DNB

NEWTON, Sir HENRY (1651–1715), British envoy in Tuscany, born 18 Aug. (N.S.) 1651, was the eldest son of Henry Newton, of Highley, Essex, and Mary, daughter of R. Hunt of the same county. His family came originally from Staffordshire. He matriculated from St. Mary Hall, Oxford, on 17 March 1665, and graduated B.A. in 1668, M.A. in 1671, B.C.L. in 1674, and D.C.L. on migrating to Merton on 17 June 1678. At the university he formed a lifelong friendship with the future Lord Somers. After some travel on the continent he became in 1678 an advocate at Doctors' Commons, and practised at the bar 'with great judgment, integrity, and applause.' In 1685 he was appointed chancellor of the diocese of London, and in 1694 judge-advocate to the admiralty. The former office he held till his death.

In 1704 Newton was sent as envoy-extraordinary to Florence, where his urbanity and eloquence won the favour of the grand duke. He obtained for the English merchants at Leghorn permission to practise the protestant religion, a privilege which had been denied them since the days of Queen Elizabeth. Towards the close of 1706 he was sent on a special mission to Genoa. He made his public entry there on 18 March 1707. The council assured Newton that the republic would carefully cultivate their friendship with Great Britain, and 'inviolably observe a perfect neutrality' in the Spanish Succession war. He left the city about the middle of June, and returned to Florence. In 1708 he visited Rome, but did not see the pope. Clement XI, however, kept up a constant correspondence with him. He was admitted a member of the Accademia della Crusca and of several other learned societies, and many odes addressed to him in Latin or Italian are printed with his works. He was recalled from Tuscany at the close of 1709. During his absence from England he had been appointed master of St. Catherine's Hospital.

On 5 Nov. 1714 Newton was made a judge of the high court of admiralty, and was knighted 4 March 1715, a ceremony which, according to his daughter, 'he wou'd gladly have dispens'd with.' He had once before refused the judgeship, according to the same authority, 'for he cou'd not bear to pronounce sentence of Death upon his Fellow creatures, tho' Pyrates.' Coote, however, attributes Newton's reluctance to the 'zeal of Toryism,' which rendered him unwilling to sanction the proceedings against the maritime partisans of James II. Newton died suddenly of apoplexy on 29 July 1715, and was buried in Mercer's Chapel, London. He had married, soon after coming to London, 'a lady of merit, by whom he had children; but the lady and children died a few years after.' By his second wife, Mary, daughter of Thomas Manning, esq., he had two daughters, besides a son who died young. The elder daughter, Mary, married Henry Rodney, esq., of Rodneystoke, Somerset. Their son was the admiral, George Bridges Rodney. The younger daughter, Catherine, married, first, Colonel Francis Alexander (who died in 1722), and, secondly, Lord Aubrey Beauclerk, youngest son of the Duke of St. Albans, who was killed at Carthagena in 1740.

Newton published: 1. 'Epistolæ, Orationes et Carmina,' Lucca, 1710, 4to, with a dedication to Lord Somers. 2. 'Orationes, quarum altera Florentiæ anno 1705, altera vero Genuæ anno 1707, habita est. Anapæsti, cum ab illustrissimo Comite Magalotti odis donaretur, Florentiæ VII Kal. Junii 1706. Vaticinium,' Amsterdam, 1710. Among the letters, twenty-five are addressed to P. H. Barcellini, six to Gisbert Cuper, four to Magliabecchi, and two each to Count Magalotti and Lord Somers. The latter is said never to have known a happy moment after Newton's death.

Newton, it appears, left ready for the press his memoirs in four large octavo volumes. These, however, were then 'unfortunately removed to a new house of a Relation, and by the damp (as 'tis said) were entirely defaced.' An engraving by Benedict Farint, from a medallion portrait executed at Florence by Soldano in 1709, bearing a eulogistic Latin inscription, is prefixed to Newton's 'Epistolæ,' 1710.

[The Latin life of Newton bound up with Christian Gebauer's Narratio de Henrico Brenkmanno, Göttingen, 1764, and probably by that writer, is founded on communications from Newton's daughters (particularly from the younger), on his own writings, and on other contemporary sources, all in Latin, except the first. See also Hist. Reg. vol. i. Chron. Diary, pp. 18, 48, 65; Boyer's Annals of Anne, 1707, pp. 202–7; Alumni Oxon. 1500–1714; Wood's Fasti, ii. 368; Catalogue of English Civilians, 1804, p. 100; and Noble's Contin. of Granger's Biog. Hist. ii. 175–6. Cf. a letter from Gisbert Cuper to Le Clerc, 16 Nov. 1706, in Cuper's Lettres de Critique (French version), pp. 361–2.]

G. Le G. N.

NEWTON, Sir ISAAC (1642–1727), natural philosopher, was born in the manor-house at Woolsthorpe, a hamlet of Colsterworth, eight miles south of Grantham, Lincolnshire, on 25 Dec. 1642. Engravings of the house, which is still standing, appear in

Thomas Maude's 'Wensleydale,' 1771, and in Turnor's 'Collections for the History of Grantham,' 1806, p. 157. He was baptised at Colsterworth 1 Jan. 1642–3. His father, Isaac Newton of Woolsthorpe, had married in April 1642 Hannah, daughter of James Ayscough of Market Overton, Rutland, but died at the age of thirty-six, in October 1642, before the birth of his son. The small estate of Woolsthorpe had been purchased by the philosopher's grandfather, Robert Newton (*d.* 1641), in 1623. Some three years after her first husband's death, 27 Jan. 1645–6, Newton's mother married Barnabas Smith, rector of North Witham, Lincolnshire, who died in 1656, leaving by him one son, Benjamin, and two daughters, Marie (wife of Thomas Pilkington of Belton, Rutland) and Hannah (second wife of Thomas Barton of Brigstock, Northamptonshire).

On his mother's second marriage Newton was left at Woolsthorpe in charge of his grandmother, Mrs. Ayscough. He was sent in 1654 to the grammar school at Grantham, then kept by a Mr. Stokes. For some time he made little advance with his books, but a successful fight with a boy older than himself awakened a spirit of emulation, and Newton soon rose to be head of the school. At the age of fourteen he was removed from school by his mother, who had returned to Woolsthorpe on the death of her second husband, in order to take part in the management of her farm. This proved distasteful to Isaac—there are various stories of the way in which he occupied himself with mathematics and other studies when he ought to have been attending to his farm duties—and by the advice of his uncle, William Ayscough, rector of Burton Coggles, Lincolnshire, he was sent back to school in 1660 with a view to preparing him for college. Ayscough was himself a Trinity man, and on 5 June 1661 Isaac Newton was matriculated as a subsizar at Trinity College, Cambridge, under Mr. Pulleyne. Few details of his undergraduate life remain. In 1664 he made some observations on halos, afterwards described in his 'Optics' (bk. ii. pt. iv. obs. 13), and on 28 April of the same year he was elected a scholar. He graduated B.A. in January 1665, but unfortunately the 'ordo senioritatis' for that year has not been preserved.

Newton's unrivalled genius for mathematical speculation declared itself almost in his boyhood. Before coming to Cambridge he had read Sanderson's 'Logic' and Kepler's 'Optics.' As an undergraduate he applied himself to Descartes's 'Geometry' and Wallis's 'Arithmetica Infinitorum,' and he attended Barrow's lectures. His mental activity immediately after taking his degree, during 1665 and 1666, was extraordinary. In a manuscript quoted in the preface to 'A Catalogue of the Newton MSS., Portsmouth Collection,' Cambridge, 1888, written probably about 1716, he writes: 'In the beginning of the year 1665 I found the method for approximating series and the rule for reducing any dignity [power] of any binomial to such a series [i.e. the binomial theorem]. The same year in May I found the method of tangents of Gregory and Slusius, and in November had the direct method of Fluxions [i.e. the elements of the differential calculus], and the next year in January had the Theory of Colours, and in May following I had entrance into the inverse method of Fluxions [i.e. integral calculus], and in the same year I began to think of gravity extending to the orb of the Moon . . . and having thereby compared the force requisite to keep the Moon in her orb with the force of gravity at the surface of the earth, and found them to answer pretty nearly. All this was in the two years of 1665 and 1666, for in those years I was in the prime of my age for invention, and minded Mathematics and Philosophy more than at any time since' (see also Appendix to RIGAUD's *Essay on the Principia*, pp. 20, 23; 'Letter to Leibnitz,' 24 Oct. 1676, No. lv. in the *Commercium Epistolicum*; PEMBERTON, Preface to *A View of Sir Isaac Newton's Philosophy*, 1728). Another statement referring to these early years, quoted by Brewster in his 'Life of Newton,' from a notebook among the Conduitt papers in the possession of Lord Portsmouth, under date 4 July 1699, runs as follows: 'By consulting an account of my expenses at Cambridge in the years 1663 and 1664, I find that in the year 1664, a little before Christmas, I being then Senior Sophister, bought Schooten's "Miscellanies" and Carte's "Geometry" (having read his "Geometry" and Oughtred's "Clavis" clean over half a year before), and borrowed Wallis's works, and by consequence made these annotations out of Schooten and Wallis in winter between the years 1664 and 1665. At such time I found the method of infinite series; and in summer 1665, being forced from Cambridge by the plague, I computed the area of the hyperbola at Boothby in Lincolnshire to two-and-fifty figures by the same method.'

Newton states here that he was driven from Cambridge in 1665 by the plague, while he wrote in the 'Philosophical Transactions' (vi. 3075): 'In the beginning of the year 1666 . . . I procured me a triangular glass prism to try therewith the celebrated phenomena of colours,' and continues (p. 3080): 'Amidst these thoughts I was forced from Cambridge by the intervening plague, and it was more than two years before I proceeded further.' The college was dismissed in consequence of the plague on 8 Aug. 1665; but Newton appears from the books to have left Cambridge before that date. The plague reappeared in 1666; the college was again dismissed 22 June 1666. It seems probable, therefore, that Newton was in Cambridge for some time between these two dates, and this is confirmed by the statement due to Conduitt that the prism was bought at Stourbridge fair. A paper in Newton's handwriting, in the possession of the Earl of Macclesfield, printed in the Appendix to Rigaud's 'Essay,' p. 20, shows that on 13 Nov. 1665 he wrote a 'Discourse on Fluxions,' and the notebooks among the 'Portsmouth Collection of Papers' have references to the same subject, dated 20 May 1665, and also May, October, and November 1666.

It was in the autumn of 1665, at Woolsthorpe, in enforced absence from Cambridge, that the idea of universal gravitation occurred to him. 'As he sat alone in a garden,' says Pemberton, his intimate friend of later years, and the editor in 1726 of the third edition of the 'Principia,' in his preface to 'A View of Sir Isaac Newton's Philosophy' (1728), 'he fell into a speculation on the power of gravity, that as this power is not found sensibly diminished at the remotest distance from the centre of the earth to which we can rise ... it appeared to him reasonable to conclude that this power must extend much farther than is usually thought. Why not as high as the moon? said he to himself, and, if so, her motion must be influenced by it: perhaps she is retained in her orbit thereby.' The story that this train of thought was aroused by seeing an apple fall is due to Voltaire, and is given in his 'Philosophie de Newton,' 3me partie, chap. iii. Voltaire had it from Newton's step-niece, Mrs. Conduitt. For many years tradition marked the tree in the garden at Woolsthorpe: it was shown to Sir D. Brewster in 1814, and was taken down in 1820.

Now Newton knew at this time, by a simple deduction from Kepler's third law, that if the moon were kept in an orbit approximately circular by a force directed to the centre of the earth, that force must be inversely proportional to the square of the distance between the moon and the earth. He tells us this in the paper in the Portsmouth MSS., of which part has already been quoted, and he proceeded therefore to compare the consequences of his theory with the observed motion of the moon, 'and found them,' to use his words, 'answer pretty nearly.' Still the matter was laid aside, and nothing more came of it for nearly twenty years.

To make the calculation a knowledge of the earth's radius was required. Now, the common estimate in use among geographers before Newton's time was based on the supposition that there were sixty miles to a degree of latitude, and Pemberton states that Newton took this common estimate, but he added: 'As this is a very faulty supposition, each degree containing about sixty-nine and a half of our miles, his computation did not answer expectation, whence he concluded that some other cause must at least join with the power of gravity on the moon.' It seems, however, impossible that Newton continued long unacquainted with the fact that the estimate he had used was exceedingly rough. Norwood's 'Seaman's Practice,' published in 1636, contained the much more correct measure of sixty-nine and a half miles to a degree, and this was a well-known work, a sixth edition having appeared in 1667, and a seventh in 1668. Snell had given nearly the same result, 28,500 Rhineland perches, in 1617, and this was referred to in Varenius's 'Geography,' an edition of which was prepared in 1672 by Newton himself. Picard made a very elaborate series of measures, published in Paris in 1671, giving sixty-nine and one-tenth miles to the degree. This was mentioned at the Royal Society on 11 Jan. and 1 Feb. 1672 (Birch, *History of Roy. Soc.* iii. 3, 8). Newton had been elected a fellow a month previously, and his telescope was discussed at the meeting at which Picard's measurement was announced. It was referred to at Royal Society meetings on other later occasions, and was discussed on 7 June 1682 at a meeting at which Newton was again present. But although Newton thus learned within a few years that his calculations of 1665 were founded on erroneous numbers, he deferred undertaking a recalculation till some time after 1682—probably in 1685—when he repeated his work with Picard's numbers, and found exact agreement between the theory and the facts. His delay in beginning the recalculation was probably due, as Professor Adams suggested, to the fact that he was unable till about 1685 to calculate the attraction of a large spherical body on a point near its surface; it was in his 'Principia' that Newton first publicly divulged the solution of that problem.

