

Remarks on Leibniz's first letter to the Abbe Conti

Author: Isaac Newton

Source: MS Add. 3968, ff. 412r-419r, Cambridge University Library, Cambridge, UK

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Pag. 1. lin. 5. Almost two years after the Principia Philosophiæ came abroad, M^r Leibnitz published the principal Propositions thereof as his own & to make himself the first inventor of this science gave it the name of Dynamic, & now instead of making restitution complains of the English as if they would pass for the sole inventors.

Ib. lin. 7. M^r Leibnitz changed the differential characters of D^r Barrow a, e, i, o, u into the characters dz, dy, dx, dv, dt: M^r Newton puts any Letters for fluxions & the rectangles under those letters & the letter o for moments or differences: but thinks it ridiculous to place the invention of the method in the invention of words & symbols. See Transact. N. 342. p. 204, 205.

Ib. l. 8. D^r Keill hath proved that M^r Bernoulli was in an error himself The candor of these Gentlemen is taxed & M^r Newton charged with plagiarism by M^r L. & the evidence against them is the testimony of M^r Leibnitz. for himself grounded upon his candour. And in all Courts of Iustice the Persons accused are allowed to enquire into the credit of the Witnesses. And the English think the following objections against his candor very material viz^t 1 that he endeavoured to make himself a witness in his own cause; that he declines to make good his charge against D^r Keill; 2 that so soon as he had found the Differential method he pretended to have found it jam tum a multo tempore a long time before; 3 that when he published the Differential method, he did not name M^r Newton; 4 that he then made no further mention of M^r Newton's methodus similis then was necessary to save his own reputation; 5 that he has not to this day acknowledged the light he received from D^r Barrows method of Tangents; 6 that he changed D^r Barrows differential notation into his own differential notation without ever acknowledging it; 7 that he did not dispute with D^r Wallis the antiquity of the method of fluxions asserted by the Doctor nor began to dispute it before the Doctor's death; 8 that in his dispute with M^r Fatio he pretended that in the year 1684 when he first published his differential method he knew nothing more of M^r Newton's inventions in that kind then that he could draw Tangents without taking away irrationals; 9 that after he had granted to M^r Fatio that Newton invented the method of fluxion by himself without receiving any light from the differential method, he affirmed in the Acta Eruditorum that M^r Newton had from the beginning used fluxions instead of Differences as Honoratus Faber had substituted motions instead of the method of Cavallerius; ¹⁰ that after he had twice received from M^r Old. a series of M^r Gregory for squaring the circle & knew that M^r Gregory had found it in the year 1671 he published it as his own without naming M^r Gregory; ¹¹ that in his Letter of 26 Octob. 1674 he pretended to have <412v> invented a series for finding any Arc of a circle whose sine is given, but afterwards when he received this series from M^r Oldenburg & again from M^r Collins by M^r Mohr, he did not know it to be his own, but in his Letter of 15 May 1676 desired M^r Oldenburg to procure from M^r Collins the method of finding it. ¹² That when M^r Newton at the request of M^r Oldenburg & M^r Collins sent to him his Method illustrated with examples of various series M^r Leibnitz pretended that he had invented four of those series before he received

them from M^r Newton, 13 that he did not yet understand the method of finding all those series but desired M^r Newton to explain it further; 14 that so soon as he understood it, he wrote back that he had found it before as he understood by his old papers, but had forgot it till he received it from M^r Newton; 15 that he changed D^r Barrow's differential notation into a new differential Notation, that he might not seem obliged to the Doctor for his method of Tangents; 16 that when M^r Newton's book of Principles came abroad he selected the principall things out of it & published them in another form above a year after in three papers, as his own: 17 that he changed the name of vis centripeta used by Newton into that of sollicitatio paracentrica, not because it is a fitter name, but to avoid being thought to build upon M^r Newtons foundation; 18 that he has set his mark upon this whole science of forces calling it Dynamick, as if he had invented it himself, & is frequently setting his mark upon things by new names & new Notations; & 19 that he has adapted an erroneous demonstration to the XIth Proposition of M^r Newtons first Book of Principles to make it his own; 20 & that he has taken from M^r Newton his general method of series, & endeavoured to explode the rest of M^r Newton's methods of series as useless. For

