

ESSAYS ON THE LIFE  
AND WORK OF NEWTON

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ON THE  
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NEWTON

BY  
AUGUSTUS DE MORGAN

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CHICAGO AND LONDON  
THE OPEN COURT PUBLISHING COMPANY

1914

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ESSAYS  
ON THE  
LIFE AND WORK OF  
NEWTON

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## EDITOR'S PREFACE

AUGUSTUS DE MORGAN'S biographical sketch entitled "Newton" appeared in *The Cabinet Portrait Gallery of British Worthies*<sup>1</sup> in 1846, and is the first essay printed in the present volume. It was, as Mrs De Morgan<sup>2</sup> said, "after Baily's *Life of Flamsteed*,<sup>3</sup> the first English work in which the weak side of Newton's character was made known. Justice to Leibniz, to Flamsteed, even to Whiston, called for this exposure; and the belief that it was necessary did not lower the biographer's estimate of Newton's scientific greatness, and of the simplicity and purity of his moral character. Francis Baily's discovery of the correspondence between the Rev. John Flamsteed, the first Astronomer Royal, and Abraham Sharp, as well as between Newton, Halley, and Flamsteed, on the publication of Flamsteed's catalogue of stars, had thrown a new light

<sup>1</sup> Vol. xi, London, 1846, pp. 78-117. This series was edited by Charles Knight. A three-columned quarto edition in one volume, and giving no editorial credit, was published in London by Henry G. Bohn in 1853 under the title *Old England's Worthies: A Gallery of Portraits*. Besides the small woodcut portraits, it contains twelve full-page "illuminated engravings." De Morgan's "Newton" occupies pp. 220-224 of this edition.

<sup>2</sup> *Memoir of Augustus De Morgan*, London, 1882, p. 256.  
<sup>3</sup> London, 1835.

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on the character of Newton. It appeared that the practical astronomer had been treated ungenerously by Newton, who failed to observe the conditions of publication agreed to by all parties; and afterwards, when remonstrated with, omitted the name of Flamsteed in places where it had formerly stood in the earlier editions of the *Principia*."

"My husband," adds Mrs De Morgan, "entered into the enquiry with keen interest, and with a power of research possible only to one who was fully master of the history of mathematical discovery." And it is not only mathematical discovery and controversy that De Morgan treats in the just, broad-minded, and high-minded way that is characteristic of him. He disclaimed any particular interest in those religious beliefs of Newton which he discussed so thoroughly; still, "notwithstanding this disclaimer," says Mrs De Morgan,<sup>1</sup> "I believe my husband felt more interest in the question, from its own nature, than he was himself aware of. Whether I am mistaken in this may be surmised by those who have read his own letter to his mother in this volume.<sup>2</sup> He says, 'Whatever Newton's opinions were, they were the result of a love of truth, and of a cautious and deliberate search after

<sup>1</sup> *Op. cit.*, p. 260. Cf. pp. 260-261, and § XI. of the first essay printed below.

<sup>2</sup> This letter of De Morgan's to his mother, which is printed in the *Memoir*, is on pp. 139-144 and there is no mention of Newton in it. The passage, however, occurs towards the end of § XI. of De Morgan's biography of Newton printed below.

it.' That Newton was a firm believer in Christianity as a revelation from God is very certain, but whether he held the opinions of the majority of Christians on the points which distinguish Trinitarians from Arians, Socinians, and Humanitarians, is the question of controversy."

The second of De Morgan's Essays printed in this volume concerns the great controversy about the invention of the fluxional or infinitesimal calculus, in which Newton and Leibniz were the principals. The essay printed is from the *Companion to the Almanac*, and is now extremely rare. It is of great interest and importance both on account of the fairness and vigour which De Morgan always showed in the defence of Leibniz against the imputations of Newton and the Royal Society, and because it first introduced the English public to Gerhardt's important discovery of Leibniz's manuscripts showing his gradual discovery of the calculus in 1673-1677. This essay also contains a summary of much of De Morgan's historical work on the controversy. In January 1846, a paper by De Morgan, "On a point connected with the Dispute between Keill and Leibnitz about the Invention of Fluxions," was read to the Royal Society, and it was afterwards printed in the *Philosophical Transactions*.<sup>1</sup>

<sup>1</sup> *Phil. Trans.*, 1846, pp. 107-109. This paper was wrongly stated in Mrs De Morgan's *Memoir* (pp. 257, 402, 406) to be printed in the *Transactions of the Cambridge Philosophical Society*. On the subject of this paper, see the second appendix to the third essay.

In 1848, De Morgan published a paper "On the Additions made to the Second Edition of the *Commercium Epistolicum*,"<sup>1</sup> and in 1852 published his three other contributions<sup>2</sup> to the literature of the fluxional controversy: "A Short Account of Recent Discoveries in England and Germany relating to the Controversy on the Invention of Fluxions,"<sup>3</sup> "On the Authorship of the 'Account of the *Commercium Epistolicum*,' published in the *Philosophical Transactions*"<sup>4</sup> and "On the Early History of Infinitesimals in England."<sup>5</sup> Where it seems advisable, notes have been added to the second essay below, giving an account of De Morgan's and others' work on the subject.

To this second essay I have added an appendix, the chief aim of which is to give the sources at which the original manuscripts written by Newton and Leibniz when they were discovering their respective calculuses may be found. This has not been done hitherto, and it is all the more necessary that it should be done, as modern authors, such as Moritz Cantor in his monumental *Vorlesungen über Geschichte der Mathematik*, neglect the fact that any early manuscripts of Newton's on fluxions are extant,

<sup>1</sup> *Phil. Mag.* (3), vol. xxxii, 1848, pp. 446-456.

<sup>2</sup> Besides the articles "*Commercium Epistolicum*" and "Fluxions" in the *Penny Cyclopædia*.

<sup>3</sup> *Companion to the British Almanac of the Society for the Diffusion of Useful Knowledge for . . . 1852*, London, pp. 5-20.

<sup>4</sup> *Phil. Mag.* (4), vol. iii, 1852, pp. 440-444.

<sup>5</sup> *Ibid.*, vol. iv, 1852, pp. 321-330.

or that some have been published—by Rigaud, for example—and some still remain unpublished.

In 1855 appeared Sir David Brewster's *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*,<sup>1</sup> and De Morgan, in a critique of this work in the *North British Review*,<sup>2</sup> showed clearly that Sir David had fallen into hero-worship. Here the faults of Newton are pointed out with an unwavering finger, and the merits of Leibniz are recognised and his character defended against Brewster more at length than in De Morgan's biography of Newton.

Biot, who had been a worshipper of Newton early in the century, wrote to De Morgan at the time, expressing his satisfaction and concurrence in the statements of the *North British Review*.<sup>3</sup>

This review is printed below as the third of De Morgan's Essays on Newton. I have added two appendices to this third essay: the first is part of a biography of Leibniz which De Morgan wrote, and which illustrates a laudatory reference to that great man in the third essay; the second is an extract from a later work of De Morgan's, which deals with Newton's character and the relation to it of the Royal Society down to De Morgan's own times.

Numerous notes of either a bibliographical, ex-

<sup>1</sup> Two vols., Edinburgh, 1855. See the first note to the first essay.

<sup>2</sup> Vol. xxiii, August 1855, pp. 307-338. This review is unsigned, but De Morgan was the author (cf. *Newton: his Friend: and his Niece*, London, 1885, p. 137; Mrs De Morgan, *op. cit.*, p. 261).

<sup>3</sup> Mrs De Morgan, *op. cit.*, p. 263.

planatory, or critical nature have been added to all the essays, but all that is not De Morgan's is put in square brackets. Such notes have become necessary, and it is hoped that the present ones will reply to all the calls of necessity and will make the book both useful and complete. Very little has to be criticised in De Morgan's history or conclusions. Like everything he wrote, these essays of his are marked by scrupulous care, sanity of judgment, and wide reading; and one hardly knows which to admire most: the breadth or the height of his mind.

Several minor structural alterations have been made: the first and third essays have been split into sections to facilitate reading and reference; the names of Huygens and Leibniz have throughout had their spelling altered from "Huyghens" and "Leibnitz" except in the titles of books and actual quotations.<sup>1</sup> Leibniz always signed himself as "Leibniz," but I have always cited the titles of books as they were printed, even though misspellings may have occurred there. This seems quite indispensable for convenience in reference.

The frontispiece is from an engraving by E. Scriven of Vanderbank's portrait of Newton in the possession of the Royal Society of London. An engraving from this picture accompanied the original

<sup>1</sup> The spelling "Leibnitz" even in titles of books where "Leibniz" is written is one of the faults in Gray's *Bibliography*.

of De Morgan's biographical sketch; but the present frontispiece is from a much finer engraving prefixed to the biography of Newton in the first volume of *The Gallery of Portraits: with Memoirs*.<sup>1</sup>

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<sup>1</sup> London, 1833, pp. 79-88. On the portraits of Newton, cf. Samuel Crompton, *Proc. Lit. and Phil. Soc. of Manchester*, vol. vi, 1866-7, pp. 1-7.

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CONTENTS

I  
NEWTON  
(1846)

## NEWTON

A BIOGRAPHY of Newton, intended for such a collection as this, must necessarily be much condensed; the account of his discoveries must be little more than allusion, and a perfect list of his writings and their editions is out of the question. The only Life which exists on any considerable scale (as justly remarked by the author) is that by Sir David Brewster in the "Family Library" (No. 24): this will be our chief reference on matters of fact.<sup>1</sup> On

<sup>1</sup> [The fullest life of Newton that has appeared was published after this biography (1846) by De Morgan, and was also written by Sir David Brewster under the title *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 2 vols., Edinburgh, 1855. A second edition—apparently unaltered, even as to the mistakes—was issued at Edinburgh in 1860. De Morgan's famous but scarce review (1855) of this work is reprinted below as the third of these Essays. An extremely valuable "Synoptical View of Newton's Life" was prefixed to J. Edleston's *Correspondence of Sir Isaac Newton and Professor Cotes*, . . . (London and Cambridge, 1850). The earlier biographies of Newton were as follows: J. B. Biot, "Newton," *Biographie Universelle*, 1794; substantially translated into English as *Life of Sir Isaac Newton* (by Lord Brougham), in the "Library of Useful Knowledge," 1829; Sir David Brewster, *Life of Sir Isaac Newton*, "Family Library," No. 24, 1831 (revised by W. T. Lynn in 1875); De Morgan, "Newton," *Penny Cyclopædia*, 1840; Fontenelle's *Eloge de Monsieur le Chevalier Newton*, 1728, translated into English in the same year; and Benjamin Martin in *Biographia Philosophica*, 1764. For biographies of Newton, see also G. J. Gray, *A Bibliography of the Works of Sir Isaac Newton*, Cambridge, second edition, 1907 (the first was published in 1888), pp. 70-76.

Various aspects of Newton's work have been dealt with in, for

those of opinion, particularly as to the social character of Newton, we must differ in some degree from our guide, as well as from all those (no small number) whose well-founded veneration for the greatest of philosophical inquirers has led them to regard him as an exhibition of goodness all but perfect, and judgment unimpeachable. That we can follow them a long way will sufficiently appear in the course of this sketch.

## I

Isaac Newton was born at Woolsthorpe, near Grantham, in Lincolnshire, on Christmas Day, 1642:<sup>1</sup> a weakly and diminutive infant, of whom it is related that, at his birth, he might have found room in a quart mug. He died on March the 20th, 1727, after more than eighty-four years of more than average bodily health and vigour; it is a proper pendant to the story of the quart mug to state that he never lost more than one of his second teeth. His father, Isaac Newton, though lord of the poor

example, (1) Stephen Peter Rigaud, *Historical Essay on the First Publication of Sir Isaac Newton's Principia*, Oxford, 1838; (2) W. W. Rouse Ball, *An Essay on Newton's "Principia"*, London and New York, 1893; (3) Ferdinand Rosenberger, *Isaac Newton und seine physikalischen Principien*, . . . Leipsic, 1895. Besides these, there is notably the account and critique of Newton's principles of mechanics in Ernst Mach's *Mechanik*, translated into English by T. J. M'Cormack under the title *The Science of Mechanics: A Critical and Historical Account of its Development*, third edition, Chicago, 1907, pp. 201-245.]

<sup>1</sup> [Old style. The new year was then reckoned from March the 25th, so that what we now call, for example, January the 6th, 1672, was then January the 6th, 1671, and is sometimes written "January the 6th, 1671/2." We will always write dates in the modern way.]

manor of Woolsthorpe, was in fact a small farmer, who died before the birth of his son. The manor, which had been in the family about a hundred years, was Newton's patrimony: it descended to the grandson of his father's brother. This heir sold it in 1732 to Edmund Turnor, to whose descendant the world is much indebted for a collection of facts connected with Newton's history.<sup>1</sup> A curious tradition of a conversation of Newton with Gregory, in which the former affirmed himself to be descended from a Scotch family, his grandfather having come from East Lothian at the accession of James I., will be found in the appendix to Brewster's *Life*,<sup>2</sup> with a careful attempt to see how far the presumption it affords can be supported by collateral evidence. But Newton himself (twenty years before the date of this conversation) gave his pedigree on oath into the Heralds' Office, stating that he had reason to believe that his great grandfather's father was John Newton, of Westby, in Lincolnshire.<sup>3</sup> To bring all that relates to his family together, his mother, when he was three years old, married Barnabas Smith, rector of North Witham, by whom she had one son and two daughters (who gained by marriage the

<sup>1</sup> [Edmund Turnor, *Collections for the History of the Town and Soken of Grantham, containing authentic Memoirs of Sir I. Newton now first published*, 1806. This book contains, among other things, Conduitt's sketch of Newton which was drawn up for the use of Fontenelle.]

<sup>2</sup> [Cf. Brewster's *Memoirs*, 1855, vol. ii, pp. 537-545.]

<sup>3</sup> [On Newton's pedigree (1705), see Turnor, *op. cit.*, p. 169, and the reference to Brewster's *Memoirs* given in the fourth note.]

names of Pilkington and Barton). The children of these three, four nephews and four nieces of Newton by the half-blood, inherited his personal property, amounting to £32,000. One of these nieces, Catherine, who married a Colonel Barton, became a widow, and afterwards lived in Newton's house. After her second marriage (to Mr Conduitt, who succeeded Newton as master of the Mint), she and her husband resided with him until his death.<sup>1</sup> They are the authority for many anecdotes given by Fontenelle in the *Eloge* read to the Academy of Science. Mrs Conduitt's only daughter, Catherine, married Mr Wallop, afterwards Viscount Lymington by inheritance; she transmitted a large collection of Newton's papers, also by inheritance, to the family of the Earl of Portsmouth. These "Portsmouth Papers" still exist unpublished,<sup>2</sup> and there is also a mass of papers in the Library of Trinity College, Cambridge, which are well known.<sup>3</sup>

<sup>1</sup> [It is a mistake that Catherine Barton, the daughter of Robert Barton and Hannah Smith, Newton's half-sister, was the widow of Colonel Barton. That this was so was stated in an anonymous *Life of the Earl of Halifax* published in 1715. Cf. Brewster, *Memoirs*, 1855, vol. ii, p. 273.]

<sup>2</sup> [The scientific part of the "Portsmouth Papers" was presented by Lord Portsmouth to the University of Cambridge, and has now been classified and deposited in the University Library. A descriptive catalogue of it was published at Cambridge, in 1888, under the title *A Catalogue of the Portsmouth collection of Books and Papers written by or belonging to Sir Isaac Newton, the Scientific Portion of which has been presented by the Earl of Portsmouth to the University of Cambridge*. This catalogue was drawn up by the Syndicate appointed on November the 6th, 1872, and the Preface was signed by H. R. Luard, G. G. Stokes, J. C. Adams, and G. D. Liveing. Only small parts of the collection have as yet been published.]

<sup>3</sup> [The correspondence with Cotes and some other letters were

At his mother's second marriage, Newton passed under the care of his grandmother. After some education at day schools, he was placed, in his twelfth year, at the public school at Grantham. He distinguished himself here by a turn for mechanics and carpentering; and among his early tastes was the love of writing verses,<sup>1</sup> and of drawing.<sup>2</sup> The dials which he made on the wall of his family house at Woolsthorpe have lasted to our day. They were lately carefully cut out by Mr Turnor, and presented, framed in glass for preservation, to the Royal Society.<sup>3</sup> While at Grantham he formed a friendship, which afterwards became a more serious feeling, with a young lady named Storey, who lived with the family in which he boarded. Their marriage was prevented by their poverty. Miss Storey was afterwards twice married, and as Mrs Vincent, at the age of eighty-two, after Newton's death, gave many particulars concerning his early life. He continued her friend to the end of his life, and was her frequent benefactor: and he lived

published by Edleston in the above-mentioned work. On other manuscripts of Newton's, see W. W. Rouse Ball, *op. cit.*, pp. 2-5, where "Shirburn Castle" is, as in G. J. Gray, *op. cit.*, p. 75, misspelt "Sherborn Castle"—a mistake that may give rise to a confusion of two different places, near Wallingford in Berkshire and in Dorset respectively.]

<sup>1</sup> [See Brewster, *Memoirs*, 1855, vol. i, pp. 12-13.]

<sup>2</sup> [According to Newton's own later confession, he was extremely inattentive to his studies and stood very low in the school; but soon, owing to the excitation of a spirit of emulation, he exerted himself in the preparation of his lessons and finally rose to the highest place in the school (Brewster, *Memoirs*, 1855, vol. i, pp. 7-8). On Newton's drawings, see *ibid.*, p. 12.]

<sup>3</sup> [But cf. Brewster, *Memoirs*, 1855, vol. i, pp. 11-12.]

and died a bachelor, though to say for her sake would perhaps be going beyond evidence; particularly when the engrossing nature of his subsequent studies is considered.<sup>1</sup>

## II

When he was fourteen years old his stepfather died, and his mother, who then took up her residence at Woolsthorpe, recalled him from school to assist in the management of the farm.<sup>2</sup> As it was found, however, that he was constantly occupied with his books when he should have been otherwise engaged, his maternal uncle recommended that he should be sent to Cambridge. He was accordingly admitted, on June the 5th, 1660, a member of Trinity College, a foundation which his name has ever since not only supported, but invigorated. According to the college books, he was subsizar<sup>3</sup> in 1661, scholar in

<sup>1</sup> [Cf. Brewster, *Memoirs*, 1855, vol. i, pp. 13-14.]

<sup>2</sup> [On Newton's early scientific experiment with the wind, see the third Essay below, § II.]

<sup>3</sup> A sizar at Cambridge was, in the original meaning of the word, a student whose poverty compels him to seek to maintain himself in whole or part by the performance of some duties which were originally of a menial character. By this institution a youth could live by the work of his hands while he pursued his studies. In our days there is but little distinction between the sizars and those above them; except in college charges, none at all. Those who look upon universities as institutions for *gentlemen* only, that is, for persons who can pay their way according to a certain conventional standard, praise the liberality with which poorer *gentlemen* than others have been gradually emancipated from what seems to them a mere badge of poverty. But those who know the old constitution of the universities see nothing in it except the loss to the labouring man and the destitute man of his inheritance in those splendid foundations. If sizarships with personal

1664, Bachelor of Arts in 1665, Junior Fellow in 1667, Master of Arts and Senior Fellow in 1668. In 1669, Dr Barrow resigned the Lucasian Professorship of Mathematics, and Newton was appointed his successor. From this period, when all money cares were removed by the emoluments of his fellowship and professorship, we must date the beginning of Newton's public career.

To go back a little; it does not appear that Newton went to Cambridge with any remarkable amount of acquired knowledge, or any results of severe discipline of mind. He had read *Euclid*, it is said, and considered the propositions as self-evident truths.<sup>1</sup> This is some absurd version of his

services had not existed, Newton could not have gone to Cambridge; and the *Principia* might never have been written. Let it be remembered, then, that, so far as we owe this immortal work and *its* immortal work to the University of Cambridge, we owe it to the institution which no longer exists, by which education and advancement were as open to honest poverty seeking a maintenance by labour, as to wealth and rank. Let the juries who find on their oaths that scores of pounds' worth of cigars are reasonable necessities for young college students, think of this, if they can think. [Cf. Edleston, *op. cit.*, p. xli.]

<sup>1</sup> [Before Newton left Woolsthorpe, his uncle had given him a copy of Sanderson's *Logic*, which he seems to have studied so thoroughly that, when he afterwards attended lectures on that work, he found that he knew more of it than his tutor. Finding him so far advanced, his tutor told him that he was about to read Kepler's *Optics* to some Gentlemen Commoners, and that he might attend the reading if he pleased. Newton immediately studied the book at home, and when his tutor gave him notice that his lectures upon it were to begin, he was surprised to learn that it had been already mastered by his pupil. About the same time, probably, he bought a book on Judicial Astrology at Stourbridge fair—a fair held yearly in Cambridge in September—and, in the course of perusing it, he came to a figure of the heavens which he could not understand without a previous knowledge of trigonometry. He therefore bought an English *Euclid* with an index of all the problems at the end of it. Having turned to two or three which he thought likely to remove his difficulties, he found the truths which they

early studies: many propositions, no doubt, are very evident; but if Newton ever gave this account of himself, which we do not believe, it proves nothing but that the lad carried to the University as much of self-conceit as the man brought away of learning and judgment. That the young mechanic, desultory in the previous reading, deep beyond his years in construction,<sup>1</sup> and practical verification, found within himself at first some dislike to the beaten road of mathematics, and was willing to make it royal by admitting all he was asked to prove, is what we can easily believe: for such is the most frequent tendency of an unbalanced exercise of manual ingenuity. That he may have stated this when he expressed his regret that he had not paid greater attention to the geometry of the ancients, is not improbable. Were such his bent, the discipline of the University would soon show a mind like his the paramount necessity of a different mode of pro-

enunciating so self-evident that he expressed his astonishment that any person should have taken the trouble of writing a demonstration of them. He therefore threw aside *Euclid* "as a trifling book," and set himself to the study of Descartes' *Geometry*, where problems not so simple seem to have baffled his ingenuity. Even after reading a few pages, he got beyond his depth and laid aside the work; and he is said to have resumed it again and again, alternately retreating and advancing, till he was master of the whole, without having received any assistance. The neglect which he has shown of the elementary truths of geometry he afterwards regarded as a mistake in his mathematical studies, and expressed his regret that "he had applied himself to the works of Descartes and other algebraic writers before he had considered the *Elements of Euclid* with that attention which so excellent a writer deserved" (Brewster, *Memoirs*, 1855, vol. i, pp. 21-22; cf. the third Essay below, § II.).]

<sup>1</sup> Let it be remembered that we are not told that Newton, when very young, took greatly to anything except arts of construction.

ceeding.<sup>1</sup> Again, we are not told anything of Newton's pupillar career at Cambridge, except that he is known to have<sup>2</sup> bought a prism (an epoch in his life) in 1664;<sup>3</sup> and that, in the same or the next year, being competitor for a college law-fellowship with a Mr Robert Uvedale, the two candidates were of perfectly equal merit, and Dr Barrow accordingly elected Mr Uvedale as the senior in standing. We have no account of any great sensation produced by the talents of Newton during his college career.

<sup>1</sup> [See § II. of the third Essay below for De Morgan's opinion on the story of Barrow forming, after an examination of Newton in *Euclid* in 1664, an indifferent opinion of Newton's knowledge (Brewster, *Memoirs*, vol. i, p. 24).]

<sup>2</sup> The *status pupillaris* lasts about seven years, that is, until the degree of Master of Arts is taken.

<sup>3</sup> [The study of Descartes' *Geometry* seems to have inspired Newton with a love of the subject, and to have introduced him to higher mathematics—the study of the works of Vieta, Schooten, and Wallis. In a note-book partly written in 1663-1664, in which mathematical notes on these writers were made, he also wrote down some observations on refraction, on the grinding of spherical lenses, and on the errors of lenses and the method of rectifying them. An entry in this same book made by Newton in 1699 is the statement that the annotations out of Schooten and Wallis were made in the winter between 1664 and 1665. At this time he found the Method of Infinite Series; and, in the summer of 1665, being forced from Cambridge by the plague, he computed the area of the hyperbola at Boothby in Lincolnshire to fifty-two figures by the same method (Brewster, *Memoirs*, 1855, vol. i, pp. 23-24; vol. ii, pp. 10-15). In 1665 Newton committed to writing his first discovery of the method of fluxions. This paper was written by his own hand, and dated May the 20th, 1665, and the notation of dotted letters was here used. On another leaf of the same note-book, the method was described under the date of May the 16th, 1666. In the same book again, with a date of November the 13th, 1665, there was written another paper on fluxions with their application to the drawing of tangents and "the finding of the radius of curvity of any curve." In October 1666, Newton drew up another small tract, in which the method of fluxions was again put down without the notation of dotted letters and applied to equations involving fractions and surds and such quantities as were afterwards called transcendent (*ibid.* See also the Appendix to the second Essay below).]

Even Barrow, the best judge in Cambridge, and, after Wallis, in England, writing to Collins in 1669 (when he was on the point of resigning the mathematical chair to Newton), mentions him as an unknown man<sup>1</sup> of great promise, in terms of high, but not unusual commendation.

### III

The first period of Newton's life is twenty-seven years, ending with his appointment to the Lucasian professorship. The second, of twenty-six years, ending with his appointment to his first office in the Mint in 1695,<sup>2</sup> was the period of the announcement of all his discoveries. The third and longest, of thirty-two years, containing his official residence in London, saw him in the uninterrupted possession of as much fame as man can have, and power never equalled over those of the same pursuits as himself. The merely biographical history of his second period is not long. On Dec. the 21st, 1671, and Jan. the 11th, 1672, the Royal Society entered on their

<sup>1</sup> "A friend of mine here, that hath an excellent genius to these things, brought me . . . papers . . . which I suppose will please you." And again, some days after, "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 (being but the second year Master of Arts), but of an extraordinary genius and proficiency in these things." [Barrow sent Newton's tract *De Analysisi* to Collins on July the 31st, 1669 (Brewster, *Memoirs*, 1855, vol. i, pp. 27, 36; vol. ii, pp. 14-15).]

<sup>2</sup> [Newton was appointed Warden of the Mint in 1696, and Master of the Mint in 1699. Cf. Edleston, *op. cit.*, pp. xxxv, lxxviii; Brewster, *Memoirs*, 1855, vol. ii, pp. 191-193.]

minutes, in such terms as people use who have not the gift of prophecy, two of the most important announcements they ever had to make. "Mr Isaac Newton, Professor of Mathematics in the University of Cambridge, was proposed candidate by the Lord Bishop of Salisbury (Dr Seth Ward)," and "Mr Isaac Newton was elected." During the whole of this second period, he was seldom out of Cambridge more than three or four weeks in one year. Having missed the Law Fellowship (which was a *lay* fellowship), he would have been required, in 1675, either to take orders or to vacate the fellowship which he did hold. But in that year he obtained a dispensation from Charles II., no doubt granted at the application of the College. He lectured on optics in the year following his appointment to the professorship; and it would appear that he lectured on elementary mathematics. The *Arithmetica Universalis* (published by Whiston, it was said, against Newton's consent, which Whiston denies) was taken from the lectures delivered on algebra and its application to geometry, which were preserved in the depositories of the University.<sup>1</sup> When, in 1687, James II., among his other attempts of the same kind, ordered the University of Cambridge to admit a Benedictine as Master of Arts without taking the oaths, and upon

<sup>1</sup> [Newton's lectures on optics, arithmetic, and algebra, on the motion of bodies, and on the system of the world, are preserved in the University Library at Cambridge, and are described in Edleston; *op. cit.*, pp. xci-xcviii. Cf. also W. W. Rouse Ball, *op. cit.*, pp. 27-28.]

the resistance of the University, Newton was appointed one of the delegates to the High Court for the purpose of stating the case. The king withdrew his order, and in the next year Newton was proposed as Member of Parliament for the University, and gained his election by a small majority. He sat accordingly in the Convention Parliament, which declared the throne vacant, though it appears by the records of the College that, except in 1688 and 1689, he was not absent from the University often enough or long enough to have taken much share in public business.

## IV

In 1692 occurred the curious episode of his history which produced abroad, as has recently appeared, a report that he had become insane. Most readers know the tradition of his dog Diamond having upset a light among the papers which contained his researches, and of the calmness with which he is said to have borne the loss. The truth, as appears by a private diary of his acquaintance Mr de la Pryme, recently discovered is, that in February 1692, he left a light burning when he went to chapel, which, by unknown means, destroyed his papers, and among them a large work on optics, containing the experiments and researches of twenty years. "When Mr Newton came from chapel, and had seen what was done, everybody thought that he would have run

mad; he was so troubled thereat that he was not himself for a month after." Such phrases, reported, gave rise to a memorandum in the diary of the celebrated Huygens (the first foreigner who understood and accepted the theory of gravitation),<sup>1</sup> stating that he had been told that Newton had become insane, either from study, or from the loss of his laboratory and manuscripts by fire—that remedies had been applied by means of which he had so far recovered as to be then beginning again to understand his own *Principia*. That Newton was in ill-health in 1692 and 1693 is known, but his letters to Dr Bentley on the Deity, written during that period, are proof that he had not lost his mind.<sup>2</sup>

We now give a slight enumeration of the matters on which Newton's attention was fixed during the second period, which we have just quitted.<sup>3</sup>

<sup>1</sup> [This is hardly correct; cf. Rosenberger, *op. cit.*, p. 234, and the whole of that chapter.]

<sup>2</sup> [See Brewster, *Memoirs*, vol. ii, pp. 123-124, 131-156; on the letters to Bentley, cf. Rosenberger, *op. cit.*, pp. 263-270.]

<sup>3</sup> [The only complete edition of Newton's works was edited by Bishop S. Horsley in five volumes from 1779-1785 under the title *Isaac Newtoni Opera quae existant omnia. Commentariis illustrabat Samuel Horsley*. Contents: Vol. i, (1) *Arithmetica Universalis*. (2) *Tractatus de Rationibus Primis Ultimisque*. (3) *Analysis per Aequationes numero terminorum Infinitas*. (4) *Excerpta quaedam ex Epistolis ad Series Fluxionesque pertinentia*. (5) *Tractatus de Quadratura Curvarum*. (6) *Geometria Analytica sive specimina Artis Analyticae*. (7) *Methodus Differentialis*. (8) *Enumeratio Linearum tertii Ordinis*. Vol. ii, *Principiorum Libri Priores duo, De Motu Corporum*. Vol. iii, (1) *Principiorum Liber Tertius, de Systemate Mundi*. (2) *De Mundi Systemate*. (3) *Theoria Lunae*. (4) *Lectiones Opticae*. Vol. iv, (1) *Opticks*. (2) *Letters on various Subjects in Natural Philosophy, published from the Originals in the Archives of the Royal Society*. (3) *Letters to Mr Boyle on the Cause of Gravitation*. (4) *Tabulae Duæ*,

## V

## OPTICS

The great discovery of the unequal refrangibility of the rays of light was made in 1666, the year in which he was driven from Cambridge by the plague. In 1668 he resumed his inquiries, and, judging that the decomposition of light which he had discovered would render it impossible to construct refracting telescopes free from colour, or achromatic, he applied himself to the improvement of the reflecting telescope. The telescope which he made with his own hands, now in possession of the Royal Society, was made in 1671. It was submitted to the Society

Colorum altera, altera Refractionum. (5) De Problematibus Bernoullianis. (6) Propositions for determining the Motion of a Body urged by two Central Forces. (7) Four Letters to Dr Bentley. (8) Commercium Epistolicum, etc., cum recensione præmissa. (9) Additamenta Commercii Epistolici ex Historia Fluxionum Raphsoni. Vol. v, (1) Chronology of Antient Kingdoms amended. (2) Short Chronicle from a MS. the property of the Rev. Dr Ekins. (3) Observations upon the Prophecies of Holy Writ, particularly the prophecies of Daniel and the Apocalypse of St John. (4) An Historical Account of two Notable Corruptions of Scripture, in a Letter to a Friend. Horsley added the following papers: (1) Logistica Infinitorum, (2) Geometria Fluxionum sive Additamentum tractatus Newtoniani de Rationibus Primis Ultimisque, in vol. i; (3) De viribus centralibus quæ rationem triplicatæ distantiarum a centro contrariam inter se constanter servant, in vol. iii. A Latin edition of Newton's works was published at Lausanne and Geneva in 1744, and is described in G. J. Gray, *op. cit.*, pp. 2-4. The various editions, from 1687 on, of the *Principia*, and its translations and commentaries were described by Gray (*ibid.*, pp. 5-35). Here we will only mention that the only complete English translation of it was by Andrew Motte, and was first published at London in 1729 (American editions, New York, 1848 and 1850), and that the selection of works mentioned in Gray's "Illustrations" is often ludicrous. Gray dealt with books on optics, fluxions, universal arithmetic, and minor works by Newton and others on pp. 35-46, 46-55, 56-59, and 59-61 respectively.]

immediately after his election as a Fellow, and was followed by the account of his discovery of the decomposition of light. This explanation of the known phenomenon of the colours of the prismatic spectrum was fully appreciated by the Society; but Newton had to reply to various objections from foreign philosophers, and to those of Hooke at home. At this time first appeared (indeed there had been nothing before to draw it out) that remarkable trait in his character of which we shall afterwards speak: extreme aversion to all kinds of opposition. "I intend," he says, "to be no further solicitous about matters of philosophy." And again, "I was so persecuted with discussions arising from the publication of my theory of light, that I blamed my own imprudence for parting with so substantial a blessing as my quiet to run after a shadow."

