Sir Isaac Newton (1642 - 1727)

From `A Short Account of the History of Mathematics' (4th edition, 1908) by W. W. Rouse Ball.

The mathematicians considered in the last chapter commenced the creation of those processes which distinguish modern mathematics. The extraordinary abilities of Newton enabled him within a few years to perfect the more elementary of those processes, and to distinctly advance every branch of mathematical science then studied, as well as to create some new subjects. Newton was the contemporary and friend of Wallis, Huygens, and others of those mentioned in the last chapter, but though most of his mathematical work was done between the years 1665 and 1686, the bulk of it was not printed - at any rate in book-form - till some years later.

I propose to discuss the works of Newton more fully than those of other mathematicians, partly because of the intrinsic importance of his discoveries, and partly because this book is mainly intended for English readers, and the development of mathematics in Great Britain was for a century entirely in the hands of the Newtonian school.

Isaac Newton was born in Lincolnshire, near Grantham, on December 25, 1642, and died at Kensington, London, on March 20, 1727. He was educated at Trinity College, Cambridge, and lived there from 1661 till 1696, during which time he produced the bulk of his work in mathematics; in 1696 he was appointed to a valuable Government office, and moved to London, where he resided till his death.

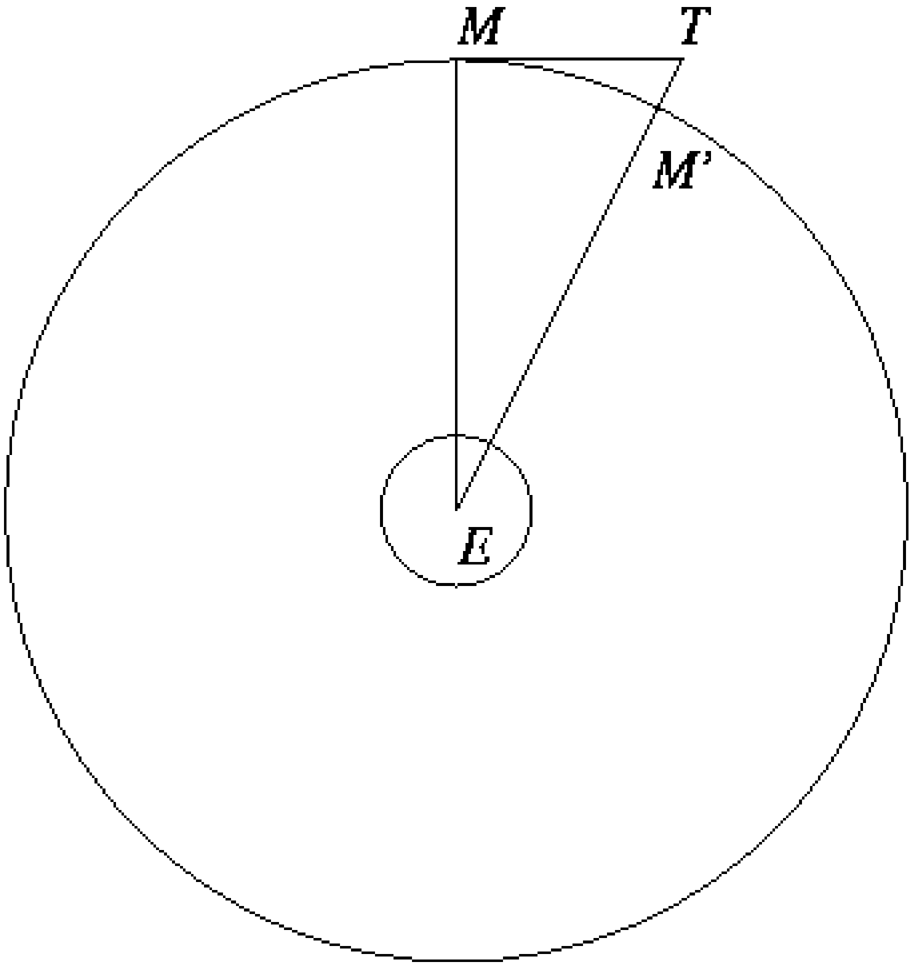
His father, who had died shortly before Newton was born, was a yeoman farmer, and it was intended that Newton should carry on the paternal farm. He was sent to school at Grantham, where his learning and mechanical proficiency excited some attention. In 1656 he returned home to learn the business of a farmer, but spent most of his time solving problems, making experiments, or devising mechanical models; his mother noticing this, sensibly resolved to find some more congenial occupation for him, and his uncle, having been himself educated at Trinity College, Cambridge, recommended that he should be sent there.

In 1661 Newton accordingly entered as a student at Cambridge, where for the first time he found himself among surroundings which were likely to develop his powers. He seems, however, to have had but little interest for general society or for any pursuits save science and mathematics. Luckily he kept a diary, and we can thus form a fair idea of the course of education of the most advanced students at an English university at that time. He had not read any mathematics before coming into residence, but was acquainted with Sanderson's *Logic*, which was then frequently read as preliminary to mathematics. At the beginning of his first October term he happened to stroll down to Stourbridge Fair, and there picked up a book on astrology, but could not understand it on account of the geometry and trigonometry. He therefore bought a Euclid, and was surprised to find how obvious the propositions seemed. He thereupon read Oughtred's *Clavis* and Descartes's *Géométrie*, the latter of which he managed to master by himself, though with some difficulty. The interest he felt in the subject led him to take up mathematics rather than chemistry as a serious study. His subsequent mathematical reading as an undergraduate was founded on Kepler's *Optics*, the works of Vieta, van Schooten's *Miscellanies*, Descartes's *Géométrie*, and Wallis's *Arithmetica Infinitorum*: he also attended Barrow's lectures. At a later time, on reading Euclid more carefully, he formed a high opinion of it as an instrument of education, and he used to express his regret that he had not applied himself to geometry before proceeding to algebraic analysis.

There is a manuscript of his, dated May 28, 1665, written in the same year as that in which he took is B. A. degree, which is the earliest documentary proof of his invention of fluxions. It was about the same time that he discovered the binomial theorem.

On account of the plague the College was sent down during parts of the year 1665 and 1666, and for several months at this time Newton lived at home. This period was crowded with brilliant discoveries. He thought out the fundamental principles of his theory of gravitation, namely, that every particle of matter attracts every other particle, and he suspected that the attraction varied as the product of their masses and inversely as the square of the distance between them. He also worked out the fluxional calculus tolerably completely: this in a manuscript dated November 13, 1665, he used fluxions to find the tangent and the radius of curvature at any point on a curve, and in October 1666 he applied them to several problems in the theory of equations. Newton communicated these results to his friends and pupils from and after 1669, but they were not published in print till many years later. It was also whilst staying at home at this time that he devised some instruments for grinding lenses to particular forms other than spherical, and perhaps he decomposed solar light into different colours.

