

Historical Annotations on the Elogium of Leibniz

Author: Isaac Newton

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<372r>

Part of Hist Annotations in Elog of Leibnitz a{illeg} 1717?

Pag 44. l. 1. What M^r Newton calls fluxions & marks sometimes with prickt letters & sometimes with other marks M^r Leibnitz doth not call differences, nor hath any name or mark for them. What M^r Newton calls moments & particles M^r Leibnitz calls differences; & fluxions are quantities of another kind. They are motions or velocities of increas{e} & not parts of the increasing quantities.

Ib. l. 3, 4, 5. When the Marquess de l'Hospital represented that the Characters of M^r Leibnitz were more convenient then those of M^r Newton, he had seen only the marks used by M^r Newton in the second Lemma of the second Book of his Principles & therefore meant those Characters & not prickt letters. They that have been used to the characters of M^r Leibnitz will be apt to think them the best.

Ib. lin. penult. Before this, D^r Wallis in the Preface to the two first Volumes of his Works published A.C. 1695 wrote that M^r Newton in his two Letters of 1676 explained to M^r Leibnitz the Method found by him ten years before or above, that is in the year 1666 or before. And the Editors of the Acta Eruditorum in giving an account of those two Books replied falsly that M^r Newton had both publickly & privately allowed the invention of the Method to M^r Leibnitz. And even before this when M^r Leibnitz first published the elements of the differential method & made no mention of correspondence he had with M^r Newton six years before by means of M^r Oldenbeg: M^r Newton described the Elements of the Method in the second Lemma of the second book of his Principles & wrote the Scholium to that Lemma, not to give away the Lemma as M^r Leibnits has pretended, but to assert it to himself.

P. 48. lin. 15. M^r Newton was first accused of Plagiary in the Acta Eruditorum for Ianuary 1705 pag 34 & 35 & the accusation was justified by M^r Leibnitz himself in his Letter of 29 Decem. 1711 to D^r Sloan And for obviating this accusation the Committe insinuated that there was more colour to suspect M^r Leibnit{z} nec lex est justior ulla.

P. 49. l. 20. M^r Leibnitz has affirmed that he found the differentia^ll method in the year 1676, & therefore could not undertake to prove by a new commercium that he found it before. And if he did not find it before, he was not the first inventor. And second Inventors have no right. The first Inventor has the sole right till another Inventor arises: & then to take away the right of which he was once justly possessed & share it with others would be injustice. All

p. 47. lin. 5, 6 This Letter was writ to M^r Collins in 1672 & a copy of it sent to M^r Leibnitz in Iune 1676

p. 51. lin. 21. M^r Leibnitz went from London to Paris in March 1673 & continued to write Letters to M^r Oldenburg about Arithmetical Matters till June 8th following. Then he intermitted his correspondence for a year & in the mean time applied himself to learn the higher Geometry by means of M^r Huygens whose Horologium oscillatorium came abroad that year in April. And by reading first this & then the other books here mentioned, he got light into the method which gave him the Theoremes like those of Barrow & Gregory. And now he had his Method by the Differentiae generatrices in Arithmetic, & that by the Triangulum characteristicum in Geometry. But he had not yet the Differential method in Algebra, for he tells us in one <372v> of his Letters that he found that in the year 1676.

Pag. 53 l. 6. He did not see M^r Newtons papers before the year 1676. Vpon the death of M^r Gregory he desired that a collection of Gregory correspondence might be made & sent to Paris. It was sent June 26 1676 & therein was, 1 M^r Gregories Letter to M^r Collins dated 15 Feb. 1671 containing Gregories Series for squaring the circle, published afterwards by M^r Leibnitz without acknowledging what he had received from London. 2 M^r Gregories Letter of 5 Sept 1670, in which Gregory mentions that by the help of D^r Barrows method of Tangents he had found a general method of Tangents without calculus, viz^t a method like that of Slusius. 3 M^r Newtons Letter of 10 Decem 1672 in which M^r Newton described his general method of Analysis & said that the method of Tangents of Gregory & Slusius was but a Corollary or branch thereof. And at the same time M^r Newtons Letter of 13 June 1676 was also sent in the end of which he writes that Analysis by the method of series & some other methods (meaning the methods of fluxions & arbitrary series, becomes very universal. And in October following M^r Leibnitz coming to London saw M^r Newtons Letter of 24 October wherein M^r Newton further describes the Method of fluxions, & mentions also his Compendium of the Method of series communicated by D^r Barrow to M^r Collins A.C. 1669, that is the Analysis per series numero terminorum infinitas. And having the year before written to M^r Oldenberg to procure M^r Newtons method of series from M^r Collins & now meeting with this further notice of it: he could scarce miss of seeing it. For he consulted M^r Collins at that very time to see what he could meet with of the correspondence of Gregory & Newton with Collins concerning these series. And if he there saw this Analysis he had a plane description of the method of fluxions at the end of it.

P. 53. l. 22. M^r Newton inserted Elements of the method of Moments into the second Lemma of the second Book of his Principles, & added a Scholium to justify his doing so. And when a Letter of M^r Leibnitz written in Autumn 1715 had been handed about some time among persons of note in London & M^r Newton was pressed to answer it, he returned an answer dated Feb 26 1716 & M^r Leibnitz sent these Letters with his Answer

P. 48. l. 6 A Letter writ by M^r Leibnitz in Autumn 1715 being handed about among persons of note in London, M^r Newton was at length prevailed with to answer it, which he did by an Letter dated Feb 26 1716. M^r Leibnitz answered that Letter by another dated 9 Apr. 1716. But because he began his Answer with a pretence that M^r N had challenged him & instead of sending his Letter directly to London he sent it with the two former Letters to Paris, † < insertion from the bottom of the page > † & it came thence to London: M^r Newton returned no answer, but only shewed privately to some of his friends < text from f 372v resumes > M^r Newton returned no answer to this, but only shewed some of his friends in private what might have been answered: & upon the death of M^r Leibnitz, because the three Letters had been dispersed, published them, with what was drawn up, but not sent.

