MEMOIRS

OF THE

LIFE AND WRITINGS OF SIR ISAAC NEWTON.

CHAPTER XV.

Nicolas Facio de Duillier attacks Leibnitz—Leibnitz appeals to Newton—He reviews Newton's "Quadrature of Curves," and accuses him of Plagiarism—Newton's opinion of the Review—Dr. Keill defends Newton as the true Inventor of Fluxions, and apparently retorts the charge of Plagiarism on Leibnitz, who complains to the Royal Society—Keill explains his Defence—The Royal Society approves of his Explanation—Leibnitz calls Keill an Upstart, and begs the Royal Society to silence him—The Society appoints a Committee to inquire into the Claims of Leibnitz and Newton—The Committee report to the Society, who publish the result in the "Commercium Epistolicum"—Instigated by Leibnitz, John Bernoulli attacks the Report, and asserts, in a private letter to Leibnitz, that he was the first Inventor of the new Calculus—Leibnitz circulates this Letter in a Charta Volans, and gives up Bernoulli as the Author of it—Keill replies to this Letter, and attacks Bernoulli as its Author, who solemnly denies it to Newton—Leibnitz attacks Newton in a Letter to the Abbé Conti—Newton replies to it—The Controversy excites great interest—Leibnitz urges Bernoulli to make a Public Declaration in his favour—Bernoulli sends to Leibnitz the celebrated Letter "Pro Eminente Mathematico," on condition of his Name being kept secret—Leibnitz and Wolf alter this Letter improperly, and publish it in such a form, that Bernoulli is proved to be its Author—Bernoulli is annoyed by the discovery, and endeavours, by improper means, to evade the truth—The Abbé Varignon reconciles Newton and Bernoulli—Death of Leibnitz—Newton writes a History of the Calculus—General view of the Controversy, and of the conduct of the parties.

Nicolas Facio de Duillier, a Genevese by birth, came to England in the spring of 1687, and, with the exception of a visit to Switzerland in 1699, 1700, and 1701, remained there during the rest of his life. He had become acquainted with the celebrated Huygens at the Hague in 1686, and had attained

to such a proficiency in mathematics, that he was introduced to Sir Isaac Newton, and visited him at Cambridge in the month of November 1692. Though only in the 28th year of his age, his health was precarious, and he seems to have consulted Newton on the subject of his spiritual as well as of his bodily condition. On his return from Cambridge, he caught a severe cold, which affected his lungs, and gave him great alarm. In communicating to Sir Isaac an account of his symptoms, he says, "I thank God that my soul is extremely quiet, in which you have had the chief hand;" and fearing that his illness would prove fatal, he expresses the "wish that his eldest brother, a man of an extraordinary integrity, should succeed him in his friendship." Sir Isaac answered this letter in course of post, making inquiries about his brother, and telling Facio that he remembered him in his prayers. In his reply, Facio gave him his most humble thanks, both for his prayers and his kindness,—requested him thus to remember him as long as he lived, and assured him that he always remembered him in a similar manner.[1]

[1] Nicolas Facio de Duillier, an eminent mathematician, was born at Basle on the 16th February 1664. In 1684 and 1685 he became acquainted with Count Fenil, a Piedmontese, who, having incurred the displeasure of the Duke of Savoy, took refuge in France, where he became captain of a troop of horse. Having quarrelled one day with the commanding officer of his regiment, when drawn up on parade, the Count shot him dead, and, being well mounted, escaped from his pursuers. He fled to Alsace, where he took refuge in the house of Mr. Facio's maternal grandfather; but, in order to assist him more effectually, he was sent to the house of Facio's father, who lived at Duillier. When walking alone with young Facio, the Count told him that he had offered to M. De Louvois to seize the Prince of Orange, and deliver him into the hands of the King; and he showed him the letter of M. Louvois, offering him the King's pardon, approving of the plan, and enclosing an order for money. The Prince of Orange was in the habit of taking a drive on the sands at Scheveling, a village three miles from the Hague, and the Count proposed, with the aid of ten or twelve men, to land in a light ship with Dutch colours, and carry off the Prince to Dunkirk. The scheme was ripe for execution in 1686: but Facio, aware of the Count's design to take the life of his son, felt it his duty to thwart him in the commission of the two crimes which he had in view. He had become acquainted with Dr. Burnet at Geneva, and knowing that he was going to Holland to visit the Prince of Orange, he acquainted the doctor with the Count's scheme, and agreed to accompany him to Holland with the view of explaining it to the Prince. The scheme was accordingly communicated to the Prince and Princess, and, though