Leaving out details and taking round numbers only, his reasoning at this time on the theory of gravitation seems to have been as follows. He suspected that the force which retained the moon in its orbit about the earth was the same as terrestial gravity, and to verify this hypothesis he proceeded thus. He knew that, if a stone were allowed to fall near the surface of the earth, the attraction of the earth (that is, the weight of the stone) caused it to move through 16 feet in one second. The moon's orbit relative to the earth is nearly a circle; and as a rough approximation, taking it to be so, he knew the distance of the moon, and therefore the length of its path; he also knew that time the moon took to go once round it, namely, a month.



Hence he could easily find its velocity at any point such as *M*. He could therefore find the distance *MT* through which it would move in the next second if it were not pulled by the earth's attraction. At the end of that second it was however at *M*', and therefore the earth *E* must have pulled it through the distance *TM*' in one second (assuming the direction of the earth's pull to be constant). Now he and several physicists of the time had conjectured from Kepler's third law that the attraction of the earth on a body would be found to decrease as the body was removed farther away from the earth inversely as the square of the distance from the centre of the earth; if this were the actual law, and if gravity were the sole force which retained the moon in its orbit, then *TM*' should be to 16 feet inversely as the square of the distance of the moon from the centre of the earth to the square of the radius of the earth. In 1679, when he repeated the investigation, *TM*' was found to have the value which was required by the hypothesis, and the verification was complete; but in 1666 his estimate of the distance of the moon was inaccurate, and when he made the calculation he found that *TM*' was about one-eighth less than it ought to have been on his hypothesis.

This discrepancy does not seem to have shaken his faith in the belief that gravity extended as far as the moon and varied inversely as the square of the distance; but from Whiston's notes of a conversation with Newton, it would seem that Newton inferred that some other force - probably Descartes's vortices - acted on the moon as well as gravity. This statement is confirmed by Pemberton's account of the investigation. It seems, moreover, that Newton already believed firmly in the principle of universal gravitation, that is, that every particle of matter attracts every other particle, and suspected that the attraction varied as the product of their masses and inversely as the square of the distance between them; but it is certain that he did not then know what the attraction of a spherical mass on any external point would be, and did not think it likely that a particle would be attracted by the earth as if the latter were concentrated into a single particle at its centre.

On his return to Cambridge in 1667 Newton was elected to a fellowship at his college, and permanently took up his residence there. In the early part of 1669, or perhaps in 1668, he revised Barrow's lectures for him. The end of the fourteenth lecture is known to have been written by Newton, but how much of the rest is due to his suggestions cannot now be determined. As soon as this was finished he was asked by Barrow and Collins to edit and add notes to a translation of Kinckhuysen's *Algebra*; he consented to do this, but on condition that his name should not appear in the matter. In 1670 he also began a systematic exposition of his analysis by infinite series, the object of which was to express the ordinate of a curve in an infinite algebraical series every term of which can be integrated by Wallis's rule; his results on this subject had been communicated to Barrow, Collins, and others in 1669. This was never finished: the fragment was published in 1711, but the substance of it had been printed as an appendix to the *Optics* in 1704. These works were only the fruit of Newton's leisure, most of his time during these two years being given up to optical researches.

In October 1669, Barrow resigned the Lucasian chair in favour of Newton. During his tenure of the professorship, it was Newton's practice to lecture publicly once a week, for from half-an-hour to an hour at a time, in one term of each year, probably dictating his lectures as rapidly as they could be taken down; and in the week following the lecture to devote four hours to appointments which he gave to students who wished to come to his rooms to discuss the results of the previous lecture. He never repeated a course, which usually consisted of nine or ten lectures, and generally the lectures of one course began from the point at which the preceding course had ended. The manuscripts of his lectures for seventeen out of the first eighteen years of his tenure are extant.

When first appointed Newton chose optics for the subject of his lectures and researches, and before the end of 1669 he had worked out the details of his discovery of the decomposition of a ray of white light into rays of different colours by means of a prism. The complete explanation of the theory of the rainbow followed from this discovery. These discoveries formed the subject-matter of the lectures which he delivered as Lucasian professor in the years 1669, 1670 and 1671. The chief new results were embodied in a paper communicated to the Royal Society in February, 1672, and subsequently published in the *Philosophical Transactions*. The manuscript of his original lectures was printed in 1729 under the title *Lectiones Opticae*. This work is divided into two books, the first of which contains four sections and the second five. The first section of the first book deals with the decomposition of solar light by a prism in consequence of the unequal refrangibility of the rays that compose it, and a description of his experiments is added. The second section contains an account of the method which Newton invented for determining the coefficients of refraction of different bodies. This is done by making a ray pass through a prism of the material so that the deviation is a minimum; and he proves that, if the angle of the prism be i and the deviation of the ray be δ, the refractive index will be sin ½ $(i + \delta)$ cosec ½ i. The third section is on refractions at plane surfaces; he here shews that if a ray pass through a prism with minimum deviation, the angle of incidence is equal to the angle of emergence; most of this section is devoted to geometrical solutions of different problems. The fourth section contains a discussion of refractions at curved surfaces. The second book treats of his theory of colours and of the rainbow.

By a curious chapter of accidents Newton failed to correct the chromatic aberration of two colours by means of a couple of prisms. He therefore abandoned the hope of making a refracting telescope which should be achromatic, and instead designed a reflecting telescope, probably on the modal of a small one which he had made in 1668. The form he used is that still known by his name; the idea of it was naturally suggested by Gregory's telescope. In 1672 he invented a reflecting microscope, and some years later he invented the sextant which was rediscovered by J. Hadley in 1731.

His professorial lectures from 1673 to 1683 were on algebra and the theory of equations, and are described below; but much of his time during these years was occupied with other investigations, and I may remark that throughout his life Newton must have devoted at least as much attention to chemistry and theology as to mathematics, though his conclusions are not of sufficient interest to require mention here. His theory of colours and his deductions from his optical experiments were at first attacked with considerable vehemence. The correspondence which this entailed on Newton occupied nearly all his leisure in the years 1672 to 1675, and proved extremely distasteful to him. Writing on December 9, 1675, he says, ``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.'' Again, on November 18, 1676, he observes, ``I see I have made myself a slave to philosophy; but if I get rid of Mr. Linus's business, I will resolutely bid adieu to it eternally, excepting what I do for my private satisfaction, or leave to come out after me; for I see a man must either resolve to put out nothing new, or to become a slave to defend it.'' The unreasonable dislike to have his conclusions doubted or to be involved in any correspondence about them was a prominent trait in Newton's character.

Newton was deeply interested in the question as to how the effects of light were really produced, and by the end of 1675 he had worked out the corpuscular or emission theory, and had shewn how it would account for all the various phenomena of geometrical optics, such as reflexion, refraction, colours, diffraction, etc. To do this, however, he was obliged to add a somewhat artificial rider, that his corpuscules had alternating fits of easy reflexion and easy refraction communicated to them by an ether which filled space. The theory is now known to be untenable, but it should be noted that Newton enunciated it as a hypothesis from which certain results would follow: it would seem that he believed that wave theory to be intrinsically more probable, but it was the difficulty of explaining diffraction on that theory that led him to suggest another hypothesis.