Newton returned to Cambridge in 1667, and on 1 Oct. was elected, with eight others, a fellow of Trinity College. There had been no election in 1665 and 1666, probably in consequence of the plague. During the next

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few years Newton turned his attention to his optical work. In 1668 he made his first reflecting telescope; it had an aperture of about one inch and was six inches long, and with it Newton saw Jupiter's satellites (*Macc. Corr.* ii. 289). He never held any college office, but in 1669 he assisted Dr. Barrow, Lucasian professor, with an edition of his 'Optical Lectures.'

At the end of 1668 Mercator had published his 'Logarithmotechnia,' in which he showed how to calculate the area of an hyperbola. A copy of this was sent by John Collins (1625–1683) [q. v.] to Barrow, and shown by him to Newton. Newton recognised that the method was in the main the same as the more general one he had already devised for finding the area of curves and for solving other problems, and showed his manuscripts to Barrow. Barrow was delighted, and wrote on 20 July 1669 to Collins, promising to send the papers of 'a Friend of mine here that hath an excellent genius to these Things.' The papers were sent, but without any mention of the name of the author, on 31 July, and on 20 Aug. Barrow writes: 'I am glad my Friend's paper gives you so much satisfaction; his name is Mr. Newton; a Fellow of our College, and very young . . . but of an extraordinary genius and Proficiency in these things' (*Comm. Epist.* pp. 1, 2, London, 1712). The title of the paper, printed from a manuscript in Collins's handwriting found among his papers after his death, and compared with Newton's own copy, is 'De Analysi per Æquationes numeri terminorum infinitas.' The main part of this manuscript was published by Newton in 1704 as an Appendix to his 'Optics.' Collins, writing to Strode in 1672, after stating that Barrow had sent him Newton's paper, proceeds: 'Equibus et aliis quæ prius ab authore cum Barrovio communicata fuerant, patet illam methodum a dicto Newtono aliquot annis antea excogitatam et modo universali applicatam fuisse.'

In the autumn of 1669 Barrow resigned the Lucasian chair, and Newton was chosen to succeed him. Part of his time during 1669 and 1670 was occupied in writing notes and additions to a Latin translation of Kinckhuysen's 'Algebra.' (See Correspondence with Collins, *Macc. Corr.* ii. 281). He also at this time was led to conclude from his optical experiments that it was impossible to perfect the refracting telescope, and he applied himself to improving his reflecting instrument. The second telescope made by him was sent up to the Royal Society in December 1671, and is described in the 'Philosophical Transactions,' vii. 4004. Towards the end of the same year he was busy enlarging his method of infinite series. This paper was never finished, but was published in 1736 in a translation by Colson. Pemberton states that he had persuaded Newton 'to let it go abroad,' and hoped to receive from him papers to supply what was wanted when he died. About the same time he prepared an edition of the 'Optical Lectures,' twenty in number, which he had delivered as Lucasian professor. These were not published till 1729, when there was printed a copy, which he had given to David Gregory, the Savilian professor at Oxford.

At the end of this year Newton was proposed for election as a fellow of the Royal Society by Seth Ward, bishop of Salisbury. He was elected on 11 Jan. 1672, and about this time his correspondence with Henry Oldenburg [q. v.], secretary of the Royal Society, commenced (see *Newton Correspondence with Cotes*, edited by Edleston, 1850, App. p. 240; *Macc. Corr.* ii. 311). The earliest letters relate mainly to the telescope. He was pleased at his election, and writes: 'I shall endeavour to show my gratitude by communicating what my poor and solitary endeavours can effect towards the promoting philosophical design.' This promise was soon fulfilled, for on 8 Feb. Oldenburg read a letter, dated 6 Feb., from Newton, containing his 'New Theory about Light and Colours' (*Phil. Trans.* vi. 3075).

The letter contained an account of the experiments with the prism bought in 1666 to try the celebrated phenomena of colours. The experiments showed conclusively that 'Light consists of Rays differently refrangible;' that 'Colours are not Qualifications of Light derived from Refractions of Natural Bodies, as is generally believed, but original and connate properties which in divers Rays are divers;' that 'to the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility. The least refrangible rays are all disposed to exhibit a red colour. . . . the most refrangible rays are all disposed to exhibit a deep violet colour,' and 'this species of colour is not mutable by refraction, nor by reflexion from natural bodies,' while 'white light is ever compounded, and to its composition are requisite all the aforesaid primary colours mixed in proper proportion.'

It was ordered that 'the author be solemnly thanked for this very ingenious discourse, and be made acquainted that the society think very much of it.' It was further ordered that this discourse be entered in the register book, and that the Bishop of Salisbury, Robert Boyle [q. v.], and Robert Hooke [q. v.] be desired to peruse and consider it, and to bring in a report of it to the society.

Hooke alone appears to have reported, and his report was read at the next meeting, 15 Feb. 1672 (BIRCH, *Hist. of Roy. Soc.* iii. 10). Hooke, in the discussions about the telescope, had already appeared as a critic of Newton. Descartes had in 1637 (*Discours de la methode pour bien conduire sa raison et chercher la verité dans les Sciences*, sect. ii. 'Meteors,' p. 190) described the rainbow colours produced by refraction of light bounded by shade through a prism, and had elaborated a theory of colours. This theory had been adopted, with modifications, by Hooke in his 'Micrographia,' published in 1664, and he had there described (p. 58) an experiment practically identical with Newton's fundamental experiment with the prism. He took a glass vessel, about two feet long, filled with water, and inclined so that the sun's rays could enter obliquely at the top surface of the water and traverse the glass. The top surface was covered with an opacous body, all but a hole through which the sunbeams were suffered to pass into the water, and were thereby refracted 'to the bottom of the glass, against which part, if a paper be expanded on the outside, there will appear all the colours of the rainbow: that is, there will be generated the two principal colours, scarlet and blue, and all the intermediate ones which arise from the composition and dilutings of these two.' But Hooke could make no use of his own observation; he attempted to substantiate from it a theory of colours of his own, and wrote pure nonsense in the attempt. Hence he was not prepared to accept Newton's reasoning; he admitted the truth of his observations, as having himself 'by many hundreds of trials found them so,' but declined to accept Newton's deductions, and wrote in a vague and unsatisfactory way about his own theory. The criticism was sent to Newton, who expressed his pleasure 'that so acute an observer had said nothing that can enervate any part' of the discourse, and promised a reply. The reply was read on 12 June 1672, and was printed in the 'Philosophical Transactions,' 18 Nov. 1672. Hooke's considerations on my theories, said Newton, 'consist in ascribing an hypothesis to me which is not mine, in asserting an hypothesis which as its principal parts is not against me, in granting the greatest part of my discourse if explicated by that hypothesis, and in denying some things the truth of which would have appeared by an experimental examination.' In the paper Newton dealt with these points seriatim. Meanwhile other objectors had appeared. Père Pardies of Clermont attempted to explain the results in a simple way, but was soon satisfied of his error. Linus of Liège denied the truth of Newton's observations, and Newton declined to reply till 1675, just previous to Linus's death. Linus's successor, Lucas, by the aid of a hint from Newton, obtained the spectrum, but its length was shorter than that found by Newton himself. Newton maintained his position, that the length of the spectrum produced at a given distance from the prism was the same for prisms of all materials, provided only that their angles were such as to produce a definite amount of deviation for one mean ray, and sent to Lucas (*Phil. Trans.* 25 Sept. 1676, p. 698) an account of his measurements, closing his letter with the desire to have full details of Lucas's experiments: 'for I know that Mr. Lucas's observation cannot hold when the refracting angle of the prism is full 60° and the day is clear, and the full length of the colours is measured.'

We know now that in this belief, to which Newton adhered with marvellous tenacity, he was wrong, and it was this faith which led him to despair of the possibility of making refracting telescopes and to turn his attention to reflectors. Thus in his 'Optics,' published in 1704, in which his optical researches are summed up, he wrote, p. 20: 'Now the different magnitudes of the hole . . . made no sensible change in the length of the image, neither did the different matter of the prisms make any, for in a vessel made of polished glass filled with water there is the like success of the experiment according to the quality of the refraction.' It is probable that in this experiment 'to increase the refraction' the water was 'impregnated strongly with saccharum saturnii;' he asserted (*Optics*, p. 51) that he sometimes adopted this plan. The sugar of lead increases the dispersion as well, and would lead to the result stated by Newton; had he used pure water he would have found a distinct difference in the length of the two spectra, and would have corroborated Lucas. Hence he concluded (*ib.* p. 74) that, 'were it not for this unequal refrangibility of rays, telescopes might be brought to a greater perfection than we have yet described;' but, as things were, Huyghens's method of enormously increasing the focal length of the object-glass was the only remedy. 'Seeing therefore (he proceeded) the improvement of telescopes of given lengths by refractions is desperate, I contrived heretofore a perspective by reflexion, using instead of an object-glass a concave metal.' He held it to be impossible to produce with lenses an achromatic or colourless image of a distant object. Shortly after the death of Newton, Chester Moor Hall [q. v.] of Essex invented the achromatic tele-

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scope, and in 1733 had made several; but his work remained unnoticed till Dollond turned his attention to the question, and in 1758 constructed satisfactory achromatic lenses by the combination of crown and flint glass (BREWSTER, *Life of Newton*, i. 99, ed. 1855).

Nor were Hook, Linus, and Lucas Newton's only opponents. Huyghens himself entered the field, but his objections (*Phil. Trans.* vii. 6086, 6108) were not very serious. Still these differences of opinion troubled Newton, and he wrote to Oldenburg (*Macel. Corr.* ii. 368, 5 Dec. 1674): 'I have long since determined to concern myself no further about the promotion of philosophy;' and again (*ib.* ii. 404, 18 Nov. 1676): 'I see I have made myself a slave to philosophy; but if I get free of Mr. Linus' business I will resolutely bid adieu to it eternally, excepting what I do for my own 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.' Collins, writing to J. Gregory (*ib.* ii. 280, 19 Oct. 1675), sadly asserted that Newton and Barrow were 'beginning to think mathematical speculations at least dry, if not somewhat barren,' and that Newton was intent on chemical studies and practices. But wiser counsels prevailed, and Newton did not yet give up philosophy. The 'Macclesfield Correspondence' contains some interesting letters from him to Collins, dated between 1672 and 1675, dealing with such topics as reflecting telescopes (Gregory's and Cassegrain's), Barrow's method of tangents, and the motion of a bullet.

On 18 Feb. 1675 'Mr. Isaac Newton and James Hoare, jun., esq., were admitted fellows of the Royal Society, to which Newton had been elected nearly three years earlier. On 28 Jan. of the same year he had been excused the weekly payment of 1*s.* to the society, and he had expressed a wish to resign, alleging as the cause the distance between Cambridge and London. It appears that at the time he was in circumstances of pecuniary difficulty. These, it seems probable, were connected with the expectation that he would have to vacate his fellowship in the autumn, owing to his not being in holy orders. The difficulty was solved by the receipt of a patent from the king permitting Newton as Lucasian professor to hold a fellowship although he was a layman. Thus encouraged, he continued his work, and towards the end of the year he wrote to Oldenburg, offering to send 'a Discourse about Colours to be read at one of your meetings.' This was accepted, and on 9 Dec. 1675 'there was produced a manuscript of Mr. Newton touching his theory of light and colours, containing partly an hypothesis to explain the properties of light discoursed of by him in his former papers, partly the principal phenomena of the various colours exhibited by thin plates or bubbles, esteemed by him to be of a more difficult consideration, yet to depend also on the said properties of light.' The experiments recorded the first measurements on the coloured rings of thin plates. The relation between the diameter of the rings and the thickness of the plate was stated, and the phenomena were explained in Newton's clear and masterly way. There was also a reference to the diffraction of light. The reading was continued 20 Jan. 1676, when 'these observations so well pleased the Society that they ordered Mr. Oldenburg to desire Mr. Newton to permit them to be published' (BIRCH, *Hist. of Roy. Soc.* iii. 278). Newton, in his reply (*Macel. Corr.* ii. 388, 25 Jan. 1676), asked Oldenburg 'to suspend the printing of them for a while, because I have some thought of writing such another set of observations for determining the manner of the production of colours by the prism, which, if done, ought to precede that now in your hands, and will do best to be joined with it.' Accordingly the paper was not printed in the 'Philosophical Transactions.' It is given in Birch (*Hist. of Roy. Soc.* iii. 247, 262, 272, &c.), while a large part of it appeared in the 'Optics,' bk. ii., in 1704, but without the hypothesis. This is printed in Brewster's 'Life of Newton' (vol. i. App. ii.) and in the 'Philosophical Magazine' (September 1846, pp. 187–213).