<373v>

1 What M^r Leibnitz calls Differences M^r Newton calls not fluxions but Moments & small particles. Fluxions are quantities of another kind & M^r Leibnitz hath no marks nor name for them. They are motions or velocities of increase & not parts of the increasing quantity.

2 to think them best: but that M^r Newtons are more universal & better adapted to just reasoning may appear by this instance. Let x, y, z be fluents $\dot{x}, \dot{y}, \dot{z}$ their fluxions & $\dot{x} \times o, \dot{y} \times o, \dot{z} \times o$ their moments: & any equation expressing the relation of the fluents, as $bbx - cxy + zz = 0$ being given let it be required to find the relation of their fluxions. Let x flow uniformly & its fluxion \dot{x} be 1 & its moment will be o & after one moment of x the fluents will become $x + o, y + \dot{y}o, z + \dot{z}o$, which being substituted for the former fluents the equation will become $bbx + bbo - cxy - coy - cx\dot{y}o - c\dot{y}oo + zz + 2z\dot{z}o + \dot{z}\dot{z}oo = 0$. And striking out $bbx - cxy + zz = 0$, there will remain $bbo - coy - cx\dot{y}o - c\dot{y}oo + 2z\dot{z}o + \dot{z}\dot{z}oo = 0$. Divide the whole by o & the Quotient will be $bb - cy - cx\dot{y} - 2c\dot{y}o + 2z\dot{z} + \dot{z}\dot{z}o = 0$. Let the moment o become infinitely little & the terms $-2c\dot{y}o + \dot{z}\dot{z}o$ will vanish, & the remaining equation $bb - cy - cx\dot{y} + 2z\dot{z} = 0$ will give the relation of the fluxions to which the moments are proportional. Here the whole operation is exact & as evident as any thing in mathematicks & all the quantities are finite (as they ought to be in Geometry) Until the operation is finished & then o is made to decrease in infinitum & vanish And if o be at any time during the operation supposed infinitely little yet it is the only infinitely small quantity in the whole operation & it is always supposed to be a constant quantity: & the Artist is at liberty to take o for a quantity finite or infinitely small as he shall find it best for his purpose whether he has a mind to be exact or to make dispatch by approximation. But in the differential notation of M^r Leibnitz, there are always two or more infinitely small quantities & one or more of them are always inconstant & the calculations are generally grounded upon approximations. The errors indeed are infinitely small, but Geometry admits of no errors. In these calculations a right line may be considered as touching a Curve in more points then one, & chord infinitely short may be considered as coincident with its Arch: but Geometry admits of no such Propositions.

M^r Leibnitz might become acquainted with M^r Huguens in 1672 but when he was in London the first time which was in Jan. & Feb. 1673 he knew nothing of the higher Geometry. The Horologium oscillatorium of M^r Huguens was published in April 1673

The Letter writ to Collins in 1672 M^r Leibnitz did not see till 1676.

Pag. 53. lin. 6. A copy of M^r Newtons Letter of 10 Decem. 1672 was sent to M^r Leibnitz June 26 1676. And M^r Leibnitz was not accused by the Committee of the R. S. of understanding all that he had seen, but only that in what he had seen, the Method of Fluxions was sufficiently described to any intelligent person. And M^r Tschurnhause & M^r James Bernoulli thought it almost described in D^r Barrows works alone. The words of Bernoulli in the Acta Erud. for Jan. 1691 pag. 14 are these Qui calculum Barrovianum — intellexerit alterum a Dⁿ Leibnitio inventum ignorare vix poterit; utpote qui in priori illo fundatus est & nisi forte in differentialium notatione & operationis aliquo compendio ab eo non differt. The Marquess de l'Hospital in the Preface to his Book said that D^r Barrows calculus stuck at fractions & surds & that where D^r Barrow left off M^r Leibnitz went on. He had not yet seen M^r Newtons Letters of 10 Decem. 1672 & 24 Octob 1672 in which M^r Newton wrote that his general method did not stick at surds. But M^r Leibnitz had seen them before he found the Differential Method, & thereby knew that M^r Newton was before him in this particular. He might learn from Gregories Letter of Sept 5 1670 that Gregory had improved Barrow's Method of Tangents so as to draw Tangents without calculation, & from Newton's of 10 Decem 1672 that this Method was the same with that of Slusius & that both Methods were a Corollary or Branch of Newtons general Method & by consequence that Barrows Method was a step to that of Gregory & Slusius & both of them were steps to Newtons general method, & this might make him write in the beginning of his Letter of 21 June 1677 Clarissimi Slusij Methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior; & then set down an example of D^r Barrows method of Tangents with the notation altered, & conclude the calculation with these words Quod coincidit cum Regula Slusiana, ostenditque eam statim occurrere hanc Methodum intelligenti. And a little after: Methodo autem Slusiana omnes ordine irrationales tollendas esse nemo non videt. <373r> Arbitror quæ celare voluit Newtonus de Tangentibus ducendis, ab his non abluere Quod addit, ex hoc eodem fundamento Quadraturas quoque reddi faciliores me in sententia hac confirmat; nimirum semper figuræ illæ sunt quadrabiles quæ sunt ad æquationem differentialem. Now M^r L. having seen M^r Newton's Letters & Papers abovementioned before he understood the method & understanding all this out of

them so soon as he understood the Method, the Committee of the R. S. may be excused for saying that in what M^r Leibnitz had seen the Method was sufficiently described to any intelligent person.

