Letter from Sir Isaac Newton to the Abbe Conti in reply to the Postscript of Leibniz to the same

Author: Isaac Newton

Source: MS Add. 3968, ff. 558r-573v, Cambridge University Library, Cambridge, UK

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Sir

3 When the Differential method began to be celebrated in Holland Dr Wallis, in the introduction to his Works printed in the year 1695, wrote that this method was the sam with the method of fluxions which Mr Newton had explained to Mr Leibnitz in his Letters written 1676 & had invented ten years before that time or above. Mr Leibnitz in the correspondence which followed thereupon between him & Dr Wallis did not deny this nor contend for any thing more then that he had added something to Mr Newtons method and what he had added was his own. Mr Newton in the Introduction to his Quadratura Curvarum published about 12 years ago wrote that he had found the method of fluxions by steps in the years 1665 & 1666. Dr Wallis being dead Mr Leibnitz now pretends that Mr Newton did not invent it so early nor was the first inventor & upon Dr Keills defending Dr Wallis against what has been published to this purpose in the Acta Eruditorum, has affirmed that what was there published, was just & he has demanded that Dr Keill should recant & that Mr Newton should declare his opinion in this matter, that is, that he should retract what he had published in the said Introduction. Vpon Mr Newton's being thus accused of plagiary, the R. Society ordered the ancient Letters & papers to be published, But Mr Leibnitz refuses to make good his accusation. huffs at what was published as trifling, complains what made for him & particularly what he had seen in England in the hands of Mr Collins about Mr Newton's ignorance has been suppressed, & appeales from the judgment of the Committee of the R Society to the jugment of his disciple & confederate Mr Iohn Bernoulli. Mr Leibnitz having thus accused Dr Keill of what he durst not accuse Dr Wallis & demanded that Mr Newton also should be questioned for what he published to the same purpose (all which amounts to an accusation of Plagiary against Mr Newton) it lies upon Mr Leibnitz by the laws of all nations either to prove his accusation, or else to be deemed guilty of calumny.

How little reason there is for Mr Leibnitz to sleight the ancient papers printed by order of the R. Society or the interpretations put upon them, may appear by this instance that there is a letter in the hand writing of Mr Leibnitz dated from Paris May 1675, in he acknowledges the receipt of a Letter with several from Mr Oldenburgh. This Letter was dated 15 April preceding series in it & conteins a series for finding the Arch whose tangents was given. which series was invented by Mr Gregory four years before. And Mr Leibnitz when he first received it from London did not know it to be his own but as appears by the Letter & yet the same year communicated it to his friends at Paris as his own & afterwards published it in Germany as his own without ever acknowledging that he had received it from London.

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And as for his complaint that what made for him & particularly what he saw in the hands of Mr Collins concerning Mr Newtons ignorance, has been suppressed in the Commercium; it is unjust That passag is there printed pag 75, lin 10, 11. And by the statutes of the R. Society it is expulsion to defame them.

What he saw in the hands of Mr Collins was in Mr Newtons Letter of 24 October 1676. Mr Leibnitz was in London some part of that Month & before he left London saw that Letter in the hands of Mr Collins but staied not to take a copy of it along with him. He tells us that Mr Collins then shewed him a part of his correspondence with Mr Collins & Mr Newton. I suppose he means the originals from whence their series had been taken.

6 Mr Leibnitz tells us that it would have been easy for Mr Newton to find the differential method, if it had been hinted to him: & was it not as easy for Mr Leibnitz to find it by the hints which he received from Mr Newtons Letters of 10 Dec 1672, 13 Iune 1676 & 24 Octob. 1676.

