BOOKS BY FLORIAN CAJORI

HISTORY OF MATHEMATICS
Revised and Enlarged Edition

HISTORY OF ELEMENTARY MATHEMATICS
Revised and Enlarged Edition

HISTORY OF PHYSICS

INTRODUCTION TO THE MODERN THEORY OF EQUATIONS

Newton

A HISTORY OF MATHEMATICS

BY

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"I am sure that no subject loses more than mathematics by any attempt to dissociate it from its history."—J. W. L. GLAISHER

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Wallis in 1685, and Edmund Halley in 1698.[1] James Gregory and Barrow gave also the integral $\int_0^\theta \tan\theta\, d\theta = \log \sec\theta$; B. Cavalieri in 1647 established the integral of $\int_0^a x^n\, dx$. Similar results were obtained by E. Torricelli, Gregory St. Vincent, P. Fermat, G. P. Roberval and B. Pascal.[2]

Newton to Euler

It has been seen that in France prodigious scientific progress was made during the beginning and middle of the seventeenth century. The toleration which marked the reign of Henry IV and Louis XIII was accompanied by intense intellectual activity. Extraordinary confidence came to be placed in the power of the human mind. The bold intellectual conquests of R. Descartes, P. Fermat, and B. Pascal enriched mathematics with imperishable treasures. During the early part of the reign of Louis XIV we behold the sunset splendor of this glorious period. Then followed a night of mental effeminacy. This lack of great scientific thinkers during the reign of Louis XIV may be due to the simple fact that no great minds were born; but, according to Buckle, it was due to the paternalism, to the spirit of dependence and subordination, and to the lack of toleration, which marked the policy of Louis XIV.

In the absence of great French thinkers, Louis XIV surrounded himself by eminent foreigners. O. Römer from Denmark, C. Huygens from Holland, Dominic Cassini from Italy, were the mathematicians and astronomers adorning his court. They were in possession of a brilliant reputation before going to Paris. Simply because they performed scientific work in Paris, that work belongs no more to France than the discoveries of R. Descartes belong to Holland, or those of J. Lagrange to Germany, or those of L. Euler and J. V. Poncelet to Russia. We must look to other countries than France for the great scientific men of the latter part of the seventeenth century.

About the time when Louis XIV assumed the direction of the French government Charles II became king of England. At this time England was extending her commerce and navigation, and advancing considerably in material prosperity. A strong intellectual movement took place, which was unwittingly supported by the king. The age of poetry was soon followed by an age of science and philosophy. In two successive centuries England produced Shakespeare and I. Newton!

[1] See F. Cajori in *Bibliotheca mathematica*, 3. S., Vol. 14, 1915, pp. 312–319.
[2] H. G. Zeuthen, *Geschichte der Math.* (deutsch v. R. Meyer), Leipzig, 1903, pp. 256 ff.

Germany still continued in a state of national degradation. The Thirty Years' War had dismembered the empire and brutalized the people. Yet this darkest period of Germany's history produced G. W. Leibniz, one of the greatest geniuses of modern times.

There are certain focal points in history toward which the lines of past progress converge, and from which radiate the advances of the future. Such was the age of Newton and Leibniz in the history of mathematics. During fifty years preceding this era several of the brightest and acutest mathematicians bent the force of their genius in a direction which finally led to the discovery of the infinitesimal calculus by Newton and Leibniz. B. Cavalieri, G. P. Roberval, P. Fermat, R. Descartes, J. Wallis, and others had each contributed to the new geometry. So great was the advance made, and so near was their approach toward the invention of the infinitesimal analysis, that both J. Lagrange and P. S. Laplace pronounced their countryman, P. Fermat, to be the first inventor of it. The differential calculus, therefore, was not so much an individual discovery as the grand result of a succession of discoveries by different minds. Indeed, no great discovery ever flashed upon the mind at once, and though those of Newton will influence mankind to the end of the world, yet it must be admitted that Pope's lines are only a "poetic fancy":—

> "Nature and Nature's laws lay hid in night;
> God said, 'Let Newton be,' and all was light."

Isaac Newton (1642–1727) was born at Woolsthorpe, in Lincolnshire, the same year in which Galileo died. At his birth he was so small and weak that his life was despaired of. His mother sent him at an early age to a village school, and in his twelfth year to the public school at Grantham. At first he seems to have been very inattentive to his studies and very low in the school; but when, one day, the little Isaac received a severe kick upon his stomach from a boy who was above him, he labored hard till he ranked higher in school than his antagonist. From that time he continued to rise until he was the head boy.[1] At Grantham, Isaac showed a decided taste for mechanical inventions. He constructed a water-clock, a wind-mill, a carriage moved by the person who sat in it, and other toys. When he had attained his fifteenth year his mother took him home to assist her in the management of the farm, but his great dislike for farmwork and his irresistible passion for study, induced her to send him back to Grantham, where he remained till his eighteenth year, when he entered Trinity College, Cambridge (1660). Cambridge was the real birthplace of Newton's genius. Some idea of his strong intuitive powers may be drawn from the fact that he regarded the theorems of ancient geometry as self-evident truths, and that, without any preliminary study, he made himself master of Descartes' *Geometry*. He

[1] D. Brewster, *The Memoirs of Newton*, Edinburgh, Vol. I, 1855, p. 8.

afterwards regarded this neglect of elementary geometry a mistake in his mathematical studies, and he expressed to Dr. H. Pemberton his regret that "he had applied himself to the works of Descartes and other algebraic writers before he had considered the *Elements* of Euclid with that attention which so excellent a writer deserves." Besides R. Descartes' *Geometry*, he studied W. Oughtred's *Clavis*, J. Kepler's *Optics*, the works of F. Vieta, van Schooten's *Miscellanies*, I. Barrow's *Lectures*, and the works of J. Wallis. He was particularly delighted with Wallis' *Arithmetic of Infinites*, a treatise fraught with rich and varied suggestions. Newton had the good fortune of having for a teacher and fast friend the celebrated Dr. Barrow, who had been elected professor of Greek in 1660, and was made Lucasian professor of mathematics in 1663. The mathematics of Barrow and of Wallis were the starting-points from which Newton, with a higher power than his masters', moved onward into wider fields. Wallis had effected the quadrature of curves whose ordinates are expressed by any integral and positive power of $(1-x^2)$. We have seen how Wallis attempted but failed to interpolate between the areas thus calculated, the areas of other curves, such as that of the circle; how Newton attacked the problem, effected the interpolation, and discovered the Binomial Theorem, which afforded a much easier and direct access to the quadrature of curves than did the method of interpolation; for even though the binomial expression for the ordinate be raised to a fractional or negative power, the binomial could at once be expanded into a series, and the quadrature of each separate term of that series could be effected by the method of Wallis. Newton introduced the system of literal indices.

Newton's study of quadratures soon led him to another and most profound invention. He himself says that in 1665 and 1666 he conceived the method of fluxions and applied them to the quadrature of curves. Newton did not communicate the invention to any of his friends till 1669, when he placed in the hands of Barrow a tract, entitled *De Analysi per Æquationes Numero Terminorum Infinitas*, which was sent by Barrow to John Collins, who greatly admired it. In this treatise the principle of fluxions, though distinctly pointed out, is only partially developed and explained. Supposing the abscissa to increase uniformly in proportion to the time, he looked upon the area of a curve as a nascent quantity increasing by continued fluxion in the proportion of the length of the ordinate. The expression which was obtained for the fluxion he expanded into a finite or infinite series of monomial terms, to which Wallis' rule was applicable. Barrow urged Newton to publish this treatise; "but the modesty of the author, of which the excess, if not culpable, was certainly in the present instance very unfortunate, prevented his compliance." [1] Had this tract

[1] John Playfair, "Progress of the Mathematical and Physical Sciences" in *Encyclopædia Britannica*, 7th Edition.

been published then, instead of forty-two years later, there probably would have been no occasion for that long and deplorable controversy between Newton and Leibniz.