Having been elected a Fellow of the Royal Society in 1687, he took an active part in its proceedings, and communicated papers to its *Transactions*. In the year 1699 he published a tract entitled a "Geometrical Investigation of the Solids of least Resistance," in which he made the following reference to the history of the new calculus.[1]

"The celebrated Leibnitz may perhaps inquire how I became acquainted with the calculus which I use. About the month

seconded by the latter, Monsieur Fagel and others had great difficulty in inducing the Prince to have the protection of a guard when he went abroad. In return for the services of Facio, it was resolved, on the strength of testimonials from Huygens, to create for him a professorship of mathematics for instructing the nobility and gentry of Holland, with a salary of 1200 florins, and a pension from the Prince.

Some delay having taken place in completing this arrangement, Facio got leave to pay a visit to England, where he arrived in 1687; but having been taken ill at Oxford, elected a Fellow of the Royal Society in 1687, and treated with much kindness by the English mathematicians, he remained till the accession of William III. When he visited Switzerland in 1699, 1700, and 1701, he learned that Count Fenil had received from the French Court a situation at Pignerol, a fortified city not far from Turin; and that in consequence of having conspired to surrender the place to the Duke of Savoy, he was condemned to be beheaded. In 1732, Facio endeavoured, but we believe unsuccessfully, to obtain, through the influence of Mr. Conduitt, some reward for having saved the life of the Prince of Orange. He assisted Conduitt in making out the design, and writing the inscription, for Newton's Monument in Westminster Abbey.

In 1704, when Facio taught mathematics in Spitalfields, he unfortunately became secretary to the Camisards, or fanatical prophets from the Cevennes, who pretended to raise the dead, and perform other miracles. Lord Shaftesbury attacked them in his *Letter on Enthusiasm;* and having been unjustly suspected of some political scheme, Facio and other two prophets were seized by the police in 1707, and condemned to the pillory. On the 2d of December 1707, Facio stood on the pillory at Charing Cross with the following inscription on his hat: "Nicolas Facio convicted for abetting Elias Moner in his wicked and counterfeit prophecies, and causing them to be printed and published to terrify the Queen's people." It is stated by Spence (*Observations, Anecdotes*, &c., 1820, p. 159), on the authority of Lockier, Dean of Peterborough, "that Sir Isaac Newton had a strong inclination to go and hear the French prophets, and was restrained from it with difficulty by some of his friends, who feared he might be infected by them as Facio had been." Facio spent the rest of his life at Worcester, where he died in 1753, nearly ninety years of age.—See *Phil. Trans.* 1713, and *Gentleman's Magazine*, 1737, 1738.

[1] Dr. Guhrauer, in his biography of Leibnitz, published in 1842, has most unjustly stated that Newton prompted this attack of Facio. We have carefully inspected all the manuscripts of Newton, and cannot discover the slightest evidence in support of a charge which deserves the severest reprobation.

of April, and the following months in the year 1687, and subsequent years, when nobody, as I thought, used such a calculus but myself, I invented its fundamental principles, and several of its rules. Nor would it have been less known to me if Leibnitz had never been born. He may, therefore, boast of other disciples, but certainly not of me. And this would be sufficiently evident if the letters which passed between me and the illustrious Huygens were given to the public.[1] Compelled by the evidence of facts, I hold Newton to have been *the first inventor* of the calculus, and the earliest by several years: And whether Leibnitz, *its second inventor*, has borrowed anything from him, I would prefer to my own judgment that of those who have seen the letters of Newton and copies of his other maunscripts. Nor will the silence of the more modest Newton, or the active exertions of Leibnitz in everywhere ascribing the invention of this calculus to himself, impose upon any person who shall examine these documents as I have done."[2]

Strong as these expressions are, they cannot be regarded as charging Leibnitz with plagiarism. He is styled *the second inventor*, the title with which he, on many occasions, expressed himself satisfied, and he is blamed only for everywhere ascrib-

[1] These letters do not appear in the Correspondence of Huygens with Leibnitz and the other distinguished men of the seventeenth century, lately published by Professor Uylenbrock. There are no letters dated between 1680 and 1690; but it appears from a letter to Leibnitz from Huygens, dated 18th November 1690, that he was acquainted with the calculus of Facio above referred to, and that it had been the subject of correspondence between these two celebrated mathematicians. Huygens tells Leibnitz that he had some share in the rule of Facio, and that it was Facio who first pointed out the mistake of Tschirnhaus. He adds that his method was a very beautiful one; and Uylenbroek, in a note on the subject, pointing at what Huygens had done in the matter, speaks of it as a fine invention. In a subsequent letter, dated 26th April 1690, Leibnitz pays a high compliment to Facio. "As Facio has much penetration," he says, "I expect from him fine things when he comes to details; and having profited by your instruction and that of Newton, he will not fail to produce works which will gain him distinction. I wish I were as fortunate as he is in being able to consult two such oracles." See *Christiani Huygenii, aliorumque seculi* xvii. *virorum celebrium. Exercit. Math. et Philos.* Fascic. i. p. 41, and Fascic. ii. pp. 56, 175. Hagæ Comitum, 1833.

