Copy by Sir Isaac Newton Newton of Leibniz's letter to Pierre Rémond De Monmort

Author: Isaac Newton

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To Montmort

Monsieur

Ie prends la liberté de vous envoyer le peces d'un procés nouveau ou renovellé, Puisque vous avés la bonté de vous interesser pour moy. M l'Abbé Condi qui avoit fait des demarches de mediateur, m'a envoyé maintenant un cartel de defy de la part la lettre & le Ie réponds à la lettre de l'un et de l'autre par la lettre & le P.S. a M. l'Abbé c'est à dire à M. Newton dans la Lettre, et à M. l'Abbé dans la Postscriptum, et je suis bien aise, Monsieur, que vous et vos amis, et particulierement M. l'Abbé Varignon, et d'autres personnes de l'Academie Royale des Sciences, à qui il en voudra faire part en soyent informés. Ie vous supplie de garder la copie des Lettres de M. l'Abbé et de M. Newton, et d'envoyer may reponse à M. l'Abbe. Vous voyes bien Monsier, pourquoy I'ay voulu me servir de la voye de la France, au lieu de repondre directement d'icy. Si vous croyés, Monsieur, que cette reponse vaille la peine qu'on en garde aussi une copie; cela depend de votre judgment. Mais je ne voudrois pas qu'on en imprimât rien sans mon consentment. Ie ne fais point d'autres reflexions sur ces Lettres; on en fera assez sans moy.

I'ay pris la liberté de vous dire dermierement que je souhaiterois que l'Academie Royale des Inscriptions vît mon Discourse <u>de Origine Francorum</u>, et que je voudrois que cela se fît avant qu'on en parlât dans les Memoires de Trevoux. Ie laisse la disposition de cela â vos bontes.

Il y a deja du temps, Monsieur, que je vous ay envoyé mon sentiment sur l'livre fait contre le P. Malbranche, peut étre que les Reverends Peres Iesuites aussi bien que les amis de ce Pere ne seront point fachés de le voir. Ce que I'ay crû conform a la verité m'a fait prendre le parti du milleu.

On me mande de Vienne, que M. le Duc d'Arenberg y reviendra. Ainsi je crois qu'ill laissera M. Sully en France. Faites moy la grace de me dire, Monsieur, si Monsieur le Duc d'Arenberg a eu une copie de mon papier que M. Sully vous avoit apporté.

Au reste je ma rapporte à ma precedent, et je sui avec zela

    Monsieur

Votre tres humble et tres obeissant Serviteur

    Leibniz

P.S.

Ie vous envoye la Lettre a M. l'Abbé Conti sub <u>sigillo volante</u>, et il n'est point necessairé que vous la firmies. Ie veux bien qu'on sache qu' vous l'avés veue, Monsieur, & que Ie suis bien aise qué vous en soyes informé.

Hanover ce 9 d'Auril. 1716.[1]

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Observations upon the foregoing Epistole.[2]

Mr Leibnitz by his Letter of 29 Decem 1711 justified the Passage in the Acta Eruditorum for Ianuary 1705 pag. 34 & 35, & thereby made it his own, & now endeavours in vain to excuse it, pretending that the words <u>adhibet semperque adhibuit</u> are malitiously interpreted by the Word <u>substituit</u>. But in the interpretation which he would put upon the place he omitts the words <u>igitur</u> & <u>quemadmodum</u>, the first of which makes the words <u>semperque adhibuit</u> a consequence of what went before & the latter makes them equipollent to <u>substituit</u> neither of which can be true in the sense which Mr Leibnitz endeavours now to put upon the words.

In his Letter of 4 March st. n. 1711 he pressed the R. Society to condemn Dr Keill without hearing both parties, & when the Doctor put in his Answer, Mr Leibnitz refused to give his reasons against the Doctor & called it injustice to expect it from him, & yet persisted in pressing them against him & thereby put them upon a necessity of appointing a Committee to search out old papers & give their opinion upon them. If they did it without him it was his own fault: he was for over-ruling them, and called it injustice to expect that he should defend his candor & plead before them. If they gave him no opportunity to except against any of the Committee it was because he refused to be heard & they had sufficient authority to appoint a Committee without him, & he had no right to except against what they did for their own satisfaction. If they have not yet given judgment against him; it is because the Committee did not act as a Iury, nor the R. Society as a formal Court of justice. The Committe examined old Letters & Papers & gave their opinion upon them alone, & left room for Mr Leibnitz to produce further evidence for himself. And it is sufficient that the Society ordered their Report with the Papers upon which it was grownded to be published, & that Mr Leibnitz in all the three years & four months which are since elapsed has not been able to produce any further proof against Dr Keill then what was then before them.

Mr Leibnitz saith that the Letter which I call defamatory being no sharper than that which has been published against him I have no reason to complain. But the sharpness of the Letter lies in accusations & reflexions without any proof which way of writing is unlawful & infamous: the sharpnes of the Commercium lies in facts which are lawfull & fit to be produced. The Letter was published in a clandestine back-biting manner (as defamatory papers use to be) without the name of the author or Mathematician or Printer or City where it was printed, & was dispersed above two years before we were told that the Mathematician was Iohn Bernoulli, the Commercium was printed openly at London by order of the R. Society.

The Mathematician to whom Mr Leibnitz appealed from the R. Society, I called a Mathematician or pretended Mathematician, not to disparage the skill of Mr Bernoulli, but because the Mathematician in his Letter of the 7th of Iune 1673 cited Mr Bernoulli as a person distinct from himself, & Mr Leibnitz lately caused that Letter to be reprinted without the citation & tells us that the Mathematician was Mr Bernoulli himself, & whether the Mathematician or Mr Leibnitz is to be believed I do not know.