Pag. 1. lin. 17, he tells us that M^r Newton indeed was before him in the invention of series, < insertion from the left margin of f 413r > but < text from f 412v resumes > he had since invented a general method of series & had no need of M^r Newtons methods. But this general method is M^r Newtons invented by him 44 years ago or above. For in his Letter of 13 June 1676 he tells us that his method of Series was not altogether universal without some further methods then those described in that Letter, & which he there omitted because he was then weary of those studies & had then abstained from them almost five years. And what those methods were he described in his Letter of Oct 24 1676 in the two following sentences exprest enigmatically, viz^t, Vna methodus consistit in extractione fluentis quantitatis ex æquatione simul involvente fluxionem ejus: altera tantum in assumptione seriei pro quantitate qualibet incognita ex qua cætera commodè derivari possunt & in collatione terminorum homologorum æquationis resultantis ad eruendos terminos seriei. The first of these two methods is a branch of the method of fluxions, the other is that very method whereof M^r Leibnitz pretends to have been the first inventor. And by all these instances of the candor of M^r Leibn. you may judge what reason he had to say that the English would be almost the sole inventors. <413r> M^r Newton found out all these methods between the years 1664 & 1672, M^r Leibnitz owns that he knew nothing of the advanced Geometry in the year 1673, & yet he would be the first inventor. M^r Newton began to study these things nine years before M^r Leibnitz & grew weary of them before M^r Leibnitz began to think of them & yet M^r Leibn. would go for the first inventor. All these methods are but several branches of M^r Newton's general method, M^r Leibnitz teares this general method in pienes that he may go for the first inventor of the principal parts of it & make the rest insignificant & useless, & this is his candour.

But M^r Leibnitz complains of the English for not printing the Letters entire, & saith that they only published what they thought capable of a bad interpretation & particularly, saith he, when I was the second time at London M^r Collins shewed me part of his correspondence with M^rs Gregory & Newton & I observed that M^r Newton acknowledged his ignorance in many things, & said amongst other things that he had found nothing about the dimension of the celebrated Curvelines except the dimension of the Cissoïd. But, saith he, they have suppressed all this. And this is another instance of his candor. For they did not omit all this, but published it, as you may see in the *Commercium* pag. 74. M^r Leibnitz came to London the second time in October 1676 & there saw these things in M^r Newtons Letter of Octob 24 1676 written to M^r Oldenburg & put into the hands of M^r Collins to be copied. He did not stay in London till the Letter could be copied, but so soon as they knew whether to send a copy after him, it was sent. This whole Letter is published in the *commercium*

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Pag 1. l. 7. M^r Leibnitz changed the differential characters of D^r Barrow a, e, i, o &c into the characters dz, dy, dx, dv: M^r Newton puts any letters for fluxions & the rectangles under those letters & the letter o for moments or differences; but thinks it ridiculous to place the invention of the method in the invention of words & characters. See *Transact. N.* 342. p. 204, 205.

Ib. l. 8. D^r Keill hath proved that M. Benoulli was in an error himself. See Journal Litt. Iul. & Aust 1674, p. 343, 344.

Ib. l. 13. Is it by a forc't interpretation that M^r Leibnitz received the Series of Gregory from M^r Oldenburg, & published it as his own?

Ib. l. 14 By the laws of all nations, if he will not prove his accusation against D^r Keill, he is guilty of calumny & If he will not write a publick answer to what has been published against him his writing against the English in private Letters to his correspondents, ought also to go for backbiting.

Ib. l. 18. His general method was found by M^r Newton many years before. See Transact Num 342 p. 212.

Ib. l. 21. NB. There was no dispute at that time tho D^r Wallis affirmed that M^r Newton found the method of fluxions in the year 1666 or before.