For a long time Newton's method remained unknown, except to his friends and their correspondents. In a letter to Collins, dated December 10th, 1672, Newton states the fact of his invention with one example, and then says: "This is one particular, or rather corollary, of a general method, which extends itself, without any troublesome calculation, not only to the drawing of tangents to any curve lines, whether geometrical or mechanical, or anyhow respecting right lines or other curves, but also to the resolving other abstruser kinds of problems about the crookedness, areas, lengths, centres of gravity of curves, etc.; nor is it (as Hudde's method of Maximis and Minimis) 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."

These last words relate to a treatise he composed in the year 1671, entitled *Method of Fluxions*, in which he aimed to represent his method as an independent calculus and as a complete system. This tract was intended as an introduction to an edition of Kinckhuysen's *Algebra*, which he had undertaken to publish. "But the fear of being involved in disputes about this new discovery, or perhaps the wish to render it more complete, or to have the sole advantage of employing it in his physical researches, induced him to abandon this design." [1]

Excepting two papers on optics, all of his works appear to have been published only after the most pressing solicitations of his friends and against his own wishes. His researches on light were severely criticised, and he wrote in 1675: "I was so persecuted with discussions arising out of my theory of light that I blamed my own imprudence for parting with so substantial a blessing as my quiet to run after a shadow."

The *Method of Fluxions*, translated by J. Colson from Newton's Latin, was first published in 1736, or sixty-five years after it was written. In it he explains first the expansion into series of fractional and irrational quantities,—a subject which, in his first years of study, received the most careful attention. He then proceeds to the solution of the two following mechanical problems, which constitute the pillars, so to speak, of the abstract calculus:—

"I. The length of the space described being continually (*i. e.* at all times) given; to find the velocity of the motion at any time proposed.

"II. The velocity of the motion being continually given; to find the length of the space described at any time proposed."

Preparatory to the solution, Newton says: "Thus, in the equation $y=x^2$, if y represents the length of the space at any time described,

[1] D. Brewster, *op. cit.*, Vol. I, 1855, p. 15.

which (time) another space x, by increasing with an uniform celerity $\dot{x}$, measures and exhibits as described: then $2x\dot{x}$ will represent the celerity by which the space y, at the same moment of time, proceeds to be described; and contrarywise."

"But whereas we need not consider the time here, any farther than it is expounded and measured by an equable local motion; and besides, whereas only quantities of the same kind can be compared together, and also their velocities of increase and decrease; therefore, in what follows I shall have no regard to time formally considered, but I shall suppose some one of the quantities proposed, being of the same kind, to be increased by an equable fluxion, to which the rest may be referred, as it were to time; and, therefore, by way of analogy, it may not improperly receive the name of time." In this statement of Newton there is contained his answer to the objection which has been raised against his method, that it introduces into analysis the foreign idea of motion. A quantity thus increasing by uniform fluxion, is what we now call an independent variable.

Newton continues: "Now those quantities which I consider as gradually and indefinitely increasing, I shall hereafter call *fluents*, or *flowing quantities*, and shall represent them by the final letters of the alphabet, v, x, y, and z; . . . and the velocities by which every fluent is increased by its generating motion (which I may call *fluxions*, or simply velocities, or celerities), I shall represent by the same letters pointed, thus, $\dot{v}$, $\dot{x}$, $\dot{y}$, $\dot{z}$. That is, for the celerity of the quantity v I shall put $\dot{v}$, and so for the celerities of the other quantities x, y, and z, I shall put $\dot{x}$, $\dot{y}$, and $\dot{z}$, respectively." It must here be observed that Newton does not take the fluxions themselves infinitely small. The "moments of fluxions," a term introduced further on, are infinitely small quantities. These "moments," as defined and used in the *Method of Fluxions*, are substantially the differentials of Leibniz. De Morgan points out that no small amount of confusion has arisen from the use of the word *fluxion* and the notation $\dot{x}$ by all the English writers previous to 1704, excepting Newton and George Cheyne, in the sense of an infinitely small increment.[1] Strange to say, even in the *Commercium epistolicum* the words *moment* and *fluxion* appear to be used as synonymous.

After showing by examples how to solve the first problem, Newton proceeds to the demonstration of his solution:—

"The moments of flowing quantities (that is, their indefinitely small parts, by the accession of which, in infinitely small portions of time, they are continually increased) are as the velocities of their flowing or increasing.

"Wherefore, if the moment of any one (as x) be represented by the product of its celerity $\dot{x}$ into an infinitely small quantity o (*i. e.* by

[1] A. De Morgan, "On the Early History of Infinitesimals," in *Philosophical Magazine*, November, 1852.

$\dot{x}o$), the moments of the others, v, y, z, will be represented by $\dot{v}o$, $\dot{y}o$, $\dot{z}o$; because $\dot{v}o$, $\dot{x}o$, $\dot{y}o$, and $\dot{z}o$ are to each other as $\dot{v}$, $\dot{x}$, $\dot{y}$, and $\dot{z}$.

"Now since the moments, as $\dot{x}o$ and $\dot{y}o$, are the indefinitely little accessions of the flowing quantities x and y, by which those quantities are increased through the several indefinitely little intervals of time, it follows that those quantities, x and y, after any indefinitely small interval of time, become $x+\dot{x}o$ and $y+\dot{y}o$, and therefore the equation, which at all times indifferently expresses the relation of the flowing quantities, will as well express the relation between $x+\dot{x}o$ and $y+\dot{y}o$, as between x and y; so that $x+\dot{x}o$ and $y+\dot{y}o$ may be substituted in the same equation for those quantities, instead of x and y. Thus let any equation $x^3-ax^2+axy-y^3=o$ be given, and substitute $x+\dot{x}o$ for x, and $y+\dot{y}o$ for y, and there will arise

$$\left.\begin{array}{l} x^3+3x^2\dot{x}o+3x\dot{x}o\dot{x}o+\dot{x}^3o^3 \\ -ax^2-2ax\dot{x}o-a\dot{x}o\dot{x}o \\ +axy+ay\dot{x}o+a\dot{x}o\dot{y}o \\ \quad +ax\dot{y}o \\ -y^3-3y^2\dot{y}o-3y\dot{y}o\dot{y}o-\dot{y}^3o^3 \end{array}\right\} =o.$$

"Now, by supposition, $x^3-ax^2+axy-y^3=o$, which therefore, being expunged and the remaining terms being divided by o, there will remain

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

But whereas zero is supposed to be infinitely little, that it may represent the moments of quantities, the terms that are multiplied by it will be nothing in respect of the rest (*termini in eam ducti pro nihilo possunt haberi cum aliis collati*); therefore I reject them, and there remains

$$3x^2\dot{x}-2ax\dot{x}+ay\dot{x}+ax\dot{y}-3y^2\dot{y}=o,$$

as above in Example I." Newton here uses infinitesimals.

Much greater than in the first problem were the difficulties encountered in the solution of the second problem, involving, as it does, inverse operations which have been taxing the skill of the best analysts since his time. Newton gives first a special solution to the second problem in which he resorts to a rule for which he has given no proof.