7 He allows that Mr Newton preceded him in the method of Series, but he saith that at length he invented a general method of series, after which he had no further use of Mr Newton's extractions. And yet his general method is Mr Newton's. In his Letter of 13 Iune 1676 Mr Newton represented that his method of Series was not general without some other Methods: & in his Letter of 24 Octob. 1676 he represented that his method of Series became general by two Methods: one of which consisted in extracting fluents out of equations involving their fluxions, the other in assuming the terms of a series & determining them by the conditions of the Probleme. The first method demonstrates that Mr Newton was then well acquainted with fluxional Equations & had then carried Analysis to a higher pitch in such Equations then Mr Leibnitz has been able to carry it in differential Equations to this day. The second is that very general method of Series which Mr Leibnitz <559r> <558v> claims from Mr Newton. And it lies upon Mr Leibnitz to make it appear that he knew either of those methods so early.

Mr Leibnitz was in London the first time in Feb. 1673 & went to Paris in or about the beginning of March following, & at that time knew nothing of the higher Geometry but some time after (suppose in the year 1674{)} began to be instructed in it at Paris by Mr Huygens as he represents in his Letter to you. The same yeare he met with a series for finding any arc of a circle whose sine was given & if the proportion of this arc to the whole circumference was known it gave him the whole circumference. The next year Mr Oldenburg sent him eight series in a letter dated 15 Apr 1675 & he acknowledged the receipt of them in his Answer dated 20 May following & said he would compare them with his own. The

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1. Mr Newton gave an instance of his method of fluxions in his Analysis per Æquationes numero termin communicated by Dr Barrow to Mr Collins in the year 1669 & described the universality of it in his Letter to Mr Collins dated 10 Decem 1672 with an example therof in drawing of Tangents, a copy of which Letter was sent to Mr Leibnitz at Paris in the year 1676, & in his Letters of 13 Iune & 24 Octob. 1676 described the method further to Mr Leibnitz as extending to Quadratures of Curves, invers Problems of Tangents & others more difficult & there also gave an example of it in a general series for squaring of Curves. Mr Leibnitz began to learn the higher Geometry in the year 1674 & came from Paris to London in October 1676, & there saw this last Letter in the hand of Mr Collins & by his Letter & that of 10 Decem 1672 ✝ ✝ understanding that the new methods of Tangents were a branch of Mr Newtons general method, fell upon considering how to make Mr Newtons method of Tangents (which was the same with that of Slusius, become general, & the next year & the next year in a Letter from Hannover dated 21 Iune 1677 sent back Dr Barrows method of Tangents, the name & characteristick , & shewed how this method gave the method of Slusius & might be improved much beyond his former method (that of Dr Barrow) so as to proceed without taking away fractions

& surds & extend to Quadratures & then he took notice that these performances being the same with those which Mr Newton had ascribed to his method, he took this his new method to be like Mr Newtons. Thus was he then endeavouring to find out Mr Newtons method. But now he contends that Mr Newton. had no such method in those days. Mr Newton in his Letter of 24 Octob represented that his method was founded in solving this Probleme Data æquatione fluentes quotcunque quantitates involvente fluxiones invenire & vice versa, & that his method of series became universal by solving this Probleme Fluentem ex æquatione fluxiones involvente extrahere. But Mr Leibnitz tells us that Mr Newton in those days had no method of fluxions no fluxional equations, no characteristick for fluxions & moments. Dr Barrow published his differential method of Tangents in the year 1670. Mr Newton knew that method some years before Mr Leibnitz & yet is accused of wanting a differential Characteristic

4 When Mr Leibnitz first published his differential Method,[1] he wrote that it reacht to such difficult Problems as could not be solved without the method or another like it. And what other method he meant you may know by his Letter to Mr Newton dated 17 March 1693 & still extant in his own hand writing. His words are Mirifice ampliaveras Geometriam tuis seriebus, sed etiam edito Principiorum opere ostendisti patere tibi etiam quæ Analysi receptæ non subsunt. Conatus sum ego quoque, notis commodis adhibitis quæ Differentias & Summas exhibeant, Geometriam illam quam transcendentem appello Analysi quodammodo subjicere; nec res male processit. And hitherto Mr Leibnitz forbore to contend with Mr Newton for the preference.