Ib. l. 20, 24. If M^r Leibnitz ③ hath any Letters in the hand writing of M^r Collins or M^r Oldenburgh with whom he corresponded, & will send the Originals to any friend in England that the hands may be viewed before the R. Society & attested copies taken thereof, ② And if he ① will appoint any friend to view the Letters kept in the Archives of the Society & extract what he thinks material to be published & is not yet published; ④ I do not question but the Society at his request will publish the same.

Pag. 2. l. 2. M^r Leibnitz corresponded with M^r Oldenburg from the year 1670 to the year 1677 & with M^r Collins from the year 1673 or 1674 & in the year 1674 began to write about M^r Newton's series, which was three or four years after M^r Collins began to communicate those series to the Mathematicians both at home & abroad & particularly to some at Paris.

Ib. l. 6. Since M^r Collins in the year 1676 shewed M^r Leibnitz part of his correspondence with M^r Newton, he might shew him the tract De seriebus infinitis.

Ib. l. 7, 8, 9. What M^r Newton there said, was not suppressed but printed in the commercium pag 74 lin. 10, 11.

Pag. 1. l. 5. Almost two years after the Principia Philosophiæ came abroad M^r Leibnitz published the principal Propositions thereof as his own, & to make the science of forces his own has christened it by the new name of Dynamik, & can he complain of the English. His adapting an erroneous demonstration to Prop. XI Lib. 1 Princip. discovers that he tried in vain to make these things his own.

Pag. 2. l. 1. Therefore M^r Leibnitz knew nothing of the advanced Geometry before the year 1674.

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P. 2. l 14. The ancient Greeks who had their Philosophy from the Phenicians & taught that all bodies were composed of Atoms, taught also that all those atoms were heavy towards the earth, & yet were not accused of introducing miracles & occult qualities of the schoolemen.

Ib. l. 18 & seq. All this is as much as to say that if God were within the world he would be the soul of the world & therefore he is intelligentia supramundana an intelligent Being beyond the bounds of the world, that is beyond all space. God & Angels the souls of men can have nothing to do with the phænomena of nature without a miracle. We must in obedience to the Church acknowledge that there is a God & that all things are conserved by his power & that the souls of men are distinct from their bodies but these things cannot be proved by any Phenomena & therefore are meere suppositions. All animal motion & all the phenomena in nature are meere mechanical & man himself is a meere machine. And upon this Hypothesis of the materialists M^r Leibnitz contends that if gravity be not mechanical it must be a miracle & an occult quality of the schoolemen. And by the same way of arguing he may tell us that its a mira cle if a man be not a meer machine or if his soul hath any thing to do with his body & that if thinking be not mechanical its a miracle &

an occult quality, & if the souls of men have nothing to do with their bodies they are occult qualities & miracles, & if God be intelligentia supramundana, his powers of conserving all things & knowing what is done in the world are miracles & occult qualities.

Ib. l. 22, 23. A miracle signifies something that is not constant but by its happening rarely creates a wonder. But M^r Leibnitz has convinced M^r Bayle that the signification of the word ought to be changed.

Ib. l. 29. & p. 4. l. 16 When the Prophets tell us that God sees & hears & has children they do not mean that he has eyes & ears & a wife. And when M^r Newton says that as the mind of man is present to the pictures of things formed in the brain & by means of its being present sees them; so God is present to all things & by presence sees them in all space as it were in his sensorium; he doth not mean that God has a brain or sensorium in a literal sense. He only means that God is omnipresent & by reason of his omnipresence sees all things within himself immediately or without a Medium. For he is not far from any of us: for in him we live & move & have our being.

Pag. 3. l. 17, 18. Is it the fault of the Watchmaker that his watches will not go eternally. If the world may go on to all eternity without falling into disorder, it may have gone on from all eternity without falling into disorder. And this is all that the Atheists contend for.

Pag. 2. l. 30. It is not lawful to deny conclusions without shewing the fault of the Premises. Bu

Pag 3. l. 1. M^r Newton doth not affirm Atoms but puts that opinion among the Quæres.