The researches on the colours of thin plates, and the explanation known by the name of the theory of "Fits of Reflexion and Transmission," was communicated to the Royal Society in 1765-66. Those on the "inflexion" of light, though probably made long before 1704, first appeared in that year, in his treatise on *Opticks*. He never would publish this work as long as Hooke lived, from that fear of opposition above noted.<sup>1</sup>

<sup>1</sup> [On Newton's optical researches, see Brewster, *Memoirs*, 1855, vol. i, pp. 37-249; Rosenberger, *op. cit.*, pp. 51-117, 289-341.]

## VI

## PRINCIPIA: THEORY OF UNIVERSAL GRAVITATION

The discoveries of Kepler<sup>1</sup> had laid down the actual laws of the planetary motions: and the idea of universal gravitation began to occupy the minds of those who thought on these subjects. "Gravitation" was a term of some antiquity, used to denote the effort of bodies on the earth to descend: *weight*, in fact. The notion of matter acting upon matter as an agent of attracting force, and the possibility of such force extending through the heavens, and being the proximate cause of the motions of the planets, was floating through men's minds when Newton first turned his attention to the subject. There has hardly ever been a great discovery in science, without its having happened that the germs of it have been found in the writings of several contemporaries or predecessors of the man who actually made it. In the case before us it had even been asserted as matter of necessity, that supposing attraction to exist, it must be according to the law of the inverse squares of the distances:<sup>2</sup> and Huygens

<sup>1</sup> [Kepler (1571-1630) discovered in 1609, from the observations of Tycho Brahe and himself, that the planets move round the sun in ellipses in one of whose foci the sun is placed, and that the line joining sun and planet describes equal areas in equal times. In 1619 he published his further discovery that the periodic times of any two planets are to one another as the cubes of their distances from the sun.]

<sup>2</sup> [On the precursors of Newton, and especially Kepler, Galileo, Descartes, Bouillaud, Borelli, and Hooke, see Brewster, *Memoirs*, 1855, vol. i, pp. 250-288; Rosenberger, *op. cit.*, pp. 135-157.]

announced, in 1673, before Newton had completed any part of his system, the relations which exist between attractive force and velocity in circular motion.<sup>1</sup> Newton first turned his attention to the subject in 1666, at Woolsthorpe; sitting alone in a garden, his thoughts turned towards that power of gravity which extends to the tops of the highest mountains, and the question whether the power which retains the moon in her orbit might not be the same force as that which gives its curvature to the flight of a stone on the earth. To deduce from what Kepler had exhibited of the laws of the planetary motions, that the force must vary inversely as the square of the distance, came within his power: but on trying the value of that force, as deduced from the moon's actual motion, with what it should be as deduced from the force of gravitation on the earth, so great a difference was found as to make him throw the subject aside. The reason of his failure was the inaccurate measure which he used of the size of the earth.<sup>2</sup> The subject was not

<sup>1</sup> [This was in his *Horologium Oscillatorium* of 1673 (see Mach, *op. cit.*, pp. 155-187). At the end of the book were given some rules for the calculation of centrifugal forces in circular motions; but no demonstrations were there given, and these demonstrations were only supplied by him in a tract published posthumously, in 1703, and translated into German in No. 138 of *Ostwald's Klassiker*. It must be remembered that Newton had used the chief result of Huygens in this direction in his earliest and unpublished investigation on gravity and the moon's orbit, in 1666.]

<sup>2</sup> [It is now usually maintained, on certain grounds that are discussed in W. W. Rouse Ball, *op. cit.*, pp. 7, 11, 16-17, 61, 157, that Newton was fairly well satisfied with the result of his approximate calculation of 1666, and had a strong suspicion of the law of universal

resumed till 1679; not, as commonly stated, because he then first became acquainted with Picard's measure of the earth (we think Professor Rigaud had shown this), but because leisure then served, and some discussions on a kindred subject at the Royal Society had awakened his attention to the question.<sup>1</sup> In 1679 he repeated the trial with Picard's measure of the earth: and it is said that when he saw that the desired agreement was likely to appear, he became so nervous that he could not continue the calculation, but was obliged to intrust to a friend.<sup>2</sup> From that moment the great discovery must be dated: the connexion of his speculations on motion with the actual phenomena of the universe was established. At the time when we write this, a distant result of that calculation has been announced, which Newton himself would hardly at any period of his life have imagined to have been

gravitation, but he was stopped by the difficulty of calculating the attractions of a number of particles massed together. This he discovered—at least in the most important case—in 1685, and thus the propositions which he had previously (1679 and 1680) found about the orbits of attracting particles could be applied at once to spherical bodies. Newton, in fact, discovered in 1685 by calculation that such bodies attract as if they were particles situated at the contents of the masses. Thus he must have only then realised that those propositions, which he had believed to be only approximately true when applied to the solar system, were almost completely exact.]

<sup>1</sup> [The subject was certainly resumed in 1679, but it was apparently in consequence of a problem proposed by Robert Hooke in a letter to Newton of November of that year. In the correspondence that followed, Hooke drew attention to Picard's measurements, and stimulated Newton's interest and curiosity by his happy insight into celestial problems and correction of a careless remark of Newton's. For this correspondence, see W. W. Rouse Ball, *op. cit.*, pp. 18–24, 139–153.]

<sup>2</sup> [This story is probably apocryphal; *cf.* W. W. Rouse Ball, *op. cit.*, p. 23.]

possible. A planetary body, unknown and unseen till after the prediction, has made itself felt by its attraction on another. Unexplained (and very trivial) irregularities in the motion of Uranus suggested the idea of there being yet another planet by the attraction of which they were produced. From those irregularities the place and distance of that planet have been inferred, and, on looking into the part of the heavens at which its silent action proved it to be, if indeed it existed—there it was found. A heavenly body has thus been calculated into existence, as far as man is concerned.<sup>1</sup>

How much Newton might have got ready it is not easy to say: all that is known is that he kept it to himself. At the end of 1683 Halley<sup>2</sup> had been considering the question, and was stopped by its difficulties; but, being in August 1684 on a visit to Newton, the latter informed him of what he had done, but was not able to find his papers. After Halley's departure, he wrote them again, and sent them: upon which Halley paid another visit to Cambridge, to urge upon Newton the continuance

<sup>1</sup> [The almost simultaneous discovery in 1846 of Uranus by Adams and Le Verrier, by calculation, created a most powerful impression on nearly everybody, including De Morgan (*cf.* Mrs De Morgan's *Memoir*, pp. 126–138).]

<sup>2</sup> [The biographical sketch of Halley (1656–1742) in the *Cabinet Portrait Gallery of British Worthies*, vol. xii, London, 1847, pp. 5–15, is, judging from the style, by De Morgan. From Mrs De Morgan's *Memoir*, p. 108 (see the first note to the first Appendix to the third Essay below), we learn that De Morgan wrote the article "Halley" on pp. 161–168 of the first volume of *The Gallery of Portraits: with Memoirs* (London, 1833). The biography of Newton on pp. 79–88 of this volume does not seem to be by De Morgan.]

of his researches; and (December, 1684) informed the Royal Society of them, and of Newton's promise to communicate them. The Society, who knew their man, and how little they should get without asking, appointed a Committee (Halley and Paget, the mathematical master in Christ's Hospital) to keep Newton in mind of his promise; so that (February, 1685) a communication was sent up, amounting to those parts of the first book of the *Principia* which relate to central forces. Newton went on with the work, and (April the 21st, 1686) Halley announced to the Society that "Mr Newton had an incomparable treatise on Motion, almost ready for the press." On the 28th, Dr Vincent (the husband, it is supposed, of Miss Storey) presented the manuscript of the first book to the Society, who ordered it to be printed, and Halley undertook to pay the expenses. But it was not yet in harbour: Hooke, who used to claim everything, asserted that he had been in possession of the whole theory before Newton; with which the latter was so disgusted, that he proposed to omit the third book (being in fact all the application to our system). Halley, the guardian angel of the work, wrote him a letter, in which he soothed him almost as if he had been a child, and prevailed upon him to complete it as first intended. It appeared under the title of *Philosophiæ Naturalis Principia Mathematica*, about midsummer, 1687, containing

the mathematical discussion of the laws of solid and fluid motion, with their application to the heavenly motions, the tides, the precession of the equinoxes, and so on. The reader who understands the terms may refer to the *Penny Cyclopædia* (article "Principia"), in which the heads of all the propositions are given. [No work on any branch of human knowledge was ever destined to effect so great a change, or to originate such important consequences.<sup>1</sup>]

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## VII

FLUXIONS, NOW CALLED THE DIFFERENTIAL  
CALCULUS

A curved figure differs from one the boundaries of which are consecutive straight lines in that there is always a *gradual* change of direction going on at the boundaries of the former, while at those of the latter the changes are made only at certain places, and as it were in the lump. To apply the doctrines of mathematics to cases in which such perfectly gradual changes take place, had been always the greatest difficulty of the science. Archimedes had conquered it in a few cases: the predecessors of Newton had greatly extended what Archimedes had done, and had given what, to those who come

<sup>1</sup> [On Newton's investigations of 1684, on the preparation and publication of the *Principia* (1685-1687), for Halley's correspondence with Newton (1686-1687) about the publication of the *Principia* and about Hooke's claims, cf. W. W. Rouse Ball, *op. cit.*, pp. 25-73, 153-174.]

after Newton and Leibniz, would appear strong hints of an organized method of treating all cases. But the method itself, and an appropriate language for expressing its forms of operation, were still wanting. About 1663, Newton turned his attention to the writings of Descartes and Wallis, and, in the path which the latter had gone over, found the celebrated Binomial Theorem: Wallis having in fact solved what would now be called a harder problem. This, far from lessening the merit of the discovery, increases it materially. In 1665 Newton arrived at his discoveries in series, and substantially at his method of fluxions. In 1669 Barrow communicated to Collins (on the occasion before referred to) a paper by Newton on series, not containing anything on fluxions. Various letters of Newton, Collins, and others, state that such a method had been discovered, without giving it. But one letter from Newton to Collins on December the 10th, 1672, states a mode of using one case of this method, confined to equations of what are called *rational terms* (it being admitted on all sides that the great pinch of the question then lay in equations of *irrational terms*). Leibniz, who had been in England in 1673, and had heard something indefinite of what Newton had done, desired to know more: and Newton, on June the 13th, 1676, wrote a letter to Oldenburg, of the Royal Society, which he desired might be communicated to Leibniz. This

letter dwells on the binomial theorem, and various consequences of it; but has nothing upon fluxions. Leibniz still desiring further information, Newton again wrote to Oldenburg, on October the 24th, 1676, explaining how he arrived at the binomial theorem, giving various other results, but nothing about fluxions, except in what is called a cipher. A cipher it was not, for it merely consisted in giving all the letters of a certain sentence, to be put together if Leibniz could do it. Thus, the information communicated was

aaaaa cc d ae eeeeeeeeeeee ff iiiiii III nnnnnnnn  
oooo qqqq rr ssss tttttttt vvvvvvvvvvvv x.

These are merely the letters of a Latin sentence which, translated word by word in the order of the words, is "given equation any whatsoever, flowing quantities involving, fluxions to find, and *vice versa*."<sup>1</sup> Even this letter had not been sent to Leibniz on March the 5th, 1677; it was sent soon after this date. But in the mean time, Leibniz, by himself, or as was afterwards said, having taken a hint from other letters of Newton, had invented his differential calculus. And, as open as Newton was secret, shortly after receipt of the above, he wrote to Oldenburg, on June the 21st, 1677, a letter giving a

<sup>1</sup> [The Latin sentence is: "Data æquatione quocunque fluentes quantitates involvente, fluxiones invenire; et vice versa." The anagram may be more shortly written:

6a 2c d æ 13e 2f 7i 3l 9n 4o 4q 2r 4s 9t 12v x.]

full and clear statement of everything he had arrived at : making an epoch as important in the pure mathematics, as was the discovery of the moon's gravitation in the physical sciences. In the *Principia*, Newton acknowledges this in the following "Scholium": "In letters which went between me and that most excellent geometer G. G. Leibniz,<sup>1</sup> ten years ago, when I signified that I was in the knowledge of a method of determining maxima and minima, of drawing tangents and the like, and when I concealed it in transferred letters involving this sentence ('Data æquatione,' and so on, as above), that most distinguished man wrote back that he had also fallen upon a method of the same kind, and communicated his method, which hardly differed from mine except in the forms of words and symbols. The foundation of both is contained in this Lemma." In 1684 Leibniz published his method : while in the *Principia*, Newton still gave nothing more than the most general description of it, and avoided its direct use entirely. By 1695 it had grown into a powerful system, in the hands of Leibniz and the Bernoullis : while in England it was very little noticed. About 1695 an alarm began to be taken in England at its progress : and the friends of Newton began to claim what they conceived to be his rights. Wallis excused himself from mentioning the differential

<sup>1</sup> [Leibniz's names were Gottfried Wilhelm ; the initials "G. G." (Gothofredus Gulielmus) stand for the Latin version of these names.]

calculus in his works, on the ground that it was Newton's method of fluxions. In 1699, Fatio de Duillier, a Genevese residing in England, published an implied charge of plagiarism on Leibniz : the latter denied the imputation and appealed to Newton's own testimony. The *Leipsic Acts*<sup>1</sup> made something very like the same charge against Newton : and in the course of the dispute, Keill, an Englishman, asserted<sup>2</sup> that Leibniz had taken Newton's method, changing its name and symbols. This accusation roused Leibniz, who complained to the Society : and after some correspondence, in which allusion was made to the Oldenburg letters as being sources from which he might have drawn knowledge of Newton's method, the Royal Society appointed a Committee, consisting of eleven members, to examine the archives, and *to defend Newton*. This latter purpose, though not stated in words, was fully understood : and since the usual impression is that it was intended for a judicial committee, meaning of course an impartial one, we give in a note<sup>3</sup> some

<sup>1</sup> [The remark referred to was in an anonymous review by Leibniz, but was by no means a charge of plagiarism. (Cf. Rosenberger, *op. cit.*, pp. 473-475).]

<sup>2</sup> *Phil. Trans.*, 1708.

<sup>3</sup> First, the Committee consisted of Halley, Jones, De Moivre, and Machin, Newton's friends, and mathematicians ; Brook Taylor, a mathematician, but not then otherwise known except as a friend of Keill, the accused party ; Robarts, Hill, Burnet, Aston, and Arbuthnot, not known as mathematicians, but the two latter intimate personal friends of Newton ; and Bonet, the Prussian minister. To call this a judicial committee would be to throw a great slur on the Society. Secondly, the names of the Committee were never published with their report, which would have been anything but creditable, if that report

heads of the proof of our assertion. The Committee, appointed at different times in March 1712, reported in April that they had examined, and so on, and that they were of opinion that Leibniz had no method till after the letter to Collins of December the 10th, 1672, had been sent to Paris to be communicated to him, and that Keill, in asserting the priority of Newton, had done Leibniz no injustice. This is, to us, the main part of the report. It was published, with abundance of extracts from letters, and letters at length, most of which had been found among Collins's papers, under the name of *Commercium Epistolicum*, and so on, in 1712 and in 1725. The conclusion was not to the point: Leibniz asked reparation for a charge of theft, and the answer is that there was no injustice to him in saying that the other party had the goods before the time when he was alleged to have stolen them.

had been a judgment: but if the Committee were only counsel for Newton's case it mattered not who they were. Thirdly, the Society had committed itself to Newton's side, by hearing his statement, and thereupon directing Keill to write the second letter in the controversy, and to "set the matter in a just light": the only light they had sought being that which Newton himself could give. Fourthly, Burnet wrote to John Bernoulli while the matter was pending, stating in express terms—not that the Royal Society was *inquiring*—but that it was *busy proving* that Leibniz might have seen Newton's letters. Fifthly, De Moivre, as appears by the statement of an intimate friend, considered himself, by merely joining that Committee, as drawn out of the neutrality which he had till then observed: which shows that he did not consider himself a jurymen. Sixthly, no notice was given to Leibniz of the proceeding, still less an invitation to produce documents on his own side. All these things put together show that the Committee was not judicial, nor meant to be so, nor asserted to be so on the part of the Society. If any one will have it that it was so, he must needs, we think, hold that it was one of the most unfair transactions which ever took place.

With regard to Collins's letter, besides its containing no more than any good mathematician could have drawn from Barrow and Fermat together, no proof<sup>1</sup> was *given to the world* of Leibniz ever having seen it, which any man who valued his character would have ventured to produce in any kind of court with rules of evidence. In truth, though the Committee were not unfair judges (simply because they were not judges at all), we cannot but pronounce them unscrupulous partisans, for the reasons given

<sup>1</sup> A parcel (*collectio*) of extracts from *Gregory's* letters are found in the handwriting of Collins, with a memorandum by Collins that they were to be sent to Leibniz and returned by him: with a letter to Oldenburg, desiring him to send them: no mention of any one but Gregory in either memorandum or letter. With the parcel is this letter to Collins: what reason the Committee have for supposing this letter belonged to the parcel they do not say: they do not even say whether it was a separate paper or not. The papers of dead mathematicians, after going through the hands of executors, are, we suspect, not always tied up exactly in the order they were untied. Whether the parcel is otherwise known to have found its way to Oldenburg than from the intention expressed in the memorandum, we are not told—nor whether Oldenburg sent it to Paris—nor whether, having arrived at Paris, it was sent on to Hanover; and finally they state, without adding how they came to know it, that it was sent to Leibniz on June the 26th, 1676. If the letter belonged to the parcel, and if the parcel were sent to Oldenburg, and if Oldenburg sent it to Paris, and if his Paris correspondent sent it to Hanover, and if it arrived safe, and if Leibniz, meaning to make an unfair use of it, was unwise enough to return this evidence against himself—the case of the Committee is good, with only one more *if*; that is, if the letter contained anything new to the purpose, which we think it palpably does not. That is to say, the letter itself is only what any strong mathematician might have drawn from Barrow and Fermat, who are almost the joint inventors of Fluxions, if that letter contained them. It is worth the remembering that Collins was not likely to tie up letters miscellaneously: he was a regular accountant, a methodical writer on and practiser of book-keeping, and a man of business. For aught we know, he may lie unquiet in his grave to this day, under the imputation of having sent a parcel which contained a paper neither mentioned in the docket nor in the letter of advice. Perhaps he never sent it at all: would not this methodical man have written on the parcel the date of its return?

and others. Leibniz never made any formal answer, but his friends retorted the charge of plagiarism upon Newton, and John Bernoulli made a short anonymous reply. The Committee, content perhaps with the number of those who were ready to swear that black was both black and white, and neither, and to believe it too, rather than yield anything to a foreigner (and it is to be remembered that Leibniz, the servant of the Elector, was particularly obnoxious to all the Jacobites), published nothing further: the Society (May the 20th, 1714), in reference to the complaint of Leibniz that he had been condemned unheard, resolved that it was never intended that the Report of the Committee should pass for a decision of the Society: but others persisted in calling it so. A mutual friend, the Abbé Conti, being in England in 1715, Leibniz at the latter end of that year wrote him a letter, in the postscript of which he adverted to the usage he had received. This letter excited curiosity in London: and Newton, whose power in matters of science was then kingly, requested and obtained the presence of all the foreign ambassadors at the Royal Society to collate and examine the papers. After this had been done, Baron Kirmansegger, one of the ambassadors, stated his opinion that the dispute could not be terminated in that manner; that Newton ought to write to Leibniz, state his own case, and demand an answer. All present agreed,

and the king (George I.), to whom the matter was mentioned that same evening, was of the same opinion. Newton accordingly wrote a letter to Conti, in which he relies mostly upon what Leibniz had either expressly or tacitly admitted. Nine times, on different points, he calls upon Leibniz to acknowledge something because he had once acknowledged it. Leibniz replied at great length. Newton did not rejoin, except in notes on the correspondence which he circulated privately among his friends. Leibniz died in November 1716, and Newton forthwith handed the whole correspondence, with his final notes, to Raphson, whose *History of Fluxions* was then in process of printing. The book appeared with this correspondence as an appendix: it is dated 1715, but the publication was retarded. And in the third edition of the *Principia*, published in 1726, Newton omitted the scholium we have quoted above, in spite of his doctrine that what was once acknowledged should be always acknowledged. In its place he put another scholium, with a similar beginning and ending, but referring not to Leibniz but to his own letter to Collins of December 1672. In the Conti correspondence—that is, in the notes which he would not print while Leibniz was alive—he had evaded the plain meaning of this scholium, asserting that it was not an admission, but a challenge to Leibniz to make it appear that the latter had the priority; and further, that by refer-

ring to the letters, he left the reader to consult them and interpret the paragraph thereby. This was the climax of blind unfairness: for Newton does not specify the dates of the letters, and gives their description wrongly (for they were written to Oldenburg, not to him). And further, the reader could not use them, for they were not published, nor at that time intended for publication.

We shall presently make some remarks on the conduct of Newton in this transaction; but we now proceed to the merits of the question. That Leibniz derived nothing from Newton except the knowledge that Newton could draw tangents, find maxima and minima, etc., by some organised method, we have no doubt whatever, nor has any one else, at this time, so far as we know. But, though we may be singular in the opinion, we agree with Bernoulli that Newton did derive from Leibniz (without being aware of the extent of his obligation, we think) the idea of the permanent use of an organized mode of mathematical expression. On a simple question of fact, opinion and construction apart, we take the words of both as indisputable; neither would have descended to bare falsehood. Now, in the first place, it is essential to observe that the genius of Newton did not shine in the invention of mathematical language: and, the disputed fluxions apart, he added nothing to it. The notation of the *Principia* is anything but a model. We know by the letter in

which Leibniz communicated his system to Newton, in 1677, that, at that period, Newton received communication of the idea of an organised and permanent language: and the question is whether he had it already. From his own Conti correspondence, written after it was within his knowledge that Bernoulli had asserted him to have taken his idea of notation from Leibniz, and when he makes the fullest and most definite assertions as to the extent to which he has carried the *use* of his method, he does not assert that before receipt of Leibniz's letter he did more than "sometimes" use one dot for a first fluxion, two for a second, and so on.<sup>1</sup> Neither of the parties knew of the importance which posterity would attach to this simple point: and it is our full conviction that Newton, who had only got the length of finding it occasionally convenient to use a specific language, would never have organised that language for permanent use had he not seen the letter of Leibniz. Even as late as the publication of the *Principia* he has no better contrivance than using small letters to represent the fluxions of great ones. We are avowedly expressing, in one point, our low estimate of Newton's power: and we believe the reason to have been, that he did not cultivate a crop for which he had no use. He who can make existing language serve his

<sup>1</sup> [We know from Newton's manuscripts that he used dots as early as 1665. Cf. the Appendix to the second Essay, below.]

purpose never invents more : and Newton was able to think clearly and powerfully without much addition to the language he found in use. The *Principia*, obscure as it is, was all light in Newton's mind ; and he did not attempt to conquer difficulties which he never knew.<sup>1</sup>

## VIII

We now pass on to the third period of Newton's life. In 1694, his old friend Charles Montague<sup>2</sup> (afterwards Lord Halifax) became Chancellor of the

<sup>1</sup> [On the genesis and development of the ideas of Newton and Leibniz on the infinitesimal calculus, and the great controversy, see De Morgan's second Essay, below.]

<sup>2</sup> Montague was deeply attached, says Sir David Brewster, to Newton's half-niece, Catherine Barton, to whom he left a large part of his fortune. Mrs Barton, to use Sir D. Brewster's words, "though she did not escape the censures of her contemporaries, was regarded by those who knew her as a woman of strict honour and virtue." Sir D. Brewster, who copies the words from the *Biographia Britannica*, declines, in his reverence for all that belonged to Newton (a feeling with which we have more sympathy than our readers will give us credit for), to state the whole case. After the death of Montague's wife, he was disappointed in a second marriage which he projected, "which was the less to be regretted as he had some time before cast his eye upon a niece of his friend Sir Isaac Newton, to be the superintendent of his domestic affairs. This gentlewoman . . . was then a celebrated toast, being young, beautiful, and gay, so that she did not escape censure, which was however passed upon her very undeservedly, since we are well assured she was a woman of strict honour and virtue. 'Tis certain she was very agreeable to his Lordship in every particular." . . . No wonder she did not escape censure, especially when the legacy left by Lord Halifax is left, to use his own words, "as a token of the sincere love, affection, and esteem I have long had for her person, and as a small recompence for the pleasure and happiness I have had in her conversation." And all this from an apologist: what, then, was the truth? On reviewing this note, we think it right to add that the statement that there were feelings of love between the parties (which, if true, puts their relation to one another beyond any reasonable doubt) is not from the author here cited, but from Sir D. Brewster, who does not give his authority. [On De Morgan's later investigations on the relations between Catherine Barton and Lord Halifax, see the third Essay, below, § VII., and the notes added to it.]

Exchequer, and it was one of his plans to restore the adulterated coinage. He served both his friend and his plan by making Newton Warden of the Mint, a place of five or six hundred a year (March the 19th, 1695<sup>1</sup>). In 1699, Newton was made Master of the Mint, on which occasion he resigned to Whiston, as his deputy, the duties and emoluments of the Lucasian professorship, and resigned to him the professorship itself of 1703. In 1701, he was again elected member for the University ; but he was turned out by two sons of Lords in 1705. In 1703, he was chosen President of the Royal Society, and was annually re-elected during the rest of his life, In 1705, he was knighted at Cambridge by Queen Anne. In 1709, he entrusted to Roger Cotes the preparation of the second edition of the *Principia*, which appeared in 1713. All the correspondence relating to the alterations made in this edition is in the Library of Trinity College.<sup>2</sup> In 1714, at the accession of George I., he became an intimate acquaintance of the Princess of Wales (wife of George II.), who was also a correspondent of Leibniz. Some observations made by the latter on the philosophy of Locke and of Newton brought on the celebrated correspondence between Leibniz and Clarke. And at the same time, an abstract of Newton's ideas on chronology, drawn up for the

<sup>1</sup> [This ought to be 1696. See note on p. 4.]

<sup>2</sup> [This correspondence was published by Edleston in 1850 (*op. cit.*).]

Princess, and at her request communicated to Conti, got abroad and was printed at Paris: on which, in his own defence, he prepared his large work on the subject. On this it is not necessary to speak: his ideas on chronology, founded on the assumption of an accuracy in the older Greek astronomers which nobody now allows them, are rejected and obsolete. But the work does honour to his ingenuity and his scholarship, showing him to be not meanly versed in ancient learning. In 1726, Dr Pemberton completed, at his request, the third edition of the *Principia*. With this he seems to have had little to do, for his health had been declining since 1722. He was relieved by gout in 1725. February the 28th, 1727, he presided for the last time at the Royal Society. He died of the stone (so far as so old a man can be said to die of one complaint) on the 20th of March. All the tributes of respect to his memory belong rather to the biographies of those who had the honour to pay them than to his: the gradual reception of his philosophy throughout Europe belongs to the history of science. We shall now offer some remarks on his character as a philosopher and as a man.

## IX

We have already adverted to the manner in which his biographers have represented him to be as

much above ordinary humanity in goodness as in intellectual power. That his dispositions were generally good and his usual conduct in the relations of life admirable to an extent which should make his worst enemy, if he had any regard to truth, hand him down as a man of high principle, no one who knows his history can deny. But when injustice is not merely concealed but openly defended; when meanness is represented as the right of a great philosopher; when oppression is tolerated, and its victims are made subjects of obloquy because they did not submit to whatever Newton chose to inflict;—it becomes the duty of a biographer to bear more hardly upon instances of those feelings, than, had they been properly represented, would have been absolutely necessary. Nor does it matter anything in such a case that the instances alluded to are the exception in the character and not the rule; forbearance and palliation are so much of injustice towards the injured parties.

The great fault, or rather misfortune, of Newton's character was one of temperament:<sup>1</sup> a morbid fear of opposition from others ruled his whole life. When, as a young man, proposing new views in opposition to the justly honoured authority of Descartes and lesser names, he had reason to look

<sup>1</sup> [On this word, Mrs De Morgan (*Memoir*, p 257) remarked: "My husband always used this word for what I should call original character or inborn disposition." Cf. § XII. of this Essay and §§ VI. and XI. of the third Essay.]

for opposition, we find him disgusted by the want of an immediate and universal assent, and representing, as he afterwards said, that "philosophy was so litigious a lady, that a man might as well be engaged in lawsuits as have to do with her." How could it be otherwise? What is scientific investigation except filing a bill of discovery against nature, with liberty to any one to move to be made a party in the suit? Newton did not feel this; and, not content with the ready acceptance of his views by the Royal Society, a little opposition made him declare his intention of retiring from the field. He had the choice of leaving his opponents unanswered, and pursuing his researches; committing it to time to show the soundness of his views. That this plan did not suit his temper shows that it was not the necessity of answering, but the fact of being opposed, which destroyed his peace. And he steadily adhered, after his first attempt, to his resolution of never willingly appearing before the world. His several works were extorted from him; and, as far as we can judge, his great views on universal gravitation would have remained his own secret if Halley and the Royal Society had not used the utmost force they could command. A discovery of Newton was of a two-fold character—he made it, and then others had to find out that he had made it. To say that he had a right to do this is allowable; that is, in the same sense in which we and our

readers have a right to refuse him any portion of that praise which his biographers claim for him. In the higher and better sense of the word, he had *no right* to claim the option of keeping from the world what it was essential to its progress that the world should know, any more than we should have a right to declare ourselves under no obligation to his memory for the services which he rendered. To excuse him, and at the same time to blame those who will not excuse him, is to try the first question in one court and the second in another. A man who could write the *Principia*, and who owed his bread to a foundation instituted for the promotion of knowledge, was as much bound to write it as we are to thank him for it when written. When he was young and comparatively unknown, this morbid temperament showed itself in fear of opposition; when he became king of the world of science it made him desire to be an absolute monarch; and never did monarch find more obsequious subjects. His treatment of Leibniz, of Flamsteed, and (*we believe*) of Whiston is, in each case, a stain upon his memory. As to Leibniz, it must of course be a matter of opinion how far Newton was behind the scenes during the concoction of the *Commercium Epistolicum*: but from the moment of his appearance *in propria persona*, his conduct is unjust. Leibniz, whose noble candour in unfolding his own discovery, in answer to

Newton's *a b c*, and so on, must have been felt at the time as a stinging reproof, is answered with arrogance (dignified severity is the other name) and treated with unfairness. Nothing can excuse Newton's circulating his reply among his friends in writing, and printing it when he heard of the death of Leibniz: this conduct tells its own story in unanswerable terms. And, if it were Newton's own act and deed, nothing can excuse in him the omission of the Scholium from the third edition, or rather the alteration of it in such manner as to resemble the former one in its general tenor. But, as Newton was then very old, and as he had allowed it to stand in the *second* edition, published when the dispute was at its height, it is possible that he left the matter to Dr Pemberton, the editor, or some other person.