After the part of the paper relating to diffraction and a portion of the observations on the colours of thin plates had been read, Hooke said 'that the main of it was contained in his "Micrographia," which Mr. Newton had only carried further in some particulars' (BIRCH, *ib.* iii. 269). Newton had moreover referred discourteously to a paper of Hooke's dealing with the inflexion of light which had been read 18 March 1675. Hooke's words were now reported to Newton, possibly with too high a colouring, by Oldenburg, who was then engaged in a dispute with Hooke on other matters, and Newton replied somewhat angrily. On this Hooke wrote privately to Newton (BREWSTER, *Life of Newton*, i. 123), expressing a desire to remove the misunderstanding. Newton modestly accepted the friendly advance. 'You defer (he wrote) too much to my ability in searching into this subject. What Descartes did was a good step. You have added much several ways, and especially in considering the colours of thin plates. If I have seen further it is by standing on the shoulders of giants.' Shortly after (*Macel. Corr.* ii. 394), he asked Oldenburg 'to leave out the last paragraph of the hypothesis, where I mention Mr. Hooke and Grimaldi together.' 'If you have opportunity (Newton added, p. 387) pray present my service to Mr. Hooke, for I suppose there is nothing but misapprehension in what has lately happened.'

This paper 'about colours' was the last separate memoir published by Newton on optical subjects. His various papers were collected in the 'Optics,' published in 1704, and to those which we have mentioned were added his researches on the colours of thick plates (bk. ii. pt. iv.) and on the diffraction or inflexion of light (bk. iii.) It will be convenient, therefore, to summarise in this place Newton's views on optics, and his position with regard to the theory which might account for his observations.

Two theories have been proposed to account for optical phenomena. Descartes was the author of one of these, the emission theory, which supposes light to consist of small particles shot out by the luminous body; Hooke, though his work was very incomplete, was the first to suggest an undulatory theory. In his 'Micrographia,' 1664, p. 56, he asserts that light is a quick and short vibrating motion, 'propagated every way through an homogeneous medium by direct or straight lines extended every way, like rays from the centre of a sphere. . . . Every pulse or vibration of the luminous body will generate a sphere which will continually increase and grow bigger just after the same manner, though indefinitely swifter, as the waves or rings on the surface of water do swell into bigger and bigger circles about a point on it.' On this hypothesis he gave an account of reflexion, refraction, dispersion, and the colours of thin plates. His reasoning was, however, utterly vague and unsatisfactory, and he convinced few of the truth of this theory. Newton followed. He may have known of Hooke's theories. The copy of the 'Micrographia' in Trinity College Library has the inscription 'Trin. Coll. Cant. A. 1664,' and below in a different hand, 'Ex dono Mgri Gale huius Colleg. Socij.' It may well have been used by Newton, for among the Portsmouth MSS. of early date are some extracts from the work. Still there was nothing in Hooke's theories but hypotheses unsupported by fact, which would have no charm for Newton. It is claimed for him, and that with justice, that he was the true founder of the rival theory, the emission theory. In Descartes's hands that theory was a vague hypothesis. Newton deduced from it by rigid dynamical reasoning the laws of reflexion and refraction; he applied it with wondrous ingenuity to explain the colours of thin and of thick plates and the phenomena of diffraction, though in the process he had to assume the existence of a mechanism which he must have felt to be almost impossible—a mechanism which in time, as it was applied to explain other and more complex phenomena, became so elaborate that, in the words of Verdet, writing a hundred years later, 'Pour renverser ce pénible échafaudage d'hypothèses indépendantes les unes des autres, il suffit presque de le regarder en face et de chercher à le comprendre.' But though Newton may with justice be called the founder of the emission theory, it is most unjust to his memory to state that he fully accepted it as giving a satisfactory account of optics. When he first began his optical work he realised that facts and measurements were needed, and his object was to furnish the facts.

Hooke's hypotheses were right: light is due to wave-motion in an all-pervading ether. But the discovery a century later of the principle of interference vaguely foreshadowed by Hooke (*Micrographia*, p. 66) was needed to remove the difficulty which Newton experienced. Newton called repeated attention to the difficulty which, unless removed, rendered the rejection of Hooke's theory inevitable. Thus, in reply to Hooke's criticism of his first paper in 1672, he wrote (*Phil. Trans.* vii. 5089, November 1672): 'For to me the fundamental supposition itself seems impossible—namely, that the Waves or Vibrations of any fluid can, like the rays of Light, be propagated in straight lines without a continual and very extravagant spreading and bending every way into the quiescent medium where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary. . . . For it seems impossible that any of those motions or pressions can be propagated in straight lines without the like spreading every way into the shadowed medium.'

Nor was there anything in the controversy which took place about 1675 to shake Newton's conviction that Hooke's 'fundamental supposition' was impossible. Hooke had (18 March 1675) read his paper describing his discovery of diffraction (*Posthumous Works*, p. 186). He had announced it two years earlier, November 1672 (Birch, *Hist. of Roy. Soc.* iii. 63). There is no doubt that this was an original discovery, and not, as Newton seemed to imply soon after, a theory borrowed from Grimaldi. But Hooke's paper did not remove the difficulty, nor was there anything more satisfactory in the lectures which he delivered as Gresham professor in 1680–2; in these he supposed the velocity of light to be infinite, and explained away Romer's observation.

Accordingly we find in the 'Principia' Newton's attempted proof (lib. ii. prop. 42) that 'motus omnis per fluidum propagatus divergit a recto tramite in spatia immota,' a 'pretended demonstration' which has convinced few of the truth of the proposition, and leaves the question unsolved. Again, in 1690, Huyghens, who in all he wrote had clearer views than Hooke, published his great 'Traité de la Lumière,' which was written in 1678. Many of his demonstrations are still completely satisfactory, but on the crucial point he was fatally weak. He, and not Hooke, may claim to be the real founder of the undulatory theory, for he showed what it would do if the rectilinear propagation could only be explained by it. The reasoning of the later pages of Huyghens's first chapter becomes forcible enough when viewed in the light of the principle of interference enunciated by Young on 12 Nov. 1801, and developed by Fresnel in his great memoir on diffraction in 1815; but without this aid it was not possible for Huyghens's arguments to convince Newton, and hence in the 'Optics' (2nd ed. 1717) he propounded the celebrated query 28: 'Are not all hypotheses erroneous in which Light is supposed to consist in pression or motion propagated through a fluid medium?' 'If it consisted in pression or in motion propagated either in an instant or in time, it would bend into the shadow. For pression or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies beyond the shadow.' These were Newton's last words on the subject. They prove that he could not accept the undulatory theory; they do not prove that he believed the emission theory to give the true explanation. And yet the emission theory had done much. Book i. sect. xiv. of the 'Principia' treats of the motion of small particles acted on by forces tending towards a body of finite size. The earlier propositions show that if a particle approaching a plane surface be acted on by a force towards the surface, depending only on the distance between the particle and the surface, it will be reflected or refracted according to the known laws of light, and the scholium to prop. xcv. calls attention to the similarity between the particles and light. Such an explanation was first given in the paper of 1675 (BIRCH, *Hist. of Roy. Soc.* iii. 256). According to it the particles move more quickly in a dense medium, such as glass or water, than in air; whereas Arago's and Fresnel's experiments in 1819 proved the reverse to be the case, thus verifying Huyghens's views, and upsetting for ever the emission theory (*Œuvres Complètes de Fresnel*, i. 75). On approaching the surface of a reflecting body the luminous particles are acted on by forces which produce in some cases reflection, in others refraction.

But to explain why some of the incident light is reflected and some refracted Newton had to invent his hypothesis of 'fits of easy reflection and refraction.' These are described in the 'Optics,' book iii. props. xi., xii., and xiii., thus: 'Light is propagated from luminous bodies in time, and spends about seven or eight minutes of an hour in passing from the sun to the earth.' 'Every ray of light in its passage through any refracting surface is put into a certain transient constitution or state, which in the progress of the ray returns at equal intervals, and disposes this ray at every return to be easily transmitted through the next refracting surface, and between the returns to be easily reflected by it.' 'Defn. The return of the disposition of any ray to be reflected I will call its Fits of easy reflection, and those of its disposition to be transmitted its Fits of easy transmission, and the space it passes between every return and the next return the interval of its Fits. . . . The reason why the surfaces of all thick transparent bodies reflect part of the light incident on them and refract the rest is that some rays at their incidence are in their Fits of easy reflection, some in their Fits of easy transmission.'

Such a theory accounts for some or all of the observed facts. But what causes 'the fits of easy transmission'? Newton states that he does not inquire, but suggests, for those who wish to deal in hypotheses, that the rays of light striking the bodies set up waves in the reflecting or refracting substances which move faster than the rays, and overtake them. When a ray is in that part of a vibration which conspires with its motion, it easily breaks through the refracting surface, and is in a fit of easy transmission; and, conversely, when the motion of the ray and the wave are opposed, the ray is in a fit of easy reflection. But he was not always so cautious. 'Were I,' says he in the 'Hypothesis' of 1675, explaining the properties of light (BIRCH, *Hist. of Roy. Soc.* iii. 249), 'to assume an hypothesis it should be this: if propounded more generally so as not to determine what light is farther than that it is something or other capable of exciting vibrations in the æther.' 'First, it is to be assumed that there is an æthereal medium. In the second place it is to be supposed that the æther is a vibrating medium like air, only the vibrations far more swift and minute. . . . In the fourth place, therefore, I suppose light is neither æther nor its vibrating motion, but something of a different kind propagated from lucid bodies. To avoid dispute and make this hypothesis general, let every man take his fancy. Fifthly, it is to be supposed that light and æther mutually act upon one another.' It is from this action that reflection and refraction came about. To explain colour Newton supposes that the rays of light impinging on a reflecting surface excite vibrations of various 'bignesses' (waves of different length, we should say), and these, transmitted along the nerves to the brain, affect the sense with various colours according to their 'bigness,' the biggest with red, the least with violet. Thus 'Optics,' query 13 (ed. 1704): 'Do not several sorts of rays make vibrations of several bignesses which, according to their bignesses, excite sensations of several colours . . . and particularly do not the most refrangible rays excite the shortest vibrations for making a sensation of deep violet, the least refrangible the largest for making a sensation of deep red?'

The above is but a development of the reply to Hooke's criticism of 1672 (*Phil. Trans.* vii. 5086), in which Newton says: ''Tis true that from my theory I argue the Corporeity of Light, but I do it without any absolute positiveness, as the word perhaps intimates, and make it at most a very plausible consequence of the doctrine, and not a fundamental supposition.' 'Certainly' my hypothesis 'has a much greater affinity with his own than he seems to be aware of, the vibrations of the æther being as useful and necessary in this as in his.'

Thus Newton, while he avoided in the 'Optics' any declaration respecting the mechanism by which the 'fits of easy reflexion and transmission' were produced, had in his earlier papers developed a theory practically identical in many respects with modern views, though without avowedly accepting it. The something propagated from luminous bodies which is distinct from the ether and its vibratory motion is energy, which, emitted from those bodies, is carried by wave motion through the ether in rays, and, falling on a reflecting or refracting surface, sets up fresh waves, by which part of the energy is transmitted, part reflected. Light is not material, but Newton nowhere states that it is. In the 'Principia' his words are 'Harum attractionum haud multum dissimiles sunt Lucis reflexiones et refractiones,' and the scholium concludes with 'Igitur, ob analogiam quæ est inter propagationem radiorum lucis et progressum corporum, visum est Propositiones sequentes in usus Opticos subjungere; interea de naturâ radiorum, utrum sint corpora necne, nihil omnino disputans, sed Trajectorias corporum Trajectoriis radiorum persimiles solummodo determinans.'

No doubt Newton's immediate successors interpreted his words as meaning that he believed the corpuscular theory of light, conceived, as Herschel says (*Encycl. Metropolitana*, p. 439), 'by Newton, and called by his illustrious name, in which light is conceived to consist of excessively minute particles of matter projected from luminous bodies with the immense velocities due to light, and acted on by attractive and repulsive forces residing on the bodies on which they impinge.' Men learnt from the 'Principia' how to deal with the motion of small particles under definite forces; the laws of wave motion were less clear, and there was no second Newton to explain them. As Whewell states (*Inductive Sciences*, vol. ii. chap. x.), 'That propositions existed in the "Principia" which proceeded on this hypothesis was with many . . . ground enough for adopting the doctrine.' A truer view of Newton's position was expressed in 1801 by Young, who writes (*Phil. Trans.* 12 Nov.): 'A more extensive examination of Newton's various writings has shown me that he was in reality the first that suggested such a theory, as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works.'

The later editions of the 'Optics' contain some additional queries. The double refraction of Iceland spar had been discussed at a meeting of the Royal Society on 12 June 1689, at which Newton and Huyghens were present. Newton's views were first given in print in 1706 in the Latin edition of the 'Optics,' query 17. In the second English edition (1718) this became query 25. In this query Newton rejected Huyghens's construction for the extraordinary ray, and gave an erroneous one of his own. The succeeding queries expressed more definitely than elsewhere the view that rays of light are particles. Thus query 29: 'Are not rays of light very small bodies emitted from shining substances?' In the advertisement to the second edition Newton, in the case of a speculation about the cause of gravity, gave the reason for putting it in the form of a query, that he was 'not yet satisfied about it for want of experiments.'

Later in the year (1676) in which Newton's important optical papers were communicated to the Royal Society he began a correspondence on his methods of analysis with Leibnitz, through his friends Collins and Oldenburg, to which, at a later date, very great importance attaches in the celebrated controversy respecting the invention of fluxions. The correspondence with Leibnitz was continued to the summer of 1677, when the death of Oldenburg put a stop to it.