Newton's corpuscular theory was expounded in memoirs communicated to the Royal Society in December 1675, which are substantially reproduced in his *Optics*, published in 1704. In the latter work he dealt in detail with his theory of fits of easy reflexion and transmission, and the colours of thin plates, to which he added an explanation of the colours of thick plates [bk. II, part 4] and observations on the inflexion of light [bk. III].

Two letters written by Newton in the year 1676 are sufficiently interesting to justify an allusion to them.

Leibnitz, who had been in London in 1673, had communicated some results to the Royal Society which he had supposed to be new, but which it was pointed out to him had been previously proved by Mouton. This led to a correspondence with Oldenburg, the secretary of the Society. In 1674 Leibnitz wrote saying that he possessed ``general analytical methods depending on infinite series." Oldenburg, in reply, told him that Newton and Gregory had used such series in their work. In answer to a request for information, Newton wrote on June 13, 1676, giving a brief account of his method, but adding the expansions of a binomial (that is, the binomial theorem) and of $\sin^{-1} x$; from the latter of which he deduced that of sin *x*: this seems to be the earliest known instance of a reversion of series. He also inserted an expression for the rectification of an elliptic arc in an infinite series.

Leibnitz wrote on August 27 asking for fuller details; and Newton in a long but interesting replay, dated October 34, 1676, and sent through Oldenburg, gives an account of the way in which he had been led to some of his results.

In this letter Newton begins by saying that altogether he had used three methods for expansion in series. His first was arrived at from the study of the method of interpolation by which Wallis had found expressions for the area of a circle and a hyperbola. Thus, by considering the series of expressions $(1-x^2)^{\frac{0}{2}}$, $(1-x^2)^{\frac{2}{2}}$, $(1-x^2)^{\frac{4}{2}}$,..., he deduced by interpolations the law which connects the successive coefficients in the expansions of $(1-x^2)^{\frac{1}{2}}$, $(1-x^2)^{\frac{3}{2}}$,...; and then by analogy obtained the expression for the general term in the expansion of a binomial, that is, the binomial theorem. He says that he proceeded to test this by forming the square of the expansion of $(1-x^2)^{\frac{1}{2}}$, which reduced to 1 - x^2; and he proceeded in a similar way with other expansions. He next tested the theorem in the case of $(1-x^2)^{\frac{1}{2}}$ by extracting the square root of 1 - x^2, *more arithmetico*. He also used the series to determine the areas of the circle and the hyperbola in infinite series, and he found that the results were the same as those he had arrived at by other means.

Having established this result, he then discarded the method of interpolation in series, and employed his binomial theorem to express (when possible) the ordinate of a curve in an infinite series in ascending powers of the abscissa, and thus by Wallis's method he obtained expressions in infinite series for the areas and arcs of curves in the manner described in the appendix to his *Optics* and in his *De Analysi per Equationes Numero Terminorum Infinitas*. He states that he had employed this second method before the plague in 1665-66, and goes on to say that he was then obliged to leave Cambridge, and subsequently (presumably on his return to Cambridge) he ceased to pursue these ideas, as he found that Nicholas Mercator had employed some of them in his *Logarithmo-technica*, published in 1668; and he supposed that the remainder had been or would be found out before he himself was likely to publish his discoveries.

Newton next explains that he had also a third method, of which (he says) he had about 1669 sent an account to Barrow and Collins, illustrated by applications to areas, rectification, cubature, etc. This was the method of fluxions; but Newton gives no description of it here, though he adds some illustrations of its use. The first illustration is on the quadrature of the curve represented by the equation

$$y = ax^m(b + cx^n)^p,$$

which he says can be effected as a sum of $(m + 1)/n$ terms if $(m + 1)/n$ be a positive integer, and which he thinks cannot otherwise be effected except by an infinite series. [This is not so, the integration is possible if $p + (m + 1)/n$ be an integer.] He also gives a list of other forms which are immediately integrable, of which the chief are

$$\frac{x^{mn-1}}{a + bx^n + cx^{2n}},$$

$$\frac{x^{(m+1/2)n-1}}{a + bx^n + cx^{2n}},$$

$$x^{mn-1}(a + bx^n + cx^{2n})^{\pm 1/2},$$

$$x^{mn-1}(a + bx^n)^{\pm 1/2}(c + dx^n)^{-1},$$

$$x^{mn-n-1}(a + bx^n)(c + dx^n)^{-1/2};$$

where m is a positive integer and n is any number whatever. Lastly, he points out that the area of any curve can be easily determined approximately by the method of interpolation described below in discussing his *Methodus Differentialis*.

At the end of his letter Newton alludes to the solution of the ``inverse problem of tangents,'' a subject on which Leibnitz had asked for information. He gives formulae for reversing any series, but says that besides these formulae he has two methods for solving such questions, which for the present he will not describe except by an anagram which, being read, is as follows, ``Una methodus consistit in extractione fluentis quantitatis ex aequatione simul involvente fluxionem ejus: altera tantum in assumptione seriei pro quantitate qualibet incognita ex qua caetera commode derivari possunt, et in collatione terminorum homologorum aequationis resultantis, as eruendos terminos assumptae seriei.''

He implies in this letter that he is worried by the questions he is asked and the controversies raised about every new matter which he produces, which shew his rashness in publishing ``quod umbram captando eatenus perdideram quietem meam, rem prorsus substantialem.''

Leibnitz, in his answer, dated June 21, 1677, explains his method of drawing tangents to curves, which he says proceeds ``not by fluxions of lines, but by the differences of numbers''; and he introduces his notation of dx and dy for the infinitesimal differences between the co-ordinates of two consecutive points on a curve. He also gives a solution of the problem to find a curve whose subtangent is constant, which shews that he could integrate.

In 1679 Hooke, at the request of the Royal Society, wrote to Newton expressing a hope that he would make further communications to the Society, and informing him of various facts then recently discovered. Newton replied saying that he had abandoned the study of philosophy, but he added that the earth's diurnal motion might be proved by the experiment of observing the deviation from the perpendicular of a stone dropped from a height to the ground - an experiment which was subsequently made by the Society and succeeded. Hooke in his letter mentioned Picard's geodetical researches; in these Picard used a value of the radius of the earth which is substantially correct. This led Newton to repeat, with Picard's data, his calculations of 1666 on the lunar orbit, and he thus verified his supposition that gravity extended as far as the moon and varied inversely as the square of the distance. He then proceeded to consider the general theory of motion of a particle under a centripetal force, that is, one directed to a fixed point, and showed that the vector would sweep over equal areas in equal times. He also proved that, if a particle describe an ellipse under a centripetal force to a focus, the law must be that of the inverse square of the distance from the focus, and conversely, that the orbit of a particle projected under the influence of such a force would be a conic (or, it may be, he thought only an ellipse). Obeying his rule to publish nothing that could land hum in a scientific controversy these results were locked up in his notebooks, and it was only a specific question addressed to him five years later that led to their publication.