[2] *Investigatio Geometrica*, &c., p. 18. Lond. 1699.

ing the invention to himself. In replying to Facio,[1] Leibnitz appealed to Newton himself as having stated, in the celebrated scholium, that the new calculus was common to them both, and that neither had received any light from the other;[2] and, without disputing or acknowledging the priority of Newton's claim, he asserted his own right to the discovery of the differential calculus. Facio sent a reply to the editors of the *Acta Eruditorum*, but they refused to print it on the ground of their aversion to controversy.[3] The controversy therefore terminated for the present, and the contending parties laid down their arms, ready to resume them on the slightest provocation.

When Newton published his Treatise on the Quadrature of Curves, along with his Optics, in 1704, he mentioned in his preface that he had gradually found the method of fluxions in the year 1665 and 1666. A review of this work, by Leibnitz,[4] but without his name, was published in the *Acta Eruditorum* for January 1705. After giving an imperfect analysis of its contents, he compared the method of fluxions with the differential calculus, and, in a sentence of some ambiguity, he states that Newton employed fluxions in place of the differences of Leibnitz, and made use of them in his Principia in the same manner as Honoratus Fabri, in his Synopsis of Geometry, had substituted progressive motion in place of the indivisibles of Cavalieri. As Fabri, therefore, was not the inventor of the method which is here referred to, but borrowed it from Cavalieri, and only changed the mode of its expression, there can be no doubt that the artful insinuation contained in the above passage was intended to convey the impression that Newton

[1] *Acta Eruditorum*, 1700, p. 203.

[2] We have already proved that Newton did not attach this meaning to his scholium; and, in replying to this passage in the *Recensio Commercii Epistolici*, he himself distinctly denies having "acknowledged that Leibnitz invented his method by his own genius, unassisted by the letters of Newton."—Newtoni *Opera*, tom. iv. p. 489.

[3] *Acta Eruditorum*, 1701, p. 134.

[4] Guhrauer, the biographer of Leibnitz, proves that he was the author of the review, and affirms that Leibnitz constantly denied any knowledge of the authorship.—See *Essays from the Edinburgh Review*, by Henry Rogers, pp. 226, 227.

had *stolen* his method of fluxions from Leibnitz. That this was the view of it taken by the friends of Newton will presently appear. That it was the view taken by Newton himself we are fortunately able to prove from the following passage in his own handwriting,[1] which is so important that we copy it without any other change than the use of Leibnitz's own words.

"In the *Acta Eruditorum* for 1705,[2] an account of the Introduction to the Book of Quadratures was published in these words:—'Quæ [Isagoge or Preface] ut MELIUS intelligatur, sciendum est, cum magnitudo aliqua continue crescit, veluti linea, (exempli gratia) crescit fluxu puncti, quod eam describit, incrementa illa momentanea [producta] appellavi DIFFERENTIAS, nempe inter magnitudinem quæ antea erat et quæ per mutationem momentaneam est producta; atque hinc natum esse calculum Differentialem, eique reciprocum summatorium,[3] cujus elementa ab INVENTORE Dn. Godefrido Guilelmo Leibnitio in his actis sunt tradita, variique usus tum ab ipso, tum a Dnn. Fratribus Bernoulliis, tum et Dn. Marchione Hospitalio sunt ostensi. Pro Differentiis IGITUR Leibnitianis Dn. Newtonus adhibet, semperque [pro iisdem] adhibuit fluxiones, iisque tum in suis Principiis Naturæ Mathematicis, tum in aliis postera editis [pro Differentiis Leibnitianis] eleganter est usus, QUEMADMODUM ut Honoratus Fabrius in sua synopsi Geometrica motuum progressus Cavallerianæ methodo SUBSTITUIT.' And all this is as much as to say that I did not invent the method of fluxions in the years 1665 and 1666, as I affirmed in this Introduction, but that after Mr. Leibnitz, in his letter of 21st June 1677, had sent me his differential method, instead of that method, I began to use, and have ever since used, the method of fluxions."[4]

[1] *A Supplement to the Remarks*, p. 6. [2] January, p. 34.