4 Afterwards in the year 1699 Mr Fatio published that Mr Newton was the oldest inventory by many years & Mr Leibnits in his Answer published in the Acta Eruditorum for May 1700 did not dispute it but granted that Mr Newton was the first who by giving a specimen of this method openly, had proved that he had it, & contended for nothing more then that each of them had found the method apart without receiving light from the other.

5 In October 1704 Dr Wallis died, the last of the old men who corresponded with Mr Oldenburg & Mr Collins in these matters. And then Mr Leibnitz began to claim the precedency. For in Ianuary 1705 in giving an Account of Mr Newton's Quadratura Curvarum in which Mr Newton had said that he found the Method of fluxion gradually in the years 1665 & 1666, it was retorted upon him that he had substituted fluxions for the differences of Mr Leibnitz the first Inentor. And when Dr Keill defended Dr Wallis & Mr Newton, Mr Leibnitz defended what had been published in the Acta Eruditorum, & demanded that Dr Keill should recant & taxed Mr Newton with knowing that Dr Keill was in the wrong & pressed that he should declare his opinion in this matter, that is, that Mr Newton should retract what had been publickly affirmed by Dr Wallis Mr Fatio Dr Keill & himself, & granted by Mr Leibnitz in his Letter of 21 Iune 1677 [See Commercium p 88 lin 14 & p. 8 9 lin penult. & p. 90 lin. 26, 27, 28] & not disputed in his correspondence with Dr Wallis & answer to Mr Fatio <559r> The accusation against Mr Newton amounts to to plagiary, & if it be not made good it ought to go for calumny, & Mr Leibnitz is the man who ought to make it good.

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Sir

When the R. Society upon a Question arising between Mr Leibnitz & Dr Keill which affected me appointed a Committee to seach out the ancient Letters & Papers found in the Archives & Letter-books of the R. Society & Library of Mr Iohn Colling & that the same should be printed with the opinion of the Committee thereupon that the matter of fact might appear to the world: instead of returning a fair answer, a defamatory Libel was printed in Germany dated the   of Iune 1713 & dispersed through Germany France & Italy

Sir

You know that the Commercium Epistolicum conteining the ancient Letters & Papers preserved in the Archives & Letter Books of the R. S. & Library of Mr Collins relating to the dispute between Mr Leibnitz & Dr Keil. They were collected & published by a Committee appointed by the R. Society for that purpose; & Mr Leibnitz has hither avoided returning an Answer to the same. for the Book is matter of fact & uncapable of an Answer. For avoiding an Answer he pretended that he had not seen this Book nor had leasure to examin it, but had desired an eminent Mathematician to examin it. And the Answer of the Mathematician or prettended Mathematician was insertd into a defamatory Libel dated   & published in Germany without the name of the author or publisher or City where it was published. And I have since seen some Letters written since by Mr Leibnitz in all which he excuses himself from returning an Answer And the Postscript which you shewed me is of the same kind. For He tells you in it that the English shall not have the pleasure of seing him return an Answer to their slender reasonings as he calls them, & the whole Postscript is reflecting & defamatory without proving any thing & he falls upon my Philosophy, which is nothing at all to the Question & in squabling about it corrupts the significations of words calling those things miracles which happen constantly & those things occult qualities which are not occult & contends for Hypotheses in opposition to Propositions proved from Experiments & observations & experiments by the argument of Induction, & ascribes opinions to me which are not mine. & at the same time he has sent a Mathematical Probleme to be solved by the English Mathematicians which is as little the purpose.

He complains of the Committee of the R. Soc. as if they had acted partially in omitting what made against me. But in proving the accusation he instances in a Paragraph concerning my ignorance which he says they omitted. And yet {For} This Paragraph is not omitted. It is in my Letter of 24 Octob. 1676, & you will find it in the Commercium Epistolicum pag   lin   . He saith that he saw this Paragraph in the hands of Mr Colling when he was in London the second time that is, in October 1676, & therefore he then saw that Letter. And in that & some other Letters writ before that time I described the method of fluxions, & in the same Letter I described two general methods of Series one of which Mr Leibnitz now claims to himself.