Ib. l. 19. & p. 4. l. 15 By the same way of arguing a man might say that to deny that God can make matter think, would be to have very narrow ideas of his wisdom & power. We are not to complete the Deity with such perfections as tend to make his Idea chimerical & his being useless & precarious

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P. 4. l. 8. M^r Newton's Philosophy is founded upon experiments applied by three general Rules set down in the beginning of the third book of his Principles. Without his third Rule it cannot be proved that all matter is impenetrable. By that Rule applied to Phenomena & experiments it appears as plainly that all matter gravitates; M^r Leibnitz denies the conclusion without finding fault with the premisses. For saith he, it would be a miracle, & there have been no miracles since the creation of the world. The body is not governed by the soul nor the world by God, but all the phenomena in the Universe are purely Mechan' mechanical: for there are no miracles. I desire to know what religion this gentleman is of.

Ib. l. 17. M^r Newton doth not endeavour to make disciples. He hath left off all correspondence about these matters almost 40 years ago.

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P. 1. l. 5 M^r Leibnitz complains that the English would be sole inventors. But he will not suffer them. He puts in his claim upon all occasions of being first inventor or at least Coinventor . After he had received the series of Gregory from M^r Oldenburgh he published it as his own without mentioning his having received it from M^r Oldenburgh. When M^r Newton in his Letter of June 13 1676 sent him several series he pretended to have invented four of them, before he understood the method of inventing them. In the year 1674 pretended to a series for finding the arch of a circle by the sine, & yet afterwards wrote for the method of finding it. Almost two years after M^r Newtons Principia Philosophiæ came abroad he published the principal Propositions thereof as his own & adapted an erroneous Demonstration to the chief of them to make it his own Galileo began to consider the effect of Gravity upon Projectiles, M^r Newton in his Principia Philosophiæ improved that consideration into a large science M^r Leibnitz christened the child by new name as if it had been his own calling it Dynamica. M^r Hygens gave the name of vis centrifuga to the force by which revolving bodies recede from the centre of their motion M^r Newton in honour of that author retained the name & called the contrary force vis centripeta M^r Leibnitz to explode this name calls it sollicitatio Paracentrica, a name much

more improper then that of M^r Newton. But his mark must be set upon all new inventions And if one may judge by the multitude of new names & characters invented by him, he would go for a great inventor.