In the general solution of his second problem, Newton assumed homogeneity with respect to the fluxions and then considered three cases: (1) when the equation contains two fluxions of quantities and but one of the fluents; (2) when the equation involves both the fluents as well as both the fluxions; (3) when the equation contains the fluents and the fluxions of three or more quantities. The first case is the easiest since it requires simply the integration of $\frac{dy}{dx}=f(x)$, to which his "special solution" is applicable. The second case demanded nothing less than the general solution of a differential equation of the first order. Those who know what efforts were afterwards needed for the complete exploration of this field in analysis, will not depreciate Newton's work even though he resorted to solutions in form of infinite series. Newton's third case comes now under the solution of partial differential equations. He took the equation $2\dot{x}-\dot{z}+x\dot{y}=0$ and succeeded in finding a particular integral of it.

The rest of the treatise is devoted to the determination of maxima and minima, the radius of curvature of curves, and other geometrical applications of his fluxionary calculus. All this was done previous to the year 1672.

It must be observed that in the *Method of Fluxions* (as well as in his *De Analysi* and all earlier papers) the method employed by Newton is strictly infinitesimal, and in substance like that of Leibniz. Thus, the original conception of the calculus in England, as well as on the Continent, was based on infinitesimals. The fundamental principles of the fluxionary calculus were first given to the world in the *Principia;* but its peculiar notation did not appear until published in the second volume of Wallis' *Algebra* in 1693. The exposition given in the *Algebra* was a contribution of Newton; it rests on infinitesimals. In the first edition of the *Principia* (1687) the description of fluxions is likewise founded on infinitesimals, but in the second (1713) the foundation is somewhat altered. In Book II, Lemma II, of the first edition we read: "Cave tamen intellexeris particulas finitas. *Momenta quam primum finitæ sunt magnitudinis, desinunt esse momenta. Finiri enim repugnat aliquatenus perpetuo eorum incremento vel decremento.* Intelligenda sunt principia jamjam nascentia finitarum magnitudinum." In the second edition the two sentences which we print in italics are replaced by the following: "Particulæ finitæ non sunt momenta sed quantitates ipsæ ex momentis genitæ." Through the difficulty of the phrases in both extracts, this much distinctly appears, that in the first, moments are infinitely small quantities. What else they are in the second is not clear.[1] In the *Quadrature of Curves* of 1704, the infinitely small quantity is completely abandoned. It has been shown that in the *Method of Fluxions* Newton rejected terms involving the quantity o, because they are infinitely small compared with other terms. This reasoning is unsatisfactory; for as long as o is a quantity, though ever so small, this rejection cannot be made without affecting the result. Newton seems to have felt this, for in the *Quadrature of Curves* he remarked that "in mathematics the minutest errors are not to be neglected" (errores quam minimi in rebus mathematicis non sunt contemnendi).

The early distinction between the system of Newton and Leibniz

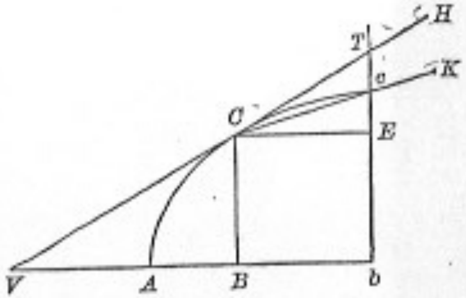
[1] A. De Morgan, *loc. cit.*, 1852.

lies in this, that Newton, holding to the conception of velocity or fluxion, used the infinitely small increment as a means of determining it, while with Leibniz the relation of the infinitely small increments is itself the object of determination. The difference between the two rests mainly upon a difference in the mode of generating quantities.

We give Newton's statement of the method of fluxions or rates, as given in the introduction to his *Quadrature of Curves.* "I consider mathematical quantities in this place not as consisting of very small parts, but as described by a continued motion. Lines are described, and thereby generated, not by the apposition of parts, but by the continued motion of points; superficies by the motion of lines; solids by the motion of superficies; angles by the rotation of the sides; portions of time by continual flux: and so on in other quantities. These geneses really take place in the nature of things, and are daily seen in the motion of bodies. . . .

"Fluxions are, as near as we please (*quam proxime*), as the increments of fluents generated in times, equal and as small as possible, and to speak accurately, they are in the prime ratio of nascent increments; yet they can be expressed by any lines whatever, which are proportional to them."

Newton exemplifies this last assertion by the problem of tangency: Let AB be the abscissa, BC the ordinate, VCH the tangent, Ec the increment of the ordinate, which produced meets VH at T, and Cc the increment of the curve. The right line Cc being produced to K, there are formed three small triangles, the rectilinear CEc, the mixtilinear CEc, and the rectilinear CET. Of these, the first is evidently the smallest, and the last the greatest. Now suppose the ordinate bc to move into the place BC, so that the point c exactly coincides with the point C; CK, and therefore the curve Cc, is coincident with the tangent CH, Ec is absolutely equal to ET, and the mixtilinear evanescent triangle CEc is, in the last form, similar to the triangle CET; and its evanescent sides CE, Ec, Cc, will be proportional to CE, ET, and CT, the sides of the triangle CET.



Hence it follows that the fluxions of the lines AB, BC, AC, being in the last ratio of their evanescent increments, are proportional to the sides of the triangle CET, or, which is all one, of the triangle VBC similar thereunto. As long as the points C and c are distant from each other by an interval, however small, the line CK will stand apart by a small angle from the tangent CH. But when CK coincides with CH, and the lines CE, Ec, cC reach their ultimate ratios, then the points C and c accurately coincide and are one and the same. Newton then adds that "in mathematics the minutest errors are not to be neglected." This is plainly a rejection of the postulates of Leibniz. The doctrine of infinitely small quantities is here renounced in a manner which would lead one to suppose that Newton had never held it himself. Thus it appears that Newton's doctrine was different in different periods. Though, in the above reasoning, the Charybdis of infinitesimals is safely avoided, the dangers of a Scylla stare us in the face. We are required to believe that a point may be considered a triangle, or that a triangle can be inscribed in a point; nay, that three dissimilar triangles become similar and equal when they have reached their ultimate form in one and the same point.

In the introduction to the *Quadrature of Curves* the fluxion of x^n is determined as follows:—

"In the same time that x, by flowing, becomes $x+o$, the power x^n becomes $(x+o)^n$, *i. e.* by the method of infinite series

$$x^n+nox^{n-1}+\frac{n^2-n}{2}o^2x^{n-2}+\text{etc.},$$

and the increments

$$o \text{ and } nox^{n-1}+\frac{n^2-n}{2}o^2x^{n-2}+\text{etc.},$$

are to one another as

$$1 \text{ to } nx^{n-1}+\frac{n^2-n}{2}ox^{n-2}+\text{etc.}$$

"Let now the increments vanish, and their last proportion will be 1 to nx^{n-1}: hence the fluxion of the quantity x is to the fluxion of the quantity x^n as $1 : nx^{n-1}$.

"The fluxion of lines, straight or curved, in all cases whatever, as also the fluxions of superficies, angles, and other quantities, can be obtained in the same manner by the method of prime and ultimate ratios. But to establish in this way the analysis of infinite quantities, and to investigate prime and ultimate ratios of finite quantities, nascent or evanescent, is in harmony with the geometry of the ancients; and I have endeavored to show that, in the method of fluxions, it is not necessary to introduce into geometry infinitely small quantities." This mode of differentiating does not remove all the difficulties connected with the subject. When o becomes nothing, then we get the ratio $\frac{o}{o}=nx^{n-1}$, which needs further elucidation. Indeed, the method of Newton, as delivered by himself, is encumbered with difficulties and objections. Later we shall state Bishop Berkeley's objection to this reasoning. Even among the ablest admirers of Newton, there have been obstinate disputes respecting his explanation of his method of "prime and ultimate ratios."