He complains that the Committee have gone out of the way in falling upon the method of Series: but he should consider that both methods are but two branches of one general method. I joyned them together in my Analysis. I interwove them in the Tract which I wrote in the year 1671 as I said in my Letters of the 10 Decem 1672 & 24 Octob 1676. In my Letter of 13 Iune 1676, I said that my method of series extended to almost all Problems but became not general without some <447v> other methods, meaning (as I said in my next Letter) the method of fluxions, and the method of arbitrary series, & now to take those other methods from me is to restrain and stint the method of series & make it ceas{e} to be general. In my Letter of 24 Octob. 1676 I called all these method{s} together my general method. See the Commercium Epistolicum pag. 86. lin. 16. And if Mr Leibnitz has been tearing this general method in pieces & taking from me first one part

& then another part whereby the rest is maimed, he has given a just occasion to the Committee to consider the whole. It is also to be observed that he is perpetually giving testimony for himself, & its allowed in all Courts of justice to speak to the credit of the witness.

He represents that the Committee of the R. Society have omitted things which made against me & printed every thing which could be turned against him by strained glosses, & to make this appear he produced in his last Letter but one an instance of my ignorance omitted by them but confesses now that he was mistaken in saying that it was omitted & saith that he will cite another instance. In one of my Letters to Mr Collins he saith that I owned that I could not find the second segments of Sphæroids & that the Committee have omitted this. If they had omitted such a passage I think they would have done right it being nothing to the purpose. But on the contrary Mr Collins in a Letter to Mr Iames Gregory 24 Decem. 1670 & in another to Mr Bertet 21 Feb 1671 both printed in the Commercium Epistolicum p. 24, 26 wrote that my method extended to second segments of round solids. And Mr Oldenburg wrote the same thing to Mr Leibnitz himself 8 Decem. 1674. See the Commer. Epist. p. 39. So you see that Mr Leibnitz hath accused the Committee of the R. Society without knowing the truth of his accusation & therefore is guilty of a misdemeanour. The Committee were so far from acting corruptly against Mr Leibnitz that omitted his ignorance of Geometry in those days & several other things which made strongly against him, such as were the two Letters in my custody, & the Paragraph in the Preface to the two first Volumes of Dr Wallis's works relating to this matter & that a copy of Gregories Letter of 5 Septem. 1670 was sent to Mr Leibnitz in Iune 1676 amongst the extracts of Gregories Letters.

Mr Leibnitz acknowledges that when he was in London the second time he saw some of my Letters in the hands of Mr Collins, & he has named two of those which he then saw, vizt that dated 24 Octob 1676, & that in which he pretends that I confessed my ignorance of second segments & no doubt he would principally desire to see the Letter which conteined the chief of my series & particularly those two for finding the Arc by the signe & the sine by the Arc with the Demonstration thereof which a few months before he had desired Mr Oldenburgh to procure from Mr Collins, that is, the <u>Analysis per Æquationes numero terminorum infinitas</u>. But yet he tells us that he never saw where I explained my method of fluxions & that he finds nothing of it in the Commercium Epistolicum where that Analysis & my Letters of 10 Decem. 1672, 13 Iune 1676 & 24 Octob. 1676 are published.

He saith also that he never saw where I explain the method claimed by me in which he assumes an arbitrary series: If he pleases to look into the Commercium Epistolicum pag. 56 & 86 he will there see that I had that Method in the year 1676 & five years before. Mr Leibnitz might find it himself but not so early, & second Inventors have no right.

He pretends that in my book of Principles pag. 253, 254, I allowed him the invention of the calculus differentialis independently of my own, & that to attribute this invention to my self is contrary to my knowledge there avowed. But in the Paragraph there referred unto, I do not find one word to this purpose. On the contrary I there represent that I sent notice of my method to Mr Leibnitz before he sent <448r> notice of his method to me, & left him to make it appear that he had found his method before the date of my Letter, that is, eight months at the least before the date of his own. And by referring to the Letters which passed between Mr Leibnitz & me ten years before, I left the Reader to consult those Letters & interpret the Paragraph thereby. For by those Letters he would see that I wrote a Tract of that method & the method of Series together five years before the writing of those Letters that is in the year 1671. And these hints were as much as was proper in that short Paragraph, it being besides the designe of that Book to enter into disputes about these matters.

He saith that when he was in London the first time which was in Ian & Feb 1673 he knew nothing of infinite series nor of the avanced Geometry nor was then acquainted with Mr Collins as some have maliciously feigned. But who hath feigned this or what need there was of feigning it I do not know. At that time Dr Pell gave him notice of Mercators series for the Hyperbola & he carried Mercators Book with him to Paris tho he did not yet understand the higher Geometry. And any of those to whom Mr Collins had communicated mine & Gregories Series might give him notice of them without his being acquainted with Mr Collins.

He saith that after his coming from London to Paris his first Letters were of other matters then Geometrical till Mr Huygens had instructed him in these things & that he found the Arithmetical Quadrature of the Circle towards the end of the year 1673 & began to write of it to Mr Oldenburg the next year, & found the general method by arbitrary series a little after & the differential Calculus in the year 1676 deducing it from the series of numbers, & that in his Letter of 27 Aug. 1676 by the words <u>certa Analysi</u> he meant the differential Analysis. And am not I as good a Witness that I found the methods of fluxions in the year 1665 & improved it in the year 1666 & that before the end of the year 1666 I wrote a small Tract on this subject which was the grownd of that larger Tract which I wrote in the year 1671 (both which are still in my custody,) & that in this smaller Tract tho I generally put letters for fluxions as Dr Barrow in his Method of Tangents put Letters for differences, yet in giving a general Rule for finding the Curvature of Curves I put the letter x with one prick for first fluxions drawn into their fluents & with two pricks for second fluxions drawn into the square of their fluents, & that when I wrote the larger of those two Tracts I had made my Analysis composed of those two methods so universal as to reach to almost all sorts of Problemes as I mentioned in my Letter of 13 Iune 1676.

In the year 1684 Mr Leibnitz published only the Elements of the Calculus differentialis & applied them to questions about Tangents & Maxima & minima, as Fermat Gregory & Barrow had done before, & shewed how to proceed in these Questions without taking away surds, but proceeded not to the higher Problemes. The Principia mathematica gave the first instances made publick of applying this Calculus to the higher Problemes & I understood Mr Leibnitz in this sense in what I said concerning the Acta Eruditorum for May 1700 pag. 206. But Mr Leibnitz observes that what was there said by him relates only to a particular artifice de maximis et minimis with which he there allowed that I was acquainted when I gave the figure of my vessel in my Principles. But this Artifice depending npon the differential method as an improvement thereof, & being the artifice by which they solved the Problemes which they value themselves most upon (those of the linea celerrimi descensus & the linea Catenaria & Velaria) & which Mr Leibnitz there calls a method of the highest moment & greatest extent, I content my self with his acknowledgement that I was the first that proved by a specimen made publick, that I had this artifice.