Pag 44 lin 10, 11. The Editors of the Acta Erudit. would print nothing in other characters.

Pag 45. l 8, 9. The assuming of right by second Inventors after it appears that he was not the first is plagiary notwithstanding that the second Inventor may have invented apart without knowing what the first had done before.

Pag 49 lin 9, 10. This writing denied in effect that M^r Newton understood the first Proposition in his own Book of Quadratures, or that the Elements of the Differential Method are contained in the second Lemma of the second Book of Principles, or that the sentence Data æquatione fluentes quotcunque quantitates involvente invenire fluxiones et vice versa, denotes the method of fluxions, or that the series for squaring of Curves which breaks off & becomes finite when the Curve may be squared by a finite equation, was found by the Method of fluxions altho M^r Newton said in his Letter of 24 Octob. 1676 that it was found by that method & in his book of Quadratures shewed how it was found by that Method, & there is no other method yet known by which it can be found.

p. 54. l. 7, 8. The Editors of the Acta Leipsica never would print any thing against M^r Leibnitz or with other characters then the Differential. When M^r Fatio sent them a Paper against M^r Leibnitz they would not print it. In giving an Account of the two first Volumes of D^r Wallis's works they would not let the reader know that the Doctor ascribed the Differential method to M^r Newton as the oldest Inventor but shuffled of the Objection by feigning that Newton had always both publickly & privately acknowledged M^r Leibnitz the inventor. In giving an Account of M^r Newtons book of Quadratures instead of giving an account of the characters used by M^r Newton in that Book they gave an account of the characters used by M^r Leibnitz. When D^r Keil sent them a paper writ in M^r Newtons characters they would not print it in those characters.

Pag. 53. lin 22. In the year 1684 M^r Leibnitz published the elements of his Method without making any mention of the correspondence between him & M^r Newton eight years before as in candour he ought to have done. For he knew by that correspondence that M^r Newton had such another Method, as is plain by his Letter of 12 Iuly 1677 & that he had it & wrote of it in the year 1671 & therefore was the oldest Inventor, as is plain by M^r Newton's Letters of 13 Iune & 24 October 1676. M^r Newton therefore in the year 1686 set down the Elements of this method as his own in the second Lemma of the second book of his Principles & added a Scholium to that Lemma not to give away that Lemma to M^r Leibnitz as has been pretended but to assert his own right to it as first inventor. And yet M^r Leibnitz continued to claim it upon a pretence that he found it apart, tho he knew that he was not the first inventor.

<374r>

Historical Annotations on the Elogium of M^r Leibnitz.

1 Pag. 44. lin. 1. Fluxions are not differences. They are quantities of another kind, & M^r Leibnitz hath no marks nor name for them. They are motions or velocities of increase & not parts of the increasing quantity. What M^r Leibnitz calls Differences M^r Newton calls Moments & particles & designes them by fluxions multiplied by the moment o.

2 Ib. lin. 3, 4, 5. When the Marquess de l'Hospital represented that the characters of M^r Leibnitz were more convenient then those of M^r Newton, he had seen only the characters used by M^r Newton in the second Lemma of the second book of his Principles & therefore meant no other.

3 Ib. lin. 22. M^r Fatio was not the first. D^r Wallis, so soon as he heard that the method of M^r Newton was spreading in Holland by the name of the Differential Method of M^r Leibnitz, wrote in the Preface to the first volume of his works, published in April 1695, that M^r Newton in his two Letters of Iune 13th & Octob. 24,

1676, had explained to M^r Leibnitz the method of fluxions found by him ten years before or above; that is, in the year 1666 or before.

8 Pag 54. lin. 3. A letter writ by M^r Leibnitz in autumn 1715 being handed about among persons of note in London, M^r Newton was pressed to return an answer, & at length did so by a Letter dated Feb. 26, 1716, & M^r Leibnitz replied Apr 9th 1716, but sent his Reply to Paris (with Copies of the two former Letters) to be communicated to his friends there before it came to London. For which reason M^r Newton wrote to him no more, but only shewed privately to a few of his friends what might have been writ in answer.

4 Pag. 48. lin. 15. M^r Newton was accused of Plagiary in the Acta Eruditorum for January 1705 pag. 34 & 35, & the accusation was justified by M^r Leibnitz himself in his Letter to D^r Sloane dated 29 Jan. 1711. And for obviating this accusation the commercium Epistolicum insinuated that the contrary was more to be suspected, M^r Leibnitz having seen severall of M^r Newton's Letters in the year 1676, it not appearing that he had the Method before he saw them.