The so-called "method of limits" is frequently attributed to Newton, but the pure method of limits was never adopted by him as his method of constructing the calculus. All he did was to establish in his *Principia* certain principles which are applicable to that method, but which he used for a different purpose. The first lemma of the first book has been made the foundation of the method of limits:—

"Quantities and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer the one to the other than by any given difference, become ultimately equal."

In this, as well as in the lemmas following this, there are obscurities and difficulties. Newton appears to teach that a variable quantity and its limit will ultimately coincide and be equal.

The full title of Newton's *Principia* is *Philosophiæ Naturalis Principia Mathematica.* It was printed in 1687 under the direction, and at the expense, of Edmund Halley. A second edition was brought out in 1713 with many alterations and improvements, and accompanied by a preface from Roger Cotes. It was sold out in a few months, but a pirated edition published in Amsterdam supplied the demand. The third and last edition which appeared in England during Newton's lifetime was published in 1726 by Henry Pemberton. The *Principia* consists of three books, of which the first two, constituting the great bulk of the work, treat of the mathematical principles of natural philosophy, namely, the laws and conditions of motions and forces. In the third book is drawn up the constitution of the universe as deduced from the foregoing principles. The great principle underlying this memorable work is that of universal gravitation. The first book was completed on April 28, 1686. After the remarkably short period of three months, the second book was finished. The third book is the result of the next nine or ten months' labors. It is only a sketch of a much more extended elaboration of the subject which he had planned, but which was never brought to completion.

The law of gravitation is enunciated in the first book. Its discovery envelops the name of Newton in a halo of perpetual glory. The current version of the discovery is as follows: it was conjectured by Robert Hooke (1635–1703), C. Huygens, E. Halley, C. Wren, I. Newton, and others, that, if J. Kepler's third law was true (its absolute accuracy was doubted at that time), then the attraction between the earth and other members of the solar system varied inversely as the square of the distance. But the proof of the truth or falsity of the guess was wanting. In 1666 Newton reasoned, in substance, that if g represent the acceleration of gravity on the surface of the earth, r

be the earth's radius, R the distance of the moon from the earth, T the time of lunar revolution, and a a degree at the equator, then, if the law is true,

$$g\frac{r^2}{R^2}=4\pi^2\frac{R}{T^2}, \text{ or } g=\frac{4\pi}{T^2}\left(\frac{R}{r}\right)^3 \cdot 180a.$$

The data at Newton's command gave $R=60.4r$, $T=2{,}360{,}628$ seconds, but a only 60 instead of $69\frac{1}{2}$ English miles. This wrong value of a rendered the calculated value of g smaller than its true value, as known from actual measurement. It looked as though the law of inverse squares were not the true law, and Newton laid the calculation aside. In 1684 he casually ascertained at a meeting of the Royal Society that Jean Picard had measured an arc of the meridian, and obtained a more accurate value for the earth's radius. Taking the corrected value for a, he found a figure for g which corresponded to the known value. Thus the law of inverse squares was verified. In a scholium in the *Principia*, Newton acknowledged his indebtedness to Huygens for the laws on centrifugal force employed in his calculation.

The perusal by the astronomer Adams of a great mass of unpublished letters and manuscripts of Newton forming the Portsmouth collection (which remained private property until 1872, when its owner placed it in the hands of the University of Cambridge) seems to indicate that the difficulties encountered by Newton in the above calculation were of a different nature. According to Adams, Newton's numerical verification was fairly complete in 1666, but Newton had not been able to determine what the attraction of a spherical shell upon an external point would be. His letters to E. Halley show that he did not suppose the earth to attract as though all its mass were concentrated into a point at the centre. He could not have asserted, therefore, that the assumed law of gravity was verified by the figures, though for long distances he might have claimed that it yielded close approximations. When Halley visited Newton in 1684, he requested Newton to determine what the orbit of a planet would be if the law of attraction were that of inverse squares. Newton had solved a similar problem for R. Hooke in 1679, and replied at once that it was an ellipse. After Halley's visit, Newton, with Picard's new value for the earth's radius, reviewed his early calculation, and was able to show that if the distances between the bodies in the solar system were so great that the bodies might be considered as points, then their motions were in accordance with the assumed law of gravitation. In 1685 he completed his discovery by showing that a sphere whose density at any point depends only on the distance from the centre attracts an external point as though its whole mass were concentrated at the centre.

Newton's unpublished manuscripts in the Portsmouth collection show that he had worked out, by means of fluxions and fluents, his

lunar calculations to a higher degree of approximation than that given in the *Principia*, but that he was unable to interpret his results geometrically. The papers in that collection throw light upon the mode by which Newton arrived at some of the results in the *Principia*, as, for instance, the famous solution in Book II, Prop. 35, Scholium, of the problem of the solid of revolution which moves through a resisting medium with the least resistance. The solution is unproved in the *Principia*, but is demonstrated by Newton in the draft of a letter to David Gregory of Oxford, found in the Portsmouth collection.[1]

It is chiefly upon the *Principia* that the fame of Newton rests. David Brewster calls it "the brightest page in the records of human reason." Let us listen, for a moment, to the comments of P. S. Laplace, the foremost among those followers of Newton who grappled with the subtle problems of the motions of planets under the influence of gravitation: "Newton has well established the existence of the principle which he had the merit of discovering, but the development of its consequences and advantages has been the work of the successors of this great mathematician. The imperfection of the infinitesimal calculus, when first discovered, did not allow him completely to resolve the difficult problems which the theory of the universe offers; and he was oftentimes forced to give mere hints, which were always uncertain till confirmed by rigorous analysis. Notwithstanding these unavoidable defects, the importance and the generality of his discoveries respecting the system of the universe, and the most interesting points of natural philosophy, the great number of profound and original views, which have been the origin of the most brilliant discoveries of the mathematicians of the last century, which were all presented with much elegance, will insure to the *Principia* a lasting pre-eminence over all other productions of the human mind."

Newton's *Arithmetica universalis*, consisting of algebraical lectures delivered by him during the first nine years he was professor at Cambridge, were published in 1707, or more than thirty years after they were written. This work was published by William Whiston (1667–1752). We are not accurately informed how Whiston came in possession of it, but according to some authorities its publication was a breach of confidence on his part. He succeeded Newton in the Lucasian professorship at Cambridge.

The *Arithmetica universalis* contains new and important results on the theory of equations. Newton states Descartes' rule of signs in accurate form and gives formulæ expressing the sum of the powers of the roots up to the sixth power and by an "and so on" makes it evident that they can be extended to any higher power. Newton's formulæ take the implicit form, while similar formulæ given earlier

[1] O. Bolza, in *Bibliotheca mathematica*, 3. S., Vol. 13, 1913, pp. 146–149. For a bibliography of this "problem of Newton" on the surface of least resistance, see *L'Intermédiaire des mathématiciens*, Vol. 23, 1916, pp. 81–84.

by Albert Girard take the explicit form, as do also the general formulæ derived later by E. Waring. Newton uses his formulæ for fixing an upper limit of real roots; the sum of any even power of all the roots must exceed the same even power of any one of the roots. He established also another limit: A number is an upper limit, if, when substituted for x, it gives to $f(x)$ and to all its derivatives the same sign. In 1748 Colin Maclaurin proved that an upper limit is obtained by adding unity to the absolute value of the largest negative coefficient of the equation. Newton showed that in equations with real coefficients, imaginary roots always occur in pairs. His inventive genius is grandly displayed in his rule for determining the inferior limit of the number of imaginary roots, and the superior limits for the number of positive and negative roots. Though less expeditious than Descartes', Newton's rule always gives as close, and generally closer, limits to the number of positive and negative roots. Newton did not prove his rule.