For the next two years (1678–9) we know little of Newton's life. He took part in various university functions. On 8 Nov. 1679 Charles Montagu, afterwards Lord Halifax, Newton's firm friend and patron, entered as a fellow commoner at Trinity College. In December 1679 he received a letter from Hooke, asking his opinion about an hypothesis on the motion of the planets proposed by M. Mallement de Messanges. His reply has only recently been discovered, though many pages were previously written as to its contents; it was bought by Dr. Glaisher for Trinity College at a sale at Messrs. Sotheby's in 1888, and is now in the library. In this letter Newton, after alluding briefly to M. Mallement de Messanges's theory, proceeds, in response to a request from Hooke for some philosophical communication, to suggest an experiment by which the diurnal motion of the earth could be verified, namely, 'by the falling of a body from a considerable height, which he alleged must fall to the eastward of the perpendicular of the earth moved' (Birch, *Hist. of Roy. Soc.* iii. 512). Newton's words are: 'And therefore it will not descend in the perpendicular AC, but, outrunning the parts of the earth, will shoot forward to the east side of the perpendicular, describing in its fall a spiral line ADEC.' A figure shows the path of the falling body relative to the earth from a point above the earth's surface down to the centre of the earth. The portion of the path above the earth does not differ much from a straight line slightly inclined to the vertical, but near the centre the path is drawn as a spiral, with one convolution closing into the centre. Writing to Halley at a later date (27 May 1686), Newton admitted that he had 'carelessly described the descent of the falling body in a spiral to the centre of the earth, which is true in a resisting medium such as our air is.' But Hooke, as will be seen in the sequel, seized upon this spiral curve as proof that Newton was ignorant of the true law of gravitation, and wrote explaining (*ib.* iii. 516) that the path 'would not be a spiral line, as Mr. Newton seemed to suppose, but an excentrical elliptoid [*sic*], supposing no resistance in the medium; but supposing a resistance, it would be an excentric ellipti-spiral.' He also called attention to the fact that the deviation would be south-east, which is right, and more to the south than to the east, which is wrong. After a short interval Hooke wrote again (6 Jan. 1680, manuscripts in Trinity College Library, in Hooke's hand): 'In the celestial motions the sun, earth, or central body are the cause of the attraction, and though they cannot be supposed mathematical points, yet they may be supposed physical, and the attraction at a considerable distance computed according to the former proportion from the centre;' while in a further letter (17 Jan. 1680, same manuscripts) he says: 'It now remains to know the properties of a curve line, not circular or concentrical, made by a central attracting power, which makes the velocity of descent from the tangent or equal straight motion at all distances in a duplicate proportion to the distance reciprocally taken. I doubt not that by your excellent method you will easily find out what that curve must be and its properties, and suggest a physical reason of the proportion. If you have had any time to consider of this matter a word or two of your thoughts will be very grateful to the Society, where it has been debated, and more particular to, sir, your very humble servant.' All these letters are printed in Ball's 'Essay on Newton's Principia,' 1893, p. 139.

Newton does not appear to have replied till 3 Dec. 1680, when, writing about another matter, he thanked Hooke for the trial he had made of the experiment (Edleston, *Cotes Corr.* p. 264). The correspondence ceased, but Hooke's letters and his statement that the motion would be elliptical had started Newton in a train of thought which resulted in the first book of the 'Principia.' 'This is true,' he says, writing to Halley on 14 July 1686 (App. to Rigaud's *Essay on the First Publication of the Principia*, p. 40), 'that his letters occasioned my finding the method of determining figures which when I had tried in the ellipsis, I threw the calculations by, being upon other studies, and so it rested for about five years, till upon your request I sought for that paper.' On 27 July (*ib.* p. 44) he wrote again, Hooke's 'correcting my spiral occasioned my finding the theorem by which I afterwards examined the ellipsis.'

Two episodes, says Dr. Glaisher in his bicentenary address, preceded the composition of the 'Principia.' One of these happened in 1665, when the idea of universal gravitation first presented itself to his mind. At that time too he knew that, at any rate approximately, and for great distances, the intensity of the gravitating force must depend upon the inverse square. The second episode was simultaneous, as we have just seen, with the correspondence with Hooke at the end of 1679 or early in 1680, when he discovered how to calculate the orbit of a body moving under a central force, and showed that if the force varied as the inverse square, the orbit would be an ellipse with the centre of force in one focus. But for five years no one was told of this splendid achievement, and it was not till August 1684 that Halley learnt the secret in Cambridge.

Halley's account of the matter is given in a letter to Newton (29 June 1686, *ib.* App. p. 35). 'And this know to be true, that in January 1684, I, having from the consideration of the sesquialterate proportion of Kepler concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came on Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and, falling in discourse about it, Mr. Hooke affirmed that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill-success of my own attempts, and Sir Christopher, to encourage the inquiry, said he would give Mr. Hooke or me two months' time to bring him a convincing demonstration thereof, and, besides the honour, he of us that did it should have from him a present of a book of 40 shillings. Mr. Hooke then said that he had it, but he would conceal it for some time, that others, trying and failing, might know how to value it when he should make it public. However, I remember that Sir Christopher was little satisfied that he could do it; and though Mr. Hooke then promised to show it him, I do not find that in that particular he has been as good as his word. The August following, when I did myself the honour to visit you, I then learned the good news that you had brought this demonstration to perfection; and you were pleased to promise me a copy thereof, which the November following I received with a great deal of satisfaction from Mr. Paget,' mathematical master at Christ's Hospital (BREWSTER, *Life of Newton*, i. 255; BALL, *Essay on the Principia*, p. 162).

In the later letter to Halley of 14 July 1686, part of which has been already quoted, Newton says that it was Halley's request which induced him to search for the paper in which he had solved the problem five years earlier, but which he had then laid aside. The original paper could not be found, but, 'not finding it,' Newton 'did it again, and reduced it into the propositions' shown to Halley by Paget. As soon as Halley had read them he paid another visit to Newton at Cambridge, and induced him to forward an account of his discoveries to the Royal Society. On 10 Dec. 1684 Halley informed the Royal Society 'that he had lately seen Mr. Newton at Cambridge, who had showed him a curious treatise, "De Motu," which upon Mr. Halley's desire was promised to be sent to the Society to be entered on their register.' A tract by Newton entitled 'Propositiones de Motu' was registered in the Royal Society archives in February 1685, with the date 10 Dec. 168$\frac{4}{5}$ affixed to the margin (see EDLESTON, *Cotes Corr.* n. 74–5, p. lv.)

This set of propositions (four theorems and seven problems) has been printed by Rigaud (*Historical Essay on Newton's Principia*, App. i.) and by Ball (*Essay on the Principia*, p. 35) from the Register of the Royal Society, vi. 218. Three other papers entitled 'Propositiones de Motu,' differing in many ways from that in the Royal Society Register, are among the Portsmouth MSS (viii. 5, 6, 7).

Meanwhile the subject of Newton's Lucasian lectures in the October term 1684 was also entitled 'De Motu Corporum;' these lectures are preserved in Newton's autograph in the Cambridge University Library (Dd. ix. 46). They must be carefully distinguished from the 'Propositiones' sent to the Royal Society, although some of the chief propositions are the same in both. The lectures 'De Motu' differ very little from the first ten sections of the published 'Principia,' of which they formed the first draft. Cotes refers to them in writing to Jones on 30 Sept. 1711 (*Newton and Cotes Correspondence*, ed. Edleston, p.209): 'We have nothing of Sir Isaac's that I know of in Manuscript at Cambridge, besides the first draught of his "Principia" as he read it in his lectures.'

Newton was away from Cambridge from February to April 1685. During that year, however, he made the third great discovery which rendered the writing of the 'Principia' possible. The discovery is referred to in the letter to Halley of 20 June 1686 (*ib.* p. 27): 'I never extended the duplicate proportion lower than to the superficies of the Earth, and before a certain demonstration I found last year have suspected that it did not reach accurately enough down so low.'

This demonstration forms the twelfth section of book i. of the 'Principia,' 'De Corporum Sphæricorum Viribus Attractivis.' According to Newton's views, every particle of matter in the universe attracts every other particle with a force which is inversely proportional to the square of the distance between them. 'Gravitatio in singulas corporis particulas æquales est reciproce ut quadratum distantiæ locorum a particulis' (*Principia*, bk. iii. prop. viii. cor. 2). The force between the earth and the moon is the resultant of the infinite number of forces between the particles of these bodies. Newton was the first to show that the force of attraction between two spheres is the same as it would be if we supposed, each sphere condensed to a point at its centre (*ib.* bk. iii. prop. viii.) Up to this time it had only been possible for him to suppose as Hooke had stated, that the theorems he had discovered as to motion were approximately true for celestial bodies, inasmuch as the distance between any two such bodies is so great, compared with their dimensions, that they may be treated as points.

But now these propositions were no longer merely approximate, save for the slight correction introduced into the simple theory by the fact that the bodies of the solar system are not accurately spherical. The explanation of the system of the universe on mechanical principles lay open to Newton, and in about a year from this time it was published to the world.

In the opinion of Professor Adams (bicentenary address of Dr. Glaisher) it was the inability to solve, previous to this date, the question of the mutual attraction of two spheres which led Newton to withhold so long his treatise on 'Motion,' and his proof that gravity extends to the moon. As soon as he mastered this problem he returned to the calculations respecting gravitation and the moon laid by in 1665, and of course he now used Picard's value for his length of a degree of latitude (PEMBERTON, *A View of Sir Isaac Newton's Philosophy*, Preface). The theorem which he had just found gave him the power of applying his analysis to the actual universe, and the problem became one of absorbing interest.

The 'Principia' was to consist of three books. The treatise 'De Motu,' enlarged in the autumn of 1685, forms the first book; the second book, 'being short,' was finished in the summer of 1685, it was written out for press next year (Newton to Halley, 20 June 1686, RIGAUD, *Essay on the First Publication of the Principia*, App. p. 29). The work of preparing his great discovery for publication thus proceeded with amazing speed. To quote again from Dr. Glaisher, 'the "Principia" was the result of a single continuous effort. Halley's first visit to Cambridge took place in August 1684, and by May 1686 the whole of the work was finished, with the exception of the few propositions relating to the Theory of Comets. It was therefore practically completed within 21 months of the day when Newton's attention was recalled to the subject of central forces by Halley. We know also, from a manuscript in Newton's handwriting in the Portsmouth collection, that, with the exception of the eleven propositions sent to Halley in 1684, the whole was completed within seventeen or eighteen months. The total interval from Halley's first visit to the publication of the book is less than three years.' The first book of the 'Principia' was exhibited at the Royal Society on 28 April 1686 (BIRCH, *Hist. of Roy. Soc.* iv. 479): 'Dr. Vincent presented to the society a manuscript treatise entitled "Philosophiæ Naturalis Principia Mathematica," and dedicated to the society by Mr. Isaac Newton, wherein he gives a mathematical demonstration of the Copernican hypothesis, and makes out all the phenomena of the celestial motions by the only supposition of a gravitation to the centre of the sun decreasing as the squares of the distances reciprocally. It was ordered that a letter of thanks be written to Mr. Newton, that the printing of his book be referred to the consideration of the council, and that in the meantime the book be put into the hands of Mr. Halley to make a report thereof to the council.' And on 19 May 1686 it was ordered (*ib.* iv. 484) that 'Mr. Newton's "Philosophiæ Naturalis Principia Mathematica" be printed forthwith in quarto in a fair letter; and that a letter be written to him forthwith to signify the Society's resolution, and to desire his opinion as to the print, volume, cuts, &c.' Halley, who was secretary, wrote on 22 May to Newton that the society 'resolved to print it at their own charge in a large quarto of a fair letter. . . . I am intrusted to look after the printing of it, and will take care that it shall be performed as well as possible.'

The minute of 19 May required the ratification of the council, and on 2 June it was ordered 'that Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it and printing it at his own charge, which he engaged to do' (*ib.* iv. 486). At the time the society were in difficulties for want of funds (RIGAUD, *Essay*, p. 34), and it appears that the council must have declined to undertake the risk of publication, and have left it to the generosity of Halley to provide for the cost.