The *Universal Arithmetic*, which is on algebra, theory of equations, and miscellaneous problems, contains the substance of Newton's lectures during the years 1673 to 1683. His manuscript of it is still extant; Whiston extracted a somewhat reluctant permission from Newton to print it, and it was published in 1707. Amongst several new theorems on various points in algebra and the theory of equations Newton here enunciates the following important results. He explains that the equation whose roots are the solution of a given problem will have as many roots as there are different possible cases; and he considers how it happens that the equation to which a problem leads may contain roots which do not satisfy the original question. He extends Descartes's rule of signs to give limits to the number of imaginary roots. He uses the principle of continuity to explain how two real and unequal roots may become imaginary in passing through equality, and illustrates this by geometrical considerations; thence he shews that imaginary roots must occur in pairs. Newton also here gives rules to find a superior limit to the positive roots of a numerical equation, and to determine the approximate values of the numerical roots. He further enunciates the theorem known by his name for finding the sum of the *n*th powers of the roots of an equation, and laid the foundation of the theory of symmetrical functions of the roots of an equation.

The most interesting theorem contained in the work is his attempt to find a rule (analogous to that of Descartes for real roots) by which the number of imaginary roots of an equation can be determined. He knew that the result which he obtained was not universally true, but he gave no proof and did not explain what were the exceptions to the rule. His theorem is as follows. Suppose the equation to be of the *n*th degree arranged in descending powers of x (the coefficient of x^n being positive), and suppose the $n + 1$ fractions

$$1,\quad \frac{n}{n-1}\frac{2}{1},\quad \frac{n-1}{n-2}\frac{3}{2},\ldots,\frac{n-p+1}{n-p}\frac{p+1}{p},\ldots,\frac{2}{1}\frac{n}{n-1},1$$

to be formed and written below the corresponding terms of the equation, then, if the square of any term when multiplied by the corresponding fraction be greater than the product of the terms on each side of it, put a plus sign above it: otherwise put a minus sign above it, and put a plus sign above the first and last terms. Now consider any two consecutive terms in the original equation, and the two symbols written above them. Then we may have any one of the four following cases: (α) the terms of the same sign and the symbols of the same sign; (β) the terms of the same sign and the symbols of opposite signs; (γ) the terms of opposite signs and the symbols of the same sign; (δ) the terms of opposite signs and the symbols of opposite signs. Then it has been shewn that the number of negative roots will not exceed the number of cases (α), and the number of positive roots will not exceed the number of cases (γ); and therefore the number of imaginary roots is not less than the number of cases (β) and (δ). In other words the number of changes of signs in the row of symbols written above the equation is an inferior limit to the number of imaginary roots. Newton, however, asserted that ``you may almost know how many roots are impossible'' by counting the changes of sign in the series of symbols formed as above. That is to say, he thought that in general the actual number of positive, negative and imaginary roots could be got by the rule and not merely superior or inferior limits to these numbers. But though he knew that the rule was not universal he could not find (or at any rate did not state) what were the exceptions to it: this problem was subsequently discussed by Campbell, Maclaurin, Euler, and other writers; at last in 1865 Sylvester succeeded in proving the general result.

In August, 1684, Halley came to Cambridge in order to consult Newton about the law of gravitation. Hooke, Huygens, Halley, and Wren had all conjectured that the force of the attraction of the sun or earth on an external particle varied inversely as the square of the distance. These writers seem independently to have shewn that, if Kepler's conclusions were rigorously true, as to which they were not quite certain, the law of attraction must be that of the inverse square. Probably their argument was as follows. If v be the velocity of a planet, r the radius of its orbit taken as a circle, and T its periodic time, $v = 2\pi r/T$. But, if f be the acceleration to the centre of the circle, we have $f = 4\pi^2 r/T^2$ Now, by Kepler's third law, T^2 varies as r^3; hence f varies inversely as r^2. They could not, however, deduce from the law the orbits of the planets. Halley explained that their investigations were stopped by their inability to solve this problem, and asked Newton if he could find out what the orbit of a planet would be if the law of attraction were that of the inverse square. Newton immediately replied that it was an ellipse, and promised to send or write out afresh the demonstration of it which he had found in 1679. This was sent in November, 1684.

Instigated by Halley, Newton now returned to the problem of gravitation; and before the autumn of 1684, he had worked out the substance of propositions 1--19, 21, 30, 32--35 in the first book of the *Principia*. These together with notes on the laws of motion and various lemmas, were read for his lectures in the Michaelmas Term, 1684.

In November Halley received Newton's promised communication, which probably consisted of the substance of propositions 1, 11 and either proposition 17 or the first corollary of proposition 13; thereupon Halley again went to Cambridge, where he saw ``a curious treatise, *De Motu*, drawn up since August.'' Most likely this contained Newton's manuscript notes of the lectures above alluded to: these notes are now in the university library and are headed ``*De Motu Corporum*.'' Halley begged that the results might be published, and finally secured a promise that they should be sent to the Royal Society: they were accordingly communicated to the Society not later than February, 1685, in the paper *De Motu*, which contains the substance of the following propositions in the *Principia*, book I, props. 1, 4, 6, 7, 10, 11, 15, 17, 32; book II, props. 2,3,4.

It seems also to have been due to the influence and tact of Halley at his visit in November, 1684, that Newton undertook to attack the whole problem of gravitation, and practically pledged himself to publish his results: these are contained in the *Principia*. As yet Newton had not determined the attraction of a spherical body on an external point, nor had he calculated the details of the planetary motions even if the members of the solar system could be regarded as points. The first problem was solved in 1685, probably either in January or in February. ``No sooner,'' to quote from Dr. Glaisher's address on the bicentenary of the publication of the *Principia*, ``had Newton proved this superb theorem - and we know from his own words that he had no expectation of so beautiful a result till it emerged from his mathematical investigation - than all the mechanism of the universe at once lay spread before him. When he discovered the theorems that form the first three sections of book I, when he gave them in his lectures of 1684, he was unaware that the sun and earth exerted their attractions as if they were but points. How different must these propositions have seemed to Newton's eyes when he realised that these results, which he had believed to be only approximately true when applied to the solar system, were really exact! Hitherto they had been true only in so far as he could regard the sun as a point compared to the distance of the planets, or the earth as a point compared to the distance of the moon - a distance amounting to only about sixty times the earth's radius - but now they were mathematically true, excepting only for the slight deviation from a perfectly spherical form of the sun, earth and planets. We can imagine the effect of this sudden transition from approximation to exactitude in stimulating Newton's mind to still greater efforts. It was now in his power to apply mathematical analysis with absolute precision to the actual problems of astronomy.''

Of the three fundamental principles applied in the *Principia* we may say that the idea that every particle attracts every other other particle in the universe was formed at least as early as 1666; the law of equable description of areas, its consequences, and the fact that if the law of attraction were that of the inverse square the orbit of a particle about a centre of force would be a conic were proved in 1679; and, lastly, the discovery that a sphere, whose density at any point depends only on the distance from the centre, attracts an external point as if the whole mass were collected at its centre was made in 1685. It was this last discovery that enabled him to apply the first two principles to the phenomena of bodies of finite size.