[3] This was the name given by Leibnitz to the integral calculus, or the inverse method of fluxions.

[4] The words within brackets are added by Newton, and bring out very distinctly the meaning of Leibnitz. In his letter to the Abbé Conti, dated 9th April 1716, Leibnitz virtually admits the authorship of the review, endeavours to give a different meaning

That Newton was virtually accused of plagiarism by the reviewer, cannot, we think, admit of a doubt. The indirect and ambiguous manner in which the charge is couched, and the artful reference to the case of Fabri and Cavalieri, make it doubly reprehensible; and we are persuaded that no candid reader can peruse the passage without a strong conviction that it justifies, in the fullest manner, the indignant feelings which it excited among the English philosophers. If Leibnitz, in place of being the author of the review, had been merely a party to it, he merited the full measure of rebuke which was dealt out to him by the friends of Newton, and deserved those severe reprisals which doubtless embittered the rest of his days. He who dares to accuse a man like Newton, or indeed any man holding a fair character in society, of the odious crime of plagiarism, places himself without the pale of the ordinary courtesies of life, and deserves to have the same charge thrown back upon himself. The man who conceives his fellow to be capable of such intellectual felony, avows the possibility of himself committing it, and almost substantiates the weakest evidence of the worst accusers.

Dr. Keill, as the representative of Newton's friends, could not brook this concealed attack upon his countryman. In a letter on the Laws of Centripetal Forces, addressed to Halley, and printed in the Philosophical Transactions for 1708,[1] he stated that Newton was "beyond all doubt" the first inventor of fluxions; and he asserted "that the same calculus was afterwards published by Leibnitz, the name and the mode of notation being changed." If the reader is disposed to consider this passage as retorting the charge of plagiarism upon Leibnitz, he will readily admit that the mode of its expression is neither so coarse nor so insidious as that which is used by

to the words *semperque adhibuit*, and maintained that Newton allowed himself to be deceived by a man who poisoned his words, and sought a quarrel by the malignant interpretation of them. Newton was himself the interpreter. See Raphson's *History of Fluxions*, p. 103.

[1] For September and October, p. 185.

the writer in the *Acta Eruditorum*. In a letter to Hans Sloane, dated 4th March 1711, Leibnitz complained to the Royal Society of the treatment he had received. "Nobody, says he, "knew better than Newton that this charge is false, for certainly I never heard of the name of the *Calculus of Fluxions*, nor saw with these eyes the characters which Newton used." He expressed his conviction that Keill had erred more from rashness of judgment than from any improper motive. He did not regard the accusation as a calumny; and he requested that the Society would desire Mr. Keill to disown publicly the injurious sense which his words might bear. When this letter was read to the Society, Keill justified himself to Sir Isaac Newton and the other members, by showing them the obnoxious article on the Quadrature of Curves in the *Acta Eruditorum*, and they all agreed in attaching the same injurious meaning to the passage in the review. The discussion excited so much interest, that, on the 5th April 1711, Newton gave, from the chair of the Society, "a short account of his invention, with the particular time of his first mentioning or discovering it;[1] upon which Mr. Keill was desired to draw up an account of the matter in dispute, and set it in a just light."[2] This account, contained in a letter to Sir Hans Sloane, was read at the Society on the 24th May 1711, and a copy of it was ordered to be sent to Leibnitz. In this letter, which is

[1] This account was probably given to the Society in consequence of the following unpublished letter from Keill to Newton, written two days before the meeting, that is on the 3d April 1711:—"I have now sent you the *Acta Lipsiæ* (1705), where there is an account given of your book (on Quadratures), and I desire you will read from page 34, &c. (namely, the passage which we have given from Newton's MS. in pages 3, 4). I hold not the volume (1710, p. 78) in which Wolfius has answered my letter, but I have sent you his letter transcribed from thence, and also a copy of my letter to him. I wish you would take the pains to read that part of their supplements, wherein they give an account of Dr. Friend's book, and from them you may gather how unfairly they deal with you; but really these things are trifles, not worth your while, since you can spend your time to much better purpose than minding anything such men can say. However, if you would look upon them so far as to let me hold your sentiments on that matter, you will much oblige, your most humble servant, Jo. KEILL."