In the year 1689 Mr Leibnitz published the principal Propositions of this Book as his own in three papers called Epistola de Lineis Opticis, Schediasma de resistentia Medij & motu projectilium gravium in Medio resistente, et Ten <448v> tamen de motuum cœlestium causis, pretending that he had found them all before that book came abroad. And to make the principal Proposition his own adapted to it an erroneous Demonstration. And this was the second specimen made publick of applying the method to the higher Problemes. Hitherto this method made no noise, but within a year or two it began to be celebrated.

Dr Barrow printed his differential method of Tangents in the year 1670. Mr Gregory from this method compared with his own deduced a general method of Tangents without calculation & by his Letter of 5 Sept. 1670 gave notice thereof to Mr Collins. Slusius in November 1672 gave notice of the like method to Mr Oldenburgh. In my Letter of 10 Decem. 1672 I sent the like method to Mr Collins & added that I mentioned it to Dr Barrow when he was printing his Lectures, & that I took the method of Gregory & Slusius to be the same with mine, & that it was but a branch or Corollary of a general method which without any troublesome calculation extended not only to tangents but also to other abstruser sorts of Problemes concerning the crookedness, areas, lengths, centers of gravity of Curves &c & did all this even without freeing equations from surds; & I added that I had interwoven this method with that of infinite series, meaning in the Tract which I wrote in the year 1671. Copies of these two Letters were sent to Mr Leibnitz by Mr Oldenburge in the Extracts of Gregories Letters in Iune 1676, & Mr Leibnitz in his Letter 21 Iune 1677 sent nothing more back then what he had notice of by these two Letters, namely, Dr Barrows Differential Method of Tangents disguised by a new notation, & extended to the method of Tangents of Gregory & Slusius, & to Equations involving surds, & to Quadratures. But this is not the case between me and Dr Barrow. He saw my Tract of Analysis in the year 1699 & was pleased with it. And before his Lectures came abroad I had deduced the method of Tangents of Gregory & Slusius from my general method. But Mr Leibnitz in those days knew nothing of the higher Geometry nor was yet acquainted with the vulgar Algebra.

In his Letter of 27 Aug. 1676, he wrote thus. Quod dicere videmini plerasque difficultates (exceptis problematibus Diophantæis) ad series infinitas reduci, id mihi non videtur. Sunt enim multa usque adeo mira et implexa ut neque ab æquationibus pendeant neque ex Quadraturis. Qualia sunt ex multis alijs Problemata methodi tangentium inversæ. And when I answered that such Problems were in my power he replied (in his Letter of 21 Iune 1677) that he conceived that I meant by infinite series but he meant by vulgar equations. See the Answer to this in the Commercium Epistolicum p. 92.

He saith that one may judge that when he wrote his Letter of 27 Aug. 1676, he had some entrance into the differential Calculus because he said there that he had solved the Probleme of Beaune certa Analysi by a certain Analysis. But what if that Probleme may be solved certa Analysi without the Differential method? For no further Analysis is requisite then this; That the Ordinate of the Curve desired increases or decreases in Geometrical Progression when the Abscissa increases in Arithmetical, & therefore the Abscissa & Ordinate have the same relation to one another as the Logarithm & its Number. And to infer from this that Mr Leibnitz had entrance into the differential method is as if one should say that Archimedes had entrance into it, because he drew tangents to the Spiral, Squared the Parabola & found the proportion between the sphere and the cylynder, or that Cavallerius Fermat & Wallis had entrance into it because they did many more things of this kind.

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Sir

Mr Leibnitz by his Letter of 29 Decem 1711 justified the Passage in the Acta Eruditorum for Ianuary 1705 pag 34 & 35, & thereby made it his own, & now endeavours in vain to excuse it, pretending that the words adhibet semperque adhibuit are malitiously interpreted by the word substituit. But in his interpretation he omits the words igitur & quemadmodum which makes the words semperque adhibuit a consequence of what went before, & the latter makes them equipollent to substituit, neither of which can be true in the sense which Mr Leibnitz now endeavours to put upon those words. And therefore he & his friends are the aggressors.

9 He pretends that in my book of Principles pag. 253, 254 I allowed him the invention of the calculus differentialis independently of my own & that to attribute this invention to my self is contrary to my knowledge there avowed. But I do not find one word in all that book to this purpose. On the contrary, in that very place which he cites, I said that I sent him notice of my method before he sent me notice of his, & by the Letters it appears that I sent him notice that I had the method six years before he sent me notice that he had it, & that he did not send me notice of his method till some months after had notice of mine & by the notice which I gave him he had light into my method so far as to be able to compare the methods & see the likeness. In the year 1685 when I was writing the book of Principles, Mr Craight brought to me the Acta eruditorum for October preceding & desired me to explain to him the elements of the calculus differentialis conteined therein & I did so & told him that the method was mine as would appear if the Letters which had passed between Mr Leibnitz & me a few years before were published & Mr Craige is still alive & remembers this; & therefore I could not mean in the Book of Principles to allow that Mr Leibnitz was the first inventor or found the Method without receiving some light from me. All that he can pretend to is in that book I did not accuse him. For he did not then pretend to be the first inventor, & I have indoustriously avoided contentions all my life.