But Halley had other difficulties to surmount. In his official letter to Newton of 22 May he felt bound to refer to the conduct of Hooke, who, when the manuscript was presented to the society, claimed to have first discovered the law of inverse squares, and to have communicated it to Newton in the correspondence with him in 1679. Hooke in 1671 (*ib.* App. p. 53; letter to A. Wood, *ib.* p. 37) had written on the attraction of gravitating power which all bodies have 'to their own centres, whereby they attract not only their own parts,' but 'all the other celestial bodies which are within the sphere of their activity.' In his 'Discourse on the Nature of Comets,' read to the Royal Society in the autumn of 1682, and printed among his posthumous works, Hooke, moreover, spoke of a gravitation by which the planets and comets are attracted to the sun, and he gave (p. 184) an ingenious hypothesis as to the cause of gravity: he supposed it due to pulsations set up in the ether by gravitating bodies, and attempted to show that on this hypothesis the law of the inverse square would follow; but all his ideas were vague and uncertain. Hooke's ingenuity was great, but he was quite incapable of conducting a piece of strict reasoning; the idea of the inverse square law had occurred to him as it had to Newton, Wren, and Halley, but he had given no proof of its truth. Hence Newton, when he received Halley's letter of 22 May, felt that Hooke's claims were small, and wrote at once, 27 May, giving his version of the events of 1679–80. This letter, which is of great importance, has only recently been printed (Ball, *Essay on Newton's Principia*, 1893, p. 155). A manuscript copy, in Hooke's handwriting, was purchased among a number of papers of Hooke by Trinity College in May 1888. Newton, in this newly recovered reply of 27 May 1686, wrote: 'I thank you for what you write concerning Mr. Hooke, for I desire a good understanding may be kept between us. In the papers in your hands there is no proposition to which he can pretend, for I had no proper occasion of mentioning him there. In those behind, where I state the system of the world, I mention him and others. But now we are upon this business, I desire it may be understood. The sum of what passed between Mr. Hooke and me, to the best of my remembrance, was this. He soliciting me for some philosophical communication or other, I sent him this notion, that a falling body ought, by reason of the earth's diurnal motion, to advance eastwards, and not fall to the west, as the vulgar opinion is; and in the scheme wherein I proposed this I carelessly described the descent of the falling body in a spiral to the centre of the earth, which is true in a resisting medium such as our air is. Mr. Hooke replied that it would not descend to the centre, but at a certain limit turn up again. I then made the simplest case for computation, which was that of gravity uniform in a medium non-resisting, imagining that he had learnt the limit from some computation, and for that end had considered the simplest case first, and in this case I granted what he contended for, and stated the limit as nearly as I could. He replied that gravity was not uniform, but increased in the descent to the centre in a reciprocal duplicate proportion of the distance from it, and that the limit would be otherwise than I had stated, namely, at the end of every entire revolution, and added that, according to his duplicate proportion, the motions of the planets might be explained and their orbs defined. This is the sum of what I remember; if there be anything more material or anything otherwise, I desire that Mr. Hooke would help my memory. Further, that I remember about nine years since Sir Christopher Wren, upon a visit Dr. Done and I gave him at his lodgings, discoursed of this problem of determining the Heavenly Motions upon philosophical principles. This was about a year or two before I received Mr. Hooke's letters. You are acquainted with Sir Christopher: pray know when and where he first learnt the decrease of the force in the duplicate ratio of the distance from the centre.' Halley called on Sir Christopher Wren, who replied that 'Mr. Hooke had frequently told him that he had done it, and attempted to make it out to him, but that he never was satisfied that his demonstrations were cogent' (Halley to Newton, 29 June 1686; Rigaud, *Essay on the First Publication of the Principia*, App. p. 36; Ball, *Essay on Newton's Principia*, p. 162).

Writing on 20 June 1686 (Rigaud, App. p. 30), Newton stated that the second book of his great work was nearly ready for press; 'the third I now design to suppress. Philosophy is such an impertinently litigious lady that a man had as good be engaged in lawsuits as have to do with her.' Fortunately for posterity, Halley prevented this. A letter announcing that the second book had been sent was read to the society on 2 March, and on 6 April 1687 the 'third book of Mr. Newton's treatise "De Systemate Mundi" was presented.'

The 'Principia' was published, but without a date, about midsummer 1687. The manuscript is kept at the Royal Society, but it is not in Newton's handwriting. For the completion and publication of the work the world owes, it should be explicitly acknowledged, an enormous debt to Halley. 'In Brewster's words, "it was he who tracked Newton to his College, who drew from him his great discoveries, and who generously gave them to the world." Newton never published anything of himself, and we may be certain that but for Halley the "Principia" would not have existed. He was the original cause of its being undertaken, and when, in consequence of Hooke's unfair claims, Newton would have suppressed the third book, it was his explanations and entreaties that smoothed over the difficulty and induced Newton to change his mind. He paid all the expenses, he corrected the proofs, he laid aside his own work in order to press forward to the utmost the printing, lest anything should arise to prevent the publication. All his letters show the most intense devotion to the work; he could not have been more zealous had it been his own' (GLAISHER).

After the publication of the 'Principia,' Newton took an active part in public affairs. In 1687 James II wished to force the university to confer the degree of M.A. on Alban Francis, a Benedictine monk, without the usual oaths. Newton, with the vice-chancellor and seven other delegates, attended before the ecclesiastical commission to represent the case for the university on 11 April. The vice-chancellor was deprived of his office and dignities, the other delegates sent home with the advice from Judge Jeffreys, 'Go! and sin no more, lest a worse thing come unto you' (MACAULAY, *History*, chap. viii.) In 1689 Newton was elected as a whig to represent the university in the Convention parliament. His chief work at this time seems to have been in persuading the university to accept the new government (*Thirteen Letters to Dr. Covel*, printed by Dawson Turner, 1848). He also became acquainted with John Locke. His friends at this time contemplated his appointment to the provostship of King's College; but this was found to be unstatutable, and rather later, 1691, he was spoken of as a candidate for the post of master of the Charterhouse. His correspondence with Locke about this period (LORD KING, *Life of Locke*) deals with some of his theological speculations. Dr. Edleston has printed (*Cotes Corr.* p. 273) an interesting paper from Newton to Bentley, who was then preparing the first Boyle lectures, giving directions as to the preliminary reading necessary to understand the 'Principia.' 'At the first perusal of my book it is enough if you understand the Propositions, with some of the Demonstrations which are easier than the rest. For when you understand the easier, they will afterwards give you light unto the harder.' Some letters to Flamsteed show that he was still working at the lunar theory, and in 1692 he drew up for Wallis two letters on fluxions (printed in WALLIS's *Works*, ii. 391–396), being the first account of the new calculus, now twenty-six years old, published by himself. Next year, 1693, there was some correspondence with Leibnitz on fluxions (RAPHSON, *History of Fluxions*, p. 119; EDLESTON, *Cotes Corr.* p. 276).

In 1693, Newton, as his letters at this time show, was in a very bad state of health (BREWSTER, *Life of Newton*, ii. 85, 132, &c.) A very exaggerated account of his illness was conveyed to Huyghens by a Scotsman named Colin, and was published by M. Biot in his life of Newton in the 'Biographie Universelle' (EDLESTON, *Cotes Corr.* App. p. lxi). Another story commonly referred to this period is that on coming from chapel one morning he found a number of his papers had been burned by a candle which he had left lighted on the table. Edleston and Brewster both assign this to an earlier date.

Throughout 1694 and 1695 Newton was very actively engaged in elaborating his lunar theory, and he held a long correspondence with Flamsteed relative to observations which he needed to complete that theory (BAILY, *Life of Flamsteed*, pp. 133–60; EDLESTON, *Cotes Correspondence with Newton*, n. 118 p. lxiv; BREWSTER, *Life of Newton*, ii. 115). The value and importance of his work on the subject have only recently been made known by Professor Adams's labours in connection with the Portsmouth collection. In a scholium in the second edition of the 'Principia' Newton states many of the principal results of the theory. The Portsmouth MSS. contain many of his calculations on the inequalities described in the scholium, and also a long list of propositions which were evidently intended to be used in a second edition, upon which it seems that Newton was engaged in 1694 (*Cat. of Newton MSS.* Pref. pp. xii, xiiii, App. p. xxiii). Another paper of probably the same date, printed for the first time in the appendix to the preface of the 'Catalogue,' deals with the problem of the solid of least resistance. In the 'Principia' he gives the solution without explaining how he obtained it. The paper in question is a letter to an Oxford friend, probably David Gregory, in which the principles employed are explained.

In a letter to Flamsteed, written in December 1694, Newton endeavoured to explain the foundations of his theory of atmospheric refraction, and a table of refractions by Newton was inserted by Halley in the 'Philosophical Transactions' for 1721. It was not known how this table was arrived at, but among the Portsmouth papers are the calculations for certain altitudes, and the method is explained: 'The papers show that the well-known approximate formula for refraction commonly known as Bradley's was really due to Newton' (*ib.* Pref. p. xv).

In 1695 the question of the reform of the currency was prominently before the nation (MACAULAY, *History*, chap. xxi.) Montagu, Newton's friend, was chancellor of the exchequer, and he, Somers the lord-keeper, Newton, and Locke met in frequent conference to discuss plans for remedying the evil without altering the standard. Montagu brought in a bill for the reform, which received the royal assent on 21 Jan. 1696. Meanwhile the wardenship of the mint became vacant, and Montagu on 19 March 1696 offered it to Newton, by whom it was accepted. The mint had been a nest of idlers and jobbers. 'The ability, the industry, and the strict uprightness of the great philosopher speedily produced a complete revolution throughout the department which was under his direction' (*ib.* chap. xxii.) Montagu's successful reform was aided to no small degree by the energy of the warden. 'Well had it been for the public,' says Haynes, 'had he acted a few years sooner in that situation' (see also RUDING, *Annals of the Coinage*). A letter to Flamsteed, which has given rise to much controversy, written in 1699, while the recoinage was in progress, may be mentioned here. In it Newton says: 'I do not love to be printed on every occasion, much less to be dunned and teased by foreigners about mathematical things, or to be thought by our own people to be trifling away my time about them when I should be about the king's business' (BAILY, *Life of Flamsteed*, p. 164; BREWSTER, *Life of Newton*, ii. 149; EDLESTON, *Cotes Corr.* n. p. lxi; MACAULAY, *History*, chap. xxii.) De Morgan, however, in opposition to Newton's other biographers, expresses regret that Newton ever accepted office under the crown, and suggests that from the time of his settling in London his intellect underwent a gradual deterioration. If, he says, after having piloted the country through a very difficult and, as some thought, impossible operation, 'he had returned to the university with a handsome pension' and his mind free to make up again to the 'litigious lady,' he would, to use his own words, have taken 'another pull at the moon;' and we suspect Clairant would have had to begin at the point from which Laplace afterwards began' (*Newton his Friend and his Niece*, p. 149).

In 1699 he became master of the mint, a member of the council of the Royal Society, and a foreign associate of the French Academy. Next year he appointed Whiston his deputy in the Lucasian chair, 'with the full profits of the place.' Whiston began his lectures on 27 Jan. 1701, and at the end of the year, when Newton resigned the professorship and his fellowship, he was elected to succeed him as professor. The same year Newton's 'Scala Graduum Caloris,' the foundation of our modern scale of temperature, was read (*Phil. Trans.* March and April). Newton had not represented the university in the parliament of 1690, but in November 1701 he was again elected, holding the seat till July 1702, when parliament was dissolved. The same year his 'Lunæ Theoria' was published in Gregory's 'Astronomy.' The following year (30 Nov. 1703) he was elected president of the Royal Society, and to this office he was annually re-elected for twenty-five years.

In February 1704 there appeared, appended to the 'Optics,' which was only then issued, two very important mathematical papers, most of which had been communicated to Barrow in 1668 or 1669. The one entitled 'Enumeratio Linearum Tertii Ordinis' (BALL, *Short Hist. of Math.* p. 346; *Trans. Lond. Math. Soc.* 1891, xxii. 104–43) was practically the same as the 'De Analysi per Equationes Numero Terminorum Infinitas' (first printed in 1711), the substance of which was communicated by Barrow to Collins in 1669. The second part of the appendix—the 'Tractatus de Quadratura Curvarum'—contains a description of Newton's method of fluxions.

In 1705 Newton, as president of the Royal Society, became involved in the difficulties relating to the publication of Flamsteed's observations, while some remarks in a review of the tract 'De Quadratura Curvarum,' published in the 'Acta Lipsica' 1 Jan. 1705, led to the controversy between Newton and Leibnitz on the priority of discovery of the fluxions.

These two controversies were pursued with much heat, and greatly embittered Newton's life for many years. That with Flamsteed lasted from 1705 to 1712; while that with Leibnitz lasted from 1705 until 1724.

Flamsteed was appointed astronomer royal (astronomical observator) in 1675, and began a correspondence with Newton about 1681 in the course of a discussion about the great comet of 1680—Halley's comet. He supplied Newton with valuable information of various matters during the preparation on the first edition of the 'Principia,' 1685–6 (*General Dictionary*, vii. 793). Their correspondence was renewed in 1691, when Newton urged Flamsteed to publish the observations he had accumulated during the past fifteen years. Flamsteed declined, and put down Newton's suggestions to Halley, with whom he had quarrelled (BAILY, *Life of Flamsteed*, p. 129). In 1694 when Newton

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was working at the lunar theory, he applied to Flamsteed for his observations, by aid of which he hoped to test his calculations. Flamsteed could not or would not understand the purpose for which Newton wanted the observations, and put difficulties in the way of communicating them. In 1694 Newton writes (p. 139): 'I believe you have a wrong notion of my method of determining the moon's motions. I have not been about making such corrections as you seem to suppose, but about getting a general notion of all the equations on which her motions depend.' Newton, on a visit to Flamsteed in September 1694, obtained a number of observations, but by no means all he needed, and during much of the early part of 1695 Newton's work was suspended while he was 'staying the time' of the astronomer royal. Again, 29 June 1695, Newton thanked Flamsteed for some solar tables, but wrote: 'These and almost all other communications will be useless to me unless you can propose some practicable way or other of supplying me with observations. . . . Pray send me first your observations for the year 1692.' Flamsteed replied with an offer of observations from 1679 to 1690, which Newton had not specially asked for. The correspondence ended 17 Sept. 1695, and Newton's work on the lunar theory was uncompleted (EDLESTON, *Cotes Corr.* p. lxiv, *n.* 117, &c.; BAILY, *Life of Flamsteed*, pp. 139 seq.; *Supplement*, p. 708). Leibnitz in a letter to Romer, 4 Oct. 1706, declared: 'Flamsteadus suas de luna observationes Newtono negaverat. Inde factum aiunt quod hic quædam in motu Lunari adhuc indeterminata reliquit.' Flamsteed's ill-health, bad temper, and extraordinary jealousy of Halley contributed to this unhappy result. Flamsteed continued to observe, and in 1703 made it known that he was willing to publish his observations 'at his own charge,' provided the public would defray the expense 'of copying his papers and books for the press.' Next year Newton, as president of the Royal Society, recommended the work to Prince George of Denmark, the husband of Queen Anne. The prince asked Newton and others to act as referees, and early in 1705 they drew up a report recommending the publication. The prince approved, and agreed to meet the expense.