5 Pag 49. lin. 20. M^r Leibnitz pretended that the Committee of the R. Society had in the commercium Epistolicum omitted many things which ought to have been published, & upon that pretence proposed to print a new commercium impartially, & in autumn 1714 desired that the original Letters in the custody of the R. Society might be sent to him for that purpose. M^r Newton replied before the R. Society that the commercium was printed out of Originals which had been constantly in their Archives & those of M^r Collins, & had been also examined by those who knew the hands, & that they ought still to be kept safe for justifying what had been printed out of them. He added further that he did not print the commercium himself, nor so much as produce any Letters or Papers which he had then in his own custody, because he would not make himself a witness in his own cause. And to prove the truth of what he said he produced two Letters written to himself the one by M^r Leibnitz March 7th 1693, the other by D^r Wallis Apr. 10th 1695. These Letters were examined before the R. Society by them who knew the hands & then they were read & laid up in the Archives of the R. Society as authentic. At the same time it was allowed that if M^r Leibnitz had any Letters in his custody which he thought material to be published, & pleased to send them to any friend in London whom he could trust with them that they might be publickly examined by those who knew the hands, & attested copies be taken of them: they might then be published either in the Phil. Transactions or by M^r Leibnitz himself. But nothing has been sent. On the contrary M^r Leibnitz having <374v> been since pressed to return a direct Answer to the commercium, replied in his Letter of April 9th 1716. Pour repondre done de point en point a l'Ouvrage publié contre moi, il falloit un autre Ouvrage aussi grand pour l{a} moins que celui là, il falloit entrer dans un grand detail de quantité de Minuties passées il y a 30 ou 40 ans dont je ne me souvenois gueres; il me falloit chercher mes veilles Lettres dont plusieurs se sont perdues, outre que le plus souvent; je n'ay pas gardé les Minutes des miennes; et les autres sont ensevelies dans une grand tas de Papiers, qui je ne pouvois debrouiller qu' avec du Temps & de la patience. Mais je n'en avois gueres le loisir, etant chargé presentmant d'Occupations d'une toute autre nature. And the author of the Elogium of M^r Leibnitz published in the Acta Eruditorum for July 1717 pag. 335, writes: Quo tamen perspicere quid de tota illa controversia sentiendum sit, Commercio Epistolico Anglorum aliud quoddam idemque amplius opponere decreverat, & paucis ante obitum diebus Cl. Wolfio significavit se Anglos famam ipsius lacescentes reipsa refutaturum: quamprimum enim a laboribus historicis vacaturus sit, daturum se aliquid in Analysisi prorsus inexpectatum & cum inventis quæ hactenus in publicum prostant, sive Newtoni, sive aliorum nil quicquam affine habens. Here M^r Leibnitz wrote only that he would refute the English (he doth not say with a new commercium, as the Author of the Elogium seems to understand, but) with a new surprizing invention in Analysis of another kind. And this he wrote a few days before his death, & we were to stay for the performance till his Historical labours should be at an end.

Pag 51. lin. 22. M^r Leibnitz might become acquainted with M^r Hugen in the year 1672; but when he was in London the first time which was in January & February 1673, & while he kept a correspondence with M^r Oldenburg about Arithmetical matters which was till Iune following, he knew nothing of the higher Geometry The Horologium oscillatorium of M^r Huguens was published in April 1673, & in studying the

higher Geometry he read first this book & then the works of Paschal & Gregory of S^t Vincent & then fell upon his Triangulum characteristicum & his many Theorems, & went on to his Method founded on his Analytical Tables of Tangents & his Combinatory Art & this was the top of his skill when he wrote his Letter of 27 August 1676. For in that Letter he said of one part of this Method Nihil est quod norim in tota Analysis momenti majoris. And of another part: Cujus vim ac potestatem nescio an quisquam hactenus sit consecutus. Ea verò nihil differat ab Analysis illa suprema ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum. In the same Letter he used the common Algebra where the Differential Calculus would have been proper had he then known it, & said that Inverse Problemes of Tangents could not be reduced to Equations. In October following he was again in England where he met with Barrows works & Newton's Letter of 24 Octob. 1676, & saw the correspondence of Gregory & Newton in the hands of Collins, having seen Newton's Letters of 10 Decem 1672 & 13 June 1676 & Gregories of 5 Sept. 1670 & 15 Feb 1671 three months before. And it doth not appear that he found the Differential method before he saw all this. See his Letter of 9 April 1716.

Pag. 53. lin. 6. The Letter was writ to M^r Collins in 1672 & seen by M^r Leibnitz in 1676.

<375r>

NB. M^r Leibnitz in his Letter of June 21. 1677 acknowledged a similitude between the Methods &

NB M^r Leibnitz observed & acknowledged this similitude of the Methods long before the publishing of the two books here mentioned, namely in his Letter of 13 June 1697. Also in th{e} Paper in which he published the elements of the Differential method A.C. 1684 he allowed a methodus similis. Also in his Letter to me dated $\frac{7}{17}$ March 1693 & printed above he allowed that by the Principia Philosophiæ mathematica I had shewed that I had such a method, & here he exprest himself with some deference to me, as if he also had endeavoured to do the like. And now after the the edition of the two first Volumes of the Works of D^r Wallis he repeats the acknowledgment allowing that the Methods are so far the same that he calls them both by the common name of the infinitesimal method But as the Analysis of Vieta & Cartes were called by the common name of Analysis speciosa & yet differed by some improvements which Cartes had made to the method of Vieta, so my infinitesimal Analysis & his might differ in some improvements which he had made to mine & which were not yet come to the knowledge of D^r Wallis. And three of those improvements he names, 1 the reduction of transcendent quantities to Equations. 2 The reduction of such Curves to Equations as Cartes called Mechanical. 3 The invention of Exponential Equations. [And thus the general infinitesimal method common to us both, & here allowed to be mine & the improvements made to it by M^r Leibnitz are his.] While M^r Leibnitz contended that my method & his differed in somethings its plain that he did not yet begin to claim the whole Nor did D^r Wallis claim for me the improvements which M^r Leibnitz had made to the Method. [The Doctor published that I had invented the Method in the year 1666 or before & M^r Leibn. in all the Letters which afterwards past between them, did not deny it. nor pretends that he had it before the year 1676. And second inventors have nor right]. M^r Leibnitz claimed the improvements & D^r Wallis claimed the rest for me as the oldest inventor by many years, & was not complained of for this by M^r Leibnitz. De te autem queri, saith M^r Leibnitz nunquam mihi in mentem venit quem facile apparet nostro [our improvements] in Actis Lipsientibus prodita, non satis vidisse. — for saying that I invented the method in the year 1666 or before & explained it ten years after in my Letters.