He represents that the Committee of the R. Society have omitted things which made against me & printed every thing which could be turned against him by strained glosses, & to make this appear he produced in his Last letter but one, an instance of something omitted but confesses now that it was not omitted & offers at another instance: He saith that in one of my Letters to Mr Collins he means that of   1672 not yet printed) I owned that I could not find the second segments of Spheroids & that the Committee have omitted this. If the Committee had omitted such a passage I think they would have done right it being nothing to the purpose. But on the contrary Mr Collins in a Letter to Mr Iames Gregory dated 24 Decem. 1670 & printed in the Commercium pag 24 wrote that my method extended to the second segments of round solids. And Mr Oldenburg wrote the same thing to Mr Leibnitz himself 8 Dec. 1674. See the Comerc. p. 39. And in the very Letter which he mentions & which was writ in the year 1672 I give the dimensions of the second segments of

the spheroid in a series, & only say that the series is not fit for gauging of vessels. And such a series I set down also in my Letter of 13 Iune 1676 & the same was publilshed in the Commercium p. 55. So you see that Mr Leibnitz has only dreamt that the Committee have acted partially. They were so far from acting partially that I my self forbore to give them two letters which were in my custody & made against him

In his Letter of 4 Mar. st. n 1711 pressed the R. Society to condemn Dr Keill without hearing both parties, & when the Doctor put in his Answer, Mr Leibnitz refused to give his reasons against the Doctor & called it injustice to expect it from him, & thereby put the R. Society upon a necessity of appointing a Committee to search out old papers & give their <449v> opinion upon them. If they did it without him it was his own fault, he was for over-ruling them & called it injustice to expect that he should defend his candor & plead before them. If they gave him no opportunity to except against any of the Committee, it was because he refused to be heard & they had sufficient authority to appoint a Committee without him. If they have not yet given judgment against him it is because the Committee did not act as a Iury nor the R. Society as a formal Court of justice. The Committee examined old Letters & Papers gave their opinion only upon them & left room for Mr Leibnitz to produce further evidence for himself. And it is sufficient that the Society ordered their Report with the Papers upon which it was grounded & to be published & that Mr Leibnitz in all the three years & four months which are since elapsed has not been able to produce any further proof against Dr Keill then what was then before them, & that Mr Newton himself forbore to produce some Letters against Mr Leibnitz which he had then in his custody, < insertion from f 450r > < text from f 449v resumes >

4 The Mathematician to whom Mr Leibnitz appealed from the R. Society I called a Mathematician or pretended Mathematician, not to disparage the skill of M^{r}Bernoulli but because the Mathematician in his Letter of 7 Iune 1673 cited Mr Bernoulli as a person different from himself, & Mr Leibnitz has lately caused the Letter to be reprinted without the citation & tells us that the Mathematician was Mr Bernoulli himself, & whether the Mathematician or Mr Leibnitz is to be believed I do not yet know.

3 Mr Leibnitz saith that the Letter which I call defamatory being no sharper then that which has been published against him I have no reason to complain. But the sharpness of the Letter lies in accusations & reflexions without any proof; which way of writing is unlawfull & infamous: the sharpness of the Commercium lies in facts which are lawfull & fit to be produced. The Letter was published in a clandestine back-biting manner (as defamatory Libels use to be) without the name of the author or Mathematician or printer or city where it was printed, & dispersed above two years before we were told that the Mathematician was Iohn Bernoulli: the Commercium was printed openly at London by order of the R. Society.

10 He saith that when he was in London the first time, he knew nothing of infinite series nor of the avanced Geometry nor was then acquainted with Mr Collins as some have maliciously feigned. But who has feigned this or what need there was to feign it I do not know Dr Pell at that time gave him notice of Mercators series for the Hyperbola & he might have notice of mine for the circle either at London or Paris without being acquainted with Mr Collins.

11 He saith that after his coming from London to Paris his first Letters were of other matters till Mr Hugens had instructed him in these things & that he found the arithmetical quadrature of the circle towards the end of the year 1673 & began to write of it to Mr Oldenburg the next ye < insertion from f 450r > ar, & found < text from f 449v resumes > the general method by arbitrary series , & the differential calculus in the year 1 < insertion from f 450r > 676 < text from f 449v resumes > , deducing it from the differences of the series of numbers & that in his Letter of 27 Aug. 1676 by the words <u>certa Analysi</u> he meant the differential Analysis. And am not I as good a witness that I found the method of fluxions in the year 1665 & improved it in the year 1666, & that before the end of the year 1666 I wrote a small Tract on this subject which was the grownd of that larger Tract which I wrote in the year 1671, both which are still in my custody & that in this smaller Tract tho I generally put letters for fluxions as Dr Barrow in his method of Tangents put letters for differences, yet in giving a general Rule for finding the curvature of curves, I put the letter x with one prick for first fluxions & with two pricks for second fluxions < insertion from between the lines of f 449v > , & that when I w <450r> rote the <449v> larger of those two Tracts I had made my Analysis composed of those two

methods so universal as to reach <450r> to almost all <449v> sorts of Problemes as I mentioned in my Letter of 13 Iune 1676. < text from f 449v resumes >

13 In the year 1684 Mr Leibnitz published only the elements of the Calculus differentialis & applied them to questions about tangents & maxima & minima, but proceeded not to the higher <450r>

13 Dr Barrow printed his differential method of Tangents in the year 1670. Mr Gregory from this method compared with his own deduced a general method of Tangents without calculation & by his Letter of 5 Sept 1670 gave notice thereof to Mr Collins. Slusius in November 1672 gave notice of the like method to Mr Oldenburge. In my Letter of 10 Decem. 1672 I sent the like method to Mr Collins, & added that I mentioned it to Dr Barrow when he was printing his Lectures & that I tooke the method of Gregory & Slusius to be the same with mine, & that it was but a branch or rather a Corollary of a general method which without any troublesome calculation extended not only to tangents but also to other abstruser sorts of Problemes concerning the crookednesses, areas lengths centers of gravity of Curves &c and did all this even without freeing equations from surds: & I added that I had interwoven this method with that of infinite series, meaning in the Tract which I wrote in the year 1671. Copies of these two Letters were sent to Mr Leibnitz by Mr Oldenburg in the Collection of Gregories Letters & Papers in Iune 1676, & Mr Leibnitz in his Letter of 21 Iune 1677 sent nothing more back then what he had notice of by these two Letters, namely Dr Barrows differential method of Tangents disguized by a new notation & extended to the method of Tangents of Gregory & Slusius & to Quadratures & equations involving surds. But this is not the case between me & Dr Barrow. He saw my Tract of Analysis in the year 1669 & found no fault with it. By other papers he knew that I had these methods above two years before And when he was publishing his Lectures I told him of my method of drawing tangents without computation, which was a step further then he had gone

14 Mr Leibnitz in his Letter of 27 Aug. 1676 wrote thus. Quod dicere videmini plerasque difficultates (exceptis Problematibus Diophantæis) ad series infinitas reduci, id mihi non videtur. Sunt enim multa usque adeo mira et implexa ut neque ab æquationibus pendeant neque ex quadraturis. Qualia sunt ex multis alijs Problemata methodi tangentium inversæ. And when I answered that such Problems were in my power he replied (in his Letter of 21 Iune 1677) that he conceived that I meant by infinite series but he meant by vulgar equations. See the Answer to this in the Commercium Epistolicum p. 92.