Difficulties began in March 1705. Newton wished to have the observations printed in one order; Flamsteed preferred a different one. For two years Flamsteed, who had conceived an intense jealousy of Newton, pursued him with recriminations which only injured their author [see FLAMSTEED, JOHN]. The first volume was finished in 1707, and preparations made for printing the second. The referees insisted on receiving the copy for this volume before the printing commenced, and it was put into their hands, Flamsteed says, in a sealed packet, 20 March 1708, copied out on to 175 sheets. Subsequently, in 1712, Flamsteed declared that this 'imperfect copy' Newton 'very treacherously broke open' in his absence and without his knowledge; but in an earlier letter of 1711 Flamsteed himself rebutted this charge of bad faith by acknowledging that the papers were unsealed in his presence. In October 1708 Prince George died, and the printing was suspended. After three years it recommenced. In 1710 the Royal Society were made visitors of Greenwich Observatory, and on 21 Feb. 1711 the secretary, Dr. Sloane, was ordered to write to the astronomer royal for the deficient part of his 'Catalogue of the Fixed Stars,' then printing by order of the queen. Flamsteed angrily declared that the proof-sheets which had been sent to him contained many errors, and asserted at a meeting with Newton, Sloane, and Mead, October 1711, that he had been robbed of the fruit of his labours. Our only accounts of this interview are the three given by Flamsteed in his 'Autobiography,' or in his papers, in which the blame is all thrown on Newton. The referees proceeded to print, and made Halley editor. Flamsteed indulged in abuse directed largely against Newton, and finally determined to reprint his observations at his own expense. These he left almost ready for publication at the time of his death in 1719. They were published in 1725. Meanwhile the copy left with Newton, together with the first volume printed in 1707, was issued, as edited by Halley, in 1712. Before his death Flamsteed, through a change of government, obtained possession of the three hundred copies which were undistributed, and, taking from them that part of the first volume which had been printed under his own care, burned the rest.

The dispute with Leibnitz about the invention of the theory of fluxions was of longer duration, and was more bitterly contested. We have seen that the discovery was made by Newton during 1665 and 1666. His tract on the subject, 'De Quadratura Curvarum,' was, however, not printed till 1704 in an appendix to his 'Optics,' though the principles of the method were given in the 'Principia,' book ii. lemma ii. in 1687. They had been communicated in letters by Newton to Collins, Gregory, Wallis, and others from 1669 onwards.

Leibnitz had been in England in 1673, and had made the acquaintance of Collins and

Oldenburg. Next year he claimed to have arrived at 'methodos quasdam analyticas generales et late fusas, quas majoris facio quam Theoremata particularia et exquisita.' On his return to Paris he maintained through Oldenburg a correspondence with various English mathematicians, and heard of Newton and his great power of analysis. Thus he wrote, 30 March 1675 (*Comm. Epist.* p. 39): 'Scribis clarissimum Newtonium vestrum habere methodum exhibendi quadraturas omnes;' and a year later, May 1676, referring to a series due to Newton, 'ideo rem gratam mihi feceris, vir clarissime, si demonstrationem transmiseris.' Collins urged Newton to comply with Leibnitz's wishes, and Newton wrote, 13 June 1676, a letter giving a brief account of his method. This was read before the Royal Society on 15 June, and was sent to Leibnitz 26 July (*ib.* p. 49), together with a manuscript of Collins, containing extracts from the writings of James Gregory, and a copy of a letter, with a highly important omission, from Newton to Collins, dated 10 Dec. 1672, about his methods of drawing tangents and finding areas. Newton's example of drawing a tangent was omitted, as has been subsequently proved. Leibnitz replied to Oldenburg on 27 Aug. 1676, asking Newton to explain some points more fully, and giving some account of his own work. Newton replied through Collins on 24 Oct., expressing his pleasure at having received Leibnitz's letter, and his admiration of the elegant method used by him (*ib.* p. 67). He gives a brief description of his own procedure, mentioning his method of fluxions, which, he says, was communicated by Barrow to Collins about the time at which Mercator's 'Logarithmotechnia' appeared (i.e. in 1669). He does not describe the method, but added an anagram containing an explanation. This is not intelligible without the key, but Newton gives some illustrations of its use (see Ball, *Short Hist. of Math.*, 2nd ed. p. 328).

Leibnitz was in London for a week in October 1676, and saw Collins, who had not then received Newton's letter of 24 Oct., and there was some delay in forwarding it to Leibnitz. But on 5 March 1677 Collins wrote to Newton that it would be sent within a week, and on 21 June 1677 Leibnitz, writing to Oldenburg, acknowledged its receipt: 'Accepi literas tuas diu expectatas cum inclusis Newtonianis sane pulcherrimis.' He then proceeded to explain his own method of drawing tangents, 'per differentias ordinatarum,' and to develop from this the fundamental principles of the differential calculus with the notation still employed by mathematicians. A second letter followed from Hanover, dated 12 July 1677, and dealt with other points. The death of Oldenburg in September 1677 put a stop to the correspondence.

Collins had in his possession a copy of Newton's manuscript 'De Analysi per Æquationes,' containing a full account of his method of fluxions, which was published in 1711. Leibnitz, in a letter to the Abbé Conti, written in 1715, and published in Raphson's 'History of Fluxions,' p. 97, admits that 'Collins me fit voir une partie de son commerce.' He states that during his first visit he had nothing to do with mathematics, and in a second letter, 9 April 1716, he writes (Raphson, *History of Fluxions*, p. 106): 'Je n'ay jamais nié qu'à mon second voyage en Angleterre j'ai vu quelques lettres de M. N. chez Monsieur Collins, mais je n'en ay jamais vu où M. N. explique sa methode de Fluxions.'

Leibnitz's recent editor, Gerhardt, found, however, among the Leibnitz papers at Hanover, a copy of a part of the tract 'De Analysi' in Leibnitz's own handwriting. The copy contains notes by Leibnitz expressing some of Newton's results in the symbols of the differential calculus (Ball, *Short Hist. of Math.* p. 364; *Portsmouth Catalogue*, p. xvi). The date at which these extracts were made is important. They must, of course, have been taken from Newton's published edition of 1704, or else, as the Portsmouth MSS. prove that Newton suspected, Leibnitz must have copied the tract when in London in 1676. The last hypothesis seems the more probable.

Leibnitz published his differential method in the 'Acta Lipsica' in 1684.

Many of the results in Newton's 'Principia,' 1687, had been obtained by the method of fluxions, though exhibited in geometrical form, and the second lemma of book ii. concludes with the following scholium: 'In literis quæ mihi cum geometra peritissimo G. G. Leibnitio annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi Maximas et Minimas ducendi Tangentes et similia peragendi quæ in terminis Surdis æque ac in rationalibus procederet, et literis transpositis hanc sententiam involventibus [Data Æquatione quotcunque Fluentes quantitates involvente, Fluxiones invenire et vice versâ] eandem celarem; rescripsit Vir Clarissimus se quoque in ejusmodi methodum incidisse, et methodum suam communicavit a mea vix abludentem præterquam in verborum et notarum formulis. Utriusque fundamentum continetur in hoc Lemmate.'

In 1692 Newton's friends in Holland informed Wallis that Newton's 'notions [of fluxions] pass there with great applause by the name of "Leibnitz Calculus Differentialis."' Wallis was then publishing his works, and stopped the printing of the preface to the first volume to claim for Newton the invention of fluxions in the two letters sent by Newton to Leibnitz through Oldenburg 13 June and 24 Oct. 1676, 'ubi methodum hanc Leibnitio exponit tum ante decem annos nedum plures ab ipso excogitatum.' Newton wrote two letters to Wallis in 1692, giving an account of the method, and they appeared in the second volume of Wallis's 'Works' (1695).

The volumes were reviewed in the 'Acta Lipsica' for June 1696 (Leibnitz's periodical), and the reviewer found no fault with Wallis for thus claiming the invention for Newton ten years before, but expressed the view that it ought to have been stated, although he admitted that Wallis might possibly be unaware of the fact, that at the date of Newton's letter of 1676 Leibnitz had already constructed his calculus. Leibnitz's letter to Oldenburg, containing a description of his method, was written in 1677.

The matter rested thus till 1699, when Fatio de Duillier referred in a tract on the solid of least resistance to the history of the calculus. He stated that he held Newton to have been the first inventor by several years, 'and with regard to what Mr. Leibnitz, the second inventor of this calculus, may have borrowed from Newton, I refer to the judgment of those persons who have seen the letters and manuscripts relating to this business.' Leibnitz replied in the 'Acta Lipsica' in May 1700. He asserted that Newton had in his scholium in the 'Principia' acknowledged his claim to be an original inventor, and, without disputing or acknowledging Newton's claims of priority, asserted his own right to the discovery of the differential calculus. Duillier sent a reply to the 'Acta Lipsica,' but it was not printed.

Newton published his treatise on 'Quadratures' in 1704, as an appendix to the 'Optics.' In the introduction he repeated the statement already made by Wallis, that he had invented the method in 1665–6. Wallis was now dead (he died in 1703). A review of Newton's work, proved by Gerhardt to have been written by Leibnitz, and admitted by Leibnitz to be his in a letter to Conti, 9 April 1716, appeared in the 'Acta Lipsica' for January 1705. In this review (Raphson, *History of Fluxions*, pp. 103–4), the author wrote, after describing the differential calculus, 'cujus elementa ab inventore D. Godofredo Gullielmo Leibnitio in his actis sunt tradita.' 'Pro differentiis igitur Leibnitianis D. Newtonus adhibet semperque adhibuit fluxiones, iisque tum in suis Principiis Naturæ Mathematicis tum in aliis postea editis eleganter est usus; quemadmodum ut Honorarius Fabrius in sua Synopsi Geometrica motuum progressus Cavallerianæ methodo substituit.' Newton's friends took this as a charge of plagiarism of a particularly gross character. Newton had copied Leibnitz, so it was suggested, changing his notation, just as Fabri had changed the method of Cavalieri. Newton's own view of it (Brewster, *Life of Newton*, vol. ii. chap. xv.) was: 'All this is as much as to say that I did not invent the method of fluxions . . . but that after Mr. Leibnitz, in his letter of 21 June 1677, had sent me his differential method I began to use, and have ever since used, the method of fluxions.' Dr. Keill, Savilian professor, replied in a letter to Halley (*Phil. Trans.* 1708), in which he states that Newton was 'sine omni dubio' the first inventor: 'eadem tamen Arithmetica postea mutatis nomine et notatione modo a Domino Leibnitio in Actis Eruditorum edita est.' Newton was at first offended at this attack on Leibnitz, but, on reading Leibnitz's review, supported Keill's action. Leibnitz complained of the charge to the Royal Society, and requested them to desire Keill to disown the injurious sense his words would bear. In his letter to Sloane, the secretary, 4 March 1711, he writes: 'Certe ego nec nomen Calculi Fluxionum fando audivi nec characteres quos adhibuit Ds Newtonus his oculis vidi antequam in Wallisianis operibus prodiêre' (*Royal Society Letter-Book*, xiv. 273; Rix, *Report on Newton-Leibnitz MSS.* p. 18). Keill drew up a letter, read to the society on 24 May 1711, and ordered to be sent to Leibnitz, in which he explained that the real meaning of the passage was that 'Newton was the first inventor of fluxions, or of the differential calculus, and that he had given in the two letters of 1676 to Oldenburg, transmitted to Leibnitz, "indicia perspicacissimi ingenii viro satis obvia unde Leibnitius principia illius calculi hausit aut haurire potuit"' (*Comm. Epist.* p. 110). Leibnitz again appealed to the Royal Society, who appointed a committee to search old letters and papers, and report on the question. In his second appeal (*ib.* p. 118) Leibnitz accepted the view of the 'Acta Lipsica' as his own, stating that no injustice had been done to any party; 'in illis enim circa hanc rem quicquam cuiquam detractum non reperio, set potius passim suum cuique tributum' are his words. The committee reported on 24 April 1712, and the report was printed with the title 'Commercium Epistolicum D. Johannis Collins et aliorum de analysi promota.' The main points of the report were that Leibnitz had been in communication with Collins, 'who was very free in communicating to able mathematicians what he had received from Mr. Newton and Mr. Gregory;' that when in London Leibnitz had claimed Mouton's differential method as his own, and that until 1677, after he had heard from Newton, there is no evidence that he knew any other method; that Newton had invented the method of fluxions before July 1669; that the differential method is one and the same as the method of fluxions; 'and therefore,' the committee continued, 'we take the proper question to be not who invented this or that method, but who was the first Inventor.' They conclude that those who reckon Leibnitz as the first inventor did not know of Newton's correspondence with Collins. 'For which reasons we reckon Mr. Newton the first inventor, and are of opinion that Mr. Keill, in asserting the same, has been in no ways injurious to Mr. Leibnitz.' Leibnitz did not publicly reply. His reasons for this were given later in a letter to Conti on 9 April 1716, already quoted (RAPHSON, *History of Fluxions*, pp. 103, 105; BALL, *Short Hist. of Math.* p. 366): he would have to refer to old letters, and had not kept his papers; he had no leisure, being occupied by business of quite another character, and so on. He circulated, however, a loose sheet entitled 'Charta Volans,' containing a letter from an eminent mathematician, and his own notes on it. The letter attacked Newton, and expressed the opinion that it appeared probable that he had formed his calculus after seeing that of Leibnitz, and had taken some of its ideas from Hooke and Huyghens without acknowledgment. The eminent mathematician was Bernoulli (letter of Leibnitz to Count Bothmar des Maizeaux); but he, when pressed to explain or justify his charges, solemnly denied that he had written such a letter. The controversy still went on. Towards the end of 1715 the Abbé Conti, on receiving a letter from Leibnitz (RAPHSON, *History of Fluxions*, p. 97), tried to terminate it, and collated the various papers at the Royal Society. Newton was persuaded to write to Conti his views of the dispute (*ib.* p. 100) for transmission to Leibnitz, and Conti, in his covering letter to Leibnitz, wrote: 'From all this I infer that, if all digressions are cut off, the only point is whether Sir Isaac Newton had the method of fluxions or infinitesimals before you, or whether you had it before him. You published it first, it is true; but you have owned that Sir Isaac Newton had given many hints of it in his letters to Mr. Oldenburg and others. This is proved very largely in the "Commercium" and in the "Extract" of it. What answer do you give? This is still wanting to the public, in order to form an exact judgment of the affair' (BREWSTER, *Life of Newton*, vol. ii. chap. xx.) The 'Extract' referred to is a paper which was published in the 'Philosophical Transactions' for January 1716, and is entitled 'An Account of the Book entituled "Commercium Epistolicum."' Professor de Morgan (*Phil. Mag.* June 1852) gave strong reasons for believing that Newton was the author, and the Portsmouth papers confirm this view. Leibnitz's reply was sent to De Montmort in Paris, to be transmitted to Conti, on 9 April 1716. It is printed in Raphson's 'History of Fluxions,' pp. 103–10. Leibnitz concludes: 'Newton finit sa Lettre en m'accusant d'être l'aggresseur et j'ai commencé celle-ci en prouvant le contraire. . . . Il y a eu du mesentendu, mais ce n'est pas ma faute.' At the same time Bernoulli wrote a second anonymous attack on Newton, which he called 'Epistola pro eminente Mathematico Domino Joanne Bernoillio contra quemdam ex Anglia antagonistam scripta;' this was published, with alterations, by Leibnitz in the 'Acta' for July 1716. Keill replied in a letter to Bernoulli, which he closed with the words, 'Si pergis dicere quæ vis, audies quæ non vis.' Leibnitz died on 14 Nov. 1716. Newton shortly afterwards published a reply which had been in circulation for some time—it was written in May—to Leibnitz's letter of 9 April (see RAPHSON, *History of Fluxions*, p. 111). Soon afterwards the Abbé Varignon reconciled Newton and Bernoulli. A fresh edition of the 'Commercium' was published in 1725, with the review or extract already mentioned and notes. The notes, like the review, were by Newton.