The story of the treatment of Flamsteed has only recently become known, by the late Mr Baily's discovery of the correspondence. Flamsteed was Astronomer Royal, and his observations were to be printed at the expense of the Prince Consort. A Committee, with Newton at its head, was to superintend the printing. If we took Flamsteed's word for the succession of petty annoyances to which he was subject, we might perhaps be wrong; for Flamsteed was somewhat irritable, and no doubt the more difficult to manage because he was the first observer in the world, and not one of the Committee

was an observer at all. But there are two specific facts which speak for themselves. The catalogue of stars (Flamsteed's own property) had been delivered sealed up, on the understanding that the seal was not to be broken unless Flamsteed refused to comply with certain conditions. After the Prince was dead, and the trust had been surrendered (it seems to have been transferred to the Royal Society), and without any notice to Flamsteed, the seal was broken, with Newton's consent, and the catalogue was printed. Halley was exhibiting the sheets in a coffee-house, and boasting of his correction of their errors. A violent quarrel was the consequence, and a scene took place on one occasion at the Royal Society which we cannot discredit (for Flamsteed's character for mere truth of narration has never been successfully impugned, any more than Newton's), but which most painfully bears out our notion of the weak point of Newton's character. As to the breaking of the seal Newton pleaded the Queen's command—an unmanly evasion, for what did the Queen do except by advice? who was her adviser except the President of the Royal Society? Shortly afterwards the second edition of the *Principia* appeared. Flamsteed, whose observations had been of more service to Newton than those of any other individual, and to whom proper acknowledgment had been made in the first edition, and who had increased the obligation in the interval, had his name erased in all

the passages in which it appeared (we have verified, for this occasion, eight or nine places ourselves).<sup>1</sup> To such a pitch is this petty resentment carried, that whereas in one place of the first edition (prop. 18, book III.) there is, in a parenthesis, "by the observations of Cassini and Flamsteed"; the corresponding place of the second is, "by the consent of the observations of astronomers."

There is a letter of Newton to Flamsteed (January the 6th, 1699), written before they were in open rupture, containing an expression which has excited much surprise and some disapprobation. Flamsteed having caused a published reference to be made to Newton's continuation of his lunar researches, the latter 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 your own people to be *trifling away my time* when I should be about the King's business." This letter was not intended for publication, still less for posterity: the phrase was pettish, unworthy even of Newton in a huff. But the feeling was the right one. If there were any thing unworthy of the dignity of Newton, it was in taking a place which required him to give up the glorious race in which

<sup>1</sup> [This is not quite correct. Edleston (*op. cit.*, p. lxxv) also questions very much whether the suppression of Flamsteed's name in several places where it had appeared in the final edition was not such as was necessary in the process of improving the work. Newton's own experiments on the old echo in Trinity College cloister gave way, in the second edition, to more accurate researches.]

he had outstripped all men, and the researches which were for him alone, while the regulation of the Mint was not above the talents of thousands of his countrymen. But, having taken it, it was his duty to attend to it in the most regular and conscientious manner, as in fact he did to the end of his days. His contemporary Swift had the sense to refuse the troop of dragoons which King William offered him before he took orders: it would have been better for Newton's fame if he had left all the coinage, clipped and unclipped, to those who were as well qualified as himself. His own share might not have been so large,<sup>1</sup> but money was not one of his pursuits. He was nobly liberal with what he got,<sup>2</sup> particularly to his own family: and it may be added that the position of his family, which was far from well off in the world, is the only circumstance which can palliate his giving up the intellectual advancement

<sup>1</sup> Sir D. Brewster represents Newton as having a very scanty income before he gained his office in the Mint. But in fact he had from his College board and lodging (both of the best) and the stipend of his fellowship: from the University the salary of his professorship: and from his patrimony about £100 a year. He could not have had less than £250 a year over and above board and lodging: which, in those days, was a very good provision for an unmarried man, and would not be a bad one now.

<sup>2</sup> [Here we may mention that Pemberton is said to have received two hundred guineas for his service in editing the third edition of the *Principia* (Brewster, *Memoirs*, 1855, vol. i, p. 318). For making a Latin translation of the *Optics*, Samuel Clarke and his children received five hundred pounds (*ibid.*, p. 248). *Cf. ibid.*, vol. ii, pp. 411-413, for other instances of Newton's sometimes rather careless generosity. Further, on July the 13th, 1719, Newton gave to Pound, the astronomer, probably in acknowledgment of astronomical observations supplied by him for the *Principia*, a "free gift" of fifty guineas. On April the 28th, 1720, Pound recorded another gift from Newton of fifty guineas. This generosity does not appear in his treatment of rivals.]

of all men, ages, and countries, to trifle away his time about the King's business.<sup>1</sup>

His treatment of Whiston, as published in the autobiography of the latter,<sup>2</sup> was always disregarded, as the evidence of a very singular person. Standing alone—for his conduct to Leibniz was defended by national feeling, and his treatment of Flamsteed was unknown—it never carried much weight. Whiston had excessive vanity and a peculiar fanaticism of his own invention, which were sure to be made the most of; for a man who loses his preferment for his conscience had need be perfect, if he would escape those who think him a fool, and those who feel him a rebuke. And in Whiston's day the number was not small of the clergy who disavowed the articles to which they had sworn, without even having the decency to provide a *non-natural sense*. Newton refused him admission into the Royal Society, declaring that he would not remain president if Whiston were elected a fellow. A reason is asserted for this which we shall presently notice; but Whiston's account is as follows. After alluding to Newton having made him his deputy, and then his successor, he adds: "So did I enjoy a large portion of his favour for twenty years together. But he then perceiving that I could not

<sup>1</sup> [For Brewster's version of the Flamsteed episode, see *Memoirs*, 1855, vol. ii, pp. 157-242.]

<sup>2</sup> [*Memoirs of the Life of Mr William Whiston by himself*, London, 1749.]

do as his other darling friends did—that is, learn of him without contradicting him when I differed in opinion from him,—he could not in his old age bear such contradiction; and so he was afraid of me the last thirteen years of his life. He was of the most fearful, cautious, and suspicious temper that I ever knew."

It would have been more pleasant merely to mention these things as what unfortunately cannot be denied, than to bring them forward as if it were our business to insist upon them. But the manner in which the biography of Newton is usually written leaves us no alternative. We are required to worship the whole character, and we find ourselves unable to do it. We see conduct defended as strictly right, and therefore, of course, proposed for imitation, which appears to us to be mean, unjust, and oppressive. As long as Newton is held up to be the perfection of a moral character, so long must we insist upon the exceptional cases which prove him to have been liable to some of the failings of humanity. But to those who can fairly admit that his conduct is proof of an unhappy temper which sometimes overcame his moral feeling, and who therefore look for the collateral circumstances which are to excuse or aggravate, there are various considerations which must not be left out of sight.

In the first place, this temperament of which we have given instances, is of all others the one which

occasionally lessens the control of the individual over his own actions. Every one knows how apt we are, from experience, to think of insanity as the possible termination of the morbidly suspicious habit. That the report which arose about Newton's mind was much assisted by a knowledge of this habit existing in him, we have little doubt: for we see, in our own day, how corroborative such a temper is held to be of any such rumour. In one instance, and in illness of a serious character, it did take a form which we can hardly hold consistent with sanity at the time. He spoke severely of Locke, his old and tried friend (in 1693), being under the apprehension that Locke had endeavoured to "embroil him with women and by other means"; he thought there was a design to "sell him an office and to embroil him." For these suspicions he wrote a letter, worthy of himself, asking pardon, and saying also that he had been under the impression that there was an evil intention, or tendency at least, in some of Locke's writings. The latter, in an affectionate answer, desired to know what passages he alluded to; and the rejoinder was that the letter was written after many sleepless nights, and that he had forgotten what he said. As we have only the letters and no further information, we must decide as we can whether Newton did really express himself to others as he said he had done, or whether he only fancied it. In either case

there is, under illness, that morbid imagination of injury done or meditated, which seems to have been but the exaggeration of an ordinary habit. If we thought, from the evidence, that Newton had ever been insane, we should see no reason whatever for concealing our opinion: we do not think so; but we think it likely that if his years from 1660 to 1680 had been passed in the excesses of the licentious court of his day, instead of the quiet retirement of his college, there might have been another story to tell.

Next, it is not fair to look upon the character of any man, without reference to the notions and morals of his time. Take Newton from his pinnacle of perfection, from the background of the picture, from the incidents of the era of political and social profligacy in which he lived, and his relative character then seems to be almost of the moral magnificence which is made its attribute. Let the sum total of his public career be compared with that of others who were "about the King's business," and we cannot help looking upon the honest and able public servant, who passed a life in the existing corruption of public affairs without the shadow of a taint upon his official morals, with an admiration which must tend to neutralise the condemnation we may not spare upon some incidents of his scientific life. Further, the idolatrous respect in which he was held at the Royal Society, and the other haunts

of learning—the worship his talents received at home and abroad, from Halley's<sup>1</sup> “*nec fas est propius mortali attingere divos,*” to de l'Hôpital's almost serious question whether Newton ate, drank, and slept—the investment of his living presence with all the honours once paid to the memory of Aristotle—make it wonderful, not that he should sometimes have indulged an unhappy disposition, but that he should have left so few decided instances of it on record. That both his person and his memory were held dear by his friends there is no doubt: this could not have been unless the cases we have cited had been exceptions to the tenor of his conduct; and, knowing the disposition of which we have spoken to be one against which none but a high power can prevail, we are to infer that it was, in general, heartily striven against and successfully opposed.<sup>2</sup>

## X

The mind of Newton, as a philosopher, is to this day, and to the most dispassionate readers of his works, the object of the same sort of wonder with which it was regarded by his contemporaries. We can compare it with nothing which the popular reader can understand, except the idea of a person

<sup>1</sup> “Nor is it possible for man to be nearer to God”: the last line of Halley's verses on the *Principia*.

<sup>2</sup> [For De Morgan's view of Newton's character, see also end of §§ II and VI. of the third Essay, below.]

who is superior to others in every kind of athletic exercise; who can outrun his competitors with a greater weight than any one of them can lift standing. There is a union, in excessive quantity, of different kinds of force: a combination of the greatest mathematician with the greatest thinker upon experimental truths; of the most sagacious observer with the deepest reflecter. Not infallible, but committing, after the greatest deliberation, a mistake in a simple point of mathematics, such as might have happened to any one: yet so happy in his conjectures, as to seem to know more than he could possibly have had any means of proving. Carrying his methods to such a point that his immediate successors could not clear one step in advance of him until they had given the weapons with which himself and Leibniz had furnished them a completely new edge, yet apparently solicitous to hide his use of the most efficient of these weapons, and to give his researches the appearance of having been produced by something as much as possible resembling older methods. With few advantages as a writer or a teacher, he wraps himself in an almost impenetrable veil of obscurity, so as to require a comment many times the length of the text before he is easily accessible to a moderately well-informed mathematician. He seems to think he has done enough when he has secured a possibility of finding one reader who can understand him

with any amount of pains : as if, seeing Halley to be of all men he knew next to himself in force, he had determined that none but Halley at his utmost stretch of thought should follow him. Accordingly one, to whom in his later years he used to send inquirers, saying, "Go to Mr De Moivre, he knows these things better than I do," avowed that when he saw the *Principia* first, it was as much as he could do to follow the reasoning. It would be difficult to name a dozen men in Europe of whom, at the appearance of the *Principia*, it can be proved that they both read and understood the work.

Newton himself attributed all his success to patience and perseverance more than to any peculiar sagacity : but on this point his judgment is worth nothing. Unquestionably, he had the two first in an enormous degree, as well as the third ; nor is it too much to say that there is no one thing in his writings which the sagacity of some of his contemporaries might not have arrived at as well as his own. But to make an extensive system many things are necessary : and one point of failure is fatal to the whole. Again, it is difficult to put before the ordinary reader, even if he be a mathematician, a distinct view of the merit of any step in the formation of a system. Unless he be acquainted with the history of *preceding* efforts, he comes to the consideration of that merit from the wrong direction ;

for he reads the history from the end. He goes to the mail-coach, back from the railroad instead of forward from the old strings of pack-horses : from a macadamised road lighted with gas to the rough stones and the oil-lamps, instead of beginning with the mud and the link-boys. Perhaps the same sort of wrong judgment may accompany the retrospect of its own labours in a mind like Newton's ; causing it to undervalue the intellectual part of which, in any case, it is least capable of judging.

The world at large expects, in the account of such things, to hear of some marvellous riddles solved, and some visibly extraordinary feats of mind. The contents of some well-locked chest are to be guessed at by pure strength of imagination : and they are disappointed when they find that the wards of the lock were patiently tried, and a key fitted to them by (it may be newly imagined) processes of art. Thus the great experiment, the trial of the moon's gravitation, seems wonderfully simple to those who have to describe it ; precisely what anybody could do. If the moon were not retained by some force, she would proceed in a straight line MB :<sup>1</sup> something causes her to describe MA instead, which is equivalent to giving a fall of BA towards the earth. Now since EM, the distance of the moon from the earth's centre, is about 60 times EC, the earth's

<sup>1</sup> [This refers to a simple figure which it is not necessary to reproduce here, as anybody can reproduce it for himself from what is said in the text.]

radius, it follows that if there be gravitation at the moon, and if it diminish as the square of the distance increases, it *ought to be* 60 times 60, or 3600 times as great at the surface of the earth as at M; or a body at the earth's surface ought to fall in one minute 3600 times as much as BA (supposing MA to be the arc moved over in one minute). A surveyor's apprentice, even in Newton's day, could with great ease have ascertained that such *is* the fact, if the data had been given to him. Now why was Newton the first to make this simple trial? The notion of gravitation was, as we have said, afloat: and Bouillaud had declared his conviction that attractive forces, if they exist, must be inversely as the squares of the distances. Did he try this simple test? Perhaps he did, and threw away his result as useless, not being able to make the next step. Or was it that neither he nor any one except Newton had any distinct idea of measuring from the centre of the earth? If so, then Newton was in possession of what he afterwards proved, namely, that a spherical body, the particles of which attract inversely as the squares of the distances, attracts as if all its particles were collected in its centre.<sup>1</sup> In either case, this may serve to illustrate what a popular reader would hardly suppose, namely, that the wonder of great discoveries consists in there

<sup>1</sup> [Newton explicitly stated that he only discovered this theorem in 1685; *cf.* above, note 30.]

being found one who can accumulate and put together many different things, no one of which is, by itself, stupendous after the fact, nor calculated to produce that sort of admiration with which the whole is regarded.

## XI

We have not yet mentioned the theological writings of Newton, as his discussion of the prophecies of Daniel, and so on. About his opinions on this subject there is a little controversy: and the various sects of opinion are in the habit of opposing to each other the great names which are on their several sides of the question. That Newton was a firm believer in Christianity as a revelation from God, is very certain: but whether he held the opinions of the majority of Christians on the points which distinguish Trinitarians from Arians,<sup>1</sup> Socinians, and Humanitarians, is the question of controversy. It is to be remembered that during the whole of Newton's life the denial of the doctrine of the Trinity was illegal, the statute of King William (which relaxed the existing law, for a man was

<sup>1</sup> These names are bandied about in vituperative discussions, until they are so misused that the chances are many readers will need explanation of them. An Arian believes in the finite pre-existence of Jesus Christ, before his appearance on earth: a Socinian believes him to be a man who did not exist before his appearance on earth, but who is still a proper object of prayer: a Humanitarian, with all others who come under the general name of Unitarian (the personal unity of the Deity being a common tenet of all), believes him to be a man, and not an object of prayer.

hanged in 1696 for denying the Trinity) making it incapability of holding any place of trust for the first offence, and three years' imprisonment with other penalties for the second. Few therefore wrote against the Trinity, except either as, in the Unitarian Tracts, without even a printer's name, or evasively, by arguing against the Trinity being an *article of faith*, that is, a necessary part of a Christian's hope of salvation. Premising this, we take the evidence, as it stands, for and against the heretical character of Newton's opinions.

There is a widespread tradition that Horsley objected to publish a part of the "Portsmouth Papers" on account of the heresy of the opinions contained in them; which statement used to be even in children's books, and was made by Dr Thomson in his *History of the Royal Society*. These papers have never been published, nor has any one of those who have had access to them denied the rumour on his own knowledge. The refusal of Horsley is not conclusive in itself; because, to use the words of one of the children's books we remember (called a "British Plutarch," or some such name), he was a "rigid high priest," and heterodoxy short even of Arianism would probably have led him to such a determination. But the suppression still continues, long after the above rumour has been very effective in aiding the probabilities drawn from other sources, that Newton's

opinions were even more heterodox than Arianism; and there is some force in this.

Two witnesses from among Newton's personal friends, Whiston, an Arian (calling himself a Eusebian), and Hopton Haynes, who was employed under him in the Mint, and who was a Humanitarian, severally bear testimony to his having held their several opinions. Whiston, whose intimate acquaintance with him terminated some time before 1720, states in two places that Newton was a Eusebian (Arian) and a Baptist, and that he was "inclined to suppose" these two sects to be the two witnesses<sup>1</sup> mentioned in the book of Revelations. Haynes<sup>2</sup> declares him to have been a Humanitarian, and stated that he much lamented that his friend Dr Clarke had stopped at Arianism. On the other hand, the writer in the *Biographia Britannica*, who

<sup>1</sup> This is strange; and if such had been Whiston's own opinion, we should not have hesitated to conclude that he had misinterpreted some civil decliner of controversy. But Whiston expressly states himself to have no such opinion. That he would intentionally utter a falsehood we believe to be out of the question.

<sup>2</sup> The testimony of Whiston is in his *Memoirs*: that of Haynes is less direct. The Unitarian minister, Richard Baron, who was a friend of Haynes, states the preceding as having passed in conversation between him and Haynes. The statement is made in the preface of the first volume of his collection of tracts, called *A Cordial for Low Spirits* (three volumes, London, 1763, third edition, 12mo), published under the name of Thomas Gordon. This is not primary evidence like that of Whiston; and it loses force by the circumstance that in the posthumous work which Mr Haynes left on the disputed points (and which was twice printed) there is no allusion to it. But those who weigh testimony will of course take into continued consideration its amount of corroborative force. And a great many writers on the Antitrinitarian side deserve blame for not stating distinctly that it is only a testimony to a testimony: Baron was a man against whose character for truth we never heard anything, but the chances of misapprehension increase very rapidly with the number of steps, in the communication of oral tradition.

cites the last edition<sup>1</sup> (1753) of Whiston's *Memoirs*, says that Whiston states that Newton was so much offended with him for having represented him as an Arian, that this was the reason why he would never consent to his admission into the Royal Society. The edition of 1749, thirteen years after Newton's

<sup>1</sup> Though aware that we should have many results of bias to encounter, we had hoped that we should have got through our task without having to expose absolute and fraudulent falsification. Since writing what is in the text, we have obtained the loan of the edition of 1753, which is scarce compared with that of 1749. The *Biogr. Brit.* informs us (p. 3241) that in pages 178, 249, 250 of Whiston's *Memoirs*, edition of 1753, 8vo, we shall find the justification of these words: "Mr Whiston, who represented Sir Isaac as an Arian, which he so much resented that he would not suffer him to be a member of the Royal Society while he was President." We look, and in p. 178 we find that Whiston states Newton to be an Arian, and in pp. 249 and 250 we find that Newton excluded Whiston from the Royal Society, for which the reason Whiston gives is that Newton could not bear contradiction, in the words we have quoted in another part of this article. The biographer distinctly implies that he is giving, not his own reason, but *Whiston's reason*. And, having diligently compared the editions of 1749 and 1753 (the latter of which *had some additions*, by which the false biographer hoped to gain credit from those who looked at the former), we find that the paragraphs cited only differ as follows: In the first, 1749 has *Revelation*, 1753 has *Revelation*. The former has "and friendly address to the Baptists" (pp. 14, 15), which the latter has not. In the second, 1749 has "desire" and 1753 has "desires" (a little instance, by the way, of the disappearance of the old English subjunctive), and the former has "through confutation," when the latter has "thorough confutation." Sir D. Brewster (p. 284) has copied the false biographer without verifying the reference—a common, but a dangerous practice. It was a mere accident that we went to the *Biogr. Brit.*, for we distrust it from old acquaintance on all matters connected with Newton. We do not know at this moment that the false biographer, as we call him, is the original falsifier: but he must bear the blame for the present. We might have had to leave the explanation to Sir D. Brewster: for he who copies a reference without verification, and without stating that he copies, must take the responsibility of that reference. But as it stands, we need not say that Sir D. Brewster is as clear in this instance from the imputation of intentionally misleading his reader, as those could wish who respect his character and admire his labours: among the number of whom we desire to place ourselves. And his candour will lead him to acknowledge that he has had a happy escape from an imminent danger of misconstruction, with no blame to those who made it.

death, shows that Whiston had then no such knowledge of the cause. But, if it were so, and Haynes's testimony be true, he might have had Priestley's objection to Arianism rather than Horsley's: and in either case, we know enough of Newton to be sure that he would be likely to take offence at any talk about opinions he did not choose to avow, particularly such as were illegal; and above all, he would fear the tongue of a man like Whiston, all honesty and no discretion, who told the world long before his death all that he knew about himself and everybody else, without the least reserve.

Newton wrote (about 1690), under the title of "Historical Account of two Notable Corruptions of Scripture," against the genuineness of two passages on which Trinitarian<sup>1</sup> writers then placed much reliance: that is, against the genuineness of 1 John v. 7, and that of the word *θεός* (God), 1 Timothy iii. 16. Now, though Trinitarians have often abandoned the first passage, and given up the Protestant reading of the second, it has rarely happened, if ever, that they have written expressly against them: the world at large sees no difference between opposing an argument, and opposing the conclusion; and parties in religion and politics require<sup>2</sup> assent,

<sup>1</sup> Protestant writers, we mean; the reading contended for by Newton in the second instance has been that of Catholics from the time of Jerome.

<sup>2</sup> Dr Chalmers, for example, states Newton to have "abetted" the leading doctrine of the Unitarians: whether upon the evidence of this writing only, or the general evidence, does not precisely appear:

not merely to their tenets, but to each and every mode of maintaining them. And writers who go so far as to say anything against one mode of supporting their own side of a question, generally make a decided profession of adherence to the *conclusion* while they reason against one mode of maintaining it. Newton does no such thing: his expressions are vague, or, if not vague, they are the formular<sup>1</sup> words under which the opponents of the

probably upon the former alone. The author of the *Life* in the *Biographia Britannica* does not mention these letters. But it appears by the testimony of Le Clerc and Wetstein, that Locke sent them to Le Clerc, who did not know their author. The possessors of Newton's papers never published them until an incomplete edition had appeared abroad.

<sup>1</sup> Sir D. Brewster, to whom the admirers of Newton have much obligation, and from whom they expect more, in the larger *Life* on which he is known to be engaged, argues from these words, which he quotes formally, that Newton received the Trinity. But, having the work before him, he should also have destroyed the effect of the following *words of Newton*:—"He (Cyprian) does not say the Father, the Word, and the Holy Ghost, as it is now in the 7th verse, but the Father, the Son, and the Holy Ghost, as it is in Baptism, *the place from which they tried at first to derive the Trinity.*" We never were quite satisfied till we saw this passage. We found the Trinitarian writers evidently shy of the question: and the Antitrinitarians as evidently laying such an undue stress on Mr Haynes's testimony, or rather Mr Baron's testimony to Mr Haynes's testimony, as made us suspect that our authorities on both sides were not fully satisfied in their own minds. But we hold it to be out of the question that a Trinitarian could have written the words in our italics. That many would not admit the baptismal form in itself to be a proof of the doctrine, is known; but what Trinitarian ever talked of a "they" who tried a text to prove the doctrine, "at first," implying that they failed, and then went to others? the clear implication being that he thought they had the doctrine before they tried any texts. Again, there is the following. Speaking of the manuscript on which Erasmus at last introduced 1 John v. 7 into his text, he says that the English, "*when they had got the Trinity into his edition, threw by their manuscript (if they had one) as an almanac out of date.*" Now most of our readers are Trinitarians, and know whether this is the way in which those who hold that doctrine speak of it. The citations above are from Horsley's *Newton*.

When M. Biot said that there was absolutely nothing in Newton's writings which was other than orthodox, he must have meant in the

received doctrine avoided imprisonment. The truth is to be purged of things spurious: the faith subsisted before these texts were introduced or changed; it is not an article of faith or a point of discipline, but a criticism, and so on. There is an expression towards the end which admits of a double interpretation: "if the ancient churches, in debating and deciding the greatest mysteries of religion, knew nothing of these two texts; I understand not, why we should be so fond of them now the debates are over." The first clause, by itself, might rather have been written by a Trinitarian: though a Unitarian might write it, more especially if he wanted a formular phrase. But the second clause looks very like a formula: for there was no time at which the debate raged so fiercely as in the day of Newton, which was that of Wallis, South, Sherlock, and so on, and hosts of anonymous writers. We find it difficult to suppose that Newton, whose friendship with Locke, Clarke, and Whiston at that time was notorious, would do that which none but Antitrinitarians, or very few, ever did, in a communication to an Antitrinitarian intended at that time for publication abroad, without making a definite avowal of the orthodoxy of his belief, if he had it to make. It is right to state, on the other

writings which he had seen. This of course may have been the case. Moreover, what is more absurd than to argue from his silence that a man does not hold an opinion for which he might be ruined and imprisoned, or, up to 1699, even hanged? [See the first note to this Essay.]

side, Bishop Burgess's argument: that this was a writing which Newton suppressed from publication. *Printing* should have been the word: Newton published it when he caused it to be sent to Le Clerc. There is to us something corroborative, or at least significative of much difference from the most common opinion, in the Scholium which he added at the end of the second edition of the *Principia*. With Jewish and Christian writers, Deity is necessarily from eternity and without superior: the word *God* implies both necessary existence and omnipotence. With the Greeks, divine power might be communicated in such a manner that a hero, for instance, after death, might become as truly the object of worship as Jupiter himself. Newton adopts the Greek definition, or one very like it. The rule of a spiritual being makes him God. "Dominatio entis spiritualis Deum constituit." And as if this were not precise enough, he adds, in the third edition, a note stating that thus the souls of dead princes were called gods by the Gentiles, *but falsely, from want of dominion*. He then proceeds to his well-known reflections on the Supreme Deity.

We have entered into this question, not from any particular interest in it—for there are too many great minds on both sides of the controversy to make one more or less a matter of any consequence to either,—but because we have a curious matter of

evidence, and an instructive view of party methods of discussion. Whatever Newton's opinions were, they were in the highest degree the result of a love of truth, and of a cautious and deliberate search after it. His very infirmity is a guarantee for the existence of this feeling in no usual measure. With a competent livelihood, and the dread of discussion so strong that he would gladly have hidden his results from the world rather than encounter even respectful opposition, he could not have worked either for the hope of wealth or office, or even for the love of fame, except in a very secondary degree. The enthusiasm which supported him through the years of patient thought out of which the *Principia* arose, must have been strong indeed when he had no ultimate worldly end to propose to himself. Who can say how much of the truth of his system we may owe to this very position? Had he been desirous of pleasing, he must have had strong temptation to build upon some of the prevailing notions; to have a little mercy upon the physics of Descartes. Or even without going so far, a small portion of the vanity which loves to present complete systems and to confess no ignorance, might have biased him to adopt such an addition to his law of attractive force (such a one as Clairaut for a little while thought necessary) as, without interfering with the main phenomena, would have served to bring out some more explanations. But he had no such bias: and

speaking of his philosophic character, it may be said that never was there more of the disinterested spirit of inquiry, unspurred by love of system, unchecked by dread of labour or of opinion. For, however much he might dislike or fear opposition, there was one tribute to it which his philosophy never paid; the pages which he would gladly have burned rather than encounter discussion, contain no concession whatever.<sup>1</sup>

## XII

In concluding this brief outline of a truly great man, one of the first minds of any age or country, of whose labours the world will reap the fruits in every year of its existence, we cannot help expressing our hope that future biographers will fairly refute, or fairly admit, the existence of those blots of temper to which the indiscriminating admiration of preceding ones has obliged us to devote so much of the present article. Of the facts, where we have stated them as facts, we are well assured; and there can be no reason why the warnings which the best and greatest of the species must sometimes hold out to the rest, should be softened, or, what is worse, converted into examples of imitation, by fear of opposing an established prejudice, or by the curious tendency of biographers to exalt those of whom

<sup>1</sup> [On Newton's religious opinions, see also § VIII. of the third Essay, below.]

they write into monsters of perfection. Surely it is enough that Newton is the greatest of philosophers, and one of the best of men—that all his errors are to be traced to a disposition which seems to have been born<sup>1</sup> with him—that, admitting them in their fullest extent, he remains an object of unqualified wonder, and all but unqualified respect.

For reasons which will be easily understood, the author of this article subscribes his name.

A. DE MORGAN.

<sup>1</sup> We cannot trace, in Newton's character, an *acquired failing*; nothing but the manifestations of the original disposition due to different circumstances.

II  
A SHORT ACCOUNT OF SOME  
RECENT DISCOVERIES RELATIVE  
TO THE CONTROVERSY ON THE  
INVENTION OF FLUXIONS

A SHORT ACCOUNT OF SOME  
RECENT DISCOVERIES IN ENGLAND  
AND GERMANY RELATIVE TO THE  
CONTROVERSY ON THE INVENTION  
OF FLUXIONS<sup>1</sup>

THE celebrated controversy on the invention of fluxions has, any one would suppose, been so fully argued that it would be difficult to make out a reasonable case for introducing the subject again. It is nevertheless true that several disclosures of great importance in the way of evidence have never been made at all until very lately.

This controversy resembles one of those well-worn law cases which must be cited and discussed whenever a certain question arises. Every dispute about

<sup>1</sup> [This Essay was printed in *The Companion to the Almanac: or, Year-Book of General Information for 1852*, pp. 5-20, which was published at London by Charles Knight as a supplement to *The British Almanac of the Society for the Diffusion of Useful Knowledge, for the year of our Lord 1852*, and of which the first part, in which the present Essay was included, contained "general information on subjects of mathematics, natural philosophy and history, chronology, geography, statistics, etc." It seems to have been the first English consideration of the fluxional controversy in the light of the discoveries of Gerhardt among Leibniz's manuscripts in the Royal Library of Hanover. Notes on the literature relating to the controversy, and on the early fluxional manuscripts of Newton and Leibniz, are given below in the Appendix to this Essay.]

priority of mathematical invention<sup>1</sup> revives it. At the same time, the main and turning points of it can be presented without any such amount of mathematical language as would render an article upon the subject unfit for the majority of readers. We therefore propose to present some of these points, with an account of the recently published materials, and of their bearing on the result.

When, after some petty and indecisive controversy, Leibniz appealed (1711) to the Royal Society for protection against imputations of plagiarism which had at last assumed a distinct form, the Society, in 1712, appointed the celebrated *partisan*<sup>2</sup> Committee to maintain the side of Newton. The report of this Committee, published with epistolary evidence in 1712, under the name of *Commercium Epistolicum*,<sup>3</sup> contains the following sentence, which is the whole

<sup>1</sup> One most fortunate circumstance about it, as a precedent, is that it fixed the meaning of the word "publication" to the genuine and legal sense. It is the sufficient answer to any one who would restrict this word to its colloquial sense of circulation by means of type.

<sup>2</sup> We have shown the Committee to have had this character in *Phil. Trans.*, part ii. for 1846, and in the life of Newton in *Knight's British Worthies*; and nobody has contested the point. It was, however, universally believed that the intended function of the Committee was judicial, and both Newton and Leibniz speak of it as if it had been so. But though the Committee itself overstepped its own proper function in the form of its decision, and thereby gave rise to the misconception, we hold the intention of its proposers to have been stated with perfect clearness. [On De Morgan's paper in the *Philosophical Transactions* for 1846, and on the subsequent occurrences, see the above Preface to these Essays and Appendix ii. to the third Essay below.]

<sup>3</sup> We cannot here detail all the circumstances. The reader may consult the articles "*Commercium Epistolicum*" and "*Fluxions*" in the *Penny Cyclopædia*, the life of Newton already cited, Brewster's *Life of Newton*, that in the Library of Useful Knowledge, or Weld's *History of the Royal Society*.

of that report, so far as it insinuates that Leibniz did take, or might have taken, his method from that of Newton:—"And we find no mention of his (*i.e.* Leibniz's) having any other *Differential Method* than *Mouton's* before his Letter of 21st of June 1677, which was a Year after a Copy of Mr *Newton's* Letter, of 10th of December 1672, had been sent to *Paris* to be communicated to him; and about four Years after Mr *Collins* began to communicate that Letter to his Correspondents; in which Letter the Method of *Fluxions* was sufficiently describ'd to any intelligent Person."