The draft of the first book of the *Principia* was finished before the summer of 1685, but the corrections and additions took some time, and the book was not presented to the Royal Society until April 28, 1686. This book is given up to the consideration of the motion of particles or bodies in free space either in known orbits, or under the action of known forces, or under their mutual attraction; and in particular to indicating how the effects of disturbing forces may be calculated. In it also Newton generalizes the law of attraction into a statement that every particle of matter in the universe attracts every other particle with a force which varies directly as the product of their masses, and inversely as the square of the distance between them; and he thence deduces the law of attraction for spherical shells of constant density. The book is prefaced by an introduction on the science of dynamics, which defines the limits of mathematical investigation. His object, he says, is to apply mathematics to the phenomena of nature; among these phenomena motion is one of the most important; now motion is the effect of force, and, though he does not know what is the nature or origin of force, still many of its effects can be measured; and it is these that form the subject-matter of the work.

The second book of the *Principia* was completed by the summer of 1686. This book treats of motion in a resisting medium, and of hydrostatics and hydrodynamics, with special applications to waves, tides, and acoustics. He concludes it by shewing that the Cartesian theory of vortices was inconsistent both with the known facts and with the laws of motion.

The next nine or ten months were devoted to the third book. Probably for this originally he had no materials ready. He commences by discussing when and how far it is justifiable to construct hypotheses or theories to account for known phenomena. He proceeds to apply the theorems obtained in the first book to the chief phenomena of the solar system, and to determine the masses and distances of the planets and (whenever sufficient data existed) of their satellites. In particular the motion of the moon, the various inequalities therein, and the theory of the tides are worked out in detail. He also investigates the theory of comets, shews that they belong to the solar system, explains how from three observations the orbit can be determined, and illustrates his results by considering certain special comets. The third book as we have it is but little more than a sketch of what Newton had finally proposed to himself to accomplish; his original scheme is among the ``Portsmouth papers,'' and his notes shew that he continued to work at it for some years after the publication of the first edition of the *Principia*: the most interesting of his memoranda are those in which by means of fluxions he has carried his results beyond the point at which he was able to translate them into geometry.

The demonstrations throughout the book are geometrical, but to readers of ordinary ability are rendered unnecessarily difficult by the absence of illustrations and explanations, and by the fact that no clue is given to the method by which Newton arrived at his results. The reason why it was presented in a geometrical form appears to have been that the infinitesimal calculus was then unknown, and, had Newton used it to demonstrate results which were in themselves opposed to the prevalent philosophy of the time, the controversy as to the truth of his results would have been hampered by a dispute concerning the validity of the methods used in proving them. He therefore cast the whole reasoning into a geometrical shape which, if somewhat longer, can at any rate be made intelligible to all mathematical students. So closely did he follow the lines of Greek geometry that he constantly used graphical methods, and represented forces, velocities, and other magnitudes in the Euclidean way by straight lines (*ex. gr*. book I, lemma 10), and not by a certain number of units. The latter and modern method had been introduced by Wallis, and must have been familiar to Newton. The effect of his confining himself rigorously to classical geometry is that the *Principia* is written in a lnaguage which is archaic, even if not unfamiliar.

The adoption of geometrical methods in the *Principia* for purposes of demonstration does not indicate a preference on Newton's part for geometry over analysis as an instrument of research, for it is known now that Newton used the fluxional calculus in the first instance in finding some of the theorems, especially those towards the end of book I and in book II; and in fact one of the most important uses of that calculus is stated in book II, lemma 2. But it is only just to remark that, at the time of its publication and for nearly a century afterwards, the differential and fluxional calculus were not fully developed, and did not possess the same superiority over the method he adopted which they do now; and it is a matter for astonishment that when Newton did employ the calculus he was able to use it to so good an effect.

The printing of the work was slow, and it was not finally published till the summer of 1687. The cost was borne by Halley, who also corrected the proofs, and even put his own researches on one side to press the printing forward. The conciseness, absence of illustrations, and synthetical character of the book restricted the numbers of those who were able to appreciate its value; and though nearly all competent critics admitted the validity of the conclusions, some little time elapsed before it affected the current beliefs of educated men. I should be inclined to say (but on this point opinions differ widely) that within ten years of its publication it was generally accepted in Britain as giving a correct account of the laws of the universe; it was similarly accepted within about twenty years on the continent, except in France, where the Cartesian hypothesis held its ground until Voltaire in 1738 took up the advocacy of the Newtonian theory.

The manuscript of the *Principia* was finished by 1686. Newton devoted the remainder of that year to his paper on physical optics, the greater part of which is given up to the subject of diffraction.

In 1687 James II, having tried to force the university to admit as a master of arts a Roman Catholic priest who refused to take the oaths of supremacy and allegiance, Newton took a prominent part in resisting the illegal interference of the king, and was one of the deputation sent to London to protect the rights of the university. The active part taken by Newton in this affair led to his being in 1689 elected member for the university. This parliament only lasted thirteen months, and on its dissolution he gave up his seat. He was subsequently returned in 1701, but he never took any prominent part in politics.

On his coming back to Cambridge in 1690 he resumed his mathematical studies and correspondence, but probably did not lecture. The two letters to Wallis, in which he explained his method of fluxions and fluents, were written in 1692 and published in 1693. Towards the close of 1692 and throughout the following two years, Newton had a long illness, suffering from insomnia and general nervous irritability. Perhaps he never quite regained his elasticity of mind, and, though after his recovery he shewed the same power in solving any question propounded to him, he ceased thenceforward to do original work on his own initiative, and it was somewhat difficult to stir him to activity in new subjects.

In 1694 Newton began to collect data connected with the irregularities of the moon's motion with the view of revising the part of the *Principia* which dealt with that subject. To render the observations more accurate, he forwarded to Flamsteed a table of corrections for refraction which he had previously made.

This was not published till 1721, when Halley communicated it to the Royal Society. The original calculations of Newton and the papers connected with them are in the Portsmouth collection, and shew that Newton obtained it by finding the path of a ray, by means of quadratures, in a manner equivalent to the solution of a differential equation. As an illustration of Newton's genius, I may mention that even as late as 1754 Euler failed to solve the same problem. In 1782 Laplace gave a rule for constructing such a table, and his results agree substantially with those of Newton.

I do not suppose that Newton would in any case have produced much more original work after his illness; but his appointment in 1696 as warden, and his promotion in 1699 to the mastership of the Mint, at a salary of £ 1500 a year, brought his scientific investigations to an end, though it was only after this that many of his previous investigations were published in the form of books. In 1696 he moved to London, in 1701 he resigned the Lucasian chair, and in 1703 he was elected president of the Royal Society.

In 1704 Newton published his *Optics*, which contains the results of the papers already mentioned. To the first edition of this book were appended two minor works which have no special connection with optics; one being on cubic curves, the other on the quadrature of curves and on fluxions. Both of them were manuscripts with which his friends and pupils were familiar, but they were here published *urbi et orbi* for the first time.

The first of these appendices is entitled *Enumeratio Linearum Tertii Ordinis*; the object seems to be to illustrate the use of analytical geometry, and as the application to conics was well known, Newton selected the theory of cubics.