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5 He complains that the Commitee have gone out of the way in falling upon the method of series: but he should consider that both methods are but two branches of one general method. I joyned them together in my Analysis. I interwove them in the Tract which I wrote in the year 1671 as I said in my Letters of the 10 Decem 1672 & 24 Octob. 1676. In my Letter of 13 Iune 1676, I said that my method of series extended to almost all Problemes, but became not general without some other methods meaning (as I said in my next Letter) the method of fluxions & the method of arbitrary series, a method which depends upon that of fluxions; & now to take those other methods from me is to restrain & stint the method of series & make it cease to be general. In my Letter of 24 Octob. 1676 I called all these methods together my general method. See the Commercium Epistolicum pag. 86. lin. 16. And if Mr Leibnitz has been tearing this general method in pieces & taking from me first one part & then another part whereby the rest is maimed he has given a just occasion to the Committee to consider the whole. It is also to be observed that Mr Leibnitz is perpetually giving testimony for himself, & it's allowed in all Courts of justice to speak to the credit of the witness.

7 Mr Leibnitz acknowledges that when he was in London the second time he saw some of my Letters in the hands of Mr Collins & he has named two of those which he then saw vizt those dated   1672 & 24 Octob 1676 & no doubt he would principally desire to see the Letter which conteined the chief of my series & the method of finding them, or Demonstration with a few months before he had desired Mr Oldenburg to procure from Mr Collins that is, the Analysis per æquationes numero terminorum infinitas. But yet he tells us that he never saw where I explained my method of fluxions & that he finds nothing of it in the Commercium Epistolicum where that Analysis & my Letters of 10 Decem. 1672, 13 Iune 1676 & 24 Octob 1676 are published.

8 He saith also that he never saw where I explain the method claimed by me in which he assumes an arbitrary series. If he pleases to look into the Commercium Epistolicum pag. 56 & 86 he will there see that I had that Method when I wrote my Letters of 24 Octob. 1676 & 13 Iune 1676 & five years before. Mr Leibnitz might find it himself but not so early: & second Inventors have no right.

In my Analysis peræquationes numero terminorum infinitas I made use of the method of fluxions direct & inverse Dr Barrow communicated that Tract to Mr Collins in Iuly 1669, & printed his differential method of Tangents

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A supplement to the Remarks.

Mr Leibnitz by telling his story put me upon doing the like in the Remarks. And since in his letters to the Co{m}tess of Kilmansegger & Baron Bothmar he has told it at large, I will go on with my narration & leave you to beleive what you please.

<center><452v></center>

In my Analysis per series numero terminorum infinitas communicated by Dr Barrow to Mr Collins in Iuly 1669 I represented that I had the Method of finding the areas & lengths of Curves exactly when it might be done, that is by series which in those cases break off & become finite equations. And this proves that I had at that time the method of fluxions direct & inverse so far as it is explained in the first six Propositions of the Book of Quadratures. There being no other way of finding such series.

In the end of the year 1669 Mr Collins – – – – right to it.

In the beginning of the year 1666 I found the Theory of colours & in the year 1671 I was upon a designe of publishing it with the methods of Series & fluxions & for that end I wrote a Tract that year upon the Method of series & fluxions together, but did not finish it & for a reason mentioned in my Letter of 24 Octob. 1676 I laid aside my designe of publishing them till the year 1704.

After the year 1671 I intermitted these studies five years that is till Iune 1676, (as I mentioned in my Letters of 13 Iun & 24 Octob 1676 but before I left of I had made my Analytical Method so general as is described in those Letters & particularly in the Letter of 13 Iune 1676, where it represented that it extended to almost all Problems except perhaps some numeral ones like those of Diophatus. And this is that Method which I described in my Letter of 10 Decem 1672. This method consisted in reducing Problemes to equations finite or infinite & applying the method of fluxions either to the Equations or to any other conditions of the Problemes

In April 1673 the Horologium Oscillatorium of Mr Huygens came abroad & this was the first book which Mr Leibnitz studied in learning the higher Geometry, Mr Huygens introducing him.

Mr Leibnitz never pretended to have found the differential Analysis before this year, & these circumstances <452r> satisfy me that he did not find it till after the writing of this Letter.

In October following he came to London & there – – – – – Mr Leibnitz was of another opinion.

Mr Iames Bernoulli in the Acta Eruditorum – – – – know that I had it before him.

I wrote the book of Quadratures – – – – – before Mr Leibnitz understood the differential method.

At the request of Dr Wallis – – – – – nor are necessary to the method.

In writing the Book of Principles I made much use of – – – – into another order & form of words.

In the year 1677 I found the Demonstration of Keplers celebrated Proposition that the Planets revolve in Ellipses about the inferior focus of their Orbs. & communicated it to the mathematicians in London A.C. 1683 & published it in the book of Principles A.C. 1687 Mr Leibnits in the Acta Eruditorum for Febr. 1689 published a Demonstration of the same Proposition, pretending that he had also found it proprie Marte, but the Demonstration (for want of skill in the differential method) is an erroneous one. After this year Mr Leibnitz grew better acquainted with the Method, the two Bernoullis coming in to his assistance & in the years 1691, 1692 & 1693 this Method began to be celebrated.

The inverse method of fluxions is imperfect, & may be improved. The improvements are his who shall make them. [I clame nothing **{inome}** then not to be abused for what has been published as mine before] I have laid aside these studies many years ago & at present concern my self no further then to have it beleived that in what I have published in the books of Principles & Quadratures I have injured no body.