The Committee in their English have "any intelligent person"; in their Latin, subjoined for foreigners, they have "idoneo harum rerum cognitori." Raphson, no stickler for accurate description, as we shall see, could not second this; so he converts the Latin into the original, and gives his own English translation, "to any proper judge of these matters." But even this was too much; so some one else (copied by Hutton in his *Dictionary*; we do not think Hutton did it himself) has invented a new report, in which we find "a man of his sagacity."

How far this celebrated letter deserves the character here given of it, is one question; whether Leibniz actually received it, is another. Comparatively little notice was taken of either; so that in many subsequent writings it reminds us of the tree

which was cut down that the action for trespass might try the ownership of the estate. It gives, nevertheless, the only possibility, such as it is, which the evidence offers of Leibniz having seen anything to the point from the pen of Newton.

In order to prove the passage quoted above, it is stated that there existed, among the papers of Collins in the possession of the Royal Society, in the handwriting of Collins, a parcel (*collectio*) of papers containing extracts from Gregory's letters, together with the letter of Newton above-mentioned (but which was not alluded to in the title or docket which Collins placed on the parcel), and that the parcel was marked as to be communicated to Leibniz, and was accompanied by a copy of a letter to Oldenburg, the party who was to make the communication. Not a word is said on the date at which the parcel was transmitted: so that the Committee, in their report, actually added a statement for which there was no pretext of evidence, namely, that Newton's letter was transmitted about a year before the 21st of June, 1677. Further, the evidence does not mention the date at which Collins died (1682), nor how his papers came into the possession of the Society, nor whether there was any guarantee that papers found tied together in 1712 had been so tied up by Collins before 1682, nor whether there was any evidence that Collins had fulfilled his intention of sending the parcel on

to Oldenburg, and so on. When Leibniz, who did not remember receiving any such letter, declared that he did not think it necessary to answer anything so weak, his contempt for this unattested statement was of course construed by the other side as being of that kind which parties who cannot answer find it convenient to assume.

The editors, whoever they were, of the reprint<sup>1</sup> of the *Commercium Epistolicum*, made under the sanction of the Royal Society in 1722, took the liberty of secretly making a few additions<sup>2</sup> and alterations. Among these, they add the date at which Collins died, and the date of transmission of the parcel: they say it was sent on June the 26th, 1676. How they got this date is not said; but as the next parcel sent by Oldenburg to Leibniz was stated to have been sent on June the 26th, it may have happened that the revisers of the second edition borrowed this date for their purpose.

So the matter rested until recently, when the publication of a portion<sup>3</sup> of Leibniz's papers took

<sup>1</sup> We say "reprint," and not "second edition," because even the old title-pages and the old date (1712) were reprinted. Everything was done which could lead the reader to suppose that he had in every respect a repetition of the original work, preceded by a preface of the new editors.

<sup>2</sup> This fact was discovered by us in 1848; and the additions are exposed in the *Philosophical Magazine* for June 1848. The first edition is now scarce. [See the above Preface and Appendix ii. to the third Essay.]

<sup>3</sup> *Leibnizens mathematische Schriften, herausgegeben von C. J. Gerhardt*, Berlin, 8vo. Erste Abtheilung, Band I, 1849, Band II, 1850. We have not seen any more, if indeed any more has yet appeared. [*Leibnizens mathematische Schriften* were edited by C. I. Gerhardt as

place. And it now appears that if the manuscripts which Leibniz left behind him, and which found their way into the Royal Library at Hanover, had been examined, it could have been ascertained *what Leibniz really did receive from Oldenburg*. It appears that the latter wrote to the former from London, with the date of *July* the 26th, 1676, not forwarding Collins's parcel, but describing its contents<sup>1</sup> himself. He gives various matters connected with Gregory's researches, and then proceeds to allude to a method in a letter from Newton of December the 10th, 1672. But though he gives, almost *verbatim*, what we may call the *descriptive*

the third series (*Dritte Folge: Mathematik*) of G. H. Pertz's edition of *Leibnizens gesammelte Werke aus den Handschriften der Königlichen Bibliothek zu Hannover*, and were published in seven volumes. In the first division (*Abtheilung*), vol. i (Berlin, 1849) contained the correspondence with Oldenburg, Collins, Newton, Galloys, and Vitale Giordano; vol. ii (Berlin, 1850) contained the correspondence with Huygens and de l'Hôpital; vol. iii (Halle, 1855) contained that with Jacob, Johann, and Nicolaus Bernoulli; and vol. iv (Halle, 1859) that with Wallis, Varignon, Guido Grandi, Zandrini, and Tschirnhaus. The second division consists of three volumes of Leibniz's mathematical writings, published and unpublished. However, none of the important papers written by Leibniz when discovering the calculus, which were published by Gerhardt in 1848 and 1855 (see the Appendix to this Essay), were included in these volumes. Vol. v (numbering consecutively to the others) was published at Halle in 1858, and contained those mathematical writings which were either published (1666-1713) or intended for publication; vol. vi (Halle, 1860) contained writings on dynamics from 1671 to 1706; and vol. vii (Halle, 1863) was on "Initia mathematica; Mathesis universalis; Arithmetica; Algebraica;" and "Geometrica." Gerhardt also published at Berlin in 1899 the *Briefwechsel* mentioned in the Appendix to this Essay.]

<sup>1</sup> Collins had desired, in the title of the parcel, that the contents after being read by Leibniz, should be returned to himself. Oldenburg appears to have thought it more prudent to write his own account than to trust the papers to accident by land and sea. (At least, this was our impression before we came to the discovery presently mentioned.)

*paragraph*<sup>1</sup> of this letter, he does not even allude to the *example of the method*, in which, according to the report of the Committee, the method of fluxions is sufficiently described to any intelligent person. So that, with reference to this asserted *description of the method of fluxions*, there is now clear and positive evidence that Leibniz did not receive it as stated, but received only an account of the *rest of the letter*, which describes the *sort of results* attainable.

Towards the end of 1850 the Master and Fellows of Trinity College, Cambridge, published (from among their manuscripts)<sup>2</sup> the correspondence of

<sup>1</sup> "Defuncto Gregorio," says Oldenburg, "congressit Collinius amplum illud commercium litterarium, quod ipsi inter se coluerant, in quo habetur argumenti hujus de seriebus historia: cui Dn. Newtonus pollicitus est se adjecturum suam methodum inventionis illius, prima quaque occasione commoda endendam; de qua interea temporis hoc scire præter rem non fuerit, quod scilicet Dn. Newtonus cum in literis suis Decbr. 10. 1672 communicaret *nobis* methodum ducendi tangentes ad curvas geometricas ex æquatione experimete relationem ordinarum ad Basin, subjicit hoc esse unum particulare, vel corollarium potius, methodi generalis, quæ extendit se absque molesto calculo, non modo ad ducendas tangentes accomodatas omnibus curvis, sive Geometricas sive Mechanicas, vel quomodocunque spectantes lineas rectas, aliisve lineis curvis; sic etiam ad resolvenda alia abstrusiora problematum genera de curvarum flexu, areis, longitudinibus, centrīs gravitatis etc. Neque (sic pergit) ut Huddeniū methodus de maximis et minimis, proinde que Slusii nova methodus de tangentibus (ut arbitror) restricta est ad æquationes, surdarum quantitatū immunes. Hanc methodum se intertextuisse, ait Newtonus (sic), alteri illi, quæ æquationes expedit reducendo eas ad infinitas series; adjicit que, se recordari, aliquando data occasione, se significasse Doctori Barrovio lectiones suas jam edituro, instructum se esse tali methodo ducendi tangentes, sed avocamentis quibusdam se præpeditum, quominus eam ipsi describeret."

The word *nobis*, put by us in italics, should be *ei*; Oldenburg forgot that he was describing, not copying, the account Collins had given him.

<sup>2</sup> *Correspondence of Sir Isaac Newton and Professor Cotes . . . now first published from the originals in the Library of Trinity College, Cambridge, together with an appendix . . . by J. Edleston, M.A., Fellow of Trinity College, Cambridge, London, 1850, 8vo.*

Newton and Cotes, with what is called a synoptical view of Newton's life. This is far below sufficient description; for the synopsis is followed by a body of notes of such research and digestion as make it difficult to give adequate praise to the whole without appearance of exaggeration. We differ much from the editor as to many matters of opinion and statements the character of which is determined by opinion; and we take particular exception to the following account<sup>1</sup> of the point before us:—

“Doubts have been expressed whether these papers<sup>2</sup> were actually sent to Leibniz. We have, however, Collins's own testimony that they were sent as had been desired,<sup>3</sup> besides Leibniz's and Tschirnhaus's acknowledgments of the receipt of them.<sup>4</sup> It may also be observed that the papers actually sent (in a letter dated July the 26th, 1676) to Leibniz by Oldenburg have been recently printed from the originals in the Royal Library at Hanover,<sup>5</sup> and that in them, as in Collins's draught, which is preserved at the Royal Society ('To Leibnitz, the 14th of June, 1676 About Mr. Gregories remains,' MSS. lxxxix.), we find the contents of Newton's letter of December the 10th, 1672, except that instead of the example of drawing a tangent to a curve, there is merely allusion made to the method.

<sup>1</sup> *Op. cit.*, p. xlvi.

<sup>2</sup> *Comm. Epist.*, p. 47; 2nd ed., p. 128.

<sup>3</sup> *Ibid.*, pp. 48 or 129 respectively.

<sup>4</sup> *Ibid.*, pp. 58, 66 or 129, 142 respectively.

<sup>5</sup> *Leibn. Math. Schrift.*, Berlin, 1849.

Collins's larger paper (called 'Collectio' and 'Historiola' in the *Commercium Epistolicum*), of which the paper just quoted 'About Mr. Gregories remains' is an abridgment, and which contains Newton's letter of December the 10th without curtailment, is stated in the second edition of the *Commercium* to have been sent to Leibniz, but whether that was the case may be fairly questioned.”

There are two things in which we have never failed. We have never examined a point of mathematical history without finding either error or difficulty arising from bad bibliography: and we have never come fresh to this controversy of Newton and Leibniz without finding new evidence of the atrocious unfairness of the contemporary partisans of Newton. Nor had we a perception, until we wrote out the preceding paragraph, of the full extent of what it proves. It proves that at the time when the Committee of the Royal Society mentioned the “collectio” which contained Newton's letter *uncurtailed* of any part relating to fluxions, and asserted in their final report (without venturing to mention it in its place) that this letter had been forwarded to Leibniz—they had, and must have seen, among the papers they were appointed<sup>1</sup> to examine, *Collins's own abridgment* of this “collectio,” headed “To Leibnitz,” and containing Newton's

<sup>1</sup> There is not the least reason to suppose that any papers of Collins's ever came into the possession of the Royal Society after the *Comm. Epist.* was published.

letter curtailed of the very part of which they asserted that it described the method of fluxions sufficiently for any intelligent person. Of this abridgment they make no mention. We now see why the statement that the "collectio" was sent to Leibniz was not allowed to appear in its place; that is, when the "collectio" was mentioned in the body of the work. Had the blot been hit, they would have pleaded some mistake or forgetfulness, would have produced the abridgment, and would have taken their stand on the fragment of the letter descriptive of results. We neither believe, nor would have others believe, that in the proceeding just described we are necessarily to impute guilty unfairness to the Committee of 1712, or to some of them: though all the circumstances make it impossible to avoid including this hypothesis among the probable ones. Independently of our knowledge of what *hero-worship* can lead to, even in our own day, we are bound to remember that all the notions as to what is fair and what is unfair in controversy, have undergone much change since the commencement of the last century. And above all, the idea that a party in literary controversy resembles one in a court of law, who may, with certainty of allowance, choose his own evidence, suppress what does not suit, and mystify what does, is now much less in force. In the particular case before us, perhaps something is to be allowed for hurry. The Committee was appointed

in parcels on March the 6th, 20th, 27th, and April the 17th; and their report was read on April the 24th. But the hurry, if any, was their own fault. This striking fact, that the very papers which were examined in 1712 prove that the celebrated letter was not<sup>1</sup> sent to Leibniz, but only a description (amounting to extract) of a part of it, and that part not the one which most appears to sustain the report of the Committee, throws into the background the remarks which we intended to make on part of the paragraph above extracted from the synoptical life of Newton. These must now be mixed up with remarks on the whole.

The editor begins by stating that doubts have been thrown on the question whether "*these papers* were *actually* sent to Leibniz." By these papers, the reference tells us we are to understand the "collectio" which has been spoken of. To remove the doubts and prove that "these papers" were actually sent, we are first referred to Collins's own testimony. The reference given would exclude

<sup>1</sup> It is now clear that the Royal Society owes the world more publication from its archives than has yet taken place: unfortunately, it is not yet alive to the feeling that such disclosures as those of the surreptitious additions to the reprint of the *Comm. Epist.*, and of the suppression now noted, would come most gracefully from itself. It is on record that in 1716, the Abbé Conti, a friend of both parties, spent some hours in looking over the letter books of the Royal Society to see if he could find anything omitted in the *Comm. Epist.* which made either for Leibniz, or against Newton; and that he found nothing. But it now appears either that he did not know what to look for, or that there were papers which did not come in his way. Be it one or the other, the credit of his search is now upset; and Mr Edleston's discovery proves that another is wanted.

Newton's letter, since nothing is there mentioned as sent to Paris except either Gregory's writings, or what had been done on the *method of series*: the drawing of tangents to curves was a perfectly distinct thing in the language of the day. But this reference leads us to a proof (though one is not needed) that the Committee actually saw the abridgment *which was sent*, and contrived to introduce reference to it in an unintelligible way; so that no one who was ignorant of the existence of the abridgment could infer that anything was sent except the complete "collectio." The reference is to the *Commercium Epistolicum*,<sup>1</sup> where we find a letter from Collins to David Gregory (the brother of James, whose papers were in question) of August the 11th, 1676, in which Collins says that he had put together an "historiola" of the writings of his brother and others, in about twelve<sup>2</sup> sheets, for preservation in the archives of the Society; and that he would find from what followed the letter (*ex sequentibus* comperies) that care had been taken to satisfy the wishes of the French mathematicians. Annexed to the letter is a memorandum to the effect that the "*sequentia*" had been sent both to the members of the French Academy,<sup>3</sup> and to

<sup>1</sup> Pp. 47, 48; 2nd ed., p. 129.

<sup>2</sup> It is now, Mr Edleston informs us, extant in thirteen sheets; from which it is clear that this "historiola," as Collins calls it, is what the Committee called the "collectio"; as the editor notes.

<sup>3</sup> Among these was Leibniz, who, as we learn from the letter of Collins to Oldenburg, attached to the "collectio," was one of the

David Gregory. Here, then, are two things; the "historiola" mentioned in the letter, and the "sequentia" of the letter: the latter was sent to Paris, and therefore by the "sequentia" we are to understand Collins's abridgment. That is to say, the Committee, which extracted as much from Collins as would prove that something was sent, did not give a word to explain what was sent: and inserted in their report a deliberate statement that the whole of what they chose to call the fluxional part of Newton's letter had been sent.

We are next told that Leibniz<sup>1</sup> acknowledged the receipt of "these papers": we look at the reference indicated, and we find that Leibniz does (August the 27th, 1676) acknowledge letters of July the 26th,

French Academy who had desired to have an account of Gregory's writings. In fact, Leibniz was at Paris when he received Oldenburg's account of Collins's abridgment. The Committee, who say that Newton's letter was sent to Paris to be communicated to him, may seem by this phrase to have supposed him to have been at Hanover.

<sup>1</sup> Our extract says, Leibniz and Tschirnhaus. Now though the latter did write from Paris, in September, acknowledging something, yet he does not sufficiently say what, and even the Committee have put a note to his letter, doubting, from its internal evidence, whether he could have seen those extracts from Gregory which were sent to Leibniz. So that the Committee knew nothing positive as to what was transmitted to Tschirnhaus. Moreover, Tschirnhaus was not Leibniz. The whole of the passage on which this note is written struck us as so singular, so contrary, in the antagonism of its two portions, to the usual clearness of the whole of which it forms a part, that we could not help suspecting that the editor had been misled by some predecessor. And at last we found out by whom. Keill, in the account of the "*Commercium Epistolicum*" published in English in the *Phil. Trans.* for 1715, and in Latin as a preface to the reprint, has the whole argument with the affirmation of Collins and the replies of Leibniz and Tschirnhaus. Keill was more noted, while alive, for getting his friends into embarrassments than for his discoveries: will he never leave off his old tricks?

which the editor himself immediately proceeds to inform us, both from the Hanoverian publication and from Collins's draught, did not contain "these papers," but only an abridgment. Finally, the editor concludes that it may be "fairly questioned" whether the transmission ever took place. How can this be? The doubts as to the transmission, he has just told us, are removed by the testimony of Collins the transmitter and Leibniz the receiver. The answer is, that the editor himself immediately proceeds to prove, both from the transmitter and the receiver, that what was transmitted was not the "collectio" of the *Commercium Epistolicum*, but an abridgment. We cannot but suppose that the editor imagined the existence of the abridgment to be known, and having no idea that he himself was the first to draw it from its retirement, considered the "collectio" and its abridgment as convertible documents, and the information they conveyed as substantially the same. We, however, had never found a trace, in any writing upon the subject, of any mention of the smaller document; and it is clear that the omission of the example of Newton's method, poor as the pretext against Leibniz would have been even if it had been there, destroys the pretext<sup>1</sup> altogether.

<sup>1</sup> If the editor meant that Newton's letter is substantially the same as to the real information it could give, whether with or without the example of the method of tangents, we not only agree with him as to the fact, but should have agreed, if he had asserted that a sheet of

We shall join the complete elucidation of the last assertion with the establishment of another statement of Leibniz, namely, that the Committee of the Royal Society had been guilty of gross suppression of facts unfavourable to themselves, and within their own knowledge. We, who have not right of access to the archives of the Society, can of course only further show this (beyond what is shown by the suppression of the abridgment) by proving suppression of documents which had been already printed; that is, by showing that the Committee either entirely suppressed what they ought to have brought forward, or contented themselves with reference where they ought to have produced extracts. We shall confine ourselves to what is immediately connected with the unlucky fragment of Newton's letter, which was never sent.

First, the Committee refer to the method which Sluse had given for drawing tangents,<sup>1</sup> and which was *printed* in the *Phil. Trans.* as early as 1673. They give Oldenburg's communication to Sluse of Newton's letter, in which Sluse learns that what he had communicated was already known to Newton.<sup>2</sup> They also give Newton's admission<sup>3</sup> that Sluse not

blank paper (after what Sluse had already published) would have done just as well. But our reader must remember that it is not the rational interpretation of the letter which is the matter in discussion, but the interpretation of the Royal Society's Committee.

<sup>1</sup> *Comm. Epist.*, p. 106; we quote the second edition as more accessible than the first.

<sup>2</sup> *Ibid.*, p. 106.

<sup>3</sup> *Ibid.*, p. 107.

only had probably an actual priority of discovery, but that, whether or no, he was the first promulgator. All this, so far as it goes, is fair, though it militates strongly against the conclusion of their report with respect to Leibniz. But it was not fair to suppress all account of the manner in which this celebrated letter of Newton was drawn out. When they state that Collins had been for four years circulating the letter in which the method of fluxions was sufficiently described to any intelligent person, they suppress two facts: first, that the letter itself was in consequence of Newton's learning that Sluse had a method of tangents; secondly, that it revealed no more than Sluse had done. In the third volume (1699) of Wallis's works<sup>1</sup> is a fragment of a letter from Collins to Newton, of June the 18th, 1673, in which he reminds Newton, for what purpose does not appear, of his having communicated the fact of Sluse's discovery, and having received an answer (which was no doubt *the* letter) for the purpose of transmission to Sluse. Again, this method of Sluse is never allowed to appear; reference is made to the *Philosophical Transactions*, though many things which had been printed before appear in the *Commercium Epistolicum* when they serve the right purpose.

To show what we assert we shall compare the two methods.

<sup>1</sup> In Latin p. 617, in English p. 636.

The paragraph of Newton's letter, from the original in the Macclesfield collection, is as follows (December the 10th, 1672):—

“I am heartily glad at the acceptance, which our rev. friend Dr. Barrow's Lectures find with foreign mathematicians, and it pleased me not a little to understand that they<sup>1</sup> are fallen into the same method of drawing tangents with me (*eandem . . . ducendi tangentes methodum*). What I guess their method to be you will apprehend by this example. Suppose *CB*, applied to *AB* in any given angle, be terminated at any curved line *AC*, and calling *AB* *x* and *BC* *y*, let the relation between *x* and *y* be expressed by any equation as

$$x^3 - 2x^2y + bx^2 - b^2x + by^2 - y^3 = 0,$$

whereby this curve is determined. To draw the tangent *CD*, the rule is this. Multiply the terms of the equation by any arithmetical progression according to the dimensions of *y*, suppose thus

$$\begin{array}{cccccccc} x^3 - 2x^2y + bx^2 - b^2x + by^2 - y^3 & & & & & & & \\ 0 & 1 & 0 & 0 & 2 & 3 & & \end{array} :$$

also according to the dimensions of *x*, suppose thus

$$\begin{array}{cccccccc} x^3 - 2x^2y + bx^2 - b^2x + by^2 - y^3 & & & & & & & \\ 3 & 2 & 2 & 1 & 0 & 0 & & \end{array} :$$

<sup>1</sup> There is no end of the curiosities of this Committee. After their Latin for the word “they,” they inserted in brackets (Sluse and Gregory), the latter not being a foreigner. If they had given the letter of Collins, just referred to, of June the 18th, 1673, the reader would have known that Sluse and Ricci are the parties understood.

The first product shall be the numerator, and the last divided by  $x$  the denominator of a fraction, which expreseth the length of  $BD$ , to whose end  $D$  the tangent  $CD$  must be drawn. The length of  $BD$  therefore is

$$-2x^2y + 2by^2 - 3y^3 \text{ divided by } 3x^2 - 4xy + 2bx - b^2."$$

Not many days afterwards (January the 17th, 1673) Sluse wrote an account of the method which he had previously signified to Collins, for the Royal Society, by whom it was printed.<sup>1</sup> The rule is precisely that of Newton, the exponents are multipliers, without any subsequent reduction of the exponents (which prevents both explanations<sup>2</sup> from describing the method of fluxions to any intelligent person), and instead of dividing by  $x$ , Sluse changes one  $x$  into  $BD$ , and then equates the two results. To have given this would have shown the world that the grand communication which was asserted to have been sent to Leibniz in June 1676 might have been seen in print, and learned from Sluse, at any time in several previous years: accordingly, it was buried under a reference. But, worse than this, the Committee had evidence before them that it *had* been

<sup>1</sup> *Phil. Trans.*, No. 90; also Lowthorp, vol. i, pp. 18-20. [J. Lowthorp abridged the *Philosophical Transactions* to the end of 1706 into three volumes.]

<sup>2</sup> If Newton's example had been sent to Leibniz, and the latter had not known the method already from Sluse, the direction to multiply by the terms of any arithmetical progression (a mere slip of the pen on Newton's part, properly preserved by the Latin translator) might have puzzled any "idoneus harum rerum cognitor."

so seen by Leibniz, and this evidence they deliberately mutilated.

On March the 5th, 1677, Collins wrote to Newton, giving him certain extracts from a letter of Leibniz, dated November the 18th, 1676. This was printed (1699) in the third volume of Wallis. Leibniz had seen Hudde at Amsterdam, and had found that Hudde was in possession of even more than Sluse; and this he states, referring to the published method of Sluse, as known to himself. He gives also an example, or rather its result, not as showing the method, which was known, but in order further to show how to eliminate one of the co-ordinates from the result. The Committee omit this example, without any notice of omission, though they give the passages between which it lies.

We are obliged frequently to recur to the assertion of the Committee that Newton's example, which we have translated, was description enough of the method of fluxions for any intelligent person. That this, which we shall believe to be the most reckless assertion ever made on a mathematical subject, until some one produces its match, was solemnly put forward by the Committee, is not in our day excuse enough for dwelling upon it. But the sufficient excuse is that writers of note, upon the Newtonian side of the question, still quote the assertion with approbation. In Sir David Brewster's *Life of Newton*, for instance, the whole Report of the Committee is

printed, and a virtual adhesion given to it. On the other hand, the defenders of Leibniz, most of whom are not English, prefer to establish his rights independently, and evade an encounter which is rendered repulsive by its dealing more with the comparison of old letters than with mathematical explanations.

Some little question has arisen as to the position in which the Royal Society stands in this matter. According to Leibniz, Chamberlayne wrote to him to the effect that the Royal Society did not wish the report to pass for a decision of its own. Mr Weld<sup>1</sup> found the minute in question (passed May the 20th, 1714), in which it is stated that "if any person had any material objection against the *Commercium*, or the Report of the Committee, it might be reconsidered at any time." This Mr Weld considers as an *adoption* of the Report of the Committee: in which we cannot join, though we admit that it throws the question open, which as long as Chamberlayne's communication stood unanswered, was settled: and enables us to infer adoption from previous acts. In all probability he informed Leibniz that the Report of the Committee was not to pass for a *decision*; meaning the stress to lie there, and stating why: and this would be correct, for a question which may be reconsidered at any time is not decided, except in a technical sense. And very likely he added "of the Society": for it was the full impression of the

<sup>1</sup> *Phil. Mag.*, 1847; *Hist. Roy. Soc.*, vol. i, p. 415.

time that the Society was one with its Committee. There can be no doubt that the hearty adherence given by the Society to the conclusions, the circulation of the *Commercium Epistolicum* throughout Europe, the admission of Keill's "recensio" into the *Transactions*, the sanction of the reprint ten years after, and the obstinate determination, which lasts down to our own time, not to confess one atom of the error nor right one atom of the wrong, amount to an adoption which could not be more than adequately represented by any quantity of minutes.

It seems the fate of this controversy that whatever the English partisans of the eighteenth century supposed to have happened between the two parties really happened the other way, the places of the parties being changed, and to no effect upon the question. Much stress was laid on Collins transmitting from Newton to Leibniz an example of the method of tangents: it appears that the example was not sent, that the *abridgment* sent did not contain it; but it appears that Collins really forwarded a result from *Leibniz* to *Newton*, which was the only one that passed between them. Not that this gave Newton any information; but neither would Newton's example, if sent, have given any to Leibniz, after Sluse's publication and Hudde's oral communication.

Again, it was frequently stated that the differential calculus was only the method of fluxions with the notation changed. Now the fact is, that as to every-

thing elementary that was *published* with demonstration under the *name of fluxions*, up to the year 1704 (when Newton *himself* first published anything under that name) the method of fluxions was nothing but the differential calculus with the notation changed. We know that Newton's letters did not treat of fluxions, nor contain anything from which the writer of a system could draw his materials. No one ventured to print an elementary treatise in England until the seed had grown into a strong plant under the care of Leibniz, the Bernoullis, and so on. When de l'Hôpital, in 1696, published at Paris a treatise so systematic, and so much resembling one of modern times, that it might be used even now, he could find nothing English to quote, except a slight treatise of Craig on quadratures, published in 1693. He mentions all that he could of Newton, and even says of the *Principia* that it was full of the *calculus*, which is not true; he should have said it was full<sup>1</sup> of the *principles* on which the calculus is founded, and of application of them in which the *reader* (whatever might have been the case with the *author*) is directed by thought without calculus. But the distinction is one which was not then appreciated: in fact it needed the calculus, such as it became, to show it. It must be remembered that, when de l'Hôpital *wrote* (for

<sup>1</sup> "C'est encore une justice dûë au sçavant M. Newton, et que M. Leibniz luy a renduë luy-même: Qu'il avoit aussi trouvé quelque chose de semblable au calcul différentiel, comme il paroît par l'excellent Livre intitulé . . . *Principia* . . . lequel est presque tout de ce calcul."  
—*Preface.*

he could then have seen the first volume of Wallis), there neither was, nor had been, one word of accusation or of national reflection, to create any bias for or against any one. The first thing of this kind took place in 1695, when Wallis, in the preface to the first volume of his collected works, not only claimed the differential calculus as derived from the method of fluxions, but (in ignorance, as he afterwards knew) grounded the claim upon the two celebrated letters of Newton to Oldenburg, of which little notice is taken here, because not even the Committee of the Royal Society venture a mention of them in their report, as any ground of confirmation against Leibniz.

The note of alarm thus sounded, our countrymen began to write upon fluxions. Some writings are so advanced that they do not define their terms: from these therefore we cannot tell whether  $\dot{x}$  means the velocity with which  $x$  changes, or an infinitely small increment of  $x$ . Such (at least so we suppose from the enlarged second edition of 1718) was the little tract of Craig, to which de l'Hôpital refers, as we have seen: and such were Dr Cheyne's tract on fluents (1703) and De Moivre's answer to it (1704). Newton himself, in the *Principia*, was not a fluxionist, but a differentialist. Though imagining quantity generated by motion or flux (in the celebrated Lemma in which he gives a brief description), he calculates, not by velocities but by *moments*, or "momentaneous increments and decrements," which are infinitely

small quantities, for "moments, so soon as they become finite magnitudes, cease to be moments." Of Wallis we shall presently speak. De Moivre<sup>1</sup> represents fluxions as momentaneous increments or decrements. And the only elementary writers, Harris<sup>2</sup> and Hayes,<sup>3</sup> are strictly writers on the differential calculus, as opposed to fluxions, in every thing but using  $\dot{x}$  instead of  $dx$ . Harris says, "By the Doctrine of Fluxions we are to understand the Arithmetick of the Infinitely small Increments or Decrements . . ." These, he says, Newton properly calls fluxions; and he proceeds to show that his own ideas are not very clear, by asserting that "'Tis much more natural to conceive the Infinitely small Increments or Decrements of the variable and Flowing Quantities, under the notion of Fluxions (that is, according to him, of infinitely small increments or decrements) than under that of Moments or Infinitely small Differences, as Leibnitz . . . chose rather to take them." And then he

<sup>1</sup> *Phil. Trans.*, 1695, No. 216.

<sup>2</sup> The first elementary work on fluxions in England is a tract of twenty-two pages in *A New short treatise of Algebra. . . Together with a specimen of the Nature and Algorithm of Fluxions.* By John Harris, M.A., London, 1702, octavo (small).

<sup>3</sup> *A Treatise of Fluxions; or an Introduction to Mathematical Philosophy. Containing a full Explication of that Method by which the Most Celebrated Geometers of the present Age have made such vast Advances in Mechanical Philosophy. A Work very Useful for those that would know how to apply Mathematicks to Nature.* By Charles Hayes, Gent., London, folio, 1704. This work, which has had very little notice (Hayes, born 1678, died 1760, wrote many works, but never set his name to any but this), is a very full treatise, nearly three times as large as that of de l'Hôpital, having 315 closely printed folio pages on fluxions, besides an introduction on conic sections.

proceeds to speak of *velocities*: in fact he jumbles de l'Hôpital, whom he did understand, with Wallis, whom he did not. Hayes, a much clearer writer, begins thus: "Magnitude is divisible in *infinitum* . . . the infinitely little Increment or Decrement is called the *Fluxion* of that Magnitude. . . . Now those infinitely little Parts being extended, are again infinitely Divisible; and these infinitely little Parts of an Infinitely little Part of a given Quantity, are by Geometers called *Infinitesimæ Infinitesimarum* or *Fluxions of Fluxions*." And again<sup>1</sup> ". . . suppose half the infinitely little increment of  $X$  to be  $\frac{1}{2}\dot{x}$ , and half the Fluxion or infinitely little Increment of  $Z$  to be  $\frac{1}{2}\dot{z}$ ." And thus it appears that all explanation that was tendered in print, up to the year 1704, whether by Newton himself, or by any of his followers (except only Wallis, as presently mentioned), was Leibnizian in principle. But when Newton, in 1704, published the treatise on the Quadrature of Curves which he had written before Leibniz communicated the differential calculus to him, he starts with nothing but the notion of quantity increasing or diminishing with velocity, and this velocity or *celerity* is the fluxion. And in the Introduction, written at the time of publication, he says, "I do not consider mathematical quantities as consisting of the smallest possible parts (*partes quam minimæ*) but as described by continuous

<sup>1</sup> *Ibid.*, p. 5.

motion." This is the first public declaration of the meaning of a "fluxion" that was made by the author of the word, *in his own name*.