M^r Leibnitz did not complain of the Doctor for saying that invented the method in the year 1666 or before & explained it ten years after in my letters, but

And so the question which then remained between them was about these improvements & particularly whether the symbols $\int \frac{aa}{b+x}$, $\int \frac{dx}{\sqrt{2x-xx}}$ & such like by which he designed transcendent quantit{illeg} & introduced them into equations or mine for the same purpose $\frac{aa}{b+x} \cdot \frac{dx}{\sqrt{2x-xx}}$ &c were the oldest, or whether the summatory method or the inverse method of fluxions was the oldest. For my part I said in my Letter of 24 Octob 1676 that I wrote a Tract five years before, concerning the method of series & another method together, & that said there that the other method proceeded without sticking at surds & was founded in this

Propositions: Data *Æquatione fluentes* quotcunque quantitates involvente invenire fluxiones & vice versa, that is in the direct & inverse methods of fluxions improperly called by M^r Leibnitz the differential & summatory methods. And I said further that this method extended to Problems about maxima & minima, & to tangents directly & inversely, & to Quadratures, & some others & gave me the series there set down for squaring of Curves & some others of like nature. And in my Letter of 10 Decem. 1672 (a copy of which was sent to M^r Leibnitz & came to his hands) I mentioned some other sorts of Problemes & particularly the finding the curvatures of Curves & the tangents of the Mechanical Curv{s} and in my letter of 13 June 1676 I said that my Analysis (composed of the method of Series & other methods, viz^t the <375v> methods of fluxions & arbitrary series] extended to almost all sorts of Problems except perhaps some numeral ones like those of Diophantus. And M^r Leibnitz in his Letter of 27 Aug. 1676, replied that he did not beleive that my method could be so general, nor extend to inverse Problems of Tangents & many others tho he had reduced one inverse probleme of Tangents (that proposed by Beaune) to a quadrature. And if all this & the method of extracting fluents out of equations involving fluxions & that of arbitrary series set down in my Letter of 24 Octob 1676 as known to me some years before, be not sufficient security: no many must hereafter communicate any thing in writing before he has secured it to himself by printing;

– Copies of the two Letters of M^r Leibnitz here desired & of some others

<377r>

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Pag. 48. lin. 15. M^r Newton was accused of Plagiarism in the Acta Eruditorum for January 1705 pag 34 & 35 & the accusation was justified by M^r Leibnitz himself in his Letter to D^r Sloan dated 29 Decem 1711. And for obviating this accusation the Commercium Epistolicum insinuated that the contrary was more to be suspected several of M^r M^r Newton's papers relating to this matter having been seen by M^r Leibnitz. in the year 1676.

Pag 48. l. 6. A Letter writ by M^r Leibnitz in Autumn 1715 being handed about among persons of note in London, M^r Newton was at length prevailed with to answer it His Letter was dated Feb. 26. 1716, & M^r Leibnitz Answered it 9 Apr. 1716, but sent his Answer to Paris (with copies of the two former Letters) to be communicated to his friends there before it came to London, For which reasons M^r Newton wrote to him no more, but only shewed privately to a few of his friends what might have been writ in answer

Pag 49. lin. 20. All the Letters of M^r Leibnitz to M^r Oldenburg from the beginning of the year 1673 to the death of M^r Oldenburg are in the hands of the R. S & so are copies of all M^r Oldenburgs Letters to M^r Leibnitz from Decem 8 1674 to the end of the year 1696. And a Commercium of a later date can signify little. M^r Leibnitz complained that the Committee of the R. S. had extracted out of these old Letters every thing that made against him & omitted every thing that made for him but foreigners who have since viewed the Letters have found it otherwise. And lately when M^r Leibnitz pretended to give two instances of things omitted he was found mistaken in them both. And in Autumn 1714 M^r Leibnitz desiring that the original Letters in the custody of the R. S. might be sent to him in order to his printing a new Commercium, he was answered, that if he had any thing in his custody which he thought material to be printed, & pleased to send the original MS

to any of his friends in London; they would order it to be printed in the P. Transactions after the hand had been examined. But MSS not examined by unconcerned persons who knew the hands they would not regard.

P. 51. lin. 22. The Horologium oscillatorium of M^r Huguens was published in April 1673. M^r Leibnitz began presently after to study the higher Geometry Then he fell upon the triangulum characteristicum & his many new Theorems, & his a{illeg} & went on to his Method by analytical Tables of Tangents & the Combinatory Art, & this was the top of his skill when he wrote his Letter of 27 Aug. 1676. For in that Letter he saith of one part of this method: Nihil est quod norim in tota Analysisi momenti majoris. And of ano <377v> ther part: Cujus vim ac potestatem nescio an quisquam hactenus sit consecutus. Ea vero nihil differt ab Analysisi illa suprema ad Cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum. † < insertion from the bottom of the page > † And there are other indications in the Letter by which it may be gathered that the differential method was not yet known to him. He seems to have learnt it in the Autumn or winter following. † And soon After this, he fell upon the differential Analysis. < text from f 377v resumes > The method therefore by differential equations he was not yet acquainted with, as may be gathered also from his words which follow in the same Letter: Quod dicere videmini plerasque difficultates ad Series infinitas reduci, id mihi non videtur. Sunt enim multa usque adeo mira et implexa ut ab æquationibus non pendeant. Qualia sunt (ex multis alijs) Problemata methodi tangentium inversæ. M^r Leibnitz in his Letter of 9 Apr 1716 said that he judged by the words certa Analysisi solvi in his Letter of 27 Aug. 1676 that he must have had some light into the Differential calculus at that time. Whether the certa Analysis were the Differential method or the Analysis illa suprema ad cujus intima Cartesius non pervenit may be doubted: but how ever nothing more can be affirmed then that M^r Leibnitz according to the best of his knowledge & memory got some light into the Differential method in the year 1676.