<453r>

A Supplement to the Remarks.[3]

Mr Leibnitz by telling his own story in his Letter of 9 April 1716 put me upon doing the like in my Remarks upon it. And since in his Letters to the Comtesse of Kilmansegger & the Comte de Bothmer he has told it at large, I will tell the rest of my story & & leave you to beleive what you please.

Mr Leibnitz was in London in the beginning of the year 1673, & going from thence to Paris in February, corresponded with Mr Oldenburg by Letters about Arithmetical matters till Iune following, being not yet acquainted with the higher Geometry. In April 1673 the Horologium oscillatorium of Mr Huygens came abroad, & this was the first book which he studied in learning the higher Geometry, Mr Huygens introducing him. In the year 1674 Mr Leibnitz after a years intermission renewed his correspondence with Mr Oldenburg & began to write to him about series for finding the Area or Circumference of a circle or any Arc whose sine was given. And in the year 1675 Mr Oldenburg sent from Mr Collins to Mr Leibnitz several of mine & Gregories series for the same purpose; & Gregory dying in the end of the year, Mr Collins at the request of Mr Leibnitz collected the Letters which he had received from Gregory, & Mr Oldenburg in Iune 1676 sent the Collection to Paris to be perused & returned, & it is now in the Archives of the R. Society In this Collection were a copy of Mr Gregories Letter of 5 Sept. 1670 & a Copy of my Letter of 10 Decem 1672 to Mr Collins: and by these Letters Mr Leibnitz had notice that Mr Barrow's method of Tangents was capable of improvement so as to give my general Analysis mentioned in my said Letter & that this Analysis proceeded without sticking at surds, & that I had interwoven it with the Method of Series, vizt in my Analysis per series abovementioned & in another Tract which I wrote upon them in the year 1671. Mr Leibnitz wrote also to Mr Oldenburg for the demonstration of some of my series, that is for the method of finding them, & promised him a Reward, & told him that Mr Collins could help him to it; & therefore he had heard that Mr Collins had my method of series, that is, my Analysis per series above mentioned. For I had sent my method of series to Mr Collins in no other Paper then that. But Mr Collins instead of sending a copy of that Tract joyned with Mr Oldenburg in solliciting me to write an Answer to Mr Leibnitz's Letter. And thereupon I wrote my Letter of 13 Iune 1676, which was sent to Mr Leibnitz at the same time with the aforesaid Collection. And Mr Leibnitz in his Answer dated 27 Aug. 1676, replied that he did not beleive that my Analysis was so general as I represented, there being many Problemes, & particulary the inverse Problemes of Tangents, not reducible to equations or quadratures. And in the same Letter he placed the perfection of Analysis not in the differential calculus as he did after he found it, but in another method founded on Analytical Tables of Tangents & the Combinatory Art. <u>Nihil est</u>, saith he, <u>quod norim in tota Analysi momenti majoris</u>. And a little after: <u>Ea verò non differt ab Analysi illa SVPREMA ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum.</u> Mr Leibnitz never pretended to have found the differential Analysis before this year, & these circumstances satisfy me that <458r> he did not find it till after the writing of this Letter.

In October following he came to London & there met with Dr Barrows Lectures & saw My Letter of 24 Octob. 1676 & therein had fresh notice of the said method & of Compendium of series sent by Dr Barrow to Mr Collins in the year 1669 under the title of Analysis per series &c & consulting Mr Collins saw in his hands several of mine & Gregories Letters, especially those relating to series; & in his way home from London was meditating how to improve the method of Tangents of Slusius as appears by his Letter to Mr Oldenburgh dated from Amsterdam $\frac{18}{28}$ Novem 1676. And the next year upon his arrival at Hannover a Copy of my Letter of 24 Octob. 1676 was sent after him, & in a letter to Mr Oldenburg dated 21 Iune 1677 he sent us his new Method with this Introduction. Clarissimi Slusij methodum tangentium nondum esse absolutam celeberrimo Newtono assentior. And in describing this method he abbreviated Dr Barrow's method of Tangents, & shewed how it might be improved so as to give the Method of Slusius & to proceed in æquations involving surds, & then subjoyned: <u>Arbitror quæ celare voluit Newtonus de tangentibus ducendis ab his non abludere: Quod addit, ex eodem fundamento quadraturas reddi faciliores me in sententia hac confirmat.</u> And after seven years vizt in October 1684 he published the elements of this method as his own without mentioning the correspondence which he had formerly had with the English about these matters. He mentioned indeed a <u>Methodus similis</u>, but whose that method was & what he knew of it, he did not say, as he should have done. And this [his silence put me upon a necessity of writing the Scholium upon the second Lemma of the second Book of Principles least it should be thought that I borrowed that Lemma from Mr Leibnitz. In my Letter of 24 Octob. 1676 when I had been speaking of the method of Fluxions I added: Fundamentum harum operationum, satis obvium quidem, quoniam non possum explicationem ejus prosequi sic potius celavi 6accdæ13eff7i3l9n404qrr4s9t12vx. And in the said Scholium I opened this ænigma, saying that it conteined the sentence <u>Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, et vice versa</u>; & was written in the year 1676. For I looked upon this as a sufficient security without entring into a wrangle: but Mr Leibnitz was of another opinion.]

Mr Iames Bernoulli in the Acta Eruditorum for December 1691 pag. 14, said that the Calculus of Mr Leibnitz was founded on that of Dr Barrow & differed not from it except in the notation of Differentials & some compendium of operation. And the Marquess de l'Hospital in the Preface to his Analysis of infinite petits published A.C. 1696 represented that where Dr Barrow left off Mr Leibnitz proceeded, & that the improvement which he made to Dr Barrow's Analysis consisted in excluding fractions & surds: but he did not then know that Mr Leibnitz had notice of this improvement from me by the two Letters above mentioned dated 10 Decem 1672 & 24 Octob. 1676. After he had notice that such an improvement was to be made, he might find it proprio Marte, but by that notice knew that I had it before him.