Some light was thrown upon it by George Campbell and Colin Maclaurin, in the *Philosophical Transactions*, of the years 1728 and 1729. But no complete demonstration was found for a century and a half, until, at last, Sylvester established a remarkable general theorem which includes Newton's rule as a special case. Not without interest is Newton's suggestion that the conchoid be admitted as a curve to be used in geometric constructions, along with the straight line and circle, since the conchoid can be used for the duplication of a cube and trisection of an angle—to one or the other of which every problem involving curves of the third or fourth degree can be reduced.

The treatise on *Method of Fluxions* contains Newton's method of approximating to the roots of numerical equations. Substantially the same explanation is given in his *De analysi per æquationes numero terminorum infinitas*. He explains it by working one example, namely the now famous cubic [1] $y^3-2y-5=0$. The earliest printed account appeared in Wallis' *Algebra*, 1685, chapter 94. Newton assumes that an approximate value is already known, which differs from the true value by less than one-tenth of that value. He takes $y=2$ and substitutes $y=2+p$ in the equation, which becomes $p^3+6p^2+10p-1=0$. Neglecting the higher powers of p, he gets $10p-1=0$. Taking $p=.1+q$, he gets $q^3+6.3q^2+11.23q+.061=0$. From $11.23q+.061=0$ he gets $q=-.0054+r$, and by the same process, $r=-.00004853$. Finally $y=2+.1-.0054-.00004853=2.09455147$. Newton arranges his work in a paradigm. He seems quite aware that his method may fail. If there is doubt, he says, whether $p=.1$ is sufficiently close to the truth, find p from $6p^2+10p-1=0$. He does not show that even this latter method will always answer. By the same mode of procedure, Newton finds, by a rapidly converging series, the value of y in terms of a and x, in the equation $y^3+axy+aay-x^3-2a^3=0$.

[1] For quotations from Newton, see F. Cajori, "Historical Note on the Newton-Raphson Method of Approximation," *Amer. Math. Monthly*, Vol. 18, 1911, pp. 29–33.

In 1690, Joseph Raphson (1648–1715), a fellow of the Royal Society of London, published a tract, *Analysis æquationum universalis*. His method closely resembles that of Newton. The only difference is this, that Newton derives each successive step, p, q, r, of approach to the root, from a *new* equation, while Raphson finds it each time by substitution in the original equation. In Newton's cubic, Raphson would not find the second correction by the use of $x^3+6x^2+10x-1=0$, but would substitute $2.1+q$ in the original equation, finding $q=-.0054$. He would then substitute $2.0946+r$ in the original equation, finding $r=-.00004853$, and so on. Raphson does not mention Newton; he evidently considered the difference sufficient for his method to be classed independently. To be emphasized is the fact that the process which in modern texts goes by the name of "Newton's method of approximation," is really not Newton's method, but Raphson's modification of it. The *form* now so familiar, $a-\frac{f(a)}{f'(a)}$ was not used by Newton, but was used by Raphson. To be sure, Raphson does not use this notation; he writes $f(a)$ and $f'(a)$ out in full as polynomials. It is doubtful, whether this method should be named after Newton alone. Though not identical with Vieta's process, it resembles Vieta's. The chief difference lies in the divisor used. The divisor $f'(a)$ is much simpler, and easier to compute than Vieta's divisor. Raphson's version of the process represents what J. Lagrange recognized as an advance on the scheme of Newton. The method is "plus simple que celle de Newton." [1] Perhaps the name "Newton-Raphson method" would be a designation more nearly representing the facts of history. We may add that the solution of numerical equations was considered geometrically by Thomas Baker in 1684 and Edmund Halley in 1687, but in 1694 Halley "had a very great desire of doing the same in numbers." The only difference between Halley's and Newton's own method is that Halley solves a quadratic equation at each step, Newton a linear equation. Halley modified also certain algebraic expressions yielding approximate cube and fifth roots, given in 1692 by the Frenchman, *Thomas Fantel de Lagny* (1660–1734). In 1705 and 1706 Lagny outlines a method of differences; such a method, less systematically developed, had been previously explained in England by John Collins. By this method, if $a, b, c, \ldots$ are in arithmetical progression, then a root may be found approximately from the first, second, and higher differences of $f(a)$, $f(b)$, $f(c)$, . . .

Newton's *Method of Fluxions* contains also "Newton's parallelogram," which enabled him, in an equation, $f(x, y)=0$, to find a series

[1] Lagrange, *Résolution des equat. num.*, 1798, Note V, p. 138.

in powers of x equal to the variable y. The great utility of this rule lay in its determining the *form* of the series; for, as soon as the law was known by which the exponents in the series vary, then the expansion could be effected by the method of indeterminate coefficients. The rule is still used in determining the infinite branches to curves, or their figure at multiple points. Newton gave no proof for it, nor any clue as to how he discovered it. The proof was supplied half a century later, by A. G. Kästner and G. Cramer, independently.[1]

In 1704 was published, as an appendix to the *Opticks*, the *Enumeratio linearum tertii ordinis*, which contains theorems on the theory of curves. Newton divides cubics into seventy-two species, arranged in larger groups, for which his commentators have supplied the names "genera" and "classes," recognizing fourteen of the former and seven (or four) of the latter. He overlooked six species demanded by his principles of classification, and afterwards added by J. Stirling, William Murdoch (1754–1839), and G. Cramer. He enunciates the remarkable theorem that the five species which he names "divergent parabolas" give by their projection every cubic curve whatever. As a rule, the tract contains no proofs. It has been the subject of frequent conjecture how Newton deduced his results. Recently we have gotten at the facts, since much of the analysis used by Newton and a few additional theorems have been discovered among the Portsmouth papers. An account of the four holograph manuscripts on this subject has been published by W. W. Rouse Ball, in the *Transactions of the London Mathematical Society* (vol. xx, pp. 104–143). It is interesting to observe how Newton begins his research on the classification of cubic curves by the algebraic method, but, finding it laborious, attacks the problem geometrically, and afterwards returns again to analysis.

Space does not permit us to do more than merely mention Newton's prolonged researches in other departments of science. He conducted a long series of experiments in optics and is the author of the corpuscular theory of light. The last of a number of papers on optics, which he contributed to the Royal Society, 1687, elaborates the theory of "fits." He explained the decomposition of light and the theory of the rainbow. By him were invented the reflecting telescope and the sextant (afterwards re-invented by Thomas Godfrey of Philadelphia [2] and by John Hadley). He deduced a theoretical expression for the velocity of sound in air, engaged in experiments on chemistry, elasticity, magnetism, and the law of cooling, and entered upon geological speculations.

During the two years following the close of 1692, Newton suffered

[1] S. Günther, *Vermischte Untersuchungen zur Geschichte d. math. Wiss.*, Leipzig, 1876, pp. 136–187.