Newton in 1721 modified in the third edition of the 'Principia' the scholium relating to fluxions, in which Leibnitz had been mentioned by name. Leibnitz and his friends had always held this scholium to be an acknowledgment of his claim to originality. Thus Biot says that 'Newton eternalised that right by recognising it in the "Principia" . . . while in the third edition he had the weakness to leave out . . . the famous scholium in which he had admitted the rights of his rival.' But this was not Newton's interpretation of the scholium; he regarded it, as Brewster says, as a statement of the simple fact that Leibnitz communicated to him a method which was nearly the same as his own, and in his reply to Leibnitz's letter of 9 April 1716 (RAPHSON, *History of Fluxions*, p. 122) we find Newton saying, 'And as for the Scholium . . . which is so much wrested against me, it was written, not to give away that lemma to Mr. Leibnitz, but, on the contrary, to assert it to myself.' And again (p. 115), writing of the same scholium, he says: 'I there represent that I sent notice of my method to Mr. Leibnitz before he sent notice of his method to me, and left him to make it appear that he had found his method before the date of my letter,' while in an unpublished manuscript, entitled 'A Supplement to the Remarks,' part of which is quoted by Brewster (*Life of Newton*, vol. ii. chap. xiv.), Newton explains that Leibnitz's silence in 1684 as to who was the author of the 'methodus similis' mentioned by him in his first paper on the calculus put on Newton himself 'a necessity of writing the scholium . . . lest it should be thought that I borrowed that lemma from Mr. Leibnitz.' In the Portsmouth papers there are various suggested forms for the new scholium (*ib.* vol. ii. chap. xiv.) In the end all reference to Leibnitz was omitted, and the scholium only contains a paragraph from the letter to Collins of 10 Dec. 1672, explaining that the method of tangents was a particular case or corollary of a general method of solving geometrical and mechanical problems.

The main facts of this controversy establish without any doubt that Newton's invention of fluxions was entirely his own. It is not so easy to decide how much Leibnitz owed to Newton.

Oldenburg clearly sent to Leibnitz on 26 July 1676, along with Newton's letter of the preceding 13 June giving a brief account of his method, a collection made by Collins from the writings of James Gregory, and a copy of part of a letter from Newton to Collins, dated 10 Dec. 1672, 'in quâ Newtonus se Methodum generalem habere dicit ducendi Tangentes, quadrandi curvilinea et similia peragendi.' The 'Commercium Epistolicum' and Newton himself assumed that the complete letter of 1672 was forwarded. It is, however, practically certain that the whole was not sent. The example of the method given by Newton was omitted. In Leibnitz's 'Mathematical Works,' published at Berlin in 1849, there are printed from manuscripts left by him the papers said to have been received by him from Oldenburg in 1676. In these, as in a draft by Collins known as the 'Abridgement,' preserved at the Royal Society (MSS. vol. lxxxi.), we find a list of problems from Newton's letter of 10 Dec. 1672, but not the example of the method of drawing a tangent which formed the second part of the letter. In the second edition of the 'Commercium' (p. 128), it is stated that a much larger 'Collectio' made by Collins, and also preserved at the Royal Society (MSS. vol. lxxxi.), was sent to Leibnitz, but there is no evidence of this, and it is almost certainly an error (EDLESTON, *Cotes Corr.* n. 35).

The papers in their possession bearing on the subject were in 1880 examined for the Royal Society by Mr. Rix, clerk of the society. They tend to prove that Leibnitz did not get that full information about Newton's method which Newton believed him to have derived from the letter of 1672.

But if Leibnitz had not seen the whole of that letter, there can be little doubt, especially after Gerhardt's discovery of Leibnitz's autograph copy of part of it at Hanover among his autograph letters, that Collins had shown him in 1676 the no less important manuscript 'De Analysi per Æquationes.' Dealing with the matter in the preface to the Portsmouth collection, Dr. Luard, Sir G. Stokes, Professor Adams, and Professor Liveing express the view 'that Newton was right in thinking that Leibnitz had been shown his manuscript' (the 'Tract de Analysi'). Mr. Ball (*Short Hist. of Math.* p. 366) comes to the same conclusion. Dr. Brewster, who wrote before Gerhardt's discovery, thought that Newton and Leibnitz borrowed nothing from each other. But it is almost certain that Leibnitz owed much to Newton, though the form in which he presented the calculus is, to quote Mr. Ball (*Short Hist. of Math.* p. 367), 'better fitted to most of the purposes to which the infinitesimal calculus is applied than that of fluxions.'

In the same year (1705) in which the two struggles with Flamsteed and Leibnitz respectively began, Newton was knighted by Queen Anne on the occasion of her visit to Cambridge (15 April), and a month later, 17 May, he was defeated in the university election. The tory candidates were successful with the cry of 'The church in danger;' it is said they were carried by the votes of the non-residents against the wishes of the residents (BREWSTER, *Life of Newton*, ii. 162). In 1709 the correspondence relative to the second edition of the 'Principia' commenced. Dr. Bentley had succeeded in the summer of 1708 in obtaining a promise to republish the work, and it was arranged that Roger Cotes, then a fellow of Trinity College, and the first Plumian professor, should edit the book.

The correspondence, which lasted till 1713, was printed, with notes and a synoptical view of Newton's life by Edleston, in 1850, and is of the greatest value to all students of Newton. Six letters on the velocity of effluent water, written by Cotes to Newton in 1710–11, are not printed by Edleston (*Cotes Corr.*), but are with the Portsmouth correspondence. The edition was not completed till 1713. Newton's various other duties contributed to cause the delay, though his friends were anxious to complete the work more rapidly. Thus (*Maccl. Corr.* i. 264, 16 March 1712) Saunderson, who succeeded Whiston as Lucasian professor in 1711, wrote: 'Sir Is. Newton is much more intent on his "Principia" than formerly, and writes almost every post about it, so that we are in great hopes to have it out of him in a very little time.'

In 1714 Newton was one of Bishop Moore's assessors at Bentley's trial (MONK, *Life of Bentley*, pp. 281–6), and the same year he gave evidence before a committee of the commons on the different methods of finding the longitude at sea (EDLESTON, *Cotes Corr.* lxxvi, *n.* 167). In 1716 Cotes died (*ib.* lxxi, *n.* 171). Newton is reported to have said on hearing of his death, 'If he had lived we might have known something.'

In 1717 and 1718 Newton presented reports to parliament on the state of the coinage. In 1724 he was engaged in preparing the third edition of the 'Principia,' which appeared, under the editorship of Pemberton, in 1726. He was laid up with inflammation of the lungs and gout in 1725, but was better after this for some time. However, he overtaxed his strength by presiding at a meeting of the Royal Society on 2 March 1727, and from this he never recovered. He died at Kensington on 20 March, in the eighty-fifth year of his age.

His body lay in state in the Jerusalem Chamber, and was buried in Westminster Abbey on 28 March 1727. A conspicuous monument, bearing a Latin inscription, was erected to his memory in the abbey in 1731. He was succeeded as master of the mint by his nephew by marriage, John Conduitt [q. v.] The family estate at Woolsthorpe went to John Newton, the heir-at-law, the great-grandson of Sir Isaac's uncle.

During the time of his residence in London Newton lived first in Jermyn Street, then for a short time at Chelsea, and afterwards in Haydon Square, Minories, in a house pulled down in 1852. From 1710 until 1727 in a large plain-built brick house (to which he added a small observatory) next Orange Street chapel in St. Martin's Street, Leicester Square. A Society of Arts tablet has been placed upon the front of the house.

At the time of his death there were living three children of his stepbrother, Benjamin Smith; three children of his stepsister, Marie Pilkington; and two daughters of his stepsister, Hannah Barton. These eight grandchildren of his mother became the heirs of his personal property, which amounted to 32,000*l.*, and they erected the monument in Westminster Abbey at a cost of 500*l.* His stepniece and heiress, Catherine Barton, married in 1717 John Conduitt, and her daughter married John Wallop, viscount Lymington, eldest son of John Wallop, first earl of Portsmouth; she was thus mother of John Wallop, second earl of Portsmouth. Through this marriage a number of Newton's manuscripts passed into the hands of the Earls of Portsmouth at Hurstbourne, and the scientific portion of them was presented to the university of Cambridge by the fifth Earl of Portsmouth in 1888; the rest remain at Hurstbourne. A full catalogue of the mathematical papers by Professors Adams and Stokes was published in 1888 ('A Catalogue of the Newton MSS.,' Portsmouth collection).

Professor Adams points out that the manuscripts show that Newton carried his astronomical investigations far further than Laplace supposed. Many theological and historical manuscripts which are in the Portsmouth collection are of no great value; some on chemistry and alchemy are of 'very little interest in themselves.' Newton left notes of chemical experiments made between 1678 and 1696. The most interesting relate to alloys.

Some of the papers left by Newton at his death dealing with theological and chronological subjects were afterwards published (BREWSTER, *Life of Newton*, vol. ii. chap. xxiii.) Leibnitz in 1710 had attacked Newton's philosophy, and in a letter written to the Princess of Wales in 1715 he made a number of charges against the religious views of the English. George I heard of the attack, and expressed a wish that Newton should reply, and he was thus brought into contact with the princess; in the course of conversation with her, he mentioned a system of ancient chronology composed by him when in Cambridge, and shortly afterwards gave her a copy. The Abbé Conti, under a strict promise of secresy, was allowed to take a copy of it. On his return to France Conti violated his promise and gave it to Freret, who wrote a refutation and then had it published without Newton's permission. Newton had neglected to answer two letters on the subject. The work was printed in 1725, and led to various discussions, in consequence of which Newton consented to prepare his complete work for the press. He died in 1727, however, before the preparation was complete, and the book was issued by Pemberton in 1728 under the title of 'The Chronology of Ancient Kingdoms Amended.' The book contains an attempt to determine the dates of ancient events from astronomical considerations. Its positive results are not of great importance, chiefly because Newton was not in a position to distinguish between mythical and historical events. Thus great attention is paid to the date of the Argonautic expedition. Newton, however, indicates the manner in which astronomy might be used to verify the views on the chronological points derived in the main from Ptolemy, which were held in his time. These views have since that date been proved, by the Babylonish and Egyptian records, to be on the whole correct. Another chronological work is entitled 'Considerations about rectifying the Julian Calendar.'