Ib. l. 7

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Pag. 1. l. 7, 8. He saith that it appears not that M^r Newton invented the infinitesimal Notation & Arithmetick before him as M^r Bernoulli has well judged. The English say that M^r Newton in his Tract communicated by D^r Barrow to M^r Collins in the year 1669 put letters for fluxions & the rectangles under the symbols of fluxions & the letter o for infinitesimals, that D^r Barrow in his method of Tangents published in the year 1670 put the vowels a, e, i, o, u for infinitesimals, & that M^r Leibnitz in the year 1677 began to put dz, dy, dx, dv, dt &c for infinitesimals & to call them differences & the method the Differential Method. They say also that M^r Newton in the same Tract, represented the summs of Ordinates of Curves by inscribing the Ordinate in a rectangle in this manner $\frac{aa}{64x}$ & that it doth not appear that M^r Leibnitz represented the same thing by the summatory symbol $\int \frac{aa}{64x}$ before the year 1686. They say also that M^r Newton in the same Tract gave a specimen of this new Arithmetic, that D^r Barrows method of Tangents is another specimen, & that M^r Newton in his Letter to M^r Collins dated 10 Decem 1672 (a copy of which was sent to M^r Leibnitz by M^r Oldenburg 13 Iune 1676) described the same method & how it extended to all sorts of Problems & proceeded without sticking at surds, & gave the method of Tangents of Slusius as a branch or Corollary thereof. They say also that M^r Newton in his Letter to M^r Oldenburg dated 24 Octob 1676, represented that his method readily gave the method of Tangents of Slusius, & maxima & minima & Quadratures & other Problemes & stuck not at surds & that it was comprehended in this sentence enigmatically exprest Data æquatione quocunque fluentes quantitates involvente, fluxiones invenire & vice versa; & that this method gave him the series for Quadratures there set down & illustrated with examples & that the book of Quadratures shews how the method of fluxions gives this series, & no man hath hitherto shewn how to find it by any other method of fluxions, & that M^r Newton five years before those days that is in the year 1671 wrote a book of this Method & of the method of series together. & that it doth not appear by any arguments that M^r Leibnitz knew the Differential method before the year 1677. They say also that when M^r Leibnitz wrote his Letter of 27 August 1676, he could not beleive that M^r Newton's methods were so general as M^r Newton had represented & that inverse Problems of Tangents could not be reduced to quadratures or equations, & therefore he had not yet found the differential Method, that at his second coming to London which was in the latter part of October 1676 he saw M^r Newtons Letter of 24 of that month in the hands of M^r Collins (For it was in that Letter that M^r Newton said that in the figures vulgarly celebrated he found little new concerning their dimensions except the dimension of the Cissoïd,) & having also a copy of M^r Newtons Letter of 10 Decem. 1672 in his return from London through Holland into Germany he was <417r> meditating how to make the method of Tangents of Slusius general & extend it to all sorts of Problems, & in a Letter to M^r Collins dated from Amsterdam 28 Novem. 1676, proposed to do it by a table of Tangents, & therefore had not yet found the differential method; & that the next year when he had newly found this method, he wrote back that he took the method which M^r Newton concealed, to be like it. They say also that all these things are taken out of Letters & Papers printed entire, (the extracts or fragments of Letters relating to the Questions about infinite or converging Series,) & that it lies upon M^r Leibnitz to prove that he had the Differential method before the year 1677.

Pag. 1. lin. 15. He complains that while the Question was about the Method of Differences the English have gone out of the way & fallen upon the Method of Series that they might attaque his candour. But the English say that in his Letter of 29 Decem. 1711. he opposed his candour to D^r Keill as if it were injustice to question it, & by making himself an unquestionable witness in his own cause, made it necessary to question his candor They say also that M^r Leibnitz in his Letter to M^r Oldenburg dated 21 Iuly 1677, when he had learnt from M^r Newton's Letters that the method of Slusius for Tangents was capable of being made general & extended to all sorts of Problems, & thereby had newly found out the Diffrential method, pretended that he had found it out long before & thereby instead of thanking M^r Newton for giving him light into the method began to claim