It may appear strange that we defer till now to mention a very *fluxional* view of fluxions which appeared as early as 1693. But we wish to give prominence to what is really Newton's first publication on the subject, though it has received but little notice until lately. The second volume of Wallis's works, containing the *Algebra*, in which the matter spoken of occurs, was published in 1693, the first in 1695, but false title-pages<sup>1</sup> make them appear as of 1699. Again, those who look at the preface to the first volume see that Wallis excuses himself from mentioning the differential calculus, because it was nothing but the fluxions which Newton, he says, had communicated to Leibniz in the celebrated Oldenburg letters, and which he (Wallis) had described, from those letters, nearly word for word, in his *Algebra*. No one of later times would thereupon refer to this *Algebra* for information; since they would know that nothing upon fluxions could be given word for word, but only letter for letter. For all that is said upon fluxions, in those celebrated

<sup>1</sup> The *Comm. Epist.* says that two volumes appeared in 1695; probably the second volume got a new title-page in that year. The third volume was published in 1699, and then the first volume certainly got a title-page of that date. This vile practice of altering title-pages will be put down by the scorn of all honest men, so soon as its tendencies are seen. A person who reads Wallis's collected works under the date of 1699 easily convicts the author, as honest a man as ever lived, of the grossest unfairness, upon his own testimony.

epistles, is, as is well known, in two anagrams, one of which is

6a 2c d æ 13e 2f 7i 3l 9n 4o 4q 2r 4s 9t 12v x,

the information given being that whoever can form a certain sentence properly out of six *a*'s, two *c*'s, a *d*, and so on, will see as much as one sentence can show about Newton's mode of proceeding. No one but Raphson<sup>1</sup> imagined that any human being derived any information from this; and probably therefore few would be induced by Wallis's preface to consult the work. They would not know (and we shall see that Wallis himself could hardly have anything to make him remember) that Wallis had been in communication with Newton, had obtained not only the key of the anagrams but their meaning, and had added a brief account of fluxions, with an extract from what Newton afterwards published in the treatise of 1704, besides other matter expressly obtained from Newton in explanation of the second

<sup>1</sup> The sentence was "Data Æquatione quocunque, fluentes quantitates involvente, fluxiones invenire, et vice versa," given any equation involving fluent quantities, to find the fluxions, and *vice versa*. Many writers have called this a *cipher*, which it is not: a cipher gives, in some way, the order of the letters as well as substitutes for the letters themselves. Raphson declared that Leibniz had first deciphered the anagram, and then detected the meaning of the word fluxion, after which he forged a resemblance. But Raphson was the unscrupulous man of the time, if any one could deserve that name. Newton stated distinctly that Leibniz sent him the details of a Method which was his own in all respects except language. Raphson says (*Hist. of Fluxions*, p. 1) that Leibniz "writ in answer that he had found out a Method not unlike it, as Sir Isaac himself had hinted, page 253, *Princip.* . . ." The impudence of this paraphrase is one of the minor gems of the controversy: and we could rub it brighter if we had room.

anagram. The reader cannot detect the new information, except in that additional part which explains the second anagram: all that can be said of the rest is, that to a reader who compares chapters 91 and 95 there are a couple of sentences which would perhaps puzzle a person who did not know that a new source of information was referred to in these sentences. The reviewer of Wallis in the *Acta Eruditorum*, in complaining of the suppression of the differential calculus, hit the real reason, namely, Wallis's ignorance of a good deal of what had been done abroad: and Wallis, who wrote to Leibniz the day after he saw this review, acknowledges that he knew nothing of what Leibniz had written, except two slight and old papers, and had never heard the name of the differential<sup>1</sup> calculus until the preface was in the press, when a friend mentioned with indignation that Newton's fluxions were current in Belgium under that name. Then, and probably without consulting what he had written, Wallis added the sentence we have mentioned to his preface. In the third volume, Wallis printed all his correspondence with Leibniz, and all the correspondence with others on the subject which he could

<sup>1</sup> Nevertheless, Leibniz and the differential method are mentioned in the second volume, that is, in the account of fluxions on which we are writing; but (as discovered by Professor Rigaud) Wallis's copy preserved in the Savilian Library has manuscript additions which note and explain this forgetfulness. It appears that the whole communication is Newton's, and is inserted in Newton's words: an author can hardly remember another person's writing, to which he gives admission, as if it were his own.

collect, and mentions fluxions and the differential calculus as two distinct things in the preface. What we have here to do with, however, is the nature of the publication of fluxions which was made in 1693.

We now come to the independent proofs of the separate invention of Leibniz, as contained in his recently published papers. Preliminary, however, to these, we may notice one which was published in 1671, and which shows the way in which the current of his ideas was setting. Dr Hales, in his *Analysis Fluxionum*,<sup>1</sup> says that Leibniz had given no obscure germs of his differential method in his *Theoria Notionum Abstractarum*, dedicated to the French Academy in 1671: and Dr Hutton<sup>2</sup> refers to this theory of abstract notions. Both are wrong in the name; for the paper which Leibniz dedicated to the Academy in that year is *Theoria Motus Abstracti*.<sup>3</sup> This paper is certainly a witness to character; throughout it there occurs a frequent approximation to the idea of infinitely small quantities having ratio to each other, but not to finite quantities. One extract (translated) will serve as a specimen: "A point is not that which has no parts, nor of which part is not considered; but which has no extension, or whose parts are indistant, whose magnitude is inconsiderable, inassignable, less than any which has ratio (except an infinitely small one)

<sup>1</sup> London, 1800, 4to.

<sup>2</sup> *Math. Dict.*, Art. "Fluxions."

<sup>3</sup> *Op. Leibn.*, vol. ii, part ii, p. 35.

to a sensible quantity, less than can be given; and this is the foundation of Cavalieri's method, by which its truth is evidently demonstrated, namely, to suppose certain rudiments, so to speak, or beginnings of lines and figures, less than any assignable." So that, in 1671, it was working in Leibniz's mind that in the doctrine of infinitely small quantities lay the true foundation of that approach to the differential calculus which Cavalieri presented.<sup>1</sup>

Dr Gerhardt, the editor of the correspondence already referred to, found among the papers of Leibniz preserved in the Royal Library at Hanover various original draughts, containing problems in which both the differential and integral calculus are employed, and has published them in a separate tract.<sup>2</sup> The editor dwells so much on the matter

<sup>1</sup> [In his paper "On the Early History of Infinitesimals in England," published in the *Philosophical Magazine* for November, 1852, and mentioned in the above Preface, De Morgan developed his thesis that *Fluxions* at first (up to 1704) had an infinitesimal basis. This thesis is supported by Newton's own early papers published by Rigaud (see the Appendix to this Essay), by Newton's *Method of Fluxions*, by the first edition of the *Principia*, as compared with the second, by Newton's *De Quadratura Curvarum*, by works of John Craig, De Moivre, Halley, Cotes, Cheyne, and Fatio de Duillier, besides the books by Harris and Hayes mentioned in the text above.]

<sup>2</sup> *Die Entdeckung der Differentialrechnung durch Leibniz.* Von Dr C. J. Gerhardt. Quarto. No date nor place; preface dated "Salzwedel, im Januar 1848:" [Accordingly we must conclude that Gerhardt's tract, in the form in which it often exists, under the title *Die Entdeckung der Differentialrechnung durch Leibniz, mit Benutzung der Leibnizischen Manuscripte auf der Königlichen Bibliothek zu Hannover*, Halle, 1848, has a different title-page from the one seen by De Morgan, which was probably the extract it was from the *Programm* of the school at Salzwedel. Two years earlier, Gerhardt had published a very important manuscript of Leibniz's under the title *Historia et*

and consequences of the manuscripts, that he forgets to satisfy curiosity as to their form, the circumstances of the discovery, and so on: they ought to be re-published with proper facsimiles of the handwriting. Not that we at all doubt them; for, independently of the full credit due to Dr Gerhardt, we do not believe that human ingenuity could have forged so genuine a mess of spoiled exercises. We cannot attempt a full account of them; but this is of little consequence, since they will of necessity be fully described in more appropriate quarters, so soon as they are better known to exist.

These papers are seven in number, dated<sup>1</sup> November the 11th, 21st, 22nd, 1675, June the 26th, July, November, 1676, and one without a date. They are not descriptions of the principles, but study exercises<sup>2</sup> in the use, of both differential and integral calculus. Except out of the problems themselves, we learn nothing of the extent to which

*Origo Calculi Differentialis a G. G. Leibnitio conscripta. Zur zweiten Säcularfeier des Leibnizischen Geburtstages aus den Handschriften der Königlichen Bibliothek zu Hannover*, Hanover, 1846. Further information about Gerhardt's publications on Leibniz is given in the Appendix to this Essay.]

<sup>1</sup> The editor tells us that some one had been meddling with the date of the first paper, and had turned the 5 of 1675 into a 3. Leibniz, speaking from recollection in 1714, says that his discovery was made, as near as he could remember, in 1676.

<sup>2</sup> Professor Rigaud has published, from the Macclesfield collection, a manuscript draught of Newton, of November 13th, 1665. But this is formally written out, proposition, resolution, and demonstration. An earlier essay, of May 20th, is not given, which is to be regretted. But from the description we see that Newton used the peculiar notation of fluxions in May, and abandoned it in November. His formal proposition uses distinct letters for fluxions of other letters. In Leibniz, everything in language is progression: no step gained is ever abandoned.

the structural operations were in the power of the writer. We find strange mistakes of operation, such as beginners now make: and it is clear that the writer is trying to push his calculus forward into discovery of new results in geometry before he has either sounded its extent or settled its language. In the first of the papers he enters (among other things) upon the examination whether

$dx \cdot dy$  is the same with  $d(xy)$  and  $d\left(\frac{x}{y}\right)$  with  $\frac{dx}{dy}$ : at

first he inclines to the affirmative, but in the next page decides in the negative. This will not surprise the mathematician of our day, who remembers that these are the private memoranda of a discoverer in the very process of investigation: but nevertheless he will look to find some particular cause of confusion of ideas at the outset. We suspect it to be as follows. Leibniz frequently supposes  $dx=1$ , or  $dy=1$ : that is, he establishes two kinds of units, without any symbolic distinction, the unit of finite, and the unit of infinitely small, quantity. In integration, he halts between the use of  $\int y$  and of  $\int y dx$ , as the expression of an integral. There are also obvious slips of the pen, and operations set down for thought, which lead to nothing.

The first problems treated are in the direct and inverse method of tangents, in which the method of Sluse is referred to by name. The two following

extracts, in which the Latin is literally translated, of the date of November the 11th, 1675, will be as much as we can afford room for. They give two of the earliest problems solved, the first and third.

The problem is to find a curve in which the subnormal ( $w$ ) is reciprocally proportional to the ordinate. Putting  $z$  instead of  $dx$ , Leibniz proceeds thus:—"It appears from what I have shown else-

where, that  $\int wz = \frac{y^2}{2}$ , or  $wz = \frac{y^2}{2d}$ ." The  $d$  in the

denominator is the symbol of differentiation of the whole: it frequently happens *in the first papers*.

"But from the quadrature of the triangle this is  $y$ ." We should write  $y dy$ , but Leibniz tacitly makes  $dy=1$ , and he afterwards says he has here thought of making an abscissa of the ordinate. "Now from

the hypothesis  $w = \frac{b}{y}$  . . . whence  $\frac{bz}{y} = y$ , and  $z = \frac{y^2}{b}$ .

But  $\int z = x$ . Therefore  $x = \int \frac{y^2}{b}$ . But  $\int \frac{y^2}{b} = \frac{y^3}{3ba}$  by the

quadrature of the parabola; therefore  $x = \frac{y^3}{3ba}$ ." This

$a$  is not of easy explanation. It is afterwards given to make the subnormal reciprocally proportional to

the abscissa. "Here  $w = \frac{a^2}{x}$ ; but  $\int w = \frac{y^2}{2}$ , whence

$y = \sqrt{2 \int w}$ , or  $\sqrt{2 \int \frac{a^2}{x}}$ . Now  $\int w$  cannot be found

except by the help of the logarithmic curve. Therefore the figure required is that in which the ordinates

are in the subduplicate ratio of the logarithms of the abscissæ."

If the Committee of the Royal Society had had these papers before them, they would have justly contended that the calculus of Leibniz, of which the principles and algorithm were settled, received a great accession of working power when Newton communicated the binomial theorem in the "epistola prior" to Oldenburg; which "epistola prior," by the rule of contraries already instanced, has been much less insisted on than the "epistola posterior" with its anagrams.

On August the 27th, 1676, Leibniz acknowledged the receipt of this communication; and his paper of November 1676 shows that Newton's algebra had borne its fruit. Previously to this date, we cannot find any fractional power differentiated except the square root. In pure algebraical discovery, Leibniz does not rank with Newton: and he always acknowledged that in the method of series (the phrase by which the algebraical improvements of the day were designated) Newton was before him and beyond him. We have every right to presume, from his conduct, and from the manner in which all subsequent disclosures establish his veracity, that had he lived to publish his own *Commercium Epistolicum*, he would have pointed out the difference between the invention of the differential calculus and the improvement of the algebra which gives it

language and guides its mechanism, and would have illustrated from his own papers the power which Newton's improvements in algebra enabled him to add to his existing differential calculus. We believe (with John Bernoulli) that Newton might have made a similar acknowledgment to Leibniz as to the idea of a fixed and uniform method of denoting operations in the fluxions of which he had already possession.

We have not alluded to the faults on the other side of the controversy, partly because they were much less gross in character, partly because they have been amply insisted on in this country. Nor have we, indeed, in this paper, given anything like a history of the unfair proceedings in this country, but have, for the most part, confined ourselves to points which are particularly effected by recent information. Whether there be anything still to be drawn out must be matter of conjecture, and will be matter of suspicion, until we can be well assured that all the private depositories of information have been exhausted.

A. DE MORGAN.

UNIVERSITY COLLEGE, LONDON,  
October 2, 1851.

the infinitesimal calculus; and (4) Brief references to the literature of the controversy about the invention of the calculus. It is hoped that this Appendix will be gradually made complete, either in future editions of the present book or as a separate publication.

## APPENDIX<sup>1</sup> ON THE MANUSCRIPTS AND PUBLICATIONS OF NEWTON AND LEIBNIZ

IN this Appendix is given, in chronological order, a list of the manuscripts and other works of Newton and Leibniz relating to the discovery and communication of the infinitesimal calculus and publications dealing with the controversy that subsequently took place between them and their respective supporters. References have been given on each point, and it is hoped that both the list and the references are complete in the sense that nothing important has been omitted. It is rather remarkable that nothing has hitherto been done in this direction, for it would seem to be very important that regard be paid to Newton's early manuscripts. Many important manuscripts of Leibniz's which relate to his discovery have been published by Gerhardt, and commented on by Gerhardt and others; but only a few of Newton's manuscripts have as yet been published, and these publications—by Raphson in 1715 and Rigaud in 1838—have apparently been completely ignored by all the modern historians of mathematics. After a list of works consulted, together with some brief comments on some of them and the abbreviations by which their titles are cited in this Appendix, are given: (1) References on the history of infinitesimal ideas before Newton and Leibniz; (2) References to Newton's fluxional manuscripts and publications; (3) References to Leibniz's manuscripts and publications on

<sup>1</sup> The whole of this Appendix is by the Editor of the present collection of Essays by De Morgan, and is supplementary to the second Essay.

### WORKS CONSULTED, WITH ABBREVIATIONS

- MORITZ CANTOR: *Vorlesungen über Geschichte der Mathematik*; vol. i (to A.D. 1200), 3rd ed., Leipsic, 1907; vol. ii (1200–1668), 2nd ed., Leipsic, 1900; vol. iii (1668–1758), 2nd ed., Leipsic, 1901 (contains an account of Leibniz's, but not of Newton's, manuscripts). Abbreviation: *Cantor*.
- KARL FINK: *Geschichte der Elementar-Mathematik*: translated by W. W. Beman and D. E. Smith under the title *A Brief History of Mathematics* (Chicago, 3rd ed., 1910; pp. 168–172 contain a brief summary of the origin and discovery of the infinitesimal calculus).
- W. W. ROUSE BALL: *A Short Account of the History of Mathematics*, London, 4th ed., 1908. In this work, a whole chapter (pp. 319–352) is devoted to "The Life and Works of Newton," in which Newton's early manuscripts are referred to, but without references, and in this chapter the communications with Leibniz are discussed; but the controversy is dealt with when an account of Leibniz's work is given (pp. 353–365), where Leibniz's manuscripts are hardly referred to, and he himself is treated with suspicion.
- JOSEPH RAPHSO: *The History of Fluxions, shewing in a compendious manner the first rise of and various improvements made in that incomparable Method*, London, 1715. A Latin translation was published at London in the same year (see G. J. Gray's work mentioned below, p. 54). Abbreviation: *Raphson*.
- STEPHEN PETER RIGAUD: *Historical Essay on the First*

*Publication of Sir Isaac Newton's Principia*, Oxford, 1838. In this book, the pages of the text of the first part and those of the Appendix are numbered separately. In the Appendix are given some of Newton's early manuscripts on fluxions from the collection of Lord Macclesfield. Abbreviation: *Rigaud*.

STEPHEN PETER RIGAUD: (though Rigaud's name does not appear on the title-page, it was he who made this collection) *Correspondence of Scientific Men of the Seventeenth Century, including Letters of Barrow, Flamsteed, Wallis, and Newton, printed from the Originals in the Collection of the Right Honourable the Earl of Macclesfield*. Two volumes (posthumous, edited by Rigaud's son, Stephen Jordan Rigaud), Oxford, 1841. Table of contents and index added by De Morgan (see Mrs De Morgan's *Memoir*, p. 414) in 1862. Fifty-nine letters from and to Newton, beginning in 1669, were published on pp. 281-437 of vol. ii. Abbreviation: *Macc. Corr.*

J. EDLESTON: *Correspondence of Sir Isaac Newton and Professor Cotes, including Letters of Other Eminent Men, now first published from the originals in the Library of Trinity College, Cambridge; together with an Appendix containing other unpublished Letters and Papers by Newton; with Notes, Synoptical View of the Philosopher's Life, and a Variety of Details illustrative of his History*, London and Cambridge, 1850. Abbreviation: *Edleston*.

Sir DAVID BREWSTER: *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 2 vols., Edinburgh, 1855. A second edition—apparently unaltered, even as to the mistakes—was published at Edinburgh, 1860. Abbreviation (to the 1855 edition): *Brewster*.

*A Catalogue of the Portsmouth Collection of Books and Papers, written by or belonging to Sir Isaac Newton, the Scientific Portion of which has been presented by the*

*Earl of Portsmouth to the University of Cambridge*. This catalogue was drawn up by the Syndicate appointed the 6th of November, 1872, and the Preface is signed by H. R. Luard, G. G. Stokes, J. C. Adams, and G. D. Liveing, and published at Cambridge in 1888. Abbreviation: *Portsmouth Catalogue*.

G. J. GRAY: *A Bibliography of the Works of Sir Isaac Newton together with a List of Books illustrating his Works*. Second edition, Cambridge, 1907. The first (and less full) edition was privately printed in 1888. Abbreviation: *Gray*.

FERDINAND ROSENBERGER: *Isaac Newton und seine physikalischen Principien. Ein Hauptstück aus der Entwicklungsgeschichte der modernen Physik*. Leipzig, 1895. Abbreviation: *Rosenberger*.

C. I. GERHARDT (herausgegeben von): *Historia et Origo Calculi Differentialis a G. G. Leibnitio conscripta. Zur zweiten Säcularfeier des Leibnizischen Geburtstages aus den Handschriften der Königlichen Bibliothek zu Hannover*, Hanover, 1846. Abbreviation: *G. 1846*.

C. J. GERHARDT: *Die Entdeckung der Differentialrechnung durch Leibniz, mit Benutzung der Leibnizischen Manuscripte auf der Königlichen Bibliothek zu Hannover, Halle, 1848*. Abbreviation: *G. 1848*.

C. I. GERHARDT: *Die Geschichte der höheren Analysis. Erste Abtheilung* [the only one which appeared]; *Die Entdeckung der höheren Analysis*, Halle, 1855. Abbreviation: *G. 1855*.

HERMANN WEISSENBORN: *Die Principien der höheren Analysis in ihrer Entwicklung von Leibniz bis auf Lagrange, als ein historisch-kritischer Beitrag zur Geschichte der Mathematik dargestellt*, Halle, 1856. Abbreviation: *W. 1856*. A further contribution of Weissenborn's is dealt with below.

*Die philosophischen Schriften von G. W. Leibniz, herausgegeben von C. J. Gerhardt*, 7 vols., Berlin, 1875-90.

*Leibnizens mathematische Schriften, herausgegeben von C. J. Gerhardt*, 7 vols., Berlin and Halle, 1849-1863. The contents of these volumes are described in note 3 on pp. 71-72.

*Der Briefwechsel von Gottfried Wilhelm Leibniz mit Mathematikern, Herausgegeben von C. I. Gerhardt*, vol. i, Berlin, 1899. Leibniz's manuscripts of October, 1675, are dealt with on pp. xii-xiv, and those of November 1675 and July 1676 on pp. xiv-xv. Leibniz's relations with Tschirnhaus are dealt with on pp. xvii-xviii. Cf. note 1 on p. 79. The volume contains the correspondence between Leibniz and Oldenburg, Newton, Collins, and Conti, from 1670 to 1716, and also many supplementary documents. Among these are reproduced (pp. 147-167) some of Leibniz's manuscripts of 1675 and (pp. 201-203) one of July 1676, which are referred to in the list given below. In the valuable introduction (pp. 3-38) to this correspondence, Leibniz's mathematical work from 1669 onwards is dealt with on pp. 5-38. Mention is made of *Die philosophischen Schriften von G. W. Leibniz*, but not of *Leibnizens mathematische Schriften*, nor of *G. 1846*, *G. 1848*, and *G. 1855*. Abbreviation: *Bw. 1899*.

G. E. GUHRAUER: *Gottfried Wilhelm Freiherr von Leibnitz: Eine Biographie*, 2 volumes, Breslau, 1846.

LÉON BRUNSCHVICG: *Les Étapes de la Philosophie mathématique*, Paris, 1912. The third book (pp. 153-249) contains: (1) A sketch of the growth of infinitesimal ideas from ancient times; (2) Accounts of the discoveries of Leibniz and Newton in the domain of the infinitesimal analysis, in which, however, almost no account is taken of the manuscripts of Leibniz and none of those of Newton; (3) An account of Leibniz's mathematical philosophy; (4) A discussion of mathematical idealism and metaphysical realism.

## I

INFINITESIMAL IDEAS BEFORE THE TIME OF NEWTON  
AND LEIBNIZ

Euclid, Archimedes, Pappus, Arabians, Middle Ages and Renaissance, Valerius, Kepler, Cavalieri, Torricelli, Fermat, Roberval, Pascal, Wallis, Mercator, St Vincent, Descartes, Huygens, Sluse, Hudde, Barrow: *Cantor*, vols. i to iii; *Brewster*, vol. ii, pp. 3-9; *G. 1855*, pp. 3-50; *Rosenberger*, pp. 424-430. Cf. also *W. 1856*, pp. 5-21 (Roberval and Barrow as precursors in the method of fluxions), and pp. 70-84 (Gregorius a St. Vincent, Barrow, etc., as precursors of the differential calculus).<sup>1</sup>

## II

NEWTON'S MANUSCRIPTS AND PUBLICATIONS ON THE  
FLUXIONAL CALCULUS

Newton's early study of mathematics at Cambridge in the years 1661-4 is dealt with by Brewster (vol. i, pp. 21-23). Having read Descartes, Schooten, and Wallis, Newton (MS. note of 1699, given in *ibid.*, pp. 23-24) found the method of infinite series in 1664-5, and, in the summer of 1665, computed the area of the hyperbola at Boothby in Lincolnshire to fifty-two places by this method.<sup>2</sup> Cf. *Brewster*, vol.

<sup>1</sup> The subsequent history of the principles of the calculus with Maclaurin, the Bernoullis, Neuentiit, Taylor, Euler, and Lagrange are also dealt with in the book mentioned.

<sup>2</sup> Among the "Portsmouth Papers" (Section I. "Early Papers by Newton") is this calculation of the area of the hyperbola (*Portsmouth Catalogue*, p. 1). All the papers of Newton on fluxions in this collection, many of which it would be important to publish, are catalogued on pp. 1-8 of this *Catalogue*. The "Early Papers" also include a little note on tangents, a tract written in 1666 on the solution of problems by motion, on the gravity of conics, and problems about curves. There are also manuscripts on "Elementary Mathematics," which include "Observations on the *Algebra* of Kinckhuysen" (*ibid.*, p. 2); and several manuscripts on fluxions and their geometrical and mechanical

ii, p. 10. See also *G. 1855*, pp. 90-92. In the following list of manuscripts use has been made of the "Synoptical Life" in *Edleston*, pp. xxi-lviii.

1665, May 20th. Paper on fluxions in which the notation of dots is used. It shows how to take the fluxion of an equation containing any number of variables. It is referred to in a paper which seems to be part of a draft of Newton's observations on Leibniz's letter of April 9th, 1716. *Rigaud*, Appendix, p. 23; *Raphson*, p. 116; *Brewster*, vol. i, p. 25, vol. ii, p. 12.

1665, Nov. 13th. Paper on fluxions and their applications to tangents and curvature of curves. *Rigaud*, Appendix, No. II, pp. 20-23 (printed at length); *Raphson* and *Brewster*, as before. Horsley, in vol. iv (p. 611) of his edition of Newton's collected works, gives this paper, from Raphson. It may be mentioned that, according to Lord Teignmouth's *Life of Sir William Jones* (p. 8), Newton saw the first sheets of Raphson's *History* and was much dissatisfied with them.

1666, May 16th. Another paper on fluxions (*Rigaud*, Appendix, p. 23; *Brewster*, vol. i, p. 25, vol. ii, p. 12).

1666, October. Small tract on fluxions and fluents, with their applications to a variety of problems on tangents, curvature, areas, lengths, and centres of gravity of curves. In this tract, Newton's previous method of taking fluxions is extended to surds. The area of a curve whose ordinate is  $y$  is denoted by a small square prefixed to the letter  $y$ . Cf. *Rigaud*, Appendix, pp. 23-24; *Brewster*, vol. i, p. 25, vol. ii, pp. 12-14. These early papers are, as De Morgan remarked (see the second Essay), infinitesimal in character. They are all in the Macclesfield Collection (*Brewster*, vol. i, p. 25, note 3).

1666, November. Tract similar to the preceding, but

applications, on the quadrature of curves, and on the fluxional controversy (*ibid.*, pp. 2-8). One of the papers on fluxions was marked by Horsley as "very proper to be published" (*ibid.*, p. 2).

apparently more comprehensive (*Raphson*, p. 116; Wilson's Appendix to Robins's *Tracts*, vol. ii, pp. 351-356). Notation by dots for first and second fluxions. Basis of his larger tract of 1671.

1669, July 31st. *De Analysisi* sent through Barrow to Collins. Cf. *Brewster*, vol. ii, pp. 14-15.

This seems a good place to give references to places where Newton's tract, *Analysis per æquationes numero terminorum infinitas*, was published or discussed. It was first published at London in 1711, and reprinted in 1712 (*Gray*, p. 59), in 1723 (*ibid.*, p. 10), in 1744 (*ibid.*, p. 2), and in vol. i (1779) of Horsley's edition of Newton's *Opera*. An English translation, with a commentary, was made by John Stewart in 1745 (*ibid.*, p. 60). See also *Cantor*, vol. iii, pp. 67-75, 105-108, 156-160; *Rosenberger*, pp. 431-434; R. Reiff, *Geschichte der unendlichen Reihen*, Tübingen, 1889, pp. 20-38; and Brill in A. Brill and M. Noether's report: "Die Entwicklung der Theorie der algebraischen Functionen in älterer und neuerer Zeit," *Jahresber. der Deutschen Mathem.-Vereinigung*, vol. iii, 1894, pp. 116-123.

1669, December. Newton writes notes upon Kinckhuysen's *Algebra* sent by Collins through Barrow (*Brewster*, vol. i, pp. 68-69, vol. ii, pp. 15-16; *G. 1855*, p. 83).

Newton's letters to Collins reporting progress on, and comments on, Kinckhuysen's *Algebra* are given in *Macc. Corr.*, and are mentioned by Edleston under the dates of Jan. 19th, Feb. 6th, Feb. 18th, July 11th, July 16th, and Sept. 27th, 1670. See also *Brewster*, vol. i, p. 69. A reference to his "discourse on infinite series" occurs in a letter to Collins, mentioned by Edleston, of July 20th, 1676.

Towards the end of 1671, Newton was occupied in enlarging his method of infinite series and preparing twenty optical lectures for the press. The method was never finished. It was published by Horsley (vol. i, pp. 391-518)

under the title of "Geometria Analytica." It first appeared in 1736 in Colson's translation; see Pemberton's preface to his *View of Newton's Philosophy*, London, 1728. See also *Cantor*, vol. iii, pp. 168-179, 108-109; *Brewster*, vol. ii, pp. 15-16; *Rosenberger*, pp. 434-438; *Gray*, pp. 46-48, 1, 2.

Newton's *Tractatus de Quadratura Curvarum* (cf. *Brewster* vol. ii, pp. 17-18) was printed at the end of the first edition of the *Opticks* (London, 1704, cf. *Gray*, pp. 35-36, 37-38). Extracts from the work had previously been printed in John Wallis's *Opera Mathematica*, of which four volumes were published at Oxford from 1693 to 1699. For other editions, see *Gray*, pp. 59, 1, 2. An English translation of it was published by John Stewart in 1745 (*ibid.*, p. 60), and a German annotated translation by G. Kowalewski is in No. 164 of *Ostwald's Klassiker*. On Newton's fluxional works, see *W. 1856*, pp. 21-58.

In a letter of May 25th, 1672, to Collins, Newton said that he did not intend to publish his lectures, but might possibly complete his method of infinite series, "The better half of which was written last Christmas" (*Macc. Corr.*, vol. ii, p. 332).

1672, Dec. 10th. Letter to Collins containing an account, requested by Collins in a letter received two days before, of his method of tangents (see *Edleston*, note 35 on p. xlvi).

1673, June 23rd. Letter to Oldenburg on Slusius's method of tangents (see *Edleston*, p. 251).

1675. In a letter of Collins to James Gregory, dated Oct. 19th, 1675. "Mr Newton . . . I have not writ to or seen these eleven or twelve months, not troubling him as being intent upon chemical studies and practices, and both he and Dr Barrow beginning to think mathematical speculations to grow at least dry, if not somewhat barren" (*Macc. Corr.*, vol. ii, p. 280).

1675, Jan. 22nd. Letter to Michael Dary on length of an elliptic arc.

1676, June 13th. Letter to Oldenburg, containing a general answer to Lucas and "some communications of an algebraic nature for M. Leibnitz, who by an express letter to Mr Oldenburg had desired them." The part for Leibniz was sent to him at Paris, July 26th, and was afterwards printed in Wallis's *Opera*, vol. iii, pp. 622-629, and from that work in the *Comm. Epist.*, where the typographical error of "26 Junii" for "Julii," which is corrected in Wallis's Errata, is also copied in the heading of the letter. Cf. second Essay, above.

1672, Sept. 5th. Letter to Collins (infinite series of no great use in the numerical solution of equations. The University Press cannot print Kinckhuysen's *Algebra*; the book is in the hands of a Cambridge bookseller with a view to its being printed: shall add nothing to it. Will alter an expression or two in his paper about infinite series, if Collins thinks it should be printed).

1676, Oct. 24th. Latin letter to Oldenburg for Leibniz, who desired explanation with reference to some points in the letter of June 13th. See note 55 in *Edleston*, pp. li-lii.

1676, Oct. 26th. Letter to Oldenburg with corrections for his letter of Oct. 24th. See note 56 in *Edleston*, p. lii.