Pag. 53. l. 6. A copy of M^r Newtons Letter dated 10 Decem 1672 was not sent to him before June 26 1676.

<378v>

Pag. 49. lin. 20. All the Letters of M^r Leibnitz to M^r Oldenburg from the beginning of the year 1683 to the death of M^r Oldenburg are in the custody of the R. Society In autumn 1714 M^r Leibnitz wrote to London that he intended to print a new commercium & desiring that the Original Letters in the custody of the R. Society might be sent to him for that purpose. The Letter being read before the R. Society M^r Newton replied that when their Committee printed the commercium out of their archives & those of M^r Collins he did not so much as produce the Letters in his own custody because he would not make himself a witness in his own cause. And to satisfy them of the truth of this he produced two Letters written to himself the one by M^r Leibnitz March 7th 1693 the other by D^r Wallis Apr. 10th 1695. The Letters were examined before the R. S by those that knew the hands of M^r Leibnitz pretended that the Committee of the R. Soc. had omitted many things which ought to have been published, & upon that pretence proposed to publish a new commercium. And in his Letters of Novem. 1715 & Apr. 9 1716 to make good his complaint he instanced in two things that they had omitted but was mistaken in them both. The Original Letters have been seen by foreigners & nothing material has been found omitted.

Pag 49. lin. 20. M^r Leibnitz pretended that the Committee of the R. S. had in their commercium Epist omitted many things which ought to have been published & upon that pretence proposed to print a new commercium impartially: And in autumn 1714 desired that the original Letters in the custody of the R. Society might be sent to him for that purpose. His Letter being read before the R. Society; M^r Newton replied that the commercium was printed out of Originals which had remained from the beginning in the Archives of the R. S. & those of M^r Collins, the hands being examined by those that knew them & were still to be kept for justifying {w} also had been printed out of them & that he himself did not intermeddle with those Originals nor so much as produce the letters & Papers in his own custody because he would not make himself a witness in his own cause. And to prove the truth of this he shewed them two old Letters written to himself the one by M^r Leibnitz March 7th 1693 the other by D^r Wallis Apr. 10th 1695. These Letters were examined — — — — unattested papers. For making good his accusation of the Committee as if they had ommitted what

made against M^r Newton, he lately charged them with two such omissions but was mistaken in them both. The Original letters have been seen by foreigners & nothing material has been found omitted.

– and that before printing they had also been examined by those who knew the hands; & ought still to be kept by the R. Society for justifying what had been printed out of them. He added further that he did not intermeddle with those Originals nor so much as produce what papers he had in his own custody because he would not

<379r>

Historical Annotations on the Elogium of M^r Leibnitz.

1 Pag. 44. lin. 1. Fluxions are not differences. They are quantities of another kind, & M^r Leibnitz hath no marks nor name for them. They are motions or velocities of increase & not parts of the increasing quantity. What M^r Leibnitz calls Differences M^r Newton calls Moments & parts.

2 Ib. l. 3, 4, 5. When the Marquess de l'Hospital represented that the characters of M^r Leibnitz were more convenient then those of M^r Newton, he had seen only the characters used by M^r Newton in the second Lemma of the second book of his Principles & therefore meant no other.

3 Ib. l. 22. M^r Fatio was not the first. D^r Wallis, so soon as he heard that the method of M^r Newton was spreading in Holland by the name of the differential method of M^r Leibnitz, wrote in the Preface to the first volume of his works published in April 1695, that M^r Newton in his two Letters of Iune 13 & Octob. 24 1676, had explained to M^r Leibnitz the method of fluxions or differences found by him ten years before or above; that is, in the year 1666 or before.

5 Pag. 48. lin. 15. M^r Newton was accused of Plagiarism in the Acta Eruditorum for January 1705 pag. 34 & 35. & the accusation was justified by M^r Leibnitz himself in his Letter to D^r Sloan dated 29 Jan 1711. And for obviating this accusation the *Commercium Epistolicum* insinuated that the contrary was more to be suspected, M^r Leibnitz having seen several of M^r M^r Newtons Papers in the year 1676.

4 Pag. 48. lin 6. A Letter writ by M^r Leibnitz in autumn 1715 being handed about among persons of note in London, M^r Newton was pressed to return an answer, & did so by a Letter dated Feb. 26. 1716 & M^r Leibnitz replied Apr. 9th 1716, but sent his Reply to Paris (with Copies of the two former Letters) to be communicated to his friends there before it came to London. For which reason M^r Newton wrote to him no more, but only shewed privately to a few of his friends what might have been writ in answer.

6 Pag. 49. lin. 20. M^r Leibnitz pretended that the Committee of the R. Society had in the *Commercium Epistolicum* omitted many things which ought to have been published, & upon that pretence proposed to print a new *Commercium* impartially, & in autumn 1714 desired that the original Letters in the custody of the R. Society might be sent to him for that purpose. M^r Newton replied before the R. Society that the *Commercium* was printed out of Originals which had been constantly in their Archives & those of M^r Collins & had been also examined by those who knew the hands, & that they ought still to be kept by the R. Society for justifying what had been printed out of them. He added further that he did not print the *Commercium* himself nor so much as produce the papers which he had then in his own custody, because he would not make himself a witness in his own cause. And to prove the truth of what he said he produced two Letters written to himself the one by M^r Leibnitz March 7th 1693, the other by D^r Wallis Apr. 10 1695. These Letters were examined before the R. Society by them who knew the hands & then they were read & laid up in the Archives of the R. Society as authentic. At the same time it was allowed that if M^r Leibnitz had any Letters in his custody which he thought material to be published & pleased to send them to any friend in London whom he could trust with them that they might be publickly examined by those who knew the hands & attested copies be taken of them: they might then be published either in the *Ph. Transactions* or by M^r Leibnitz himself. But nothing has been sent. On the contrary M^r Leibnitz having been since pressed to ‡ < insertion from f 379v > ‡ to return a