He begins with some general theorems, and classifies curves according as their equations are algebraical or transcendental; the former being cut by a straight line in a number of points (real or imaginary) equal to the degree of the curve, the latter being cut by a straight line in an infinite number of points. Newton then shews that many of the most important properties of conics have their analogues in the theory of cubics, and he discusses the theory of asymptotes and curvilinear diameters.

After these general theorems, he commences his detailed examination of cubics by pointing out that a cubic must have at least one real point at infinity. If the asymptote or tangent at this point be a finite distance, it may be taken for the axis of *y*. This asymptote will cut the curve in three points altogether, of which at least two are at infinity. If the third point be at a finite distance, then (by one of his general theorems on asymptotes) the equation can be written in the form

$$xy^2 + hy = ax^3 + bx^2 + cx + d,$$

where the axes of *x* and *y* are the asymptotes of the hyperbola which is the locus of the middle points of all chords drawn parallel to the axis of *y*; while, if the third point in which this asymptote cuts the curve be also at infinity, the equation can be written in the form

$$xy = ax^3 + bx^2 + cx + d.$$

Next he takes the case where the tangent at the real point at infinity is not at a finite distance. A line parallel to that direction in which the curve goes to infinity may be taken as the axis of *y*. Any such line will cut the curve in three points altogether, of which one is by hypothesis at infinity, and one is necessarily at a finite distance. He then shews that if the remaining point in which this line cuts the curve be at a finite distance, the equation can be written in the form

$$y^2 = ax^3 + bx^2 + cx + d;$$

while if it be at an infinite distance, the equation can be written in the form

$$y = ax^3 + bx^2 + cx + d.$$

Any cubic is therefore reducible to one of four characteristic forms. Each of these forms is then discussed in detail, and the possibility of the existence of double points, isolated ovals, etc., is worked out. The final result is that in all there are seventy-eight possible forms which a cubic may take. Of these Newton enumerated one seventy-two; four of the remainder were mentioned by Stirling in 1717, one by Nicole in 1731, and one by Nicholas Bernoulli about the same time.

In the course of the work Newton states the remarkable theorem that, just as the shadow of a circle (cast by a luminous point on a plane) gives rise to all the conics, so the shadows of the curves represented by the equation

$$y^2 = ax^3 + bx^2 + cx + d$$

give rise to all the cubics. This remained an unsolved puzzle until 1731, when Nicole and Clairaut gave demonstrations of it; a better proof is that given by Murdoch in 1740, which depends on the classification of these curves into five species according as to whether their points of intersection with the axis of *x* are real and unequal, real and two of them are equal (two cases), real and all equal, or two imaginary and one real.

In this tract Newton also discusses double points in the plane and at infinity, the description of curves satisfying given conditions, and the graphical solution of problems by the use of curves.

The second appendix to the *Optics* is entitled *De Quadratura Curvarum*. Most of it had been communicated to Barrow in 1668 or 1669, and probably was familiar to Newton's pupils and friends from that time onwards. It consists of two parts.

The bulk of the first part is a statement of Newton's method of effecting the quadrature and rectification of curves by means of infinite series; it is noticeable as containing the earliest use in print of literal indices, and also the first printed statement of the binomial theorem, but these novelties are introduced only incidentally. The main object is to give rules for developing a function of x in a series in ascending powers of x, so as to enable mathematicians to effect the quadrature of any curve in which the ordinate y can be expressed as an explicit algebraical function of the abscissa x. Wallis had shewn how this quadrature could be found when y was given as a sum of a number of multiples of powers of x, and Newton's rules of expansion here established rendered possible the similar quadrature of any curve whose ordinate can be expressed as the sum of an infinite number of such terms. In this way he effects the quadrature of the curves

$$y = \frac{a^2}{b+x},$$

$$y = (a^2 \pm x^2)^{\frac{1}{2}},$$

$$y = (x - x^2)^{\frac{1}{2}},$$

$$y = \left(\frac{1+ax^2}{1-bx^2}\right)^{\frac{1}{2}},$$

but naturally the results are expressed as infinite series. He then proceeds to curves whose ordinate is given as an implicit function of the abscissa; and he gives a method by which y can be expressed as an infinite series in ascending powers of x, but the application of the rule to any curve demands in general such complicated numerical calculations as to render it of little value. He concludes this part by shewing that the rectification of a curve can be effected in a somewhat similar way. His process is equivalent to finding the integral with regard to x of $(1 + \dot{y}^2)^{\frac{1}{2}}$ in the form of an infinite series. I should add that Newton indicates the importance of determining whether the series are convergent - an observation far in advance of his time - but he knew of no general test for the purpose; and in fact it was not until Gauss and Cauchy took up the question that the necessity of such limitations was commonly recognized.

The part of the appendix which I have just described is practically the same as Newton's manuscript *De Analysi per Equationes Numero Terminorum Infinitas*, which wa subsequently printed in 1711. It is said that this was originally intended to form an appendix to Kinckhuysen's *Algebra*, which, as I have already said, he at one time intended to edit. The substance of it was communicated to Barrow, and by him to Collins, in letters of July 31 and August 12, 1669; and a summary of it was included in the letter of October 24, 1676, sent to Leibnitz.

It should be read in connection with Newton's *Methodus Differentialis*, also published in 1711. Some additional theorems are there given, and he discusses his method of interpolation, which had been briefly described in the letter of October 24, 1676. The principle is this. If $y = \phi(x)$ be a function of x, and if, when x is successively put equal to $a_1, a_2, \ldots$, the values of y be known and be $b_1, b_2, \ldots$, then a parabola whose equation is $y = p + qx + rx^2 + \ldots$ can be drawn through the points $(a_1, b_1), (a_2, b_2), \ldots$, and the ordinate of this parabola may be taken as an approximation to the ordinate of the curve. The degree of the parabola will of course be one less than the number of given points. Newton points out that in this way the areas of any curves can be approximately determined.

The second part of this appendix to the *Optics* contains a description of Newton's method of fluxions. This is best considered in connection with Newton's manuscript on the same subject which was published by John Colson in 1736, and of which it is a summary.

The invention of the infinitesimal calculus was one of the great intellectual achievements of the seventeenth century. This method of analysis, expressed in the notation of fluxions and fluents, was used by Newton in or before 1666, but no account of it was published until 1693, though its general outline was known to his friends and pupils long anterior to that year, and no complete exposition of his methods was given before 1736.

The idea of a fluxion or differential coefficient, as treated at this time, is simple. When two quantities - e. g. the radius of a sphere and its volume - are so related that a change in one causes a change in the other, the one is said to be a function of the other. The ratio of the rates at which they change is termed the differential coefficient or fluxion of the one with regard to the other, and the process by which this ratio is determined is known as differentiation. Knowing the differential coefficient and one set of corresponding values of the two quantities, it is possible by summation to determine the relation between them, as Cavalieri and others had shewn; but often the process is difficult, if, however, we can reverse the process of differentiation we can obtain this result directly. This process of reversal is termed integration. It was at once seen that problems connected with the quadrature of curves, and the determination of volumes (which were soluble by summation, as had been shewn by the employment of indivisibles), were reducible to integration. In mechanics also, by integration, velocities could be deduced from known accelerations, and distances traversed from known velocities. In short, wherever things change according to known laws, here was a possible method of finding the relation between them. It is true that, when we try to express observed phenomena in the language of the calculus, we usually obtain an equation involving the variables, and their differential coefficients - and possibly the solution may be beyond our powers. Even so, the method is often fruitful, and its use marked a real advance in thought and power.

