transmutation and Immutability," in *Ambix*, **14** (1967), 69–95. On alchemy, see R. J. Forbes, "Was Newton an Alchemist?," in *Chymia*, **2** (1949), 27–36; F. Sherwood Taylor, "An Alchemical Work of Sir Isaac Newton," in *Ambix*, **5** (1956), 59–84; E. D. Geoghegan, "Some Indications of Newton's Attitude Towards Alchemy," *ibid*., **6** (1957), 102–106; and A. R. and M. B. Hall, "Newton's Chemical Experiments," in *Archives Internationales d'histoire des sciences*, **11** (1958), 113–152.

A salutary point of view is expressed by Mary Hesse, "Hermeticism and Historiography: An Apology for the Internal History of Science," in Roger H. Stuewer, ed., *Historical and Philosophical Perspectives of Science*, vol. **V** of Minnesota Studies in the Philosophy of Science (Minneapolis, 1970), 134–162. But see also P. M. Rattansi, "Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy," in Mikuláš Teich and Robert Young, eds., *Changing Perspectives in the History of Science, Essays in Honour of Joseph Needham* (London, 1973), pp. 148–166.

10. *The London Years: the Mint, the Royal Society, Quarrels With Flamsteed and With Leibniz*. On Newton's life in London and the affairs of the mint, see the biographies by More and Brewster (1), supplemented by Manuel's *Portrait* (1). Of special interest are Augustus De Morgan, *Newton: His Friend: and His Niece* (London, 1885); and Sir John Craig, *Newton at the Mint* (Cambridge, 1946). On the quarrel with Flamsteed, see Francis Baily, *An Account of the Rev*^{d}. *John Flamsteed* (London, 1835; supp., 1837; repr. London, 1966); the above-mentioned biographies of Newton; and Newton's *Correspondence* (II). On the controversy with Leibniz, see the *Commercium epistolicum*(I). Newton's MSS on this controversy (U.L.C. MS Add. 3968) have never been fully analyzed; but see Augustus De Morgan, "On the Additions Made to the Second Edition of the *Commercium epistolicum"* in *Philosophical Magazine*, 3rd ser., **32** (1848), 446–456; and "On the Authorship of the Account of the *Commercium epistolicum*, Published in the *Philosophical Transactions*" *ibid*., 4th ser., **3** (1852), 440–444. The most recent ed. of *The Leibniz-Clarke Correspondence* was edited by H. G. Alexander (Manchester, 1956).

11.*Newton's Philosophy: The Rules of Philosophizing, the General Scholium, the Queries of the Opticks*. Among the many books and articles on Newton's philosophy, those of Rosenberger, Bloch, and Koyré (VI) are highly recommended. On the evolution of the General Scholium, see A. R. and M. B. Hall, *Unpublished Papers* (II), pt. IV, intro, and sec. 8; and 1. B. Cohen, *Transformations of Scientific Ideas* (the Wiles Lectures, in press) (5) and "Hypotheses in Newton's Philosophy," in *Physis,***8** (1966), 163–184.

The other studies of Newton's philosophy are far too numerous to list here; authors include Gerd Buchdahl, Ernst Cassirer, A. C. Crombie, N. R. Hanson, Ernst Mach, Jürgen Mittelstrass, John Herman Randall, Jr., Dudley Shapere, Howard Stein, and E. W. Strong.