1 I beleive you will think it reasonable that Mr Leibnitz be constant to himself & still acknowledge what he acknowledged above 15 years ago & still forbeare to contradict what he forbore to contradict in those days. In his Letter of 20 May 1675 with several converging Series conteined therein & I expect that he still acknowledge the receipt thereof.

2 In a Letter datd 12 May 1676 he acknowledged that he then wanted the method for finding a Series for the Arc whose sine was given, & by consequence that <560v> when he wrote his Letter of 26 Octob. 1674 he wanted that method. 3 In the Acta Eruditorum for May 1700, he acknowledged that no body So far as he knew had the method of Fluxions or Differences before me & him, & that no body before me had proved by a specimen made publick that he had the method: & I expect that he continues to make the same acknowledgement . ⌐7⌐ Dr Wallis in the Preface to the two first volumes of his works published in April or May 1695 wrote that I in two Letters written in the year 1676 had explained to Mr Leibnitz the Method (called by me the Method of fluxions & by him the method of differences <560v> invented by me ten years before or above, that is in the year 1666 or before, & in the Letters which followed between them, Mr Leibnitz had notice of this Paragraph & did not then contradict it nor found any fault with it & I expect that he still forbears to contradict it But as he has attaqued me with an accusation which amounts to plagiary if he goes on to accuse me it lies upon him by the laws of all nations to prove his accusation on pain of being accounted guilty of calumny.

4. ② In his Letter to me dated 7. March 1693 & now in the custody of the R. S. he wrote Mirifice ampliaveras Geometriam tuis seriebus, sed edito Principiorum opere ostendisti patere tibi quæ Analysi receptæ non subsunt Conatus sum Ego quoque notis commodis adhibitis quæ differentias & summas exhibent, Geometriam illam quam transcendentem appello, Analysi quodammodo subjicere, nec res male processit. And ① in the Acta Eruditorum for October 1684, when he had described the differential method | elements of his method of tangents & Maxima & Minima he added that the method extended to the

difficulter sort of Problems which without this method or ANOTHER LIKE IT could not be managed so easily. ③ And what he then acknowledged he ought still to acknowledge.

5 In his Letter of 21 Iune 1677 in answer to mine of 24 Octob 1676 wherein I had described my method partly in plane words & partly in cyphers he said that he agreed with me that the method of tangents of Slusius was not yet perfect & then set down a differential method of tangents published by Dr Barrow in the year 1670 & shewed how it might be improved to perform those things which I had attributed to my method & thence concluded that mine differed not much from his, especially since it faciliated Quadratures: & in candor he ought still to acknowledge that he then understood that when I wrote my Letter of 24 Octob. 1676 I had such a method.

6 In his Letter of 27 Aug. 1676 he represented that he did not beleive that my methods were so general as I had described them, & affirmed that there were many Problemes so difficult that they did not depend upon Equations & Quadratures: such as (amongst many others) were the inverse Problemes of tangents. And by these words that he had not yet found the reduction of Problems to Differential Equations . And what he then acknowledged he ought in candor to acknowledge still.

8 And after this I beleive you will see that I described the extent & nature of this method in my Letter of 10 Decem 1672 & that I couched both the Differential & the Summatory Method in my Tract of Analysis communicated by Dr Barrow to Mr Collins in Iuly 1669. At which time Mr Leibnitz had not yet begun to learn Algebra & the higher Geometry.

But if he goes on still to accuse me of plagiary, it lies upon {him} by the laws of all nations to prove his accusation on pain of being deemed guilty of calumny. He is the aggressor & it lies upon him to prove his charge.

9 I descend not further into particulars [those being described in the Commercium Epistolicum & the Account thereof to which] but refer you to the Commercium Epistolicum. & the Account thereof where you will find the whole matter distinctly stated & represented.