I wrote the Book of Quadratures before Mr Leibnitz understood the Differential Analysis. For I wrote it in the year 1676, ex <454r> cept the Introduction & Conclusion, extracting most of it out of old Papers. And when I had finished it & the 7th 8th 9th & 10th Propositions with their Corollaries were fresh in memory, I wrote upon them to Mr Collins that Letter which was dated 8 Novem. 1676 & published by Mr Iones. The Tables at the end of the tenth Proposition for squaring of some Curves & comparing others with the Conic Sections were invented by the inverse Method of fluxions before the year 1671 as may be understood by my Letter of 24 Octob. 1676, where the Ordinates of the Curves are set down. And all the ten first Propositions of the Book of Quadratures except the fift & sixt are in the Tract which I wrote in the year 1671 tho not in the same words & some of them not in words but in equations, & most of them are in a little Tract which I wrote in autumn A.C. 1666.

In my Analysis above mentioned I said of this new Method: Ad Analyticam merito pertinere censeatur cujus beneficio Curvarum areæ et longitudines &c, id modo fiat, exacte et Geometrice determinantur: sed ista narrandi non est locus. And Mr Collins in his Letter to Mr Strode above mentioned, said that by this Tract of Analysis & other things communicated to Dr Barrow, it appeared that I knew this method some years before the Logarithmotechnia of Mr Mecator came abroad so as to find the area of any figure accurately if it may be or at least by approximation in infinitum. And in my Letter of 24 Octob. 1676 I represented that the Quadrature of Curves was improved by the method of fluxions & that by that method I had found some general Theoremes for that end & there set down one of those Theoremes & illustrated it with examples. And in the six first Propositions of the Book of Quadratures I shewed how such Theoremes were to be found by

that Method. And therefore that Method so far as it is conteined in the first six Propositions of the Book of Quadratures was known to me before I wrote the said Letter & even before I wrote the said Analysis & before Mercators Logarithmotechnia came abroad there being no other method then that conteined in those six Propositions by which such Theoremes could be found.

By the inverse Method of fluxions I found in the year 1677 the demonnstration of Keplers Astronomical Proposition vizt that the Planets move in Ellipses about the lower focus with an angular velocity reciprocally proportional to their distance from the focus: & in the year 1683 at the importunity of Dr Halley I resumed the consideration thereof, & added some other Propositions about the heavenly bodies & sent them to him that year in autumn, & they were by him communicated to the R. Society & by their order entred in their Letter Book. And all this was done before Mr Leibnitz published the elements of the Differential calculus.

In writing the Book of Principles I made much use of the method of fluxions direct & inverse but did not set down the calculations in the Book it self because the Book was written by the method of composition, as all Geometry ought to be. And this Book was the first specimen made publick of the use of this method in the difficulter Problemes. The Marquess de l'Hospital said that this Book was presque tout de ce calcul And Mr Leibnitz in a Letter to me dated $\frac{7}{17}$ Mart 1693: Mirifice ampliaveras Geometriam tuis seriebus, sed edito Principiorum opere ostendisti patere tibi etiam quæ Analysi receptæ non subsunt. Conatus sum ego quoque Notis commodis adhibitis quæ differentias et summas exhibent, Geometriam illam quam transcendentem appello, Analysi quodammodo subjicere. And in the Acta Eruditorum for May 1700 pag. 203. Certe cum Elementa calculi <457r> mea edidi anno 1684, ne constabat quidem mihi aliud de inventis ejus [Newtoni] in hoc genere quam quod ipse olim significaverat in litteris, posse se tangentes invenire non sublatis irrationalibus &c sed majora multo consecutum Newtonum, viso demum libro Principiorum ejus satis intellexi. And pag 206 speaking of the method by which they solved the Problem of the linea celerrimi descensus & I found the solid of least resistance, he calls it methodum summi momenti valdeque diffusam — quam ante Dominum Newtonum et me nullus quod sciam Geometra habuit; uti hunc maximi nominis Geometram nemo specimine publice dato se habere probavit.

The Book of Principles came abroad in Spring 1687 & in the Acta eruditorum for Ianuary 1689 Mr Leibnitz published a schediasma de resistentia Medij & motu Projectorum gravium in Medio resistente. This Tract was writ in words at length without any calculations, & in the end of it Mr Leibnitz added: Et fortassis attente consideranti vias quasdam novas vel certe satis antea impeditas aperuisse videbimur. Omnia autem respondent nostræ Analysi infinitorum, hoc est, calculo summarum et differentiarum (cujus elementa quædam in his Actis dedimus) communibus quoad licuit verbis hic expresso. And this was the second specimen made public of the use of this method in the difficulter Problems. And yet it was nothing else then the two first Sections of the second book of Principles reduced into another order & form of words, & enlarged by an erroneus Proposition. To find the Curve described in a Medium where the resistance was in a duplicate ratio of the velocity he composed the horizontal & perpendicular motions of the projectile.

In the same Acta Eruditorum for February he published another Paper entituled Tentamen de motuum cœlestium causis & therin he endeavoured to demonstrate Keplers Proposition above mentioned, by the Differential Method. But the Demonstration proved an erroneous one. And this was the third specimen made publick of the use of this Method in the difficulter Problems.

At the request of Dr Wallis I sent to him in two Letters dated 27 Aug. & 17 Septem 1692 the first Proposition of the Book of Quadratures copied almost verbatim from the Book & also the method of extracting fluents out of equations involving fluxions mentioned in my Letter of 24 Octob 1676 & copied from an older Paper, & an explication of the Method of fluxions direct & inverse comprehended in the sentence Data æquatione quotcunque fluentes quantitates involvente, invenire fluxiones & vice versa: & the Doctor printed them all the same year (viz anno 1692) in the second volume of his works pag. 391, 392, 393, 394, 395, 396, this Volume being then in the press & coming abroad the next year, two years before the first Volume was printed off. And this is the first time that the use of letters with pricks & a Rule for finding second third & fourth fluxions were published, tho they were long before in Manuscript When I considered only first fluxions I seldome used letters with a prick: but when I considered also second third & fourth fluxions &c I distinguished them by Letters with one two or more pricks: & for fluents I put the fluxion either included

withn a square (as in the aforesaid Analysis) or with a square prefixed (as in some other papers) or with an oblique line upon it. And then notations by pricks & oblique lines are the most compendious yet used, but were not known to the Marquess de l'Hospital when he <455r> recommended the differential notation, nor are necessary to the method.