direct Answer to the *Commercium*, replied in his Letter of April 9th 1716 Pour repondre done de point en point a l'Ouvrage publié contre moi, il falloit une autre Ouvrage aussi grand pour le moins que celui là, il falloit entrer dans une grand detail de qualite de Minuties passees il y a 30 ou 40 ans dont le ne me souvenois gueres, il me falloit chercher mes vieilles Lettres, dont plusieurs sont perdues, outre que le plus souvent, je nay pas gardé les Minutes des miennes, et les autres sont ensevelies dans une grand tas de Papiers, qui je ne pouvois de brouiller qui avec du Temps & de la patience. Mais je n'en avois gueres le loisir, etant chargé presentmant d'Occupations d'une toute autre nature. And the author of the Elogium of M^r Leibnitz published in the *Acta Eruditorum* for July 1717 pag. 335 writes: Quo tamen perspicerent intelligentes quid de tota illa controversia sentiendum sit, Commercio Epistolico Anglorum aliud quoddam suum idemque amplius opponere decreverat, & paucis ante obitum diebus Cl. Wolfio significavit se Anglos famam ipsius lacescentes reipsa refutaturum: quam primum enim a laboribus historicis vacaturus sit, daturum se aliquid in *Analysi* prorsus inexpectatum & cum inventis quæ hactenus in publicum prostant, sive Newtoni, sive aliorum nil quicquam affine habens. We were to stay till his Historical labours were at an end, & then he would confute the English; he doth not say with a new *Commercium*, but with a new surprizing invention in Analysis of another kind ; & this he wrote a few days before his death. And perhaps he meant his *Alphabetum cogitationum humanarum* or something like it. The author of the Elogium tells us of an aliud quoddam [*Commercium*] idemque amplius, but M^r Leibnitz himself wrote only that he would refute the English (he doth not say with a new *Commercium* as the Author of the Elogium seems to understand but) with a new surprizing invention in Analysis of another kind; & this he wrote a few days before his death, & we were to stay for this till his historical labours should be at an end. < text from f 379r resumes >

Pag 51. lin. 22. He returned from London to Paris in Febr. 1673 & began to study the higher Geometry a few months after. The *Horologium oscillatorium* of M^r Huygens was published <379v> in April 1673. M^r Leibnitz soon after read this Book & then some others [began presently after to study the higher Geometry] & then fell upon his *Triangulum characteristicum* & his many Theorems, & went on to his Method by Analytical Tables of Tangents, & by his Combinatory Art. And this was the top of his skill when he wrote his Letter of 27 Aug. 1676. For in that Letter he said of one part of this method: Nihil est quod norim in tota Analysi momenti majoris. And of another part: Cujus vim ac potestatem nescio an quisquam hactenus sit consecutus. Ea vero nihil differt ab Analysi illa suprema ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum. And soon after this, he fell upon the Differential Analysis.

Pag. 53. l. 6. A copy of M^r Newton's Letter dated 10 Decem. 1672 was not sent to him before June 26, 1676.

<380v>

M^r Leibnitz [Might be acquainted with M^r Hugen in 1672 but he] came to London in the beginning of the year 1673 & returned to Paris in March following & kept a correspondence with M^r Oldenburg about Arithmetical questions till June 8 following, & hitherto he was unacquainted with the higher Geometry. The *Horologium oscillatorium* of M^r Huygens was published in April 1673 & when he began to study the higher Geometry he read first this book & then the works of Paschal & Gregory of S^t Vincent & then fell upon his *Triangulum characteristicum* & his many Theorems, & went on to his method by Analytical Tables of Tangents & the combinatory Art. And this was the top of his skill when he wrote his Letter of 27 Aug. 1676. — — — humanarum. In the same Letter he used the common Algebra where the Differential calculus would have been proper had he then known it, & said that inverse problems of Tangents could not be reduced to equations. It was therefor after the writing of this Letter that he fell upon the Differential calculus. In October following he was again in England where he met with Barrows works &c & Newton's Letter of 24 Octob 1676. [And James Bernoulli in the *Acta Eruditorum* for Jan 1691 pag 14 wrote, Qui calculum Barrovium — intellexerit, alterum a Dn. L. inventum ignorare vix poterit; utpote qui in prior illo fundatus est & nisi forte in differentialium notatione & operationis aliquo compendio ab eo non differt. And the Marques de l'Hospital in his Preface that D^r Barrow's calculus taught not how to take away fractions & surds; and that where he & others left off M^r Leibnitz began. [And M^r Newton in his Letters of 10 Decem. 1672 & 24. Octob. 1676 that his method proceeded in equations involving surds.] But the Marquess had not seen M^r Newtons Letters of 10 Dec. 1672 & 24 Octob. 1676] & saw the correspondence of Gregory & Newton in the hands of Collins.