[2] F. Cajori, *Teaching and History of Mathematics in the U. S.*, Washington, 1890, p. 42.

from insomnia and nervous irritability. Some thought that he labored under temporary mental aberration. Though he recovered his tranquillity and strength of mind, the time of great discoveries was over; he would study out questions propounded to him, but no longer did he by his own accord enter upon new fields of research. The most noted investigation after his sickness was the testing of his lunar theory by the observations of Flamsteed, the astronomer royal. In 1695 he was appointed warden, and in 1699 master of the mint, which office he held until his death. His body was interred in Westminster Abbey, where in 1731 a magnificent monument was erected, bearing an inscription ending with, "Sibi gratulentur mortales tale tantumque exstitisse humani generis decus." It is not true that the Binomial Theorem is also engraved on it.

We pass to Leibniz, the second and independent inventor of the calculus. **Gottfried Wilhelm Leibniz** (1646–1716) was born in Leipzig. No period in the history of any civilized nation could have been less favorable for literary and scientific pursuits than the middle of the seventeenth century in Germany. Yet circumstances seem to have happily combined to bestow on the youthful genius an education hardly otherwise obtainable during this darkest period of German history. He was brought early in contact with the best of the culture then existing. In his fifteenth year he entered the University of Leipzig. Though law was his principal study, he applied himself with great diligence to every branch of knowledge. Instruction in German universities was then very low. The higher mathematics was not taught at all. We are told that a certain John Kuhn lectured on Euclid's *Elements*, but that his lectures were so obscure that none except Leibniz could understand them. Later on, Leibniz attended, for a half-year, at Jena, the lectures of Erhard Weigel, a philosopher and mathematician of local reputation. In 1666 Leibniz published a treatise, *De Arte Combinatoria*, in which he does not pass beyond the rudiments of mathematics, but which contains remarkable plans for a theory of mathematical logic, a symbolic method with formal rules obviating the necessity of thinking. Vaguely such plans had been previously suggested by R. Descartes and Pierre Hérigone. In manuscripts which Leibniz left unpublished he enunciated the principal properties of what is now called logical multiplication, addition, negation, identity, class-induction and the null-class.[1] Other theses written by him at this time were metaphysical and juristical in character. A fortunate circumstance led Leibniz abroad. In 1672 he was sent by Baron Boineburg on a political mission to Paris. He there formed the acquaintance of the most distinguished men of the age. Among these was C. Huygens, who presented a copy of his work on the oscillation of the pendulum to Leibniz, and first led the gifted young German to the study of higher mathematics. In 1673 Leibniz

[1] See Philip E. B. Jourdain in *Quarterly Jour. of Math.*, Vol. 41, 1910, p. 329.

P. E. B. Jourdain,[1] "Leibniz himself attributed all of his mathematical discoveries to his improvements in notation."

Before tracing the further development of the calculus we shall sketch the history of that long and bitter controversy between English and Continental mathematicians on the invention of the calculus. The question was, did Leibniz invent it independently of Newton, or was he a plagiarist?

We must begin with the early correspondence between the parties appearing in this dispute. Newton had begun using his notation of fluxions in 1665.[2] In 1669 I. Barrow sent John Collins Newton's tract, *De Analysi per equationes*, etc.

The first visit of Leibniz to London extended from the 11th of January until March, 1673. He was in the habit of committing to writing important scientific communications received from others. In 1890 C. J. Gerhardt discovered in the royal library at Hanover a sheet of manuscript with notes taken by Leibniz during this journey.[3] They are headed "Observata Philosophica in itinere Anglicano sub initium anni 1673." The sheet is divided by horizontal lines into sections. The sections given to Chymica, Mechanica, Magnetica, Botanica, Anatomica, Medica, Miscellanea, contain extensive memoranda, while those devoted to mathematics have very few notes. Under Geometrica he says only this: "Tangentes omnium figurarum. Figurarum geometricarum explicatio per motum puncti in moto lati." We suspect from this that Leibniz had read Isaac Barrow's lectures. Newton is referred to only under Optica. Evidently Leibniz did not obtain a knowledge of fluxions during this visit to London, nor is it claimed that he did by his opponents.

Various letters of I. Newton, J. Collins, and others, up to the beginning of 1676, state that Newton invented a method by which tangents could be drawn without the necessity of freeing their equations from irrational terms. Leibniz announced in 1674 to H. Oldenburg, then secretary of the Royal Society, that he possessed very general analytical methods, by which he had found theorems of great importance on the quadrature of the circle by means of series. In answer, Oldenburg stated Newton and James Gregory had also discovered methods of quadratures, which extended to the circle. Leibniz desired to have these methods communicated to him; and Newton, at the request of Oldenburg and Collins, wrote to the former the celebrated letters of June 13 and October 24, 1676. The first contained the Binomial Theorem and a variety of other matters relating to infinite series and quadratures; but nothing directly on the method of

[1] P. E. B. Jourdain, *The Nature of Mathematics*, London, p. 71.

[2] J. Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes*, London, 1850, p. xxi; A. De Morgan, "Fluxions" and "Commercium Epistolicum" in the *Penny Cyclopædia*.

[3] C. J. Gerhardt, "Leibniz in London" in *Sitzungsberichte der K. Preussischen Academie d. Wissensch. zu Berlin*, Feb., 1891.

fluxions. Leibniz in reply speaks in the highest terms of what Newton had done, and requests further explanation. Newton in his second letter just mentioned explains the way in which he found the Binomial Theorem, and also communicates his method of fluxions and fluents in form of an anagram in which all the letters in the sentence communicated were placed in alphabetical order. Thus Newton says that his method of drawing tangents was

6a cc d æ 13e ff 7i 3l 9n 4o 4qrr 4s 9t 12vx.

The sentence was, "Data æquatione quotcunque fluentes quantitates involvente fluxiones invenire, et vice versa." ("Having any given equation involving never so many flowing quantities, to find the fluxions, and vice versa.") Surely this anagram afforded no hint. Leibniz wrote a reply to John Collins, in which, without any desire of concealment, he explained the principle, notation, and the use of the differential calculus.

The death of Oldenburg brought this correspondence to a close. Nothing material happened till 1684, when Leibniz published his first paper on the differential calculus in the *Acta eruditorum*, so that while Newton's claim to the priority of invention must be admitted by all, it must also be granted that Leibniz was the first to give the full benefit of the calculus to the world. Thus, while Newton's invention remained a secret, communicated only to a few friends, the calculus of Leibniz was spreading over the Continent. No rivalry or hostility existed, as yet, between the illustrious scientists. Newton expressed a very favorable opinion of Leibniz's inventions, known to him through the above correspondence with Oldenburg, in the following celebrated scholium (*Principia*, first edition, 1687, Book II, Prop. 7, scholium):—

"In letters which went between me and that most excellent geometer, G. G. Leibniz, ten years ago, when I signified that I was in the knowledge of a method of determining maxima and minima, of drawing tangents, and the like, and when I concealed it in transposed letters involving this sentence (Data æquatione, etc., above cited), that most distinguished man wrote back that he had also fallen upon a method of the same kind, and communicated his method, which hardly differed from mine, except in his forms of words and symbols."

As regards this passage, we shall see that Newton was afterwards weak enough, as De Morgan says: "First, to deny the plain and obvious meaning, and secondly, to omit it entirely from the third edition of the *Principia*." On the Continent, great progress was made in the calculus by Leibniz and his coadjutors, the brothers James and John Bernoulli, and Marquis de l'Hospital. In 1695 John Wallis informed Newton by letter that "he had heard that his notions of fluxions passed in Holland with great applause by the name of 'Leibniz's Calculus Differentialis.'" Accordingly Wallis stated in the preface to a volume of his works that the calculus differentialis was Newton's method of fluxions which had been communicated to Leibniz in the Oldenburg letters. A review of Wallis' works, in the *Acta eruditorum* for 1696, reminded the reader of Newton's own admission in the scholium above cited.