Newton's theological writings were begun at an early period of his life. An account of them will be found in Brewster's 'Life,' vol. ii. chap. xxiv. Some of them passed from Lady Lymington to her executor, and thence into the hands of the Rev. J. Ekins, rector of Little Sampford, Essex. Newton was known previous to 1692 as an 'excellent Divine' (*Pryme's MSS.*), and from 1690 onwards corresponded with Locke on questions relating to the interpretation of prophecy and other theological speculations. M. Biot endeavours to connect some of these writings with the serious illness of 1693, but without much success.

In 1690 he sent to Locke his 'Historical Account of Two Notable Corruptions of the Scriptures,' dealing with the texts 1 John v. 7: 'For there are three that bear record in heaven, the Father, the Son, and the Holy Ghost, and these three are one;' and 1 Timothy iii. 16: 'Great is the mystery of godliness, God manifested in the flesh.' With regard to the first text, Hort (*New Testament Appendix*, p. 104) states that it is certainly an interpolation: 'There is no evidence for the inserted words in Greek or in any language but Latin before cent. xiv. . . . The words occur at earliest in the latter part of cent. v.' They appear to have been unknown to Jerome, and were omitted by Luther in the last edition of his 'Bible,' though they were afterwards restored by his followers. They were also omitted by Erasmus in his first two editions, but inserted in the edition of 1522. They were discussed by Simon in 1689, and by Bentley in a public lecture.

Newton was of the same opinion as these divines, and argued for the omission of the words. In the second text, 1 Timothy iii. 16, Newton maintained that the word θεὸς was a corruption effected by changing ὅ, which he supposed to be the correct reading, into θε. The correct reading is almost certainly ὃς, not ὅ. Hort says 'that there is no trace of θεὸς till the last third of cent. iv.' Newton placed its introduction at a later date.

Newton's design in writing to Locke was that he should take the manuscript to Holland and have it translated into French and published there. Locke's contemplated journey was put off, and he sent the manuscript, but without Newton's name, to Le Clerc, who undertook to translate and publish it. Newton, who was not at once informed that the manuscript had been sent, and, knowing that Locke had not gone, supposed that the matter had been dropped, changed his mind when he was told of Le Clerc's wishes, and stopped the publication. Le Clerc deposited the manuscript in the library of the Remonstrants, and a copy was published in an imperfect form in 1754. A genuine edition appeared in vol. v. of Horsley's 'Newtoni Opera,' 1779–85. It was reprinted in 1830, in support of the Socinian system, and the views expressed in it have been quoted as proving Newton to be an anti-Trinitarian. They can hardly be pressed so far; they are rather the strong expression of his hostility to the unfair manner in which, in his opinion, certain texts had been treated with a view to the support of the Trinitarian doctrine.

A third work, first printed in 1733, is entitled 'Observations upon the Prophecies of Daniel and the Apocalypse.' In it an interpretation is given of Daniel's dreams, and the relation of the Apocalypse to the Books of Moses and to the prophecy of Daniel is considered.

A bibliography of Newton's works, together with a list of books illustrating his life and works, was published by G. J. Gray in 1888. This contains 231 entries. To these some ten additions have been made in the interleaved copy in Trinity College Library. The only collected edition of his works is that by Samuel Horsley (five vols. 4to, 1779–85), and this is not complete. Some of his mathematical works were reprinted by Castillon at Lausanne in 1744. Of the 'Principia' three editions appeared in England in Newton's lifetime, the last, edited by Pemberton, being published in 1726. Editions were published at Amsterdam in 1714 and 1723. Pemberton's edition was reprinted in facsimile at Glasgow by Sir William Thomson (Lord Kelvin) and Professor Blackburne in 1871. In 1739–42 Le Sueur and Jacquier's edition appeared at Geneva. The 'Principia' was translated into English by Motte in 1729, and a second edition of Motte's translation, revised by W. Davis, was printed in 1803. Various editions of particular sections have appeared. The one chiefly used at Cambridge is that of book i. sections i–iii., by Percival Frost, 1854; 4to edition, 1883. There are numerous works illustrating and commenting on the 'Principia.' Brougham and Routh published an 'Analytical View' in 1855. Dr. Glaisher's bicentenary address (*Cambridge Chronicle*, 20 April, 1888) has been often referred to above, and is specially important as containing Professor Adams's view on various points.

The 'Optics' first appeared in English in 1704, with the two tracts 'Enumeratio Linearum tertii Ordinis' and 'Tractatus de Quadratura Curvarum.' It was translated into Latin in 1706 by Samuel Clarke. A second English edition without the tracts appeared in 1718; a third in 1721; and a fourth, 'corrected by the author's own hand, and left before his death with the bookseller,' in 1730.

The 'Optical Lectures read in the Publick Schools of the University of Cambridge, Anno Domini, 1669,' were first printed in English in 1728, and in Latin in 1729. The tract 'Enumeratio' closely resembled the famous 'De Analysi per Æquationes,' which was first published in 1711, and was edited by William Jones. Newton's method of fluxions appeared in an English translation made by John Colson from an unpublished Latin manuscript under the title, 'Method of Fluxions and Infinite Series,' in 1736 [cf. HODGSON, JAMES]. This was translated into French by M. de Buffon in 1740. The more important of the works written in connection with the dispute with Leibnitz have been already quoted. Biot and Lefort's edition of the 'Commercium Epistolicum' of 1856 contains additional information. The 'Arithmetica Universalis' first appeared in 1707, edited by Whiston.

The personal reminiscences of Newton are not very numerous. He was not above the middle size. According to Conduitt, 'he had a very lively and piercing eye, a comely and gracious aspect, with a fine head of hair as white as silver.' Bishop Atterbury, however, does not altogether agree with this. 'Indeed,' he says, 'in the whole air of his face and make there was nothing of that penetrating sagacity which appears in his compositions.' 'He never wore spectacles,' says Hearne, 'and never lost more than one tooth to the day of his death.' In money matters he was very generous and charitable. In manners his appearance was usually untidy and slovenly. There are many stories of his extreme absence of mind when occupied with his work. In character he was most modest. 'I do not know what I may appear to the world' were his words shortly before his death, 'but to myself I seem to have been only like a boy playing on the seashore, 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' (SPENCE, *Anecdotes*, quoting Chevalier Ramsay, p. 54). Bishop Burnet speaks of him as the 'whitest soul' he ever knew. At the same time, as Locke points out, he was a little too apt to raise in himself suspicions where there was no ground for them. In the controversies with Hooke, Flamsteed, and Leibnitz, he does not appear as a generous opponent; he was himself transparently honest, and anything in an adversary which appeared to him like duplicity or unfair dealing aroused his fiercest anger. De Morgan, who has taken a severer view of his actions in these controversies than his other biographers, says that 'it is enough that Newton is the greatest philosopher, and one of the best of men: we cannot find in his character an acquired failing. All his errors are to be traced to a disposition which seems to have been born with him. . . . Admitting them to the fullest extent, he remains an object of unqualified wonder, and all but unqualified respect.'

An estimate of his genius is impossible. 'Sibi gratulentur mortales tale tantumque extitisse Humani generis Decus' are the words on his monument at Westminster, while on Roubiliac's statue in Trinity College chapel the inscription is 'Newton qui genus humanum ingenio superavit.' All who have written of him use words of the highest admiration. On a tablet in the room in which Newton was born at Woolsthorpe manor-house is inscribed the celebrated epitaph written by Pope:

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

Laplace speaks of the causes 'which will always assure to the "Principia" a pre-eminence above all the other productions of the human intellect.' Voltaire, who was present at Newton's funeral, and was profoundly impressed by the just honours paid to his memory by 'the chief men of the nation,' always spoke of the philosopher with reverence—'if all the geniuses of the universe assembled, he should lead the band' (MARTIN SHERLOCK, *Letters from an English Traveller*, 1802, i. 98–108). 'In Isaac Newton,' wrote Macaulay in his 'History' (i. 195), 'two

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s 'History' (i. 195), 'two

kinds of intellectual power which have little in common, and which are not often found together in a very high degree of vigour, but which are nevertheless equally necessary in the most sublime department of physics, were united as they have never been united before or since. . . . In no other mind have the demonstrative faculty and the inductive faculty co-existed in such supreme excellence and perfect harmony.'

Among the portraits of Newton the chief are: In the possession of Lord Portsmouth, Hurstbourne Priors, not damaged at the fire in 1891, (1) in the hall, head signed G. Kneller, 1689; (2) in the billiard-room, head by Kneller, 1702; (3) in the library, head by Thornhill. In the possession of Lord Leconfield, Petworth House, (4) head by Kneller. In the possession of the Royal Society, (5) in the meeting-room, over the president's chair, portrait by Jervas, given in 1717 by Newton; (6) in the library, portrait by Vanderbank, 1725, given by Vignolles in 1841; (7) portrait by Vanderbank, given by M. Folkes, P.R.S. In the possession of Trinity College, Cambridge, (8) in the drawing-room of the lodge, portrait by Thornhill, 1710, given by Bentley; (9) in the drawing-room of the lodge, portrait given by Sam Knight in 1752; (10) in the dining-room of the lodge, head by Enoch Seeman, given by Thomas Hollis; (11) in the college hall, full-length portrait by Ritts, 1735, given by R. Gale, probably taken from Thornhill's picture, No. 8; (12) in the large combination-room, portrait given in 1813 by Mrs. Ring of Reading, whose grandmother was Newton's niece; (13) in the small combination-room, portrait by Vanderbank, 1725 (?), given by R. Smith, 1760; (14) in library, portrait by Vanderbank (taken at the age of eighty-three, after the publication of the third edition of the 'Principia'), purchased by Trinity College in 1850. In the Pepys collection there is a drawing, probably from Kneller's portrait (No. 1).

Many of the above have been engraved. The engraving which is best known is one of No. 4 by J. Smith in 1712. This was done again by Simon 1712, Faber, Esplen 1743, and Fry. The engraving from the picture in the Pepys collection is also well known. The Vanderbank portrait of 1725 was engraved by Vertue in 1726, A. Smith, and Faber. There is a mezzotint by MacArdell, 1760, of Enoch Seeman's picture, and an engraving by T. O. Barlow of the Kneller picture of 1689 (No. 1 above).

A very beautiful statue by Roubiliac was given to Trinity College by the master, Dr. Robert Smith, in 1750, and is now in the ante-chapel. Wordsworth in his 'Prelude' (bk. iii.) detected in Newton's 'silent face,' as depicted in this work of art,

The marble index of a mind for ever
Voyaging through strange seas of Thought, alone.

There is also a bust by Roubiliac, 1751, in Trinity College Library, and a cast of Newton's face, taken, in the opinion of competent judges, during life. The Royal Society and Trinity College possess other interesting relics. Copies of the bust exist at Bowood Park, and elsewhere.

[The most complete life of Newton is that by Sir D. Brewster, Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton, 1855; 2nd ed. 1860. Materials for a life collected by Conduitt are among the Portsmouth MSS. By far the most valuable collection of facts relating to him is the Synoptical View of Newton's Life contained in Newton's correspondence with Cotes, edited by Edleston in 1850. Shorter notices have been published by Biot, Biographie Universelle, translated in the Library of Useful Knowledge, 1829, and by De Morgan, Knight's Portrait Gallery, 1846. An Eloge de M. le Chevalier Newton was written by Fontenelle in 1728, partly from materials collected by Conduitt. This and the account given in Turnor's collection for the History of the Town and Soke of Grantham, 1806, are based on a sketch drawn up by Conduitt soon after Newton's death. Pemberton's View of Sir Isaac Newton's Philosophy, 4to, 1728, is interesting as being the account of a near friend, and Rigaud's Historical Essay on the first publication of Sir I. Newton's Principia abounds with important and accurate information. Maclaurin's Account of Sir I. Newton's Philosophical Discoveries, 1775, should be mentioned. Ball's Short History of Mathematics, Cambridge, 1893, contains a valuable account of Newton's mathematical writings; while Ball's Essay on Newton's Principia, Cambridge, 1893, gives a full account of the writing of the Principia, and contains several letters not previously printed. In addition to the works already mentioned important collections of letters are to be found in Raphson's History of Fluxions, 1715; Rigaud's Correspondence of Scientific Men, reprinted from originals in the possession of the Earl of Macclesfield, Oxford 1841; Leibnitz's Math. Schriften, Berlin, 1849; Baily's Life of Flamsteed, London, 1835; Des Maizeaux' Recueil de diverses pièces sur la Philosophie, &c., Amsterdam, 1720; 2nd ed. 1740; and Birch's History of the Royal Society, 1756; Spence's Anecdotes, 1820; Stukeley's Memoirs (Surtees Soc.)]

R. T. G.

NEWTON, JAMES (1670 ?–1750), botanist, born probably about 1670, graduated M.D., and subsequently, according to Noble, kept a private lunatic asylum near Islington turnpike (*Biogr. Hist. of England,*