Criticism of Fontenell {CI}{illeg}

to Varignon {endof} 1718 or beginning of 1719

Sir

I am much obliged to you for your Letter in order principally to bring M^r Iohn Bernoulli & me to a better understanding. M^r Leibnitz has attributed to him the Letter of Iune 7th 1713 inserted into a flying Paper dated 29 Iuly following & it that Letter his he cites himself by the title of Eminens quidam Mathematicus & took upon him to give as judge between M^r Leibnitz & the Committee of the R. Society & in that sentence denied (in effect that when I wrote the book of Quadratures, which I can assure you was above 40 years ago, except the Introduction & Conclusion) I did not understand the first Proposition of the Book

† And I return you my hearty thanks for your kind present of the Elogia of the Academiciens. In that of M^r Leibnitz M^r Fontenelle has been very candid. There are some mistakes in matter of fact but not by designe. I recon that M^r Fontenell was not sufficiently informed. He seems to follow the Marquess de l'Hospital in saying that the differential characters are the more convenient, but the Marquess whom I recon a very candid person when he published his book of infinitely littles had seen no other characters of mine then those in the Book of Principles. In pag. 44 lin 22 he should have mentioned D^r Wallis before M^r Fatio if he had known what the Doctor wrote in the Preface to the two first Volumes of his works. Pag. 48 lin 15 he should have mentioned that I was first accused of Plagiarism in the Acta Eruditorum for Ianuary 1705 pag. 34 & 35 & by M^r Leibnitz himself in his Letter to D^r Sloan dated 29 Ian. 1711, had he been advised of it. For this was in opposition to my saying in the Introduction to the book of Quadratures that I found the method of fluxions gradually in the years 1665 & 1666. Pag. 49, l. 20 they have given him a wrong information from Germany. For in the Elogium of M^r Leibnitz printed in the Acta Eruditorum for Iuly 1717 page 335 were are told that a few days before his death he wrote to M^r Wolfius that he would refute the English after his historical labours were over. Pag. 51 lin. 22 M^r Leibnitz became acquainted with M^r Hugens in 1672 came to London & returned to Paris in the beginning of 1673, kept a correspondence with M^r Oldenburg about Arithmetical questions till Iune following & hitherto knew nothing of the higher Geometry. The Horologium oscillatorium of M^r Hugens was published in April 1673 & he begun with this book. And it doth not appear that he knew any thing of the Differential Method before his second coming to London at which time he met with D^r Barrows works & my Letter of 24 Octob 1676 & saw in the hands of Collins the Letters & Papers sent to him by me & Gregory Pag. 53. lin. 6, the Letter here referred unto was writ by Collins in 1672 but not seen by M^r Leibnitz till 1676. [Pag. 53 l. 22 M^r Leibnitz is blamed here in point of candor for concealing what he knew of other mens inventions. He received Gregories series twice from England & by a copy of Gregories Letter dated 15 Feb 1671 knew that Gregory had invented it before the date of that Letter & yet printed it as his own in 1682 without making any mention that he had received it from London & that it was first invented by Gregory. He might have said that he invented it himself before he knew that Gregory had invented it, but he should not have concealed his having receiving it from London, & much less should he have concealed his knowledge that Gregory had invented it before him. And so when he printed the elements of the Differential Method he should not have concealed his knowledge of what he seen in my Letters concerning such a method. For its very plane by his Letter of 21 Iune 1677 that he then understood that my method was of the same kind with his own. And much less should he have concealed his knowledg after I had in the second Lemma of the second Book of Principles set down the Elements of my method & in the Scholium upon that Lemma explained the sentence in which I had couched that Method in my Letter of 24 Octob. 1676 where I said that I had explained it in a Tract written five years before. He knew therefore that I was before him & should not have concealed his knowledge. In his Letter of 7 March 1693 he acknowledged of his own that I had shewed by my Book of Principles that I had such a method when I wrote it & to deny this afterwards proceeded from the want of candor.] D^r Barrows works & Gregories Letters of 5 Sept 1670, & 15 Sept 1671 & mine of 10 Decem 1672 <381v> 13 Iune 1676 & 24 Octob 1676 were all of them seen by him when he was the second time in England or within less then four months before & it doth not appear that he understood the Differential Method before he saw all these Papers. And the Committee of the R. Society have

affirmed nothing more then that he saw them before he had had the Method, & that in them the Method of fluxions it sufficiently described to any intelligent person. But if the friends of M^r Leibnitz beleive that he did not understand those Papers the Gentlemen of

When the Commer. Epist. came abroad M^r Leibnitz appealed from the Committee of the R. Society to a primary & impartial Mathematician & the judgement of that Mathematician dated 7 Iuly 1713 was inserted into a flying paper which was dated 24 Iuly 1713 & was written by somebody who knew what passed at Paris between M^r Leibnitz & M^r Hugens 37 years before. And the Mathematician cited M^r Iohn Bernoulli by the name of an Eminent Mathematician [& [thereby denied that M^r Iohn Bernoulli was the Iudge to whom M^r Leibnitz appealed,] but M^r Leibnitz has since told us that M^r Bernoulli was the Mathematician. This Iudg in that Paper of 7 Iune 1613 has given a Iudgment which our people look upon as if he had said that I did not understand the first Proposition of my book of Quadratures & that in the Introduction to that book & in the 2^d Lemma of the 2^d Book of Principles & the Scholium upon it I said nothing of the Method of Fluxions because there are no prickt letters in them, & that in the years 1665 & 1666, in which I said in that Introduction that I found the Method of fluxions gradually, I did not so much as dream of it. nor understood the Method till it was commonly known by others. And M^r Leibnitz hath subscribed to this judgment & endeavoured to support it , by telling the world that it is the judgment of a primary & impartial Mathematician & that is the judgment of M^r Iohn Bernoulli himself, & by challenging the English Mathematicians to solve a Problem of that great man. And that he beleived that I found hte Method by my self till M^r Bernulli gave a contrary judgment. And thus M^r Leibnitz has made M^r Iohn Bernulli the Principal in this controversy.