1676, Nov. 8th. Letter to Collins thanking him for copies of the letters of Leibniz and Tschirnhaus, with remarks showing that Leibniz's method is not more general or easy than his own (*Macc. Corr.*, vol. ii, p. 403).

1676, Oct. 14th. Letter to Oldenburg (further alterations of his letter of Oct. 24th). Cf. note 58 of *Edleston*, p. lii.

1677, March 5th. Letter of Collins to Newton, printed in Wallis's *Opera*, vol. iii, p. 646 (extracts from it in the *Comm. Epist.*).

1687. Method for finding volume of a segment of a parabolic conoid (*Edleston*, end of note 90 on p. lviii).

1692, August 27th and Sept. 17th. Letters to Wallis, with illustrations of the calculus of fluxions and fluents sent at Wallis's request (*Wallis, Opera*, vol. ii, p. 391).

1693, March 14th. Letter to Fatio (proposing to make him such an allowance as might make his subsistence at Cambridge easy to him; *Edleston*, note 108 on p. lx).

1693, Oct. 16th. Letter to Leibniz (*Edleston*, pp. 276-279).

1697, Jan. 30th. Solution of John Bernoulli's two problems (*Edleston*, note 128 on p. lxxviii): read to the Royal Society Feb. 24th, and printed without Newton's name in the *Philosophical Transactions* for January.

1704. Equivocal expressions in the review of Newton's tract, *De Quadratura Curvarum* in the *Leipsc Acta* (*Edleston*, note 148 on pp. lxxi-lxxiii). This was the origin of the dispute as to priority.

### III

#### LEIBNIZ'S MANUSCRIPTS AND PUBLICATIONS ON THE INFINITESIMAL CALCULUS

Development of Leibniz's mathematical education. *G. 1848*, pp. 7-20 (also on Descartes, Fermat, and others), 29-32; *G. 1855*.

Leibniz's first discoveries in mathematics (Pascal's influence, *G. 1846*, pp. 1-20 (Hist. et origo; see notes 21-31); *G. 1855*, p. 33; Leibniz and St Vincent, *G. 1855*, pp. 37-38; Leibniz and Barrow, *G. 1855*, p. 48; *G. 1848*, p. 15). Cf. also *Cantor*, vol. iii, pp. 76-84, 161-164; *Rosenberger*, pp. 438-441.

Leibniz's manuscripts.<sup>1</sup>

1673, August. Method of tangents (inverse problem also dealt with). *G. 1855*, pp. 55-57; *G. 1848*, pp. 20-22.

1674, October. Inverse problem is that of quadratures. *G. 1855*, p. 57; *G. 1848*, p. 22.

1674, October. Summation of series. *G. 1855*, pp. 57-58; *G. 1848*, pp. 22-23.

<sup>1</sup> If the manuscript is printed at length, it is stated so explicitly. On the genuineness of the dates, see *G. 1848*, p. 6.

1675, January. Descartes' method not sufficient for inverse problem. *G. 1855*, p. 58; *G. 1848*, pp. 23-24.

1675, Oct. 25th. Method of quadrature. *G. 1855*, pp. 58, 117-119 (printed in full); *Bw. 1899*, pp. 147-149 (printed in full).

1675, Oct. 26th. The same subject. *G. 1855*, pp. 58, 119-121 (printed in full); *Bw. 1899*, pp. 149-151 (printed in full).

1675, Oct. 29th. The same subject (uses  $\int$ ). *G. 1855*, pp. 58-59, 121-127, 161-162; *Bw. 1899*, pp. 151-156.

1675, Nov. 1st. The same subject. *G. 1855*, pp. 60, 127-131; *Bw. 1899*, pp. 157-160.

1675,<sup>1</sup> Nov. 11th. Example of the inverse method (*d* used). *G. 1855*, pp. 160-161, 132-139; *G. 1848*, pp. 23-24, 32-40; *Bw. 1899*, pp. 161-167.

1675, Nov. 21st. On *d* (*xy*). *G. 1855*, pp. 62-63; *G. 1848*, pp. 41-45, 24-25.

1675, Nov. 22nd. Problem of tangents. *G. 1848*, pp. 25, 46-48.

1676, June 16th. Direct problem of tangents can also be treated. *G. 1855*, pp. 63-64; *G. 1848*, pp. 49-50, 25.

1676, July. *G. 1848*, pp. 25-26, 51-54; *Bw. 1899*, pp. 201-203.

1676. Leibniz in England, Holland, and Germany. *G. 1848*, pp. 54-56 (*Bw. 1899*, pp. 228-230), 26-27.

1676, Nov. Differential calculus of tangents. *G. 1855*, pp. 65, 140-142; *G. 1848*, pp. 27, 56-59; *Bw. 1899*, pp. 229-231.

1677. Correspondence with Newton.

1677, July 11. Tangents (for publication). *G. 1855*, pp. 66, 143-148; *G. 1848*, pp. 27-28, 59-65.

1684. Leibniz's publication; his relations with Tschirnhaus. *G. 1855*, pp. 66-72; *G. 1848*, p. 28.

<sup>1</sup> Here somebody has tried to turn the 5 into a 3.

MS. "Elementa calculi novi . . .," *G.* 1855, pp. 72, 149-155; *G.* 1846, pp. 32-38.

Another MS., *G.* 1846, pp. 39-50.

On Leibniz's manuscripts, see also *Cantor*, vol. iii, pp. 164-168; *Rosenberger*, p. 447, note; and *W.* 1856, pp. 84-115.

Gerhardt<sup>1</sup> published a note on the history of the controversy about the first discovery of the differential calculus, together with some critical remarks on Weissenborn's book.

In Weissenborn's book reference was often made to an essay of his in explanation of some points in Leibniz's manuscripts in Vol. XXV of *Grunert's Archiv*. As this essay did not appear, Weissenborn published the most important part of it under the title "Bemerkungen zu einigen in Dr C. J. Gerhardt's 'Entdeckung der höheren Analysis' veröffentlichten Manuscripten Leibnizens" in *Schlömilch's Zeitschrift* for 1856.<sup>2</sup> This should be read in connection with Gerhardt's publications.

On the letters and publications of Newton and Leibniz, see *Cantor*, vol. iii, pp. 179-215, and *Rosenberger*, pp. 441-455. Leibniz's publications are reprinted in Vol. V of his *Mathematische Schriften* edited by Gerhardt (see note 3 on pp. 71-72); and annotated German translations, by G. Kowalewski, of papers in the *Acta Eruditorum* of 1684, 1691, 1693, 1694, 1702 and 1703; and the *Miscellanea Berolinensia*, in No. 162 of *Ostwald's Klassiker*.

#### IV

##### THE CONTROVERSY

See, in the first place, *Cantor*, vol. iii, pp. 285-328; *Rosenberger*, pp. 423, 460-506. Various letters, from 1714-1719, on the controversy are mentioned in *Edleston*, pp. xxxviii-xxxix (see also the notes referred to). An

<sup>1</sup> *Archiv der Mathematik und Physik*, vol. xxvii, 1856, pp. 125-132.

<sup>2</sup> *Zeitschrift für Mathematik und Physik*, vol. i, 1856, pp. 240-244.

account of the controversy, from the point of view of a partisan of Newton, is given in *Brewster*, vol. ii, pp. 23-83; and, from this point of view, reference may be made to H. Sloman's book, *The Claims of Leibniz to the Invention of the Differential Calculus*, translated from the German, with considerable additions and new addenda by the author, Cambridge, 1860 (*cf.* also *Gray*, p. 55).<sup>1</sup> On the editions of the *Commercium Epistolicum*, and so on, see *Gray*, pp. 49-52, 1, 2-3.

<sup>1</sup> With reference to this book, it must be remarked that Gerhardt (*cf.* *Bw.* 1899, p. 25) found that Leibniz first saw, and made extracts from, Newton's *Analysis* in 1676.

III

REVIEW (1855) OF BREWSTER'S  
"MEMOIRS OF NEWTON"

REVIEW (1855) OF BREWSTER'S  
"MEMOIRS OF NEWTON"

*Memoirs of the Life, Writings, and Discoveries of  
Sir Isaac Newton.* By Sir David Brewster,  
K.H., etc., etc. Two volumes, 8vo. Constable  
& Co., Edinburgh, 1855.<sup>1</sup>

I

NOTHING is more difficult than to settle who is the most illustrious, the most to be admired, in any walk of human greatness. Those who would brain us—if they could but imagine us to have any brains—for hinting that it may be a question whether Shakspeare be the first of poets, would perhaps have been *Homerites* a century ago. In these disputes there is more than matter of opinion, or of taste, or of period: there is also matter of quantity, question of how much, without any possibility of bringing the thing to trial by scale. This element of difficulty is well illustrated by an exception. Among inquirers into what our ignorance calls the

<sup>1</sup> [On this book, see note 1 on p. 3; and, on the subject of this review, see the Preface.]

“laws of nature,” an undisputed pre-eminence is given to Isaac Newton, as well by the popular voice, as by the deliberate suffrage of his peers. The right to this supremacy is almost demonstrable. It would be difficult to award the palm to the swiftest, except by set trial, with one starting-place and one goal: nor could we easily determine the strongest among the strong, if the weights they lifted were of miscellaneous material and bulk. But if we saw one of the swiftest among the runners keep ahead of nearly all his comrades, with one of the heaviest of the weights upon his shoulders, we should certainly place him above all his rivals, whether in activity alone, or in strength alone. Though Achilles were the swifter, and Hercules the stronger, a good second to both would be placed above either. This is a statement of Newton's case. We cannot say whether or no he be the first of mathematicians, though we should listen with a feeling of possibility of conviction to those who maintain the affirmative. We cannot pronounce him superior to all men in the sagacity which guides the observer of—we mean rather deducer from—natural phenomena, though we should be curious to see what name any six competent jurors would unanimously return before his. But we know that, in the union of the two powers, the world has never seen a man comparable to him, unless it be one in whose case remoteness of circumstances creates great difficulty of comparison.

Far be it from us to say that if Newton had been Cænopolis, a Sicilian Greek, he would have surpassed Archimedes; or that if Archimedes had been Professor Firstrede, of Trinity College, Cambridge, he would have been below Newton. The Syracusan is, among the ancients, the counterpart of the Englishman among the moderns. Archimedes is perhaps the first among the geometers: and he stands alone in ancient physics. He gave a *new geometry*—the name was afterwards applied to the infinitesimal calculus—out of which he or a successor would soon have evolved an infinitesimal *calculus*, if algebra had been known in the West. He founded the sciences of statics and hydrostatics, and we cannot learn that any hint of application of geometry to physics had previously been given. No Cavalieri, no Fermat, no Wallis, went before him in geometry: there was not even a chance of a contemporary Leibniz. We cannot decide between Archimedes and Newton: the two form a class by themselves into which no third name can be admitted; and the characteristic of that class is the union, in most unusual quantity, of two kinds of power not only distinct, but so distinct that either has often been supposed to be injurious to the favourable development of the other.

The scientific fame of Newton, the power which he established over his contemporaries, and his own general high character, gave birth to the desirable

myth that his goodness was paralleled only by his intellect. That unvarying dignity of mind is the necessary concomitant of great power of thought, is a pleasant creed, but hardly attainable except by those whose love for their faith is insured by their capacity for believing what they like. The hero is *all* hero, even to those who would be loath to pay the compliment of perfect imitation. Pericles, no doubt, thought very little of Hector dragged in the dust behind the chariot: and Atticus we can easily suppose to have found some three-quarter excuse for Romulus when he buried his sword in his brother's body by way of enforcing a retort. The dubious actions of Newton, certainly less striking than those of the heroes of antiquity, have found the various gradations of suppressors, extenuators, defenders, and admirers. But we live, not merely in sceptical days, which doubt of Troy and will none of Romulus, but in discriminating days, which insist on the distinction between intellect and morals. Our generation, with no lack of idols of its own, has rudely invaded the temples in which science worships its founders: and we have before us a biographer who feels that he must abandon the demigod, and admit the impugners of the man to argument without one cry of blasphemy. To do him justice, he is more under the influence of his time, than under its fear: but very great is the difference between the writer of the present volumes and that of the shorter *Life* in the

"Family Library" in 1831; though, if there be any truth in metaphysics, they are the same person.

The two deans of optical science, in Britain and in France, Sir David Brewster and M. Biot, are both biographers of Newton, and take rather different sides on disputed points. Sir D. Brewster was the first writer on optics in whose works we took an interest; but we do not mean printed works. We, plural as we are, remember well the afternoon, we should say the half-holiday, when the kaleidoscope which our *ludi-magister*—most aptly named for that term—had just received from London was confided to our care. We remember the committee of conservation, and the regulation that each boy should, at the first round, have the uninterrupted enjoyment of the treasure for three minutes; and we remember, further, that we never could have believed it took so very short a time to boil an egg. A fig for Jupiter and his satellites, and their inhabitants too, if any! What should we have thought of Galileo, when placed by the side of the inventor of this wonder of wonders, who had not only made his own telescope, but his own starry firmament? The inventor of the kaleidoscope must have passed the term allotted to man, before he put his hand to the actual concoction of these long-meditated volumes, in which we find the only life of Newton written on a scale commensurate with Newton's fame. But though he has passed the term, he has not incurred the penalty;

his strength is labour without sorrow. We trust therefore that the still later age, the full fourscore, will find him in the enjoyment of the additional fame which he has so well earned. And since his own scientific sensibilities are keen, as evidenced by many a protest against what he conceives to be general neglect on the part of ruling powers, we hope they will make him fully feel that he has linked his own name to that of his first object of human reverence for as long as our century shall retain a place in literary history. This will be conceded by all, how much soever they may differ from the author in opinions or conclusions; and though we shall proceed to attack several of Sir D. Brewster's positions, and though we have no hesitation in affirming that he is still too much of a biographer, and too little of an historian, we admire his earnest enthusiasm, and feel as strongly as any one of his assentients the service he has rendered to our literature. When a century or two shall have passed, we predict it will be said of our day that the time was not come when both sides of the social character of Newton could be trusted to his follower in experimental science. Though biography be no longer an act of worship, it is not yet a solemn and impartial judgment: we are in the intermediate stage, in which advocacy is the aim, and in which the biographer, when a thought more candid than usual, avows that he is to *do his best* for his

client. We accept the book as we find it; we expect an *ex parte* statement, and we have it. The minor offence is sometimes admitted, with what we should call the art of an able counsel, if we did not know that the system of the advocate in court is but the imitation of all that is really telling in the natural practices of the partisan defender. But Sir D. Brewster stands clear of the imputation of art by the mixture of all which art would avoid. A judicious barrister, when he has to admit some human nature in his client, puts an additional trump upon the trick by making some allowance for the other side; and nothing puts the other side in so perilous a predicament. It is not so with Sir D. Brewster. When sins against Newton are to be punished, we hear Juvenal; when Newton is to be reprimanded, we hear a nice and delicate Horace, who can

"In reverend bishops note some small defects;  
And own the Spaniard did a waggish thing,  
Who cropt our ears, and sent them to the king."

We have more reasons than one for desiring that it should have been so, and not otherwise. Sir D. Brewster is the first biographer who has had restricted access to the "Portsmouth Papers"; he has been allowed to have this collection in his own possession. Had the first *Life* written upon knowledge of these papers taken that view of Newton's social conduct which stern justice to others requires,

a condonation of all the previous offences of biographers would have followed. There was not full information; the fault lay with those who suppressed the truth; and so forth. And every great man who has left no hoard of papers would have had a seal of approval placed upon all his biographies; for, you see, Newton was exposed by the publication of the "Portsmouth Papers," that is easily understood; but A B left no papers, therefore no such exposure can take place, etc., etc. We, who hold that there is, and long has been, ample means of proving the injustice with which Newton and his contemporaries once and again treated all who did not bow to the idol, should have been loath to see the garrison which our opponents have placed in the contested forts march out with the honours of war, under a convention made on distant ground, and on a newly-discovered basis of treaty. Again, there is a convenient continuity in the first disclosure of these documents coming from an advocate; the discussion which they excite will be better understood when the defender of Newton is the first to have recourse to Newton's own papers.

## II

Of Newton's birth, of his father's death, and the subsequent marriage of his mother, we need say nothing. He was not born with a title, though

he was the son of a lord of a very little manor, a yeoman's plot of land with a baronial name. But the knighthood clings strongly to his memory. Sir David (and on looking back, we see that the doctor did just the same) seldom neglects it. When the school-boy received a kick from a school-fellow, it was "Sir Isaac" who fought him in the churchyard, and it was "Sir Isaac" who rubbed his antagonist's nose against the wall in sign of victory.<sup>1</sup> Should we survive *Sir David*, we shall *Brewster* him: we hold that those who are gone, when of a certain note, are entitled to the compliment of the simplest nomenclature. The childhood and boyhood of Newton were distinguished only by great skill in mechanical contrivance. No tradition, no remaining record, imputes any very early progress either in mathematics or general learning, beyond what is seen in thousands of clever boys in any one year of the world. That he was taken from farming occupations, and sent back to school, because he loved study, is told us in general terms; but what study we are not told. We have always been of opinion that the diversion of Newton's flow of reason into its proper channel was the work of the University and its discipline. He was placed at Trinity College as a subsizar in his nineteenth year. We have no proof, but rather the contrary, that he had then opened *Euclid*. That he was caught solving a

<sup>1</sup> [Cf. Brewster, *Memoirs*, 1855, vol. i, pp. 7-8.]

problem under a hedge is recorded : perhaps a knotty question of wheelwork. He bought a *Euclid at Cambridge*, and threw it aside as a trifling book, because the conclusions were so evident : he betook himself to Descartes, and afterwards lamented that he had not given proper attention to Euclid. All this is written, and Sir David is bound to give it ; but what Newton has written belies it. We put faith in the *Principia*, which is the work of an inordinate Euclidian, constantly attempting to clothe in the forms of ancient geometry methods of proceeding which would more easily have been presented by help of algebra. Shall we ever be told that Bacon complained of the baldness of his own style, and wished he had obtained command over metaphor ? Shall we learn that Cobbett lamented his constant flow of Gallicism and west-end slang, and regretted that his English had not been more Saxon ? If we do, we shall have three very good stories instead of one. We may presume as not unlikely, that Newton, untrained to any *science*, threw away his *Euclid* at first, as very evident : no one need be Newton to feel the obvious premise, or to draw the unwise conclusion. But it would belong to his tutor to make him know better : and Newton was made, as we shall see, to know better accordingly. Our reader must not imagine that deep philosophy and high discovery were discernible in the young subsizar. He was, as to what had come

out, a clever and somewhat self-willed lad, rather late at school, with his heart in the keeping of a young lady who lived in the house where he had boarded, and *vice versa*, more than commonly ingenious in the construction of models, with a good notion of a comet as a thing which might be imitated, to the terror of a rustic neighbourhood, by a lantern in a kite's tail, and with a tidy and more than boyish notion of an experiment, as proved by his making an anemometer of himself by trial of jumping with and against the wind. In that tremendous storm in which many believed that Oliver Cromwell's reputed patron came to carry him away, and in which he certainly died, the immortal author of the theory of gravitation was measuring he little knew what, by jumping to and fro. We do not desire to see boys take investiture of greatness from their earliest playtime : we like to watch the veneration of a biographer growing with its cause, and the attraction varying with some inverse power of the distance. And further, we are rather pleased to find that Newton was what mammas call a *great boy* before he was a great man.

Of all the books which Newton read before he went to Cambridge, only one is mentioned—Sanderson's *Logic* : this he studied so thoroughly that when he came to college lectures he was found to know it better than his tutor. The work is, for its size, unusually rich in the scholastic distinctions and the

*parva logicalia*; very good food for thought to those who can sound the depths. Newton's Cambridge successors are apt to defend their neglect of logic by citing his supposed example, and that of other great men: but it now appears that Newton was not only conversant with *Barbara, Celarent*, etc., but even with *Fecana, Cajeti, Dafenes, Hebare, Gadaco*, etc. We have often remarked that Newton, as in the terminal scholium of the *Principia*, had more acquaintance with the mode of thought of the schoolmen than any ordinary account of his early reading would suffice to explain. We strongly suspect that he made further incursions into the old philosophy, and brought away the idea of fluxions, which had been written on, though not in mathematical form, nor under that name. Suisset's tract on intension and remission is fluxional, though not mathematical: in the very first paragraph he says that the word "intension" is used "uno modo pro alteratione mediante qua qualitas acquiritur: et sic loquendo intensio est motus." For "qualitas" read "quantitas," and we are as near to Newton's idea as we can well be.

In less than four years from the time concerning which we have presumed to ridicule the joint attempt of Conduitt and the biographers to create a dawn for which there is no evidence, the sun rose indeed. Shortly after Newton took his B.A. degree, in 1665, he was engaged on his discovery

of fluxions: but there is neither record nor tradition of his having taken his degree with any unusual distinction. Conduitt's information on this period must be absurdly wrong in its dates. We are to believe that the young investigator who conceived fluxions in May 1665, was, at some time in 1664, found wanting in geometry by Barrow, and thereby led not only to study *Euclid* more attentively, but to "form a more favourable estimate of the ancient geometer when he came to the interesting propositions on the equality of parallelograms. . . ." And this when he was deep in Descartes's geometry of co-ordinates. We entertain no doubt that the unwise contempt for demonstration of evident things, so often cited as a proof of great genius, and its correction by Barrow, all took place in the first few months of his residence at Cambridge.<sup>1</sup> His copy of Descartes, yet existing, is marked in various places, "Error, error, non est Geom."<sup>2</sup> No such phrase as "non est Geometria" would have been used, except by one who had not only read *Euclid*, but had contracted some of that bias in favour of Greek geometry which is afterwards so manifest in the *Principia*. Pemberton, who speaks from communication with Newton, and is a better authority than Conduitt, tells us that Newton regretted he had not paid *more* attention to *Euclid*. And Doctor

<sup>1</sup> [Cf. note 1 on pp. 9-10, and Brewster, *Memoirs*, 1855, vol. i, pp. 21-22, 24.]

<sup>2</sup> [Brewster, *Memoirs*, 1855, vol. i, p. 22, note.]

Sangrado, when the patient died, regretted that he had not prescribed more bleeding and warm water. The *Principia* bears already abundant marks of inordinate attachment to the ancient geometry; in one sense, it has *died* in consequence. If Newton had followed his own path of invention, and written it in fluxions, the young student of modern analysis could have read it to this day, and would have read it with interest: as it is, he reads but a section or two, and this only in England. Before 1669, the year of his appointment to the Lucasian chair, all Newton's discoveries had germed in his mind. The details are notorious, and Sir D. Brewster is able to add a remarkable early paper on fluxions to those already before the world.<sup>1</sup>

We here come upon the well-known letter to Mr Aston, a young man about to travel, which, as Sir David says, "throws a strong light on the character and opinions of its author." It does indeed, and we greatly regret that the mode in which that character has been represented as the perfection of high-mindedness compels us to examine this early exhibition of it, in connexion with one of a later date. Newton is advising his young friend how to act if he should be insulted. Does he recommend him, as a Christian man, to entertain no thought of revenge, and to fear his own conscience more than the contempt of others? Or, as a rational man,

<sup>1</sup> [See the Appendix to the second Essay, above.]

does he dissuade him from the folly of submitting the decision of his difference to the logic of sword or pistol? Or, supposing him satisfied by well-known sophisms that the duel is noble and necessary, does he advise his friend to remember that dishonour is dishonour everywhere? He writes as follows:—

"If you be affronted, it is better, in a forraine country, to pass it by in silence, and with a jest, though with some dishonour, than to endeavour revenge; for, in the first case, your credit's ne'er the worse when you return into England, or come into other company that have not heard of the quarrell. But, in the second case, you may beare the marks of the quarrell while you live, if you outlive it at all."

This letter has often been printed, in proof of Newton's sagacity and wisdom. If Pepys or Boswell had written the preceding advice, they would not have been let off very easily. Again, when, many years after, Newton wrote, as member for the University in the Parliament which dethroned King James, to Dr Covel the Vice-Chancellor, he requests a reasonable decorum in proclaiming William and Mary, "because," says he, "I hold it to be their interest to set the best face upon things, after the example of the London divines." And again, "Those at Cambridge ought not to judge and censure their superiors, but to obey and honour them, according to the law and the doctrine of passive obedience." What had Newton and passive obedience just been doing with King James?

These instances, apart from science, show us the character of Newton out of science: he had not within himself the source from whence to inculcate high and true motives of action upon others; the fear of man was before his eyes.<sup>1</sup> But his mind had been represented as little short of godlike: and we are forced upon proof of the contrary. Had it been otherwise, had his defects been duly admitted, it would have been pleasant to turn to his uncompromising philosophic writings, and to the manner in which, when occupied with the distinction between scientific truth and falsehood, no meaner distinction ever arose in his mind. This would have been, but for his worshippers, our chief concern with him. The time will come when his social weaknesses are only quoted in proof of the completeness with which a high feeling may rule the principal occupation of life, which has a much slighter power over the subordinate ones. Strange as it may seem, there have been lawyers who have been honest in their practice, and otherwise out of it: there have been physicians who have shown humanity and kindness, such as no fee could ever buy, at the bedside of the patient and nowhere else.

### III

Sir David Brewster gives Newton's career in optics at great length; it is his own subject, and

<sup>1</sup> [The letter to Aston is given, with comments, in Brewster's *Memoirs*, 1855, vol. i, pp. 34, 385-389.]

he makes us feel how completely he is at home. He gives a cursory glance at the science even down to our own time; and he does the same with astronomy. The writer would rather have had more of the time of Newton, and particularly, more extracts from the "Portsmouth Papers." But we must think of our neighbours as well as of ourselves; and the general reader will be glad to know that so much of the work is especially intended for him. We have not space to write an abstract; but the book is very readable. In the turmoil of discussion which arose out of his optical announcements, Newton made the resolution, which he never willingly broke, of continuing his researches only for his own private satisfaction. I see, said he, that a man must either resolve to put out nothing new, or to become a slave to defend it. It seems that he expected all his discoveries to be received without opposition.

About 1670, or later, Newton drew up a scheme for management of the Royal Society, which Sir D. Brewster found among the papers. Certain members, some in each department, should be paid, and should have fixed duties in the examination of books, papers, experiments, etc. In this paper our writer, whose views on this subject are very large and of old standing, sees the recommendation of an Institute, which indeed, on a small scale, the plan seems to advocate. Sir David would have all

the societies congregated at Kensington Gore, under liberal patronage, and images to himself that "each member of the now insulated Societies would listen to the memoirs and discussions of the assembled Academy,<sup>1</sup> and science and literature would thus receive a new impulse from the number and variety of their worshippers!" If all *Fellows* were *savants*, and if all *savants* studied all sciences, this might be practicable. There is one body in London which cultivates a large range of subjects, the Royal Society itself: and all the world knows that the meetings of this Society, abounding in Fellows of such universality of knowledge as in our time is practicable, are less interesting and worse attended than those of any of the societies for special objects. And reason good: the astronomer or the geologist goes down to his own place for he knows what; but the astronomer is shy of a society of which it is as likely that any one evening may give him a treat of physiology as of astronomy, and the geologist, who wants a stone when he asks for bread, turns very sleepy under a dose of hyper-determinants or definite integrals.

Newton's reputation rests on a tripod, the feet

<sup>1</sup> The members of the French Institute receive a part of their emoluments at the Board, and the quatum of each day on which any one is absent is forfeited. This insures good attendance, and we have, on pay-day, seen men of profound science, during the memoirs and discussions of the assembled Academy, practising the first rule of arithmetic, called numeration, upon rouleaux of five-franc pieces. To this it must be added that the Institute has much patronage, and constant attendance is necessary to keep up influence and connexion.

of which are fluxions, optics, gravitation. Each one of these words must be used in a very large sense: thus by fluxions we mean all mathematics as bearing upon a system of which the fluxional calculus is at the completion. Of the three supports of this tripod one only has received any damage, though left quite strong enough, in conjunction with the rest, to support the fabric through all time. In optics only, the subject on which Newton showed his first impatience of opposition, his opinion, even his system, has been set aside in our own day. The hypothesis of an undulating ether, as the immediate agent in the production of light, has superseded that of particles emanating from the luminous body: and though the undulationists, now a large majority, have long maintained their theory with a higher order of certainty than they were entitled to, yet it seems that time is drifting their conclusions to a stable anchorage. There is something like coincidence in the almost simultaneous appearance of the first elaborate biography of Newton, who well-nigh strangled the undulatory theory in its cradle, and of that of Young, who first played a part of power in its resuscitation. As yet, Young is fully known but to a few: his early education was not, like that of Newton, conducted under a system which corrects the false impressions of green age. Had he been trained in a University, he would have been, as they say of the globe, rectified for the latitude of

the place: but speculation on what he might have become may be deferred until what he did become is of more popular notoriety. Dean Peacock's *Life* is one of the best of scientific biographies, and the three volumes of Young's collected writings are treasures to all who know what intellectual wealth is.

## IV

We come to the *Principia*, and we confess that we heartily wish it were but just and right to persuade ourselves that the author of this work could do no wrong. One of the greatest wonders about it is the manner in which it was thrown off in eighteen months. Certainly the matter had fermented in Newton's mind many years before: but it was not the irresistible call of his own genius which drew him to the work in December 1684; it was Halley, and the influence of the Royal Society brought to bear by Halley. Sir D. Brewster very properly contends that to Halley, not to the Society, the *Principia* is due. Who found out, casually, that Newton had had some great success in the question which had occupied many of the first minds, the connexion of the planetary motions with mechanical second causes? Who went to Cambridge to learn the truth of the report, obtained specimens from Newton with a promise to go on, got himself appointed by the Royal Society to "keep Mr Newton

in mind of his promise," did keep Mr Newton in mind, and doubtless let him have no peace unless he continually reported progress? Who, when Newton, disgusted with the unfair claim of Hooke, proposed to leave out the third book (that is, all the application of the previous books to the *actual solar system*), soothed him with skilful kindness, and made what Sir D. Brewster calls his "excellent temper" recover its serenity? Who paid the expense of printing, when the Royal Society found it could not afford to fulfil its engagement? To all those questions the answer is—Halley, who shines round the work, as Newton shines in it. When Newton proposed to leave out the third book, he felt that *Philosophiæ Naturalis Principia Mathematica* was no longer the true title, but rather *De Motu Corporum Libri Duo*; but, feeling this, he intended to preserve the wrong title, because, as he says to Halley, "'Twill help the sale of the book, which I ought not to diminish now 'tis yours." The greatest of all works of discovery, with a catch-penny title! We can hardly excuse this, even though the penny were angled for by a feeling of gratitude. We never liked the "*Eme, lege, fruere*," which figures in the title-page of Copernicus: this was the work of an injudicious friend; but Newton was only saved from worse by his incomparable adviser.

We are come to the time when the morbid dislike of opposition which would, but for Halley, first have

prevented the *Principia* from being written, and next have deprived it of its essential conclusions, is no longer regarded as the modesty of true greatness, and served up for us to admire, as we shall answer the contrary at our peril. It is passed without comment; we are now in slack water, and the turn of tide will be here in due season. The sooner the better; for the indulgence due to the mother failings of a great public benefactor cannot be cheerfully and cordially given so long as our gratitude is required to show itself in misnomers and make-believes. Candid acknowledgment would convert censure into regret: sufficient acknowledgment would turn the reader into an extenuator: the *Principia* would neutralise greater faults than Newton's; but it will not convert them into merits. The quarrel is not with Newton for his weaknesses, but with the biographer for his misconception of his own office. How indeed would it be possible to think for a moment with harshness of a great man of all time, and a good man of an evil time, on account of errors which we never could have known but for the benefits to ourselves in the achievement of which they were committed?

If faults had exhibited themselves in matters affecting society at large, by offences, as it were, against the Crown, the fountain of justice would also have been that of mercy, and the evidence to character and services would have secured a nominal

sentence. But the suits we have to deal with are in civil process. The memory of more than one illustrious contemporary brings an action for damages, and palliation of the defendant is injustice to the plaintiff.