I proceed to describe somewhat fully Newton's methods as described by Colson. Newton assumed that all geometrical magnitudes might be conceived as generated by continuous motion; thus a line may be considered as generated by the motion of a point, a surface by that of a line, a solid by that of a surface, a plane angle by the rotation of a line, and so on. The quantity thus generated was defined by him as the fluent or flowing quantity. The velocity of the moving magnitude was defined as the fluxion of the fluent. This seems to be the earliest definite recognition of the idea of a continuous function, though it had been foreshadowed in some of Napier's papers.

Newton's treatment of the subject is as follows. There are two kinds of problems. The object of the first is to find the fluxion of a given quantity, or more generally ``the relation of the fluents being given, to find the relation of their fluxions.'' This is equivalent to differentiation. The object of the second or inverse method of fluxions is from the fluxion or some relations involving it to determine the fluent, or more generally ``an equation being proposed exhibiting the relation of the fluxions of quantities, to find the relations of those quantities, or fluents, to one another.'' This is equivalent either to integration which Newton termed the method of quadrature, or to the solution of a differential equation which was called by Newton the inverse method of tangents. The methods for solving these problems are discussed at considerable length.

Newton then went on to apply these results to questions connected with the maxima and minima of quantities, the method of drawing tangents to curves, and the curvature of curves (namely, the determination of the centre of curvature, the radius of curvature, and the rate at which the radius of curvature increases). He next considered the quadrature of curves and the rectification of curves. In finding the maximum and minimum of functions of one variable we regard the change of sign of the difference between two consecutive values of the function as the true criterion; but his argument is that when a quantity increasing has attained its maximum it can have no further increment, or when decreasing it has attained its minimum it can have no further decrement; consequently the fluxion must be equal to nothing.

It has been remarked that neither Newton nor Leibnitz produced a calculus, that is, a classified collection of rules; and that the problems they discussed were treated from first principles. That, no doubt, is the usual sequence in the history of such discoveries, though the fact is frequently forgotten by subsequent writers. In this case I think the statement, so far as Newton's treatment of the differential or fluxional part of the calculus is concerned, is incorrect, as the foregoing account sufficiently shews.

If a flowing quantity or fluent were represented by x, Newton denoted its fluxion by $\dot{x}$, the fluxion of $\dot{x}$ or second fluxion of x by $\ddot{x}$, and so on. Similarly the fluent of x was denoted by $\boxed{x}$, or sometimes by x' or $[x]$. The infinitely small part by which a fluent such as x increased in a small interval of time measured by o was called the moment of the fluent; and its value was shewn to be $\dot{x}o$. Newton adds the important remark that thus we may in any problem neglect the terms multiplied by the second and higher powers of o, and we may always find an equation between the co-ordinates x, y of a point on a curve and their fluxions $\dot{x}$, $\dot{y}$. It is an application of this principle which constitutes one of the chief values of the calculus; for if we desire to find the effect produced by several causes on a system, then, if we can find the effect produced by each cause when acting alone in a very small time, the total effect produced in that time will be equal to the sum of the separate effects. I should here note the fact that Vince and other English writers in the eighteenth century used $\dot{x}$ to denote the increment of x and not the velocity with which it increased; that is $\dot{x}$ in their writings stands for what Newton would have expressed by $\dot{x}o$ and what Leibnitz would have written as dx.

I need not discuss in detail the manner in which Newton treated the problems above mentioned. I will only add that, in spite of the form of his definition, the introduction into geometry of the idea of time was evaded by supposing that some quantity *ex. gr*. the abscissa of a point on a curve) increased equably; and the required results then depend on the rate at which other quantities (*ex. gr*. the ordinate or radius of curvature) increase relatively to the one so chosen. The fluent so chosen is what we now call the independent variable; its fluxion was termed the ``principal fluxion''; and, of course, if it were denoted by x, then $\dot{x}$ was constant, and consequently $\ddot{x} = 0$.

There is no question that Newton used a method of fluxions in 1666, and it is practically certain that accounts of it were communicated in manuscript to friends and pupils from and after 1669. The manuscript, from which most of the above summary has been taken, is believed to have been written between 1671 and 1677, and to have been in circulation at Cambridge from that time onwards, though it is probable that parts were rewritten from time to time. It was unfortunate that it was not published at once. Strangers at a distance naturally judged of the method by the letter to Wallis in 1692, or by the *Tractatus de Quadratura Curvarum*, and were not aware that it had been so completely developed at an earlier date. This was the cause of numerous misunderstandings. At the same time it must be added that all mathematical analysis was leading up to the ideas and methods of the infinitesimal calculus. Foreshadowings of the principles and even of the language of that calculus can be found in the writings of Napier, Kepler, Cavalieri, Pascal, Fermat, Wallis, and Barrow. It was Newton's good luck to come at a time when everything was ripe for the discovery, and his ability enabled him to construct almost at once a complete calculus.

The infinitesimal calculus can also be expressed in the notation of the differential calculus: a notation which was invented by Leibnitz probably in 1675, certainly by 1677, and was published in 1684, some nine years before the earliest printed account of Newton's method of fluxions. But the question whether the general idea of the calculus expressed in that notation was obtained by Leibnitz from Newton, or whether it was discovered independently, gave rise to a long and bitter controversy. The leading facts are given in the next chapter.

The remaining events of Newton's life require little or no comment. In 1705 he was knighted. From this time onwards he devoted much of his leisure to theology, and wrote at great length on prophecies and predictions, subjects which had always been of interest to him. His *Universal Arithmetic* was published by Whiston in 1707, and his *Analysis by Infinite Series* in 1711; but Newton had nothing to do with the preparation of either of these for the press. His evidence before the House of Commons in 1714 on the determination of longitude at sea marks an important epoch in the history of navigation.

The dispute with Leibnitz as to whether he had derived the ideas of the differential calculus from Newton or invented it independently originated about 1708, and occupied much of Newton's time, especially between the years 1709 and 1716.

In 1709 Newton was persuaded to allow Cotes to prepare the long-talked-of second edition of the *Principia*; it was issued in March 1713. A third edition was published in 1726 under the direction of Henry Pemberton. In 1725 Newton's health began to fail. He died on March 20, 1727, and eight days later was buried in Westminster Abbey.

His chief works, taking them in their order of publication, are the *Principia*, published in 1687; the *Optics* (with appendices on *cubic curves*, the *quadrature and rectification of curves by the use of infinite series*, and the *method of fluxions*), published in 1704; the *Universal Arithmetic*, published in 1707; the *Analysis per Series, Fluxiones*, etc., and the *Methodus Differentialis*, published in 1711; the *Lectiones Opticae*, published in 1729; the *Method of Fluxions*, etc. (that is *Newton's manuscript on fluxions*), translated by J. Colson and published in 1736; and the *Geometrica Analytica*, printed in 1779 in the first volume of Horsley's edition of Newton's works.