In the end of the year 1672 Mr Iames Bernoulli spake contemptibly of the Differential Method as above. In the years 1692, 1693 & 1694 it grew into reputation. And in spring 1695 Dr Wallis hearing that my Method of fluxions began to be celebrated in Holland under the name of the Differential Method of Mr Leibnitz, wrote in the Preface to the first Volume of his works that the two methods were the same & that in my Letters of 13 Iune & 24 October 1676 I explained to Mr Leibnitz this method found by me ten years before or above. Dr Wallis was not homo novus & rerum anteactarum parum peritus as Mr Leibnitz objected against Dr Keill. He was homo vetus & rerum anteactarum peritus, having received copies of my said two Letters from Mr Oldenburgh in the very year 1676 when they were newly written, & having had sufficient opportunity in those days to inform himself about this matter. By my explaining the Method to Mr Leibnitz, I suppose he meant no more then that in those two Letters I had said so much of it as to make it easy to find the rest.

In the yeare 1696 the Marquess de l'Hospital published his Analysis de infinitement petits & this I think was the first time that a Rule for finding second third & fourth differences was published. This Book being an Introduction to the differential Method made it spread much more then before. But in the year 1699 Mr Fatio published that I was the oldest inventor by many years & Dr Wallis published by Letters in the third Volume of his works. Mr Leibnitz returned a reply to Mr Fatio in the year 1700 & Mr Fatio wrote an Answer & sent it to the Editors of the Acta Eruditorum A.C. 1701, but they refused to print it, pretending an aversion to controversies. See the Acta for March pag. 134.

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In the beginning of the year 1666 I found the Theory of colours & in the year 1671 I was upon a designe of publishing it together with the methods of Series & fluxions: but for a reason given in my Letter of 24 Octob. 1676 I laid aside my designe of publishing them & neglected to publish them till the year 1704 & then published the Theory of colours & the Book of Quadratures together. And writing an introduction to the Book of Quadratures with relation to the method of fluxions upon which that Book depended: I said in that Introduction that I found the Method gradually in the years 1665 & 1666; this being not so much as Dr Wallis had said nine years before without being then contradicted. And thus the Method was claimed from time to time from Mr Leibnitz till this year so that he never was in quiet possession.

But in the Acta Eruditorum for Ianuary 1705, an account of the Introduction to the Book of Quadratures was published in these words. Quæ [Isagoge] ut MELIVS intelligatur, sciendum est cum magnitudo aliqua continue crescit, veluti linea (exempli gratia) crescit fluxu puncti quod eam describit, incrementa illa momentanea appellari DIFFERENTIAS, nempe inter magnitudinem quæ antea erat & quæ per mutationem momentaneam est producta; atque hinc natum esse Calculum Differentialem, eique reciprocum Summatorium; cujus element{a} ab INVENTORE D. Godofredo Guillielmo Leibnitio in his Actis sunt tradita varijque usus tum ab ipso, tum a DD Fratribus Bernoullijs tum a D. Marchione Hospitalio sunt ostensi. Pro Diffentijs IGITVR Leibnitianis D. Newtonus adhibet semperque [pro ijsdem] adhibuit, fluxiones, — ijsque tum in suis Principijs Naturæ Mathematicis, tum in alijs postea editis [pro Differentijs Leibnitianis] eleganter est usus, QVEMADMODVM et Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianæ methodo SVBSTITVIT. And all this is as much as to say that I did not invent the method of fluxions in the years 1665 & 1666 as I affirmed in this Introduction, but after Mr Leibnitz in his Letter of 21 Iune 1677 had sent me his differential method, instead of that method I began to use & have ever since used the method of fluxions.

In the Philosophical Transaction for September & October 1708 Dr Keill published to the contrary: Fluxionum Arithmeticam sine omni dubio primus invenit D. Newtonus ut cuilibet ejus epistolas a Wallisio editas legenti facile constabit. Eadem tamen Arithmetica postea mutatis Nomine & Notationis modo a Domino Leibnitio in Actis Eruditorum edita est. And Mr Leibnitz in a Letter to Sir Hans Sloane dated 4

March 1711 complained of Dr Keill for this saying: <u>Cogor remedium ab Inclyta vestra Societate Regia patere. Nempe æquum esse vos ipsi credo judicabitis ut D. Keillius testetur publice, non fuisse sibi animum imputandi mihi quod versa insinuare videntur, quasi ab alio hoc quicquid est Inventi didicerim & mihi attribuerim</u>. And Dr Keill in an Epistle read before the R. Society 24 May 1711 replied: Agnosco me dixisse fluxionum Arithmeticam a D. Newtono inventam fuisse, quæ mutato nomine & notationis modo a Leibnitio edita fuit: sed nollem hæc verba ita accipi, quasi aut nomen quod Methodo suæ imposuit Newtonu**{s}** aut Notationis formam quam adhibuit, D. Leibnitio innotuisse contenderem; sed hoc solum innuebam, D. Newtonum fuisse primum inventorem <456r> Arithmeticæ fluxionum seu Calculi Differentialis; eum autem in duabus ad Oldenburgum scriptis Epistolis & ab illo ad Leibnitium transmissis, indicia dedisse perspicacissimi ingenij Viro satis obvia; unde Leibnitius principia illius calculi hausit, vel saltem haurire potuit. At cum loquendi et Notandi formulas quibus usus est Newtonus, ratiocinando assequi nequiret Vir illustris, suas imposuit. Hæc ut scriberem impulerunt Actorum Lipsiensium Editores qui in ea quam exhibent operis Newtoniani de Fluxionibus seu Quadraturis enarratione, diserte affirmant D. Leibnitium fuisse istius methodi INVENTOREM et Newtonum aiunt pro Differentijs Leibnitianis fluxiones adhibere semperque adhibuisse. And Mr Leibnitz in a Letter to Dr Sloane replied: Frustra [Keilius] ad

[1] Died November 1716 **{illeg}**

[2] Published in Raphson p. 111

[3] Scholium ref to in p. 2