For fifteen years Leibniz had enjoyed unchallenged the honor of being the inventor of his calculus. But in 1699 *Fatio de Duillier* (1664–1753), a Swiss, who had settled in England, stated in a mathematical paper, presented to the Royal Society, his conviction that I. Newton was the first inventor; adding that, whether Leibniz, the second inventor, had borrowed anything from the other, he would leave to the judgment of those who had seen the letters and manuscripts of Newton. This was the first distinct insinuation of plagiarism. It would seem that the English mathematicians had for some time been cherishing suspicions unfavorable to Leibniz. A feeling had doubtless long prevailed that Leibniz, during his second visit to London in 1676, had or might have seen among the papers of John Collins, Newton's *Analysis per æquationes*, etc., which contained applications of the fluxionary method, but no systematic development or explanation of it. Leibniz certainly did see at least part of this tract. During the week spent in London, he took note of whatever interested him among the letters and papers of Collins. His memoranda discovered by C. J. Gerhardt in 1849 in the Hanover library fill two sheets.[1] The one bearing on our question is headed "Excerpta ex tractatu Newtoni Msc. de Analysi per æquationes numero terminorum infinitas." The notes are very brief, excepting those *De resolutione æquationum affectarum*, of which there is an almost complete copy. This part was evidently new to him. If he examined Newton's entire tract, the other parts did not particularly impress him. From it he seems to have gained nothing pertaining to the infinitesimal calculus. By the previous introduction of his own algorithm he had made greater progress than by what came to his knowledge in London. Nothing mathematical that he had received engaged his thoughts in the immediate future, for on his way back to Holland he composed a lengthy dialogue on mechanical subjects.

Fatio de Duillier's insinuations lighted up a flame of discord which a whole century was hardly sufficient to extinguish. Leibniz, who had never contested the priority of Newton's discovery, and who appeared to be quite satisfied with Newton's admission in his scholium, now appears for the first time in the controversy. He made an animated reply in the *Acta eruditorum* and complained to the Royal Society of the injustice done him.

Here the affair rested for some time. In the *Quadrature of Curves*, published 1704, for the first time, a formal exposition of the method and notation of fluxions was made public. In 1705 appeared an un-

[1] C. J. Gerhardt, "Leibniz in London," *loc. cit.*

favorable review of this in the *Acta eruditorum*, stating that Newton uses and always has used fluxions for the differences of Leibniz. This was considered by Newton's friends an imputation of plagiarism on the part of their chief, but this interpretation was always strenuously resisted by Leibniz. *John Keill* (1671–1721), professor of astronomy at Oxford, undertook with more zeal than judgment the defence of Newton. In a paper inserted in the *Philosophical Transactions* of 1708, he claimed that Newton was the first inventor of fluxions and "that the same calculus was afterward published by Leibniz, the name and the mode of notation being changed." Leibniz complained to the secretary of the Royal Society of bad treatment and requested the interference of that body to induce Keill to disavow the intention of imputing fraud. John Keill was not made to retract his accusation; on the contrary, was authorized by Newton and the Royal Society to explain and defend his statement. This he did in a long letter. Leibniz thereupon complained that the charge was now more open than before, and appealed for justice to the Royal Society and to Newton himself. The Royal Society, thus appealed to as a judge, appointed a committee which collected and reported upon a large mass of documents—mostly letters from and to Newton, Leibniz, Wallis, Collins, etc. This report, called the *Commercium epistolicum*, appeared in the year 1712 and again in 1722 and 1725, with a Recensio prefixed, and additional notes by Keill. The final conclusion in the *Commercium epistolicum* was that Newton was "the first inventor." But this was not to the point. The question was not whether Newton was the first inventor, but whether Leibniz had stolen the method. The committee had not formally ventured to assert their belief that Leibniz was a plagiarist. In the following sentence they insinuated that Leibniz did take or might have taken, his method from that of Newton: "And we find no mention of his (Leibniz's) having any other *Differential Method* than *Mouton's* before his Letter of 21st of *June*, 1677, which was a year after a Copy of Mr. *Newton's* Letter, of the 10th of *December*, 1672, had been sent to *Paris* to be communicated to him; and about four years after *Mr. Collins* began to communicate that Letter to his Correspondents; in which Letter the Method of *Fluxions* was sufficiently describ'd to any intelligent Person."

About 1850 it was shown that what H. Oldenburg sent to Leibniz was not Newton's letter of Dec. 10, 1672, but only excerpts from it which omitted Newton's method of drawing tangents and could not possibly convey an idea of fluxions. Oldenburg's letter was found among the Leibniz manuscripts in the Royal Library at Hanover, and was published by C. J. Gerhardt in 1846, 1848, 1849 and 1855,[1] and again later.

[1] See *Essays on the Life and Work of Newton* by Augustus De Morgan, edited, with notes and appendices, by Philip E. B. Jourdain, Chicago and London, 1914. Jourdain gives on p. 102 the bibliography of the publications of Newton and Leibniz.

Moreover, when J. Edleston in 1850 published the *Correspondence of Sir Isaac Newton and Professor Cotes*, it became known that the Royal Society in 1712 had not one, but two, parcels of Collins. One parcel contained letters of James Gregory, and Isaac Newton's letter of Dec. 10, 1672, in full; the other parcel, which was marked "To Leibnitz, the 14th of June, 1676 About Mr. Gregories remains," contained an abridgment of a part of the contents of the first parcel, with nothing but an allusion to Newton's method described in his letter of Dec. 10, 1672. In the *Commercium epistolicum* Newton's letter was printed in full and no mention was made of the existence of the second parcel that was marked "To Leibnitz. . . ." Thus the *Commercium epistolicum* conveyed the impression that Newton's uncurtailed letter of Dec. 10, 1672, had reached Leibniz in which fluxions "was sufficiently described to any intelligent person," while as a matter of fact the method is not described at all in the letter which Leibniz received.

Leibniz protested only in private letters against the proceeding of the Royal Society, declaring that he would not answer an argument so weak. John Bernoulli, in a letter to Leibniz, which was published later in an anonymous tract, is as decidedly unfair towards Newton as the friends of the latter had been towards Leibniz. John Keill replied, and then Newton and Leibniz appear as mutual accusers in several letters addressed to third parties. In a letter dated April 9, 1716, and sent to Antonio Schinella Conti (1677–1749), an Italian priest then residing in London, Leibniz again reminded Newton of the admission he had made in the scholium, which he was now desirous of disavowing; Leibniz also states that he always believed Newton, but that, seeing him connive at accusations which he must have known to be false, it was natural that he (Leibniz) should begin to doubt. Newton did not reply to this letter, but circulated some remarks among his friends which he published immediately after hearing of the death of Leibniz, November 14, 1716. This paper of Newton gives the following explanation pertaining to the scholium in question: "He [Leibniz] pretends that in my book of principles I allowed him the invention of the calculus differentialis, independently of my own; and that to attribute this invention to myself is contrary to my knowledge there avowed. But in the paragraph there referred unto I do not find one word to this purpose." In the third edition of the *Principia*, 1726, Newton omitted the scholium and substituted in its place another, in which the name of Leibniz does not appear.