Though not much relying on Conduitt's memoranda of mathematical conversations, we trust that which follows, and it will much please young mathematicians to read of Newton in one of their own scrapes. When Halley visited him in 1684,—

... "he at once indicated the object of his visit by asking Newton what would be the curve described by the planets on the supposition that gravity diminished as the square of the distance. Newton immediately answered, *an Ellipse*. Struck with joy and amazement, Halley asked him how he knew it? Why, replied he, I have calculated it; and being asked for the calculation, he could not find it, but promised to send it to him. After Halley left Cambridge, Newton endeavoured to reproduce the calculation, but did not succeed in obtaining the same result. Upon examining carefully his diagram and calculation, he found that in describing an ellipse coarsely with his own hand, he had drawn the two axes of the curve instead of two conjugate diameters, somewhat inclined to one another. When this mistake was corrected, he obtained the result which he had announced to Halley."

This anecdote<sup>1</sup> carries truth on the face of it, for Conduitt was neither mathematician enough to have conceived it, nor to have misconceived it into any-

<sup>1</sup> [Brewster, *Memoirs*, 1855, vol. i, p. 297.]

thing so natural and probable as what he has given. Little things illustrate great ones. Newton, whose sagacity in pure mathematics has an air of divination, who has left statements of results without demonstration, so far advanced that to this day we cannot imagine how they were obtained, except by attributing to him developments of the doctrine of fluxions far, far beyond what he published, or any one of his time—this Newton was liable, both in his own closet and in his printed page, to those little *incuriæ* which the man of pen and ink must sometimes commit, and which the man who can push through a mental process may indeed commit, but is almost sure to detect when he empties his head upon paper. Now join what precedes to Newton's own assertion that he had no peculiar sagacity, but that all he had done was due to patience and perseverance; an assertion at any common interpretation of which we may well smile, but which, all things put together, may justify us in such an irreverent simile as the supposition that he hunted rather by scent than by sight.

## V

We now come to the second volume, and to those points on which we more especially differ from Sir D. Brewster. Our plan must be to take one or two prominent cases, and to discuss them with the biographer. We do not express disapprobation at

the facility with which he credits the opponents of Newton with bad motives: we are glad of it, and thank him for it. There is a pledge of earnest sincerity in the wildness with which the barbed arrow is fired at Leibniz or at Flamsteed; and if the partisan be too much led away by his feelings to be a judicious counsel, it is not we, to whom trouble is saved, who ought to blame him for it. We take the following as an instance, chiefly because we can be brief upon it.

Newton and others, acting for Prince George, entered into an agreement with Flamsteed: articles of agreement were signed, out of the execution of which quarrels arose. We must know, as Sir David justly observes, what these articles were before we can judge. No signed copy appears: Mr Baily found none among Flamsteed's papers, Sir David found none among Newton's. But draught articles occur in *both* repositories: and, wonderful to relate, the unsigned draughts actually differ; Flamsteed's draughts bind him less, Newton's draughts bind Flamsteed more. The case is a very common one; the manner in which Sir David treats it is not quite so common. Speaking of Flamsteed, he informs us that "of these he has left no copy, because he had wilfully violated them": speaking of the draughts in Newton's possession, he says, "I regret to say that they are essentially different from those published by Mr. Baily"; by which he means that

Newton's unsigned papers are of course copies of the signed agreement, and Flamsteed's of course no such thing; the false draughts being purposely retained by Flamsteed, in preference to the final articles purposely destroyed. We need not tell our readers that a man is not to be pronounced dishonest because his draught proposals do not agree with his signed covenants, still less because they do not agree with the other parties' draught proposals. Newton and Flamsteed were both honest men, with very marked faults of different kinds: we may be sure neither of them privately destroyed a document for the suppression of evidence. When Sir D. Brewster not merely *opines*, but *narrates*, that Flamsteed left no copy because he had wilfully violated them, he is our very good friend, and lightens our task very much.

When Newton allowed himself to perpetrate, not the suppression of a document, for a third edition does not suppress the first and second, but a revocation so made as to do all that could be done towards suppression, Sir David Brewster is his defender, and in this instance, we really believe, one of the last of his defenders. He thinks the step was "perhaps unwise," but proceeds to say that Newton was "not only entitled but constrained" to cancel the passage.

When Leibniz applied to Newton for information on the nature of the discoveries with rumours of which the English world was ringing, Newton com-

municated some of his algebraic discoveries, but studiously concealed a descriptive mention of fluxions under the celebrated anagrams, or sentences with their letters transposed into alphabetical order. Leibniz (1677) replied, almost immediately, with a full and fair disclosure of his own differential calculus, and in so doing became the first publisher of that method, and under the symbols which are now in universal use. He adds that he thinks Newton's concealed method must resemble his own; thus holding out an invitation to Newton to say yes or no. Not one word of answer from Newton. Accordingly, when Leibniz printed his discovery in the *Leipsic Acts* for 1684, he did not affirm that Newton was in possession of a method similar to his own. What ought he to have done, we ask of our readers, under these circumstances? Ought he to have given Newton's assertions about his method, as assertions, leaving it to a suspicious temper to surmise that the reader was desired not to believe without proof? Ought he, as a matter of compliment, to have promulgated what Newton was doing everything in the power to conceal? Seven years had passed, and Newton had made no sign: was Leibniz bound, either in fairness or in courtesy, to take on himself to affirm that he had a method similar to his own? Not in fairness; for if a man studiously conceal and continue to conceal his discovery, those to whom he may have stated that he

had a discovery are not bound to be his trumpeters until such time as he shall please to reveal himself. Not in courtesy; a man who sends only anagrams, and when he receives from his correspondent a full and open account of that correspondent's discoveries, and an invitation to state whether his own resemble them, returns no answer, cannot complain of want of courtesy if his correspondent keep silence about him thenceforward. What Leibniz did was merely to state that no one would successfully treat such problems as he had treated, except by his own calculus, or one similar to it. Sir D. Brewster calls his silence with respect to Newton the first fault in the controversy: we see no fault at all; and if we did, we should call it the second. The paper had no historical allusions; Cavalieri, Fermat, and Hudde, each of whom had shown the world something approaching to *calculus*, are not named in it: and either of these had more claim to mention than Newton at that time. But, two years afterwards, in 1686, Leibniz published a paper in the same *Leipsic Acts*, a paper which Newton did not cite when, long after, he was writing against Leibniz, a paper which the Newtonians are very shy of citing, and of which, apparently, Sir David knows nothing. In this paper he explains the foundation of the *integral calculus*, the matter of which was much more likely to recall Newton to mind than his former paper on the differential calculus: for

his application to Newton, in the first instance, was to know what he had done on series, and especially with reference to their use in *quadratures*, which we now call *integration*. Here he gives an historical summary; and speaking of those who had performed quadratures by series, he proceeds thus;—"A geometer of the most profound genius, Isaac Newton, has not only arrived at this point independently of others, but has solved the question by a certain universal method: and if he would publish, which I understand he is now preparing to do, beyond doubt he would open new paths, to the great increase, as well as condensation, of science." A passing word on Leibniz. We shall not stop to investigate the various new forms in which Sir D. Brewster tries to make him out tricking and paltry. We have gone through all the stages which a reader of English works can go through. We were taught, even in boyhood, that the Royal Society had made it clear that Leibniz stole his method from Newton. By our own unassisted research into original documents we have arrived at the conclusion that he was honest, candid, unsuspecting, and benevolent. His life was passed in law, diplomacy, and public business; his leisure was occupied mostly by psychology, and in a less degree by mathematics. Into this last science he made some incursions, produced one of the greatest of its inventions, almost simultaneously with one of its greatest names, and made himself

what Sir D. Brewster calls the "great rival" of Newton, in Newton's most remarkable mathematical achievement.<sup>1</sup>

Newton, in the first edition of the *Principia*, gave a fair and candid account of the matter. But, many years after, when this important passage was quoted against those (and we now know that Newton was *always* one of them) who endeavoured to prove Leibniz a plagiarist, he tried to explain away the force of his own admission. This he did twice; once in a private paper which Sir D. Brewster has published—and, strange to say, in vindication of the suppression of the passage which took place in the third edition—and once in those observations on Leibniz's last letter which he circulated among friends until Leibniz died and then sent at once to press. We give the Scholium from the *Principia*, and the two *explanations*.

*Scholium from the "Principia" (first edition).*  
 "In letters which passed between me and that most skilful geometer G. G. Leibnitz ten years ago, when I signified that I had a method of determining maxima and minima, of drawing tangents to curves, and the like, which would apply equally to irrational as to rational quantities, and concealed it under transposed letters which would form the following sentence—'Data æquatione quocunque fluentes quantitates

<sup>1</sup> [De Morgan wrote a biography of Leibniz, an extract from which is given in the first Appendix to this Essay.]

involvente, fluxiones invenire, et vice versa'—that eminent man wrote back that he had fallen upon a method of the same kind, and communicated his method, which hardly differed from mine in anything except language and symbols. The foundation of both is contained in the preceding Lemma."

*Newton's explanation, left in manuscript.*

*Newton's explanation circulated in writing, and printed in Raphson's "Fluxions" (1716, date of title 1715) after Leibniz's death.*

"After seven years, viz. in October 1684, he published the elements of this method as his own, without referring to the correspondence which he formerly had with the English about these matters. He mentioned indeed, a *methodus similis*, but *whose that method was*, and *what he knew of it*, he did not say, as he should have done. And thus *his silence put me upon a necessity* of writing the Scholium upon the second Lemma of the second Book of Principles, *lest it should be thought that I borrowed* that Lemma from Mr Leibnitz."

P. 115. "He pretends that in my book of *Principles*, pp. 253, 254, I allowed him the invention of the *Calculus Differentialis*. independently of my own; and that to attribute this invention to myself, is contrary to my knowledge. But in the paragraph there referred unto, I do not find one word to this purpose. On the contrary, 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; that is, eight months at least before the date of his own. And by referring to the letters which passed between Mr Leibnitz and me ten years before, I left the reader to consult these letters, and interpret the paragraph thereby."

The first explanation is from a manuscript supplement to that printed answer to Leibniz of which the second explanation is part. We think better of Newton in 1687 than to believe either, though we do not doubt that Newton in 1716 saw his former self through the clouds of 1712. Though the morbid suspicion of others, which was the worst fault of temperament, the fault alluded to by Locke, did act to some extent throughout his whole life, yet we do not believe that it was in 1687 what it afterwards became when he had sat on the throne of science for many years, the object of every form of admiration, and every form of flattery. Could we believe his first explanation, could we think that in 1687 his hidden anagrams, answered by Leibniz's candid revelations, produced no effect except a diseased feeling that perhaps Leibniz would rob him, instead of a generous confidence that Leibniz would not suspect him, we should turn from him with pity. We must now change our position, and defend him from his biographer. Sir D. Brewster does not quote the second explanation; he only cites the page, and quotes a few words occurring further on, which are much less to the purpose, and which he says "fortunately" give us Newton's opinion. Now we say that the second explanation, as quoted by us, fortunately saves Newton from his own imputation upon himself. The two explanations cannot stand together: according to the

first Newton was guarding himself from a charge of plagiarism; according to the second, he was putting upon Leibniz the *onus* of averting a similar charge from himself. Both motives might have been simultaneous; but both could not be so much the chief motives as to be separately worthy of standing alone. But the most precious inference in Newton's favour is that the second explanation<sup>1</sup> is demonstrably not the true one, and the disorder of mind which perverted the best-known facts may as easily, and more easily, have perverted the memory of impressions. Those letters which Newton *referred to* that the reader might *consult* them, for interpretation of his printed paragraph, had never been published, had never been announced, were not then likely to be published, and in fact never were published till 1699, thirteen

<sup>1</sup> In reference to both explanations, the following is remarkable. Just after Leibniz made his publication of 1684, a young Scotchman, Craig, then of Cambridge, took it up, and published a short tract upon the quadrature of curves, in which he uses, with high praise, the differential calculus of Leibniz. He had been in communication with Newton, had asked for help in this very subject of quadrature, and had received the binomial theorem, then unprinted. But not one word did Newton drop to the effect that *he* also had a method like that of Leibniz, and that he and Leibniz had communicated seven or eight years before. Craig says, long after, in 1718, that Newton examined the manuscript: it is clear, however, that his memory is at fault here, and that it was the second edition (1693) which Newton examined. Are we to believe that Newton was brooding over the matter of the two explanations, at a time when he allowed his young friend to proclaim Leibniz as the author of the new calculus, with that negation of himself which was implied in acknowledgment of assistance on *another point*? We rather suspect that, at the time, when the geometrical form which is so prominent in the *Principia*, then on the anvil, was in his mind, he greatly undervalued his own fluxions. And we think they never would have been heard of if the mighty force which the calculus had developed by 1693 had not shown him how much there was to contend for.

years afterwards. Moreover, the letters were not written by Leibniz and Newton to one another, but by both to Oldenburg: how could the readers of the *Principia* have known what to go to; or how could they have gone to the letters, if they had known? The truth we suspect to be as follows:— In 1712, when those letters were first republished, the *second* edition of the *Principia* was in preparation, and the battle of fluxions was raging: we believe that in 1716, all that Newton said of himself in reference to the first edition of the *Principia* must be referred to the Newton of the *second* edition. On any other supposition, except morbid confusion of ideas, Newton must be charged with worse than we ever believed of him. What well-read and practised investigator, with his mind in its normal state, and all his books before him, ever mistakes the date of first publication of any of his own works by thirteen years, in a deliberate answer to an acute opponent? Again, Newton is quite wrong as to the *eight months* which he gives Leibniz to execute his alleged fraud in. His own *Commercium Epistolicum* would have taught him better. Though his second letter to Oldenburg (the one in question) was dated October the 24th, 1676, and Leibniz's answer June the 21st, 1677, yet Collins informs Newton that the copy intended for Leibniz was in his hands on March the 5th, 1677, but that in a week it would be despatched to Hanover by a private hand.

We are of opinion that the *moral intellect* of Newton—not his moral *intention*, but his power of judging—underwent a gradual deterioration from the time when he settled in London. We see the faint traces of it in his manner of repudiation of the *infinitesimal* view of fluxions, in 1704. A man of sound judgment as to what is right does not abandon a view which he has held in common with a great rival, and this just at a time when the world is beginning to ask which came first in their common discovery, without a clear admission of the abandonment: he does not imply that *some* have held that view, and declare against the opinion of those *some*, without a distinct statement that he himself had been one of them: still less does he quietly and secretly alter what he had previously published, or allowed to be published, so as to turn the old view into the new one, and to leave the reader to understand that he had never changed his opinion. The Newton of the mythologists would have felt to his fingers' ends that such a proceeding had a tendency to give false impressions as to the case, and to throw suspicion on his own motives. This is a small matter, but it is a commencement of worse. We come to the *Commercium Epistolicum*, the name given to the collection of letters, accompanied by notes and a decision of the question, on the part of a Committee of the Royal Society. To this well-known

part of the history Sir D. Brewster has a very important addition to make; and he makes it fairly, though we confess we wish he had given us what they call chapter and verse. "It is due to historical truth to state that Newton supplied all the materials for the *Commercium Epistolicum*, and that though Keill was its editor, and the Committee of the Royal Society the authors of the Report, Newton was virtually responsible for its contents.<sup>1</sup>

Before we proceed further, we must address a respectful word to Lord Portsmouth, the descendant of Newton's niece, the representative of his blood, and the possessor of these valuable papers, to whose liberality and judgment the permission to publish their contents is due, after long concealment from fear of hurting Newton's reputation, and long abeyance from family circumstances. We submit to him that either too much is done, or not enough. Great harm arose out of the rumours which circulated during the period in which the papers were concealed: both the opponents and the defenders of Newton's conduct were, without any fault of their own, put in a wrong position as to interpretation of facts and appreciation of probabilities. Much more

<sup>1</sup> [See Brewster, *Memoirs*, 1855, vol. ii, p. 75. From a study of the "Portsmouth Papers," Brewster was enabled to confirm De Morgan's contention of 1852 that Newton wrote the anonymous preface to the second edition of the *Commercium Epistolicum*. On De Morgan's rather later view of Newton's character, see the second Appendix to this Essay.]

harm will be done if the regretful admissions of so warm a partisan as Sir D. Brewster be allowed to stand instead of these rumours. The papers cannot possibly contain anything from which any such injury would arise as unquestionably will arise from the above substitution, which, to all the indefiniteness of mere rumour, adds all the authority of a judicial decision. For when Sir D. Brewster declares against Newton, it is as if a counsel threw up his brief: we mean nothing disrespectful, for we remember when we ourselves would have held it, on such retainers as the *Principia*, the fluxions, and the optics. Why should not these papers be published? It must come to this at last. We have little doubt that the Government would defray the expense, which would be considerable: and the Admiralty publication of the Flamsteed papers would be a precedent of a peculiarly appropriate character. Those who were scandalised at the idea of the nation paying for the printing of an attack upon Newton would take it as reparation: while those who entirely approved of the proceeding would as heartily approve of the new measure. It is impossible that the matter should rest here. Sir D. Brewster himself will probably desire, for his own sake, for that of Newton, and for that of truth, that these documents should undergo public scrutiny. And we have no delicacy in saying that they ought to come under the eyes of persons familiar with the

higher parts of mathematics, which Sir D. Brewster neither is, nor pretends to be.<sup>1</sup>

The Committee of the Royal Society was always considered in England as judicial, not as expressly defensive of Newton. A few years ago, Professor De Morgan, a decided opposer of Newton and the Committee in the fluxional dispute—and one whose views Sir D. Brewster states himself to have confirmed on several points—rescued the objects of his censure from the inferences which this notion would lead to, and showed that the Royal Society intended its Committee for purposes of advocacy, and that the members of the Committee had no other idea of their own function. Sir D. Brewster says that Newton himself asserted this also: he does not say where, and this is only one of several *obiter dicta* which ought to have been supported by reference; we remember no such statement. It is now of course perfectly settled that the Committee was *not* judicial; and we find Newton to have been the real source of the materials of the *Commercium Epistolicum*, and answerable for all the running notes which accompany the published correspondence. We might easily proceed to justify our assertion that his moral intellect was undergoing deterioration: but, for want of space, we shall pass on to 1716, and shall make one extract from his letter to Conti,

<sup>1</sup> [For a later utterance of De Morgan's about the necessity of publishing the "Portsmouth Papers," see *Newton: his Friend: and his Niece*, London, 1885, pp. 148-149.]

in which, in his own name, he makes the assertion that Leibniz had stolen from him. He says that he had explained his "method" to Leibniz, "partly in plain words and partly in cyphers," and that Leibniz "disguised it by a new notation pretending that it was his own." His statement contains two untruths, which we impute to the forgetfulness of irritation. He did not describe part of his method in plain words: all that he described in plain words was the species of problems which he could solve. When Glendower said, "I can call spirits from the vasty deep," no one ever supposed that he "partly described" the "method" of doing it. Secondly, he did not describe the rest in *cypher*: he put the letters of his sentences into alphabetical order, and gave what was called an *anagram*. There are many good decypherers in the country, and the task is one for a mathematician: Wallis in past times, and Mr Babbage now, may be cited as instances. But no one will undertake to say what the sentence is which we have decomposed into the following string of letters: 6a 2c 5d 19e 2f 3h 5ij 3kl 6n 5o 8r 9s 9t 3u 2vw 3y; ninety-three letters in all, six of which are a's, two are c's, etc.

Yet a few years more, and the deterioration is more decided. In 1722, Newton himself wrote a preface and an *Ad Lectorem* to the reprint of the *Commercium Epistolicum*, and caused to be prefixed a Latin version of the account of that work which

he had inserted anonymously in the *Philosophical Transactions* for 1715. His authorship of this paper, constantly denied, and for very cogent reasons, by his partisans, but proved from evidence internal and external, is now admitted by Sir D. Brewster. Much is to be got from those documents, but we shall only add that a few years ago Mr De Morgan discovered that some alterations, one in particular of great importance, had been made in this reprint, *without notice*. Of this Sir D. Brewster says not one word. He calls the *reprint* a *new edition*, which it was not: so completely does it profess to be only a reprint, that the old title-page, and the *old date*, are reprinted after the new title, and the avowedly new matter at the beginning. We now believe that Newton was privy to the alterations, and especially to the most important of all: we believe it independently of what may possibly arise from further scrutiny; and we suppose from Sir D. Brewster's silence that he has no means of contradicting this natural inference. The famous letter of Newton to Collins, on which the Committee (very absurdly) made the whole point turn, was asserted to have been sent to Leibniz, but no date of transmission was given with the letter, though the *report* of the Committee affirmed a rough date of which nothing was said in their *evidence*. A date of transmission was smuggled into the reprint. Where does this date first appear? Who first gave it?

Newton himself in the *Philosophical Transactions*, anonymously, and without stating any authority.

Lastly, in the third edition of the *Principia*, Newton struck out the scholium in which he had recognised the rights of Leibniz. It has been supposed that Pemberton, who assisted him, was the real agent in this "perhaps unwise" step: but it appears distinctly that Newton alone is responsible. He struck out this scholium; did he state openly why, and let his reader know what had been done? He supplied it by another scholium, beginning and ending in words similar to the old one, but describing, not the correspondence with Leibniz, but the celebrated letter to Collins. If the old scholium had been misunderstood, as Newton affirms it was, nothing would have been more easy than to annex an explanation: if the suppression were done openly. Newton, in the second edition of the *Principia*, had revenged himself on Flamsteed by omitting Flamsteed's name in every place in which he could possibly do without it: the omission of his candid and proper acknowledgment of what had passed between himself and Leibniz was but a repetition of the same conduct under more aggravated circumstances. Of this letter to Collins, asserted to have been sent to Leibniz, and falsely, as proved in our own day both from what *was* sent to Leibniz, now in the Library at Hanover, and from the draught which has turned up in the archives of the Royal

Society, we shall only say that it proved that Newton was more indebted to Hudde than Leibniz would have been to him if he had seen the letter. But the relations of Hudde to the two inventors of the differential calculus would be matter for a paper apart.

## VII

To discuss every subject would require volumes ; and we shall therefore now pass on to Sir D. Brewster's treatment of the curious question of the relation which existed between Newton's half niece, Catherine Barton, and his friend and patron, Charles Montague, Earl of Halifax. Sir D. Brewster declares that for a century and a half no stain has been cast on the memory of Mrs C. Barton, and then proceeds to quote Voltaire's insinuation as scarcely deserving notice ; so that by "no stain" we are to understand no stain which *he* thinks worthy of notice. Now the fact is that, though respect for Newton has kept the matter quiet, there has always been a general impression that it was a doubtful question, a thing to be discussed, whether or no Mrs C. Barton was the mistress of Lord Halifax. Mr De Morgan took up this subject in the *Notes and Queries* (No. 210), and, perfectly satisfied that she was either a wife or a mistress, came to a balanced conclusion that, as he says, "the supposition of a private marriage, generally

understood among the friends of the parties, seems to me to make all the circumstances take an air of likelihood which no other hypothesis will give them : and this is all my conclusion." Sir D. Brewster, whose mind admits no such balance, makes this the "inference" of a private marriage. The grounds of the alternative are that she was publicly declared, by the writer of the *Life* of Halifax, to have lived, when very young, and she herself distinguished by beauty and wit, in the house of Lord Halifax as "superintendent of his domestic affairs" : and this not in attack, but defensively, with a declaration that she was a virtuous woman, though "those that were given to censure passed a judgment upon her which she no ways merited." Further, Lord Halifax held in trust an annuity for her of £200 a year, bought in Newton's name : besides which he left her £5000, with Bushy Park and a manor for life : while neither she nor any one of her friends contradicted the admission made in the *Life* of Halifax, which came out at the time when the legacies and the annuity would have turned public attention upon Miss Barton. This is a subject unconnected with mathematics ; and we dwell upon it more than its intrinsic importance deserves, because it will enable us to show to every reader the kind of reasoning which can be pressed into the service of biography, when biography herself has been tempted into the service of partisanship. We may judge

from the arguments which Sir David is driven to employ, that he would have followed the example of other biographers in slurring this subject, if Mr De Morgan's closing words had not reminded him that the day for such a suppression was past: "such points, relating to such men as Newton, will not remain in abeyance for ever, let biographers be as timid as they will." And we may also judge from these arguments why it is that the subject has been allowed to remain in abeyance.

And first, as to the annuity. Halifax holds in trust an annuity for Miss Barton, and directs his executor to give her all aid in the transfer: this annuity was bought in Newton's name. Sir D. Brewster declares that "an annuity purchased in Sir Isaac Newton's name can mean nothing else than an annuity purchased by Sir Isaac Newton." This is an assertion of desperation—it could have meant, not thereby saying that it did mean, a settlement by Halifax on Miss Barton, done in Newton's name, with or without Newton's knowledge; and done in Newton's name purposely that people might think it was made by Newton, or, at least, not by Halifax. This may appear impossible to Sir D. Brewster in 1855, and yet it may have been done in 1706. We may fairly infer that Halifax did not draw his will with the intention of giving colour to those reports against which his biographer protests, or with the intention of exciting such reports: if the annuity were bought by

Newton, what more easy than to have said so? In spite of Sir D. Brewster, who is neither lawyer nor actuary, we affirm positively that the description of an annuity upon the life of A B as bought in the name of C D, does not imply that C D paid for it, and that so far as it implies anything on the point, which is little enough, it is the very contrary. Again, Conduitt does not mention this annuity in his list of the benefactions which Newton, who was very generous to his family, bestowed on his poorer relations. For this Sir D. Brewster has to find a reason; Conduitt was the husband of Catherine Barton, knew of the assertions in Halifax's biography, had read Halifax's will, and must have been cognisant of the fact that the existence of a scandal had been asserted in print. And he finds a curious reason.

"But the annuity was not a benefaction like those contained in Conduitt's list. It was virtually a debt due to his favourite niece whom he had educated, and who had for twenty years kept his house; and if she had not received it from Sir Isaac, his conduct would have been very unjust, as, owing to his not having made a will, she got only the eighth part of his personal estate along with his four nephews and (three other) nieces."

Let us first take Sir D. Brewster's statement, as here given, erroneous as it is. When a single man educates a favourite niece, thereby distinguishing her from his other nieces, and gives her shelter and main-

tenance until she marries (for we must here take Sir D. Brewster's assertion that she did not leave him to live with Lord Halifax), all the world knows that the least that favourite niece can do is to keep house for him, and that the idea of her services in looking after the dinner, which he pays for and gives her share of, running him into debt, actual or *virtual* (oh, the *virtue* of this word!), is an absurdity. No doubt a man ought to provide for such a niece after his death: but if he should leave her, as Newton did to Miss Barton, the eighth part of £32,000, producing an income of more than £200 a year, he treats her very handsomely: especially if a friend of his should have left her a large fortune, and his introduction should have married her to a member of Parliament. Now to Sir D. Brewster's statement. Just before our quotation begins, he informs us that by the act of transference it appears that this trust was created in 1706, so that he seems to say that Miss Barton, aged six years, began to keep Newton's rooms in Trinity College, when he was writing the *Principia*: for he says she "had" kept his house for twenty<sup>1</sup> years. He does not mean this: but here and elsewhere he

<sup>1</sup> Conduitt tells us that his wife lived with her uncle nearly twenty years, before and after her marriage: it is believed that the Conduitts resided with Newton from the very marriage. Newton lived in London thirty years; therefore, ten or more of those years his niece did not live with him. The annuity was bought in 1706 and Halifax died in 1715. Miss Barton, being sixteen years old when Newton came to London, must have finished her school education shortly afterwards. Either Newton did not invite his favourite niece, whom he had educated, to live with him for ten years afterwards, or there is a gap which tallies most remarkably with the hypothesis of her residence under the roof of

heaps circumstances together without sufficient attention to consistency. We very much doubt if Newton *could* have afforded the price of that annuity in 1706. He came to London with very little in 1696: by 1706 he had enjoyed £600 a year for four years, and £1500 a year for six years. An annuity of £200 on a life of twenty-six, money making five per cent., now costs about £3000: if we say, which is straining the point to the utmost, that Miss Barton's annuity cost £2000, we confess we think it not very likely that Newton could have bought it, or that he would have held it just to his other relatives to have bought so large an annuity. But we are quite sure that Conduitt, under all the circumstances, would never have held this annuity as payment of a debt due to his wife; he would not have made the twenty years end with 1706, to speak of nothing else.

Next, we come to the way in which Sir D. Brewster treats the assertions of Halifax's biographer. Those assertions are not in attack, but in defence; the witness is a friendly one, and the publication was made at the very time when Halifax's will had just drawn public attention to the legacies.

Halifax. But, as a presumption against the first supposition, there is extant a short letter from Newton to his niece, written in 1700, which by the contents seems written to an inmate of his house, absent for change of air.

Newton has been charged with avarice; of which there is really no proof, unless his dying worth more than £30,000 be one. But Conduitt was in easy circumstances, and his wife also: their daughter was said to have had £60,000. Supposing, as is probable, that they bore their fair share of the joint expenses, Newton might have saved nearly all his income for the last ten years of his life.

"I am likewise to account for another Omission in the Course of this History, which is that of the Death of the Lord *Halifax's* Lady; upon whose Decease his Lordship took a Resolution of living single thence forward, and cast his Eye upon the Widow of one Colonel *Barton*, and Neice to the famous Sir *Isaac Newton*, to be Superintendent of his domestic Affairs. But as this Lady was young, beautiful and gay, so those that were given to censure, pass'd a Judgment upon her which she no Ways merited, since she was a Woman of strict Honour and Virtue; and tho' she might be agreeable to his Lordship in every Particular, that noble Peer's Complaisance to her, proceeded wholly from the great Esteem he had for her Wit and most exquisite Understanding, as will appear from what relates to her in his Will at the Close of these Memoirs."

Now Sir D. Brewster is so far biased by the necessities of his case, as to affirm that it is *not* here stated that Miss Barton (that she had been married is a mistake) lived under Halifax's roof. "His biographer makes no such statement. . . . How could any person contradict the *cast of an eye*—the only act ascribed to Halifax by his biographer?" The writer of "Newton" in the *Biographia Britannica*—as strong a partisan as Sir David—could not get so far as this ingenious solution: for he makes Halifax's continuance in his widowed state "the less to be regretted" on account of this "cast of an eye." We are to infer, according to Sir David, that this friendly biographer, wishing to defend Miss Barton from censure she no

ways deserved, and alluding to rumours which had no source except a "plan or a wish" of Lord Halifax, omitted to state that the plan was all Montague's eye; and forgot to assert the very material circumstance that she did *not* accede to the plan, that she did *not* live in the house of her earnest admirer. We make no doubt, on the other hand, that the apologist means to say that she did live there, and made her a widow to give some colour of respectability to it. Her noble admirer left his large legacy "as a token," he writes, "of the sincere love, affection, and esteem, I have long had for her person, and as a small recompence for the pleasure and happiness I have had in her conversation." Sir D. Brewster appends a note to prove that *love and affection* "had not, in Halifax's day, the same meaning which they have now." Does he really think that they mean nothing now except conjugal love and its imitations? Does not a man still love his friends, and might not Pope write to H. Cromwell now, as then, of his affection and esteem? If we come to *old meanings*, we might remember that *conversation* did not always mean *colloquy*.<sup>1</sup> If Miss Barton did live with Halifax under one roof, and if Halifax did buy the annuity, these words are to be interpreted accordingly. And they must be looked at jointly with the other things.

<sup>1</sup> [On the old meaning of the word "conversation," see De Morgan, *Newton: his Friend: and his Niece*, London, 1885, pp. 58-64.]

There is a fallacy which has no name in books of logic, but is of most frequent occurrence. It is that because neither A, nor B, nor C, will separately give moral conviction of D, that therefore they do not give it when taken together.