a share in it as coinventor. His words are, Clarissimi Slusij Methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium longe generalius tractavi; scilicet per differentias Ordinatarum. I agree with M^r Newton saith he that the method of Tangents of Slusius is not yet perfected; & I have a long time ago handled the buisiness of Tangents far more generally namely by the differences of the Ordinates. And yet M^r Leibnitz in this Letter acknowledges that when he was in England the first time & for some time after, that is, in the year 1673, he knew nothing of the advanced Geometry, & in his Letter of 27 August 1676 he disputed against the universality of M^r Newton's methods & contended that inverse Problemes of Tangents & many others could not be reduced to Equations, & by consequence he had not then found the differential method as we noted above. And in the Acta Eruditorum for April 1691, pag 178, he represented that in the year 1675 he had a little Tract concerning an Arithmetical Quadrature, but the matter increasing under his hand till other employments came upon him he had not leasure to fit it for the press, nor thought it then worth the while to explain that Quadrature any further in the prolix vulgar manner, which his new Analysis comprehended in short. He found this new Analysis therefore after his return into Germany to enter upon new employments which was in the winter between the years 1676 & 1677, & after he began to enter upon them, & by consequence not above three or four months before he wrote that he had found it jam a multo tempore a long time before he wrote. The English say further that M^r Leibnitz first attacked the candor of D^r Wallis & M^r Newton. For, say they, D^r Wallis in the Preface to his works <418r> printed in March or April 1695, said that the Method of fluxions is of the same nature with the Differential Calculus, & that M^r Newton in his letters of Iune 13 & Aug. 24 (he means Octob. 24) 1676 written to M^r Oldenburgh to be communicated to M^r Leibnitz, explained to M^r Leibnitz this method found by him ten years before that time or above; that is, in the year 1666 or before. And M^r Newton in the Preface to his book of Quadratures published in the year 1704, affirmed that he found the method of fluxions by degrees in the years 1665 & 1666. D^r Keill affirmed no more then his Predecessor D^r Wallis had affirmed many years before. M^r Leibnitz questions the candor of them all, & D^r Wallis being dead, demands that D^r Keill recant & that M^r Newton declare his opinion in this matter, that is, against D^r Keill & himself & D^r Wallis, & the Royal Society must condemn them & see the sentence executed & this without any other evidence against them then his own testimony for himself. For saith he, at so great an age & after so many documents of my life to question my candour & expect that I should defend it, would be injustice. In his Letter of 21 Iune 1677 he was examining whether the Differential method was of as great extent as M^r Newton's. In the Acta Eruditorum for November 1684 he called the Differential Method a sublimer Geometry reaching to the most difficult Problemes which were not to be resolved without this method or another like it, meaning M^r Newtons. In his Letter to M^r Newton March $\frac{7}{17}$ 1693 he acknowledged that M^r Newton had a method of doing what the Vulgar Analysis did not reach to to as appeared by his book of Principles, & subjoyned that he had also by convenient symbols of Summs & differences, endeavoured to extend Analysis to the transcendent Geometry. And in the years 1695, 1696 when D^r Wallis in the Preface to his works had affirmed that M^r Newton in his Letters written in the year 1676 had explained to M^r Leibnitz the method of fluxions found by him 10 years before the writing of those Letters or above, & the Editors of the Acta Eruditorum (in the stile of M^r Leibnitz as some think) gave an account of the Doctors works, & therein took notice of this Paragraph of the said Preface, & there followed thereupon a correspondence by Letters between the Doctor & M^r Leibnitz, & the Doctor in his Letters of Decem. 1, 1696 & Apr. 6, 1697 gave notice of that Paragraph to M^r Leibnitz himself, & represented that the Differential method was the same with M^r Newtons method of fluxions M^r Leibnitz did not question the truth of what D^r Wallis had published, granted that the Methods were the same in the main & said that he therefore called them both by the common name of the infinitesimal method, but as the Analysis of Vieta & Cartes were both called by the common name of Analysis speciosa & yet differed in some things, so perhaps M^r Newtons method & his own might differ in some things. Thus he then compared M^r Newtons method to the Analysis of Vieta & his own to that of Des Cartes in point of antiquity & chalenged only to himself those things wherein his method differed from M^r Newton; & in his present Letter he represents that D^r Wallis had not the least dispute with him. And when M^r Fatio (in the year 1699) had published that M^r Newton was the first & <419r> by many years the oldest inventor of this Method, & that M^r Leibnitz was the second Inventor thereof; And M^r Leibnitz in the Acta Eruditorum for May 1700 allowed that M^r Newton had found the method of fluxions without receiving any light from the

differential method & did not deny that M^r Newton was the oldest Inventor by many years nor contend for any thing more then that he also had found the Differential Method without receiving any light from M^r Newton & added that no man before M^r Newton had proved by a specimen publickly exhibited that he had this method. But after the death of D^r Wallis, in giving an Account of M^r Newton's Tract de Quadratura Curvarum in the Acta Eruditorum for Ianuary 1705, the Editors (in the style of M^r Leibnitz) represented that M^r Leibnitz was the first Inventor, & that M^r Newton had substituted fluxions for differences. And thus by attacking the candor of D^r Wallis & M^r Newton & demanding that D^r Keill who defended them should retract provoked & authorised D^r Keill to retort the accusation & made it necessary for him to do so in defense of himself & his friends.