National pride and party feeling long prevented the adoption of impartial opinions in England, but now it is generally admitted by

We recommend J. B. Biot and F. Lefort's edition of the *Commercium epistolicum*, Paris, 1856, which exhibits all the alterations made in the different reprints of this publication and reproduces also H. Oldenburg's letter to Leibniz of July 26, 1676, and other important documents bearing on the controversy.

nearly all familiar with the matter, that Leibniz really was an independent inventor. Perhaps the most telling evidence to show that Leibniz was an independent inventor is found in the study of his mathematical papers (collected and edited by C. J. Gerhardt, in seven volumes, Berlin, 1849–1863), which point out a gradual and natural evolution of the rules of the calculus in his own mind. "There was throughout the whole dispute," says De Morgan, "a confusion between the knowledge of fluxions or differentials and that of a *calculus* of fluxions or differentials; that is, a digested method with general rules."

This controversy is to be regretted on account of the long and bitter alienation which it produced between English and Continental mathematicians. It stopped almost completely all interchange of ideas on scientific subjects. The English adhered closely to Newton's methods and, until about 1820, remained, in most cases, ignorant of the brilliant mathematical discoveries that were being made on the Continent. The loss in point of scientific advantage was almost entirely on the side of Britain. The only way in which this dispute may be said, in a small measure, to have furthered the progress of mathematics, is through the challenge problems by which each side attempted to annoy its adversaries.

The recurring practice of issuing challenge problems was inaugurated at this time by Leibniz. They were, at first, not intended as defiances, but merely as exercises in the new calculus. Such was the problem of the isochronous curve (to find the curve along which a body falls with uniform velocity), proposed by him to the Cartesians in 1687, and solved by Jakob Bernoulli, himself, and Johann Bernoulli. Jakob Bernoulli proposed in the *Acta eruditorum* of 1690 the question to find the curve (the catenary) formed by a chain of uniform weight suspended freely from its ends. It was resolved by C. Huygens, G. W. Leibniz, Johann Bernoulli, and Jakob Bernoulli himself; the properties of the catenary were worked out methodically by David Gregory [1] of Oxford and himself. In 1696 Johann Bernoulli challenged the best mathematicians in Europe to solve the difficult problem, to find the curve (the cycloid) along which a body falls from one point to another in the shortest possible time. Leibniz solved it the day he received it. Newton, de l'Hospital, and the two Bernoullis gave solutions. Newton's appeared anonymously in the *Philosophical Transactions*, but Johann Bernoulli recognized in it his powerful mind, "tanquam," he says, "ex ungue leonem." The problem of orthogonal trajectories (a system of curves described by a known law being given, to describe a curve which shall cut them all at right angles) was proposed by Johann Bernoulli in a letter to G. W. Leibniz in 1694. Later it was long printed in the *Acta eruditorum*, but failed at first to receive much

[1] *Phil. Trans.*, London, 1697.

attention. It was again proposed in 1716 by Leibniz, to feel the pulse of the English mathematicians.

This may be considered as the first defiance problem professedly aimed at the English. Newton solved it the same evening on which it was delivered to him, although he was much fatigued by the day's work at the mint. His solution, as published, was a general plan of an investigation rather than an actual solution, and was, on that account, criticised by Johann Bernoulli as being of no value. Brook Taylor undertook the defence of it, but ended by using very reprehensible language. Johann Bernoulli was not to be outdone in incivility, and made a bitter reply. Not long afterwards Taylor sent an open defiance to Continental mathematicians of a problem on the integration of a fluxion of complicated form which was known to very few geometers in England and supposed to be beyond the power of their adversaries. The selection was injudicious, for Johann Bernoulli had long before explained the method of this and similar integrations. It served only to display the skill and augment the triumph of the followers of Leibniz. The last and most unskilful challenge was by John Keill. The problem was to find the path of a projectile in a medium which resists proportionally to the square of the velocity. Without first making sure that he himself could solve it, Keill boldly challenged Johann Bernoulli to produce a solution. The latter resolved the question in very short time, not only for a resistance proportional to the square, but to any power of the velocity. Suspecting the weakness of the adversary, he repeatedly offered to send his solution to a confidential person in London, provided Keill would do the same. Keill never made a reply, and Johann Bernoulli abused him and cruelly exulted over him.[1]

The explanations of the fundamental principles of the calculus, as given by Newton and Leibniz, lacked clearness and rigor. For that reason it met with opposition from several quarters. In 1694 *Bernhard Nieuwentijt* (1654–1718) of Holland denied the existence of differentials of higher orders and objected to the practice of neglecting infinitely small quantities. These objections Leibniz was not able to meet satisfactorily. In his reply he said the value of $\frac{dy}{dx}$ in geometry could be expressed as the ratio of finite quantities. In the interpretation of dx and dy Leibniz vacillated.[2] At one time they appear in his writings as finite lines; then they are called infinitely small quantities, and again, *quantitates inassignabiles*, which spring from *quantitates assignabiles* by the law of continuity. In this last presentation Leibniz approached nearest to Newton.

[1] John Playfair, "Progress of the Mathematical and Physical Sciences" in *Encyclopædia Britannica*, 7th Ed., continued in the 8th Ed. by Sir John Leslie.

[2] Consult G. Vivanti, *Il concetto d'Infinitesimo. Saggio storico.* Nuova edizione. Napoli, 1901.

In England the principles of fluxions were boldly attacked by Bishop **George Berkeley** (1685–1753), the eminent metaphysician, in a publication called the *Analyst* (1734). He argued with great acuteness, contending, among other things, that the fundamental idea of supposing a finite ratio to exist between terms absolutely evanescent—"the ghosts of departed quantities," as he called them—was absurd and unintelligible. Berkeley claimed that the second and third fluxions were even more mysterious than the first fluxion. His contention that no geometrical quantity can be exhausted by division is in consonance with the claim made by Zeno in his "dichotomy," and the claim that the actual infinite cannot be realized. Most modern readers recognize these contentions as untenable. Berkeley declared as axiomatic a lemma involving the shifting of the hypothesis: If x receives an increment i, where i is expressly supposed to be some quantity, then the increment of x^n, divided by i, is found to be $nx^{n-1} + n(n-1)/2\; x^{n-2}i + \ldots$ If now you take $i=0$, *the hypothesis is shifted* and there is a manifest sophism in retaining any result that was obtained on the supposition that i is not zero. Berkeley's lemma found no favor among English mathematicians until 1803 when Robert Woodhouse openly accepted it. The fact that correct results are obtained in the differential calculus by incorrect reasoning is explained by Berkeley on the theory of "a compensation of errors." This theory was later advanced also by Lagrange and L. N. M. Carnot. The publication of Berkeley's *Analyst* was the most spectacular mathematical event of the eighteenth century in England. Practically all British discussions of fluxional concepts of that time involve issues raised by Berkeley. Berkeley's object in writing the *Analyst* was to show that the principles of fluxions are no clearer than those of Christianity. He referred to an "infidel mathematician" (Edmund Halley), of whom the story is told[1] that, when he jested concerning theological questions, he was repulsed by Newton with the remark, "I have studied these things; you have not." A friend of Berkeley, when on a bed of sickness, refused spiritual consolation, because the great mathematician Halley had convinced him of the inconceivability of the doctrines of Christianity. This induced Berkeley to write the *Analyst*.

Replies to the *Analyst* were published by *James Jurin* (1684–1750) of Trinity College, Cambridge under the pseudonym of "Philalethes Cantabrigiensis" and by *John Walton* of Dublin. There followed several rejoinders. Jurin's defence of Newton's fluxions did not meet the approval of the mathematician, **Benjamin Robins** (1707–1751). In a Journal, called the *Republick of Letters* (London) and later in the *Works of the Learned*, a long and acrimonious controversy was carried on between Jurin and Robins, and later between Jurin and Henry Pemberton (1694–1771), the editor of the third edition of

[1] Mach *Mechanics*, 1907, pp. 448–449.