We have seen that Sir D. Brewster can omit, as in the case of the secret alterations in the reprint above mentioned: we shall now see that he can omit when he distinctly declares he has not omitted. We are far from charging him with any unfair intention: we know the effect of bias, and nothing disgusts us more than the readiness with which suppressions and misrepresentations are set down to deliberate intention of foul play. Sir D. Brewster informs us that he has given in an appendix "all the passages" in which Swift mentions Miss Barton or Halifax. He has *not* given all. When he wrote this (vol. ii, p. 278), he intended to give all; but when he came to the appendix, he altered his mind, omitted two, and forgot his previous announcement. It was not oversight, because Mr De Morgan had particularly mentioned these curious passages, in which Swift quotes to Stella some of Miss Barton's conversation, which has the freedom of a married woman (we mean of that day; our matrons are more particular). Either the Professor, who declines to repeat the stories, is overfastidious, or is unskilful in rendering the license of the seventeenth century into the decorums of the nineteenth: we

think we can convey an idea of the good joke over which Catherine Barton, aged 31, and Jonathan Swift, aged 43, enjoyed a hearty laugh. A man had died, leaving small legacies to those who should bear him to the grave, who were to be an equal number of males and females: provided always that each bearer, male or female, should take a declaration that he or she had always been a strict votary of Diana. The joke was, that there lay the poor man, unburied, and likely to remain so: and this was the joke which Miss Barton introduced, in a *tête-à-tête* with Swift; at least so says Swift himself. Mr De Morgan thinks that "Swift's tone with respect to the stories, combined with his obvious respect for Mrs Barton, may make any one lean to the supposition that he believed himself to be talking to a married woman." Certainly it can hardly be credited that the maiden niece of Newton (then living in Newton's house, according to Sir D. Brewster) would bring up such a joke for the entertainment of a bachelor friend: and Swift's great and obvious respect for Catherine Barton will justify us in thinking that he never would have invented such a story as coming from her.

We do not intend to decide the question whether the lady was the platonic friend, the mistress, or the secretly married wife, of Lord Halifax: in consequence of the reserve of biographers, it has never been fully put forward until our own day. Further

research may settle it : what we have to do with is our biographer's mode of dealing with his case. Sir D. Brewster certainly handles the phenomena of mind and conduct as if they were phenomena of matter : he requires that any conclusion shall be a theory, which is to explain how all the circumstances arose. No such thing is possible in grappling with circumstantial evidence as to the dealings of human beings with one another. Never a day passes without the prisoner's counsel triumphantly bringing to notice a circumstance which is perfectly inexplicable on the supposition of his client's guilt. So says the judge too, and so feel the jury : and both parties are in a difficulty. If it were a question about an explanatory theory, as of light, an obstinate dark band or coloured fringe might put the undulations out of the question, till further showing. But the court asks the jury, not for their theory, but for their verdict : that verdict is guilty, and the prisoner generally confirms it, at least in capital cases, and explains the difficulty. The matter we have been discussing has two counts : the first opens the question whether, under the circumstances, the conclusion that Miss Barton lived with Halifax can be avoided ; the second, on the supposition that it cannot be avoided, opens the question whether she lived with him as a mistress or as a secretly married wife. Sir D. Brewster works hard against the supposition of the marriage, and, by an *ignoratio*

*elenchi*, believes himself to be forwarding his own alternative ; but we strongly suspect that his reasons against the marriage, be their force what it may, will not avail against the other alternatives of our second count.<sup>1</sup>

## VIII

We will now take the vexed question of Newton's religious opinions, a vexed question no more, for the papers so long, and, in the first instance, so unworthily suppressed, are now before the world. Sir D. Brewster, in his former *Life*, followed his predecessors in stoutly maintaining *orthodoxy*, by which,

<sup>1</sup> [De Morgan made many further investigations on this subject. An article on Catherine Barton and Halifax was written by him in 1858 for *The Companion to the Almanac*. This article was rejected by Charles Knight, the editor, who thought that the question discussed in it would not be held generally interesting (see also Mrs De Morgan's *Memoir*, 1882, p. 264). The original manuscript was revised, and received some additions in the years 1864-6. And, later still, on the accession of new evidence, it was enlarged again. It was published posthumously, under the editorship of his widow and his pupil A. C. Ranyard, under the title *Newton: his Friend: and his Niece* (London, 1885). This book contains many digressions, most of which are interesting and some of which are amusing ; and De Morgan concluded that a private marriage between Halifax and Catherine Barton was contracted in 1706. The most important piece of evidence is a letter in Newton's handwriting, dated in May 1715, bought by De Morgan's friend Guglielmo Libri—who was accused and proceeded against by the French government, unjustly it seems, of having stolen books from public libraries in France—in 1856, which contains the sentence : "The concern I am in for the loss of my Lord Halifax, and the circumstances in which I stand related to his family, will not suffer me to go abroad till his funeral is over." See also Mrs De Morgan's *Memoir*, p. 288. Macaulay's view of the question was (*Newton: his Friend: and his Niece*, p. 70) that Catherine Barton was neither Halifax's mistress nor his wife, and that the relation between them was of the same sort as that between Congreve and Mrs Bracegirdle, as that between Swift and Stella, as that between Pope and Martha Blount, and as that between Cowper and Mrs Unwin. For De Morgan's view of Brewster's treatment of the Halifax case, see *ibid.*, pp. 107-130: the case is discussed in Brewster's *Memoirs*, 1855, vol. ii, pp. 270-281.]

in this article, we mean a belief of at least as much as the churches of England and Scotland hold in common. But many circumstances seemed to point the other way. There was a strong and universal impression that Horsley had recommended the concealment of some of the "Portsmouth Papers," as heterodox: and here and there was to be found, in every generation, a person who had been allowed to see them, and who called them dubious, at least. Newton was the friend of the heretics Locke and Clarke, and sent abroad, for publication, writings on the critical correction of texts on which Trinitarians relied, without a word against the conclusion which might be drawn respecting himself. Nay, he spoke of the Trinity in a manner which Sir D. Brewster admits would make any one *suspect* his orthodoxy. Whiston, always indiscreet, but always honest, declared from his own conversation with Newton, that Newton was an Arian; Haynes, Newton's subordinate at the Mint, declared to Baron, a Unitarian minister, that Newton was what we now call a Unitarian. He himself, in the *Principia*, allowed himself a definition of the word "God" which would have permitted him to maintain the Deity of the second and third persons of the Trinity. He said that every spiritual being having dominion is God: *Dominatio entis spiritualis Deum constituit*. And he enforces his definition by so many exemplifications that it is beyond question he means that,

if the Almighty were to grant some power, for only five minutes, to a disembodied spirit, that spirit would be, for that time, a God.

In the papers now produced for the first time, we have certain *paradoxical questions* (the word "paradox" then meant an unusual opinion) concerning Athanasius and his followers, in which many historical opinions of a suspicious character are maintained; but no matters of doctrine are touched upon. In "A Short Scheme of the True Religion," the purpose is rather to describe religion as opposed to irreligion, and all who are conversant with opinion know that a Trinitarian and a Unitarian use the same phrases against atheism and idolatry. Hence, some language which in controversy would be heterodox, may be counted orthodox. But in another manuscript, "On our Religion to God, to Christ, and the Church," there is an articulate account of Newton's creed, in formal and dogmatical terms. This we shall give entire: and it is to be remembered that Newton destroyed many papers before his death, which adds to those he left behind him additional meaning and force.

"Art. 1. There is one God the Father, ever living, omnipresent, omniscient, almighty, the maker of heaven and earth, and one Mediator between God and man, the man Christ Jesus.

"Art. 2. The Father is the invisible God whom no eye hath seen, nor can see. All other beings are sometimes visible.

"Art. 3. The Father hath life in himself, and hath given the Son to have life in himself.

"Art. 4. The Father is omniscient, and hath all knowledge originally in his own breast, and communicates knowledge of future things to Jesus Christ; and none in heaven or earth, or under the earth, is worthy to receive knowledge of future things immediately from the Father, but the Lamb. And, therefore, the testimony of Jesus is the spirit of prophecy, and Jesus is the Word or Prophet of God.

"Art. 5. The Father is immovable, no place being capable of becoming emptier or fuller of him than it is by the eternal necessity of nature. All other beings are movable from place to place.

"Art. 6. All the worship (whether of prayer, praise, or thanksgiving), which was due to the Father before the coming of Christ, is still due to him. Christ came not to diminish the worship of his Father.

"Art. 7. Prayers are most prevalent when directed to the Father in the name of the Son.

"Art. 8. We are to return thanks to the Father alone for creating us, and giving us food and raiment and other blessings of this life, and whatsoever we are to thank him for, or desire that he would do for us, we ask of him immediately in the name of Christ.

"Art. 9. We need not pray to Christ to intercede for us. If we pray the Father aright he will intercede.

"Art. 10. It is not necessary to salvation to direct our prayers to any other than the Father in the name of the Son.

"Art. 11. To give the name of God to angels or kings, is not against the First Commandment. To give the worship of the God of the Jews to angels

or kings is against it. The meaning of the commandment is, Thou shalt worship no other God but me.

"Art. 12. To us there is but one God, the Father, of whom are all things, and one Lord Jesus Christ, by whom are all things, and we by him. That is, we are to worship the Father alone as God Almighty, and Jesus alone as the Lord, the Messiah, the Great King, the Lamb of God who was slain, and hath redeemed us with his blood, and made us kings and priests."

In a paper called "Irenicum," or "Ecclesiastical Polity tending to Peace," are many remarks on church-government, but on doctrine only as follows. After insisting, in one place, that those who introduce any article of communion not imposed from the beginning are teaching another gospel, he gives, in another place, the fundamentals, by which he means the terms of communion imposed from the beginning.

"The fundamentals or first principles of religion are the articles of communion taught from the beginning of the Gospel in catechising men in order to baptism and admission into communion; namely, that the catechumen is to repent and forsake covetousness, ambition, and all inordinate desires of the things of this world, the flesh, and false gods called the devil, and to be baptized in the name of one God, the Father, Almighty, Maker of Heaven and Earth, and of one Lord Jesus Christ, the Son of

God, and of the Holy Ghost. See Heb. v. 12, 13, 14, and vi. 1, 2, 3."

In some queries on the word *ὁμοούσιος*, Newton asks, among many questions of a similar tendency, whether *unius substantiæ* ought not to be *consubstantialis*—whether *hypostasis* did not signify *substance*—whether Athanasius, etc., did not acknowledge three substances—whether the worship of the Holy Ghost was not "set on foot" after the Council of Sardica—whether Athanasius, etc., were not Papists. We prefer giving the reader Newton's opinions in full to arguing on them ourselves. It would be difficult, we think, to bring him so near to orthodoxy as Arianism. Though his exposition of his own opinions goes far beyond the simple terms of communion, there is not a direct word on the divinity of Christ, on his pre-existence, on the miraculous conception, on the resurrection, on the personality of the Holy Ghost, or on the authority of Scripture. Those who think that some of these points (as we think of the fourth and sixth) must be implied, will perhaps bring in the rest: but those who look at the emphatic *first article* of the twelve, unmodified and unqualified by the rest, though enforced by the eighth and ninth, will, we think, give up the point, and will class Newton, as Haynes did, with the Humanitarians, and not, as Whiston did, with the Arians. Sir D. Brewster leaves it to be implied that he does not any longer dispute the heterodoxy

of Newton's creed; that is, its departure from the creed most commonly believed by Christians. Of this we have no doubt, that in his theological opinions, Newton was as uncompromising and as honest as in his philosophical ones. And he was no dabbler in the subject, having in truth much reading, both as a scholar and a theologian.<sup>1</sup>

## IX

We cannot easily credit the story of Newton in love at sixty years of age. In Conduitt's handwriting is a letter entitled "Copy of a letter to Lady Norris by . . .," docketed, *in another hand*, "A letter from Sir I. N. to . . ." The letter is amusing. After informing the lady that her grief for her late husband is a proof she has no objection to live with a husband, he advises her, among other things, that a widow's dress is not acceptable in company, and that it will always remind her of her loss: and that "the proper remedy for all these mischiefs is a new husband"; the question being whether she "should go constantly in the melancholy dress of a widow, or flourish once more among the ladies." Sir D. Brewster seems rather staggered by this letter: but there is no authority for it coming from Newton, and surely we may rather

<sup>1</sup> [On Newton's religious opinions, see, besides § XI. of the first Essay, above, De Morgan, *Newton: his Friend: and his Niece*, London, 1885, p. 107.]

suspect that his friend, Lady Norris, sent him, or perhaps Miss Barton, a copy of a letter from some coxcomb of a suitor.<sup>1</sup> Newton was always a man of feeling, right or wrong, and, though perhaps he would have been awkward at the expression of it, he never would have addressed a woman for whom he experienced a revival of what he once felt for Miss Storey, in such terms as the young bucks in the *Spectator* address rich widows. The letter reminds us much more of Addison's play, and of the puppy who was drummed away from the widow by the ghost, than of Newton.

## X

To us it has always been matter of regret that Newton accepted office under the Crown. Sir D. Brewster thinks otherwise. "At the age of fifty, the high-priest of science found himself the inmate of a college, and, but for the generous patronage of a friend, he would have died within its walls." And where should a high-priest of science have lived and died? At the Mint? Very few sacrifices were made to science after Newton came to London. One year of his Cambridge life was worth more to his philosophical reputation and utility than all his long official career. If, after having piloted the

<sup>1</sup> The original letter, written shortly after 1702, is copied in the handwriting of Conduitt, who did not become a member of Newton's family till 1717. Say that Lady Norris sent it to Mrs Conduitt, to amuse her, and that Conduitt copied it.

country safely through the very difficult, and as some thought, impossible, operation on the coinage, 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 Clairaut would have had to begin at the point from which Laplace afterwards began. Newton was removed, the high-priest of science was translated to the temple of Mammon, at the time when the differential calculus was, in the hands of Leibniz and the Bernoullis, beginning to rise into higher stories. Had Newton remained at his post, coining nothing but ideas, the mathematical science might have gained a century of advance.

## XI

We now approach the end of our task, and, in spite of our battle with the biographer, we cannot express the pleasure with which we have read his work. It is very much superior, new information apart, to the smaller *Life* which he published long ago. Homer's heroes are very dry automatons so long as they are only godlike men: but when they get into a quarrel with one another, out come the points on which we like and dislike. Newton always right, and all who would say otherwise excathedrally reproved is a case for ostracism; we are tired of hear-

ing Aristides always called the just. But Newton of whom wrong may be admitted, Newton who must be defended like other men, and who cannot always be defended, is a man in whom to feel interest even when we are obliged to dissent from his eulogist. As we have said before, it is the defence which provokes the attack. Newton, with the weak points exposed and unprotected, is not and cannot be an object of assault: our blow is on the shield which the biographers attempt to hold before him. A great predecessor was guilty of delinquencies before which the worst error of Newton is virtue itself: he sold justice for bribes, so committing wilful perjury—for who may dare to deny that the oath of the false judge rose before his mind when he fingered the price of his conscience—that the perjury itself is forgotten in the enormity of the mode of committing it. But how often is this remembered when we think of Bacon? The bruised reed is not broken, because even biographers admit that it is a bruised reed: let them hold it up for a sturdy oak, and the plain truth shall be spoken whenever the name is mentioned. And so, in its degree, must it be with the author of the *Principia*.

All Newton's faults were those of a temperament which observers of the human mind know to be incapable of alteration, though strong self-control may suppress its effects. The jealous, the suspicious nature, is a part of the man's essence, when it exists

at all: it is no local sore, but a plague in the blood. Think of this morbid feeling as the constant attendant of the whole life, and then say, putting all Newton's known exhibitions of it at their very worst, how much they will amount to, as scattered through twenty years of controversy with his equals, and thirty years of kingly power over those who delighted to call themselves his inferiors. Newton's period of living fame is longer than that of Wellington: it is easy to talk of sixty years, but think of the time between 1795 and 1855, and we form a better image of the duration. In all this life, we know of some cases in which the worst nature conquered the better: in how many cases did victory, that victory which itself conceals the battle, declare for the right side? Scott claims this allowance even for Napoleon; how much more may it be asked for Newton? But it can only be asked by a biographer who has done for the opponents of his hero what he desires that his readers should do for the hero himself. When once the necessary admissions are made, so soon as it can be done on a basis which compromises no truth, and affords no example, we look on the errors of great men as straws preserved in the pure amber of their services to mankind. If we could but know the real history of a flaw in a diamond, we might be made aware that it was a necessary result of the combination of circumstances which determined that the product should be a

diamond, and not a bit of rotten wood. Let a flaw be a flaw, because it is a flaw: Newton is not the less Newton; and without the smallest rebellion against Locke's maxim—whatever it is—*nobis gratulamur tale tantumque exitisse humani generis decus.*

## APPENDIX I. TO THE THIRD ESSAY

(See note 1 on p. 148.)

DE MORGAN'S VIEW OF LEIBNIZ'S CHARACTER<sup>1</sup>

THE Leibniz of our day is either the mathematician or the metaphysician.

In the first of these two characters he is coupled in the mind of the reader with Newton, as the co-inventor of what was called by himself the Differential Calculus, and by Newton the Method of Fluxions. Much might be instanced which was done by him for the pure sciences in other respects; but this one service, from its magnitude as a

<sup>1</sup> [The following is from a biographical sketch entitled "Leibnitz" which appeared anonymously in the *Gallery of Portraits: with Memoirs* (vol. vi, 1836, pp. 132-136) which was published by Charles Knight at London under the superintendence of the Society for the Diffusion of Useful Knowledge. We know from Mrs De Morgan's *Memoir* (p. 108), that this article was by De Morgan. "The Life of Maskelyne," she says, "is one of a series of lives of Astronomers written by him for the *Gallery of Portraits*, published by C. Knight two or three years before this time (1839). They are those of Bradley, Delambre, Descartes, Dollond, Euler, Halley, Harrison, W. Herschel, Lagrange, Laplace, Leibnitz, and Maskelyne. They are bound up together, and illustrated in his own way, under the title of 'Mathematical Biography, extracted from the *Gallery of Portraits*, by Augustus De Morgan, H.O.M.O. P.A.U.C.A.R.U.M. L.I.T.E.R.A.R.U.M.' The letters of his literary tail were only B.A., F.R.A.S., besides those expressing membership of one or two lesser scientific societies. On account of the declaration of belief at that time required by the University, he never took his M.A. degree." On the reference to Halley, cf. note 2 on p. 21. The extract printed above is on pp. 134-136 of the *Gallery*. The portrait of Leibniz given in this article is an engraving after the well-known picture in the Florence Gallery, which is reproduced in the Open Court Company's series of portraits of philosophers.]

discovery, and its notoriety as the cause of a great controversy, has swallowed up all the rest.

Leibniz was in London in 1673, and from that time began to pay particular attention to mathematics. He was in correspondence with Newton, Oldenburg, and others, on questions connected with infinite series, and continued so more or less till 1684, when he published his first ideas on the Differential Calculus in the *Leipsic Acts*. But it is certain that Newton had been in possession of the same powers under a different name, from about 1665. The English philosopher drops various hints of his being in possession of a new method, but without explaining what it was, except in one letter of 1672, of which it was afterwards asserted that a copy had been forwarded to Leibniz in 1676. Leibniz published both on the Differential and Integral Calculus before the appearance of Newton's *Principia* in 1687; and indeed, before 1711, the era of the dispute, this new calculus had been so far extended by Leibniz and the Bernoullis, that it began to assume a shape something like that in which it exists at the present day. In the first edition of the *Principia*, Newton expressly avows that he had, ten years before (namely, about 1677), informed Leibniz that he had a method of drawing tangents, finding maxima and minima, etc.; and that Leibniz had, in reply, actually communicated his own method, and that he (Newton) found it only differed from his own in symbols. This passage was, not very fairly, suppressed in the third edition of the *Principia*, which appeared in 1726, after the dispute; and the space was filled up by an account of other matters. It was obvious that, on the supposition of plagiarism, it only gave Leibniz a year to infer, from a hint or two, his method, notation, and results.

Some discussion about priority of invention led Dr Keill to maintain Newton's title to be considered the sole inventor of the fluxional calculus. Leibniz had asserted that he had been in possession of the method eight years

before he communicated it to Newton. He appealed to the Royal Society, of which Newton was President, and that body gave judgment on the question in 1712. Their decision is now worth nothing; firstly, because it only determined that Newton was the first inventor, which was not the whole point, and left out the question whether Leibniz had or had not stolen from Newton; secondly, because the charge of plagiarism is insinuated in the assertion that a copy of Newton's letter, as above mentioned, had been sent to Leibniz. Now they neither prove that he had received this letter in time sufficient to enable him to communicate with Newton as above described, or, if he had received it, that there was in it a sufficient hint of the method of fluxions. The decision of posterity is, that Leibniz fairly invented his own method; and though English writers give no strong opinion as to the fairness with which the dispute was carried on, we imagine that there are few who would now defend the conduct of their predecessors. Whoever may have had priority of invention, it is clear that to Leibniz and the Bernoullis belongs the principal part of the superstructure, by aid of which their immediate successors were enabled to extend the theory of Newton; and thus Leibniz is placed in the highest rank of mathematical inventors.

The metaphysics of Leibniz have now become a by-word. He is pre-eminent, among modern philosophers, for his extraordinary fancies. His monads, his pre-established harmony, and his best of all possible worlds, are hardly caricatured in the well-known philosophical novel of Voltaire. If any thinking monad should find that the pre-established harmony between his soul and body would make the former desire to see more of Leibniz as a metaphysician, and the latter able to second him, we can inform him that it was necessary, for the best of all possible universes, that Michael Hansch should in 1728 publish the whole system at Frankfort and Leipsic, under the title,

*Leibnitii Principia philosophica more geometrico demonstrata*; and also that M. Tenneman should give an account of this system, and M. Victor Cousin translate the same. It is not easy to give any short description of the contents, nor would it be useful. A school of metaphysicians of the sect of Leibniz continued to exist for some time in Germany, but it has long been extinct.

The mathematical works of Leibniz were collected and published at Geneva in 1768. His correspondence with John Bernoulli was also published in 1745, at Lausanne and Geneva. It is an interesting record, and exhibits him in an amiable light. He gives his friend a check for his manner of speaking of Newton, at the time when the partisans of the latter were attacking his own character, both as a man and a discoverer. He says,<sup>1</sup> "I thank you for the animadversions which you have sent me on Newton's works; I wish you had time to examine the whole, which I know would not be unpleasant even to himself. But in so beautiful a structure, non ego paucis offender maculis." He also says that he has been informed by a friend in England, that hatred of the Hanoverian connexion had something to do with the bitterness with which he was assailed; "Non ab omni veri specie abest, eos qui parum Domui Hanoveranæ favent, etiam me lacerare voluisse; nam amicus Anglus ad me scribet, videri aliquibus non tam ut mathematicos et Societatis Regiæ Socios in socium, sed ut Toryos in Whigium quosdam egisse."<sup>2</sup>

<sup>1</sup> *Ibid.*, vol. ii, p. 234.

<sup>2</sup> *Ibid.*, p. 321.

## APPENDIX II. TO THE THIRD ESSAY

(See note I on p. 154.)

### NOTE BY DE MORGAN ON THE CHARACTER OF NEWTON AND ON THE ACTIONS OF THE ROYAL SOCIETY.<sup>1</sup>

RECENT knowledge has recoloured the mythical portrait of Newton's character. He was not a simple-minded man in the sense propounded: he was not like the old philosopher who knocked his foot against a stone while he was looking at the stars. Though not learned in human nature, he was very much the man of the world. He stuck to the main chance, and knew how to make a cast. He took good care of his money, and left a large fortune, though very—even magnificently—liberal on suitable occasions, especially to his family. He was observant of small things, as are all men of suspicious temperament; and he had a strong hatred of immorality, whether in word or deed, which no doubt would have turned his acuteness of observation, and his tendency to suspicion, upon anything from which inference could have been drawn. Those who imagine that Newton was always thinking of gravitation might just as well imagine that Wellington was always thinking of strategy. The following description applies to both. After this (the *Principia* or Waterloo, according to the person thought of), he lived about forty years, during which his attention to what had been his main pursuit was inter-

<sup>1</sup> [This Appendix is extracted from De Morgan's book, *Newton: his Friend: and his Niece* (London, 1885, the first paragraph on pp. 70-71, and the rest on pp. 130-136), which was, for the most part, written in 1858 (see note I on p. 171).]

mittent and casual, and rather directive of others than executive. He had a new career before him, in which again he was eminently successful; and in the last years of his life he was of all his contemporaries the most famous and the most respected.

It was in Britain the temper of the age, before Baily's *Life of Flamsteed* rudely broke in upon the illusion, to take for granted that Newton was human perfection. There is a class in this country which has a perennial<sup>1</sup> existence among all that is middle, from nobility down to handicraft; into both of which it throws its shoots. It is a respectable class: it can truly be described as so respectable, you can't think! It is a useful class; it is part of the ballast of our good ship; and though our middle ranks furnish a much larger percentage of that which is ballast and cargo, both, yet no ballast is useless. Who does not know the smug individual of this species, as he sees him picking his way through the world? His highest model is aristocracy; his social life is silver-forkery; his main pursuit is money-grubbery; and his whole religion is Sunday-prayer. This is the complete specimen, fit for the museum; but the characteristics are variously interfused through an immense mass, often lost in other and better features, except to a close observer. This class is, in every case in which its members knew the name of Newton, the one in which you were safe to be reckoned as in the broad way if you imputed anything wrong to the man who bore that name at the Mint—a position which was mysteriously connected with wonderful discoveries in the heavens.

“And, so you think that Newton told a lie;  
Where do you hope to go to when you die?”

By help of this class, without which the man of science could not have put Newton on the pedestal which had been made for him, it was practicable to allow what had the

<sup>1</sup> [“Percental” is misprinted in the original.]

clearest appearance of a direct and deliberate falsehood on Newton's part to stand unexamined for more than a century. Newton, in his final conflict with Leibniz, declared that the decision at the Royal Society against Leibniz had been voted by a “numerous committee of gentlemen of different nations.” The world was never told of more than six, all British subjects of English mother-tongue; no list of the committee was published with the decision. Here was, to all appearance, if not a falsehood, worse—the evasion of calling the English, Scotch, etc., different nations in reference to a dispute between Britain and the Continent. If the faith in Newton had been anything but a formula, some would have reasoned thus:—“Newton could not be false: he says the committee had members of different nations; let us look at the minute-books of the Royal Society, and find them out.” But this was not thought necessary. I had long been puzzled with this statement of Newton's; though I knew him to be capable of being betrayed by the necessities of his case into that culpable evasion in which self-love finds excuse, I did not believe that his principles would allow him directly and wilfully to falsify a fact; or that his acuteness would allow him to do it on so small a matter and to so little purpose. It chanced to me, in 1845, to look at a *Life of De Moivre* of the rarest character, by his friend Dr Matthew Maty, Sec., R.S. I never saw more than one separate copy; but I long afterwards found it in the *Journal Britannique* for 1755—a French journal, published in England by the “little black dog,” as Sam Johnson called him—Maty himself. Here I found eleven members named, two of them aliens—De Moivre himself and Bonet the Prussian minister. And though they were the only two foreigners, yet De Moivre was a host: the only one among the rest who was fit to stand up against him for one moment on a mathematical question was Halley. On application to the Royal Society, the facts were verified immediately: the six who have passed for the

whole were those first appointed; the remaining five were added piecemeal in the five weeks following the first nomination.

I drew up a few words on this discovery, and sent them to the Royal Society. I thought they would be a *charta volans* for the *Proceedings*, etc. To my very great surprise they were printed in all the dignity of the *Philosophical Transactions*, in which no historical paper has ever appeared, that I know of—certainly none within the century. But the matter concerned the character of Newton. The little bit of two and three-quarter pages, with the facts about the Committee and some anecdote—as how, for instance, Newton said nothing but his age prevented him from having “another pull at the moon”—looks curious among the elaborate mathematical and physical papers. This is so far a mere anecdote: it takes meaning in connexion with what follows.

About a year after the preceding paper was sent, some of those accidents, by which those who are prepared can snap surmises, as well as facts, led me to a surmise that perhaps the reprint (1722) of the *Commercium Epistolicum* (1712)—as the work containing the reasons and decisions of the above-named Committee is called—had not been quite fairly made. I say reprint, not second edition; for the very title-page was reprinted with the old date, after the avowedly new matter and a new title over all, which amounted to the most positive declaration that not a comma was intentionally changed. I had no copy of the first edition, so I applied to the Council of the Royal Society for the loan of their copy, stating why I wanted it. The request was instantly granted; and I found, on examination, that some alterations had been made, of which some were decidedly unfair in matter, all being of course unjustifiable under the old date and without notice. The worst among them was, that whereas the old Committee did not say precisely, in the evidence, when the letter on which the most depended was forwarded to Leibniz,

a date for this transmission was foisted into the reprint. It ought to be said that the notions of the literary world,<sup>1</sup> in that day, about the sanctity of documents were by no means so rigid as they are now; so that what, done by one of us, would be sheer rascality, may be let off with a much softer name. I drew up an account of the alterations, and sent it to the Royal Society; to have sent it elsewhere would have been to say, in effect, that though I knew the Society would go out of the way to clear the fame of Newton, I could not trust them to clear their own wrong to Leibniz. That they had some hand in it was clear from the reprint having cuts from the old wood blocks which were the property of the Society. The Society proved itself worthy of the reflection which I could not venture to cast; it declined to print the second paper. I gathered that the council thought it would be necessary to submit my paper and the documents to a special committee of examination. The documents were two printed books, and the question was whether certain passages in one book were accurate reprints of certain passages in the other; and if not, how they differed. I have no doubt the real reason was, that in the paper was seen danger of danger to Newton's character. I afterwards saw a published reason, of which I was not cognisant at the time, for thinking that Newton himself was the editor of this reprint, and the writer of the preface which preceded the old title. Sir D. Brewster, from the “Portsmouth Papers,” found that I was quite right. When I made this last discovery, it crossed my mind for one moment that the fact was known in the Council of the Royal Society, and the refusal to investigate the question was in part the consequence of disinclination to bring it out. But this notion took no root; I soon felt satisfied that, whatever unconscious bias might do, there was no reason to fear a definite intention to suppress a definite fact. And further, so small and so inexact is the know-

<sup>1</sup> [“Word” is misprinted in the original.]

ledge of the history of science among scientific men, that I could easily imagine not one single person on the council knew so much as that there had been a reprint, much less that Newton's active share in the reprint had been matter of discussion, of affirmation, and denial.

I applied for permission to withdraw the paper, hoping thus to nullify the proceedings in form at least. But the laws of the Society prevent the withdrawal of any communication which has undergone adjudication; hence this little matter must have its little place in the history of the Society, and its somewhat larger place in mine. A copy would have been allowed me if I had requested it; but I preferred to write another paper, and to request its insertion in the *Philosophical Magazine* (June, 1848).

One testimony to the significance of the *variantes* is that of Sir D. Brewster, who holds it wise to omit all mention of them. After my paper, which I took care he should have, and with full knowledge of the new work being *reprinted under the old date*, he calls it "a new edition with notes, a general review of it and a preface of some length."<sup>1</sup> He did not even give the true date (1722), but sticks by that of the second title-page (1725). This is of some consequence; for three years, at Newton's age, then made a difference in the palliation which years and infirmity may be made to give. But it must be remembered that persons unused to bibliography are often not even aware of the distinction between a *reprint* and a *new edition*.

I freely and unreservedly blame the Council of the Royal Society—collectively, of course—for not printing the account of the variations mentioned above; they missed a golden opportunity. They might have shown that the beautiful edition of the *Commercium Epistolicum*, published in 1856, by Biot and Lefort, at the expense of the French Government, "avec l'indication des variantes de l'édition de 1722," would have recorded that these *variantes* were first made

<sup>1</sup> [Brewster, *Memoirs*, 1855, vol. ii, p. 75.]

known by the Royal Society itself, the body which was most concerned in the publication of them. Considered as an act of reparation, the opportunity is lost, and the revelations of the "Portsmouth Papers" and of those of Leibniz have left little chance of another. The Royal Society, in this matter, reminds one much of those old managers of the impeachment, who, when Warren Hastings, many and many a year after his acquittal, appeared before a House of Commons, the members of which rose and uncovered at his retirement, remained sitting with their hats on, to show their sullen consistency. As a question of curiosity, I asked myself whether Leibniz ever found as stubborn an adherent in spite of all that could be learnt? I could not remember such a thing in real life, but the optimist of Voltaire's fiction hits the case exactly: "Eh bien! mon cher Pangloss,' lui dit Candide, 'quand vous avez été pendu, disséqué, roué de coups, et que vous avez ramé aux galères, avez-vous toujours pensé que tout allait le mieux du monde?' 'Je suis toujours de mon premier sentiment,' répondit Pangloss; 'car enfin je suis philosophe; il ne me convient pas de me dédire. Leibnitz ne pouvant pas avoir tort. . . .'"

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