In appearance Newton was short, and towards the close of his life rather stout, but well set, with a square lower jaw, brown eyes, a broad forehead, and rather sharp features. His hair turned grey before he was thirty, and remained thick and white as silver till his death.

As to his manners, he dressed slovenly, was rather languid, and was often so absorbed in his own thoughts as to be anything but a lively companion. Many anecdotes of his extreme absence of mind when engaged in any investigation have been preserved. Thus once when riding home from Grantham he dismounted to lead his horse up a steep hill; when he turned at the to remount, he found that he had the bridle in his hand, while his horse had slipped it and gone away. Again, on the few occasions when he sacrificed his time to entertain his friends, if he left them to get more wine or for any similar reason, he would as often as not be found after the lapse of some time working out a problem, oblivious alike of his expectant guests and of his errand. He took no exercise, indulged in no amusements, and worked incessantly, often spending eighteen or nineteen hours out of the twenty-four in writing.

In character he was religious and conscientious, with an exceptionally high standard of morality, having, as Bishop Burnet said, ``the whitest soul'' he ever knew. Newton was always perfectly straightforward and honest; but in his controversies with Leibnitz, Hooke and others, though scrupulously just, he was not generous; and it would seem that he frequently took offence at a chance expression when none was intended. He modestly attributed his discoveries largely to the admirable work done by his predecessors; and once explained that, if he had seen further than other men, it was only because he had stood on the shoulders of giants. He summed up his own estimate of his work in the sentence, ``I do not know what I may appear to the world; but to myself I seem to have been only like a boy, playing on the sea-shore, and diverting myself, in now and then finding a smoother pebble, or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.'' He was morbidly sensitive to being involved in any discussions. I believe that, with the exception of his papers on optics, every one of his works was published only under pressure from his friends and against his own wishes. There are several instances of his communicating papers and results on condition that his name should not be published: thus when in 1669 he had, at Collins's request, solved some problems on harmonic series and on annuities which had previously baffled investigation, he only gave permission that his results should be published ``so it be,'' as he says, ``without my name to it; for I see not what there is desirable in public esteem, were I able to acquire and maintain it: it would perhaps increase my acquaintance, the things which I chiefly study to decline.''

Perhaps the most wonderful single illustration of his powers was the composition in seven months of the first book of the *Principia*, and the expression of the numerous and complex results in classical geometrical form. As other illustrations of his ability I may mention his solutions of the problems of Pappus, of John Bernoulli's challenge, and of the question of orthogonal trajectories. The problem of Pappus, here alluded to, is to find the locus of a point such the rectangle under its distances from two given straight lines shall be in a given ratio to the rectangle under its distances from two other given straight lines. Many geometricians from the time of Apollonius had tried to find a geometrical solution and had failed, but what had proved insuperable to his predecessors seems to have presented little difficulty to Newton who gave an elegant demonstration that the locus was a conic. Geometry, said Lagrange when recommending the study of analysis to his pupils, is a strong bow, but it is one which only a Newton can fully utilize. As another example I may mention that in 1696 John Bernoulli challenged mathematicians (i) to determine the brachistochrone, and (ii) to find a curve such that if any line drawn from a fixed point O cut it in P and Q then $OP^n + OQ^n$ would be constant. Leibnitz solved the first of these questions after an interval of rather more than six months, and then suggested that they be sent as a challenge to Newton and others. Newton received the problems on Jan. 29, 1697, and the next day gave the complete solutions to both, at the same time generalising the second question. An almost exactly similar case occurred in 1716 when Newton was asked to find the orthogonal trajectory of a family of curves. In five hours Newton solved the problem in the form in which it was propounded to him, and laid down the principles for finding trajectories.

It is almost impossible to describe the effect of Newton's writings without being suspected of exaggeration. But, if the state of mathematical knowledge in 1669 or at the death of Pascal or Fermat be compared with what was known in 1700 it will be seen how immense was the advance. In fact we may say that it took mathematicians half a century or more before they were able to assimilate the work produced in those years.

In pure geometry Newton did not establish any new methods, but no modern writer has shewn the same power in using those of classical geometry. In algebra and the theory of equations he introduced the system of literal indices, established the binomial theorem, and created no inconsiderable part of the theory of equations: one rule which he enunciated in this subject remained till a few years ago an unsolved riddle which had overtaxed the resources of succeeding mathematicians. In analytical geometry, he introduced the modern classification of curves into algebraical and transcendental; and established many of the fundamental properties of asymptotes, multiple points, and isolated loops, illustrated by a discussion of cubic curves. The fluxional or infinitesimal calculus was invented by Newton in or before the year 1666, and circulated in manuscript amongst his friends in and after the year 1669, though no account of the method was printed till 1693. The fact that the results are nowadays expressed in a different notation has led to Newton's investigations on this subject being somewhat overlooked.

Newton, further, was the first to place dynamics on a satisfactory basis, and from dynamics he deduced the theory of statics: this was in the introduction to the *Principia* published in 1687. The theory of attractions, the application of the principles of mechanics to the solar system, the creation of physical astronomy, and the establishment of the law of universal gravitation are due to him, and were first published in the same work, but of the nature of gravity he confessed his ignorance, though he found inconceivable the idea of action at a distance. The particular questions connected with the motion of the earth and moon were worked out as fully as was then possible. The theory of hydrodynamics was created in the second book of the *Principia*, and he added considerably to the theory of hydrostatics which may be said to have been first discussed in modern times by Pascal. The theory of the propagation of waves, and in particular the application to determine the velocity of sound, is due to Newton and was published in 1687. In geometrical optics, he explained amongst other things the decomposition of light and the theory of the rainbow; he invented the reflecting telescope known by his name, and the sextant. In physical optics, he suggested and elaborated the emission theory of light. The above list does not exhaust the subjects he investigated, but it will serve to illustrate how marked was his influence on the history of mathematics. On his writings and on their effects, it will be enough to quote the remarks of two or three of those who were subsequently concerned with the subject-matter of the*Principia*. Lagrange described the *Principia* as the greatest production of the human mind, and said he felt dazed at such an illustration of what man's intellect might be capable. In describing the effect of his own writings and those of Laplace it was a favourite remark of his that Newton was not only the greatest genius that had ever existed, but he was also the most fortunate, for as there is but one universe, it can happen but to one man in the world's history to be the interpreter of its laws. Laplace, who is in general very sparing of his praise, makes of Newton the one exception, and the words in which he enumerates the causes which ``will always assure to the *Principia* a pre-eminence above all the other productions of human genius'' have been often quoted. Not less remarkable is the homage rendered by Gauss; for other great mathematicians or philosophers he used the epithets magnus, or clarus, or clarissimus: for Newton alone he kept the prefix summus. Finally Biot, who had made a special study of Newton's works, sums up his remarks by saying, ``comme géomètre et comme expérimentateur Newton est sans égal; par la réunion de ces deux genres de génies à leur plus haut degré, il est sans exemple.''