[2] Weld's *History of the Royal Society*, vol. i. p. 410.

one of considerable length, Dr. Keill declares that he never meant to state that Leibnitz knew either the name of Newton's method or the form of notation, and that the real meaning of the passage was, "that Newton was the first inventor of fluxions, or of the differential calculus, and that he had given, in two letters to Oldenburg, and transmitted to Leibnitz, indications of it sufficiently intelligible to an acute mind,[1] from which Leibnitz derived, or was able to derive, the principles of his calculus."

The charge of plagiarism which Leibnitz thought was implied in the former letter of his antagonist, is here greatly modified, if not altogether denied. Keill expresses an *opinion* that the letters *seen* by Leibnitz contained intelligible indications of the fluxionary calculus, from which he either derived, or might derive, the principles of his calculus. Even if this opinion were correct, it is no proof that Leibnitz either saw these indications, or availed himself of them; or if he did perceive them, it might have been in consequence of his having previously been in possession of the differential calculus, or having enjoyed some distant view of it. Leibnitz should, therefore, have allowed the dispute to terminate here; for no ingenuity on his part, and no additional facts, could affect an opinion which any other person as well as Keill was entitled to maintain.[2]

[1] "Indicia perspicacissimi ingenii viro satis obvia, unde Leibnitius principia illius calculi hausit aut haurire potuit."

[2] These sentiments, which we had formerly expressed, and which we again repeat, have been singularly misrepresented by Dr. Guhrauer in his *Life of Leibnitz*. A distinguished writer (Mr. Henry Rogers), in giving an account of this work, has defended us better than we could have done ourselves. "Dr. Guhrauer," he remarks, "is not a little indignant with Sir David Brewster for the supposed injustice which, in his *Life of Newton*, he has done to Leibnitz, and to which he frequently refers with much bitterness. Never was a complaint more unreasonable. Our distinguished countryman does not question Leibnitz's claim to be regarded as a true inventor of the calculus; he merely asserts the undoubted *priority* of Newton's discovery. He expressly affirms that there is no reason to believe Leibnitz a plagiarist; but that if there were any necessity for believing either to be so, it must be Leibnitz, and not Newton, who is open to the charge. Guhrauer angrily replies, not simply by saying (which is true) that there

Leibnitz, however, took a different view of the subject, and wrote a letter to Sir Hans Sloane, dated December 29, 1711, which excited new feelings, and involved him in new embarrassments. Insensible to the mitigation which had been kindly impressed upon the supposed charge against his honour, he alleges that Keill had attacked his candour and sincerity more openly than before; that he acted without any authority from Sir Isaac Newton, who was the party interested; and that it was in vain to justify his proceedings by referring to the provocation in the *Acta Eruditorum*, because, in that journal, *no injustice had been done to any party, but every one had received what was his due.* He asserts that he discovered the calculus *some* years before he published it, that is in 1675, or earlier. He brands Keill with the odious appellation of an upstart, and one little acquainted with the circumstances of the case;[1] and he calls upon the Society to silence his vain and unjust clamours,[2] which, he believed, were disapproved by Newton himself, who was well acquainted with the facts, and who, he was persuaded, would willingly give his opinion on the matter.

is no sufficient evidence of Leibnitz's having stolen Newton's invention, but by denying the essential identity of the two methods, and by affirming that they are so different as to be considered 'unlike things,' than which nothing can, in our judgment, be more uncandid.

"There is only one statement which, as respects Leibnitz, Dr. Gubrauer could fairly find fault with in Sir David Brewster's work; and that is, that Keill had a 'right to express his opinion' that the letters of Newton of 1676 gave indications from which Leibnitz 'derived, or might derive,' the principles of his calculus. For reasons already assigned, we do not think that any man *had a right to say this*, nor that any one could say it without being *of a different opinion from Newton himself*, who undoubtedly must have thought that he had not disclosed what he designed to conceal. With no other statement of Sir David Brewster, as regards Leibnitz, are we disposed to find fault."—*Essays from the Edinburgh Review*, by Henry Rogers, vol. i. pp. 227, 228. *Edin. Review*, vol. lxxxiv. pp. 43, 44. Mr. Rogers has certainly misapprehended the meaning of our statement, which amounts to nothing more than that Dr. Keill, or any other man, had a right to express his opinion on any subject whatever, whether they are sound or unsound. We have already proved that the opinion of Keill was the opinion of Newton himself, and, as he knew this, he had a right of a higher kind to express the same opinion.

[1] Homo doctus, sed novus, et parum peritus rerum anteactarum cognitor.

[2] Vanæ et injustæ vociferationes.