I forbear to descend further into particulars. You have them represented in the Commercium Epistolicum & the Extract thereof to both which I referr

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He hath hitherto written Letters to his correspondents full of affirmations & reflexions without proving any thing. But he is the aggressor & it lies upon him to prove his charge. But if he goes on still to

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Hitherto he avoided returning an answer to the Commercium Epistolicum by pretending that he had not seen it being at Vienna, And he still excuses his answering it, telling you that the English shall not have the pleasure to see him returne an answer to their slender reasoning as he calls them, & by endeavouring to engage me in disputes about Philosophy & about solving of Problems which are nothing to the Question in hand

I do not contend about skill in Mathematicks having left off that study 20 years ago & look upon solving of Problems a very unfit method argument to decide who was the best Mathematician or invented any thing above 40 years ago . And as to Philosophy And as to Philosophy it is as little to the purpose. He colludes in the signification of words he takes words in new significations peculiar to himself, preferrs Hypotheses to Arguments of Induction taken from Phenomena accuses me of opinions that are not mine & instead of proposing Questions to be examined by experiments before they are admitted into Philosophy, he would have Hypotheses admitted & beleived before they are examined. But all this is nothing to the Commercium Epistolicum.

He complains of the Committee

And this is as much as to acknowledge that I had the method before it was published in Germany & that the Principia Philos were a proof that I had it & the first specimen made publick of applying it to the difficulter Problems.

– There you will see that in my Letter of 10 Decem 1672 I described the extent of the method & some of its Characters & that in my Analysis communicated by Dr Barrow to Mr Collins in the year 1669 I couched both the Differential method & the summatory.

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<center>**after 1715.**</center>

Sir

You know that the Commercium Epistolicum conteins the ancient Letters & Papers preserved in the Archives & Letter Books of the Royal Society & Library of Mr Collins relating to the dispute between Mr Leibnitz & Dr Keill & that they were collected & published by a numerous Committee of Gentlemen of several nations appointed by the Royal Society for that purpose. Mr Leibnitz hath hitherto avoided returning an Answer to the same: for the book is matter of fact & uncapable of an Answer. To avoid answering it he pretended the first year that he had not seen this book nor had leasure to examin it, but had desired an eminent Mathematician to examin it. And the Answer of the Mathematician (or pretended Mathematician) dated 7 Iune 1713 was inserted into a defamatory Letter dated 29 Iuly following & published in Germany without the name of the Author or Printer or city where it was printed. And the whole has been since translated into French & inserted into another abusive Letter (of the same Author as I suspect) & answered by Dr Keill in Iuly 1714 & no answer is yet given to the Doctor.

Hitherto Mr Leibnitz avoided returning an Answer to the Commercium Epistolicum by pretending that he had not seen it: And now he avoids it by telling you that the English shall not have the pleasure to see him return an answer to their slender reasonings (as he calls them) & by endeavouring to engage me in disputes about Philosophy & about solving of Problems both which are nothing to the Question.

As to Philosophy, he colludes in the significations of words, preferrs Hypotheses to arguments of Induction from experiments, accuses me of opinions which are not mine, & instead of proposing Questions to be examined by experiments before they are admitted into Philosophy he proposes Hypotheses to be admitted & beleived before they are examined. But all this is nothing to the Commercium Epistolicum.

He complains of the Committee of the Royall Society as if they had acted partially in omitting what made against me. But he fails in proving the accusation. For he instances in a Paragraph concerning my ignorance pretending that they omitted it, & yet you will find it in the Commercium Epistolicum pag. 74 lin. 10, 11, & I am not ashamed of it. He saith that he saw this Paragraph in the hands of Mr Collins when he was in London the second time, that is, in October 1676. It is in my Letter of 24 Octob. 1676, & therefore he then saw that Letter. And in that & some other Letters writ before that time I described my method of fluxions. And in the same Letter I described also two general methods of series, one of which is now claimed from me by M^{r}Leibnitz.

I beleive you will think it reasonable that Mr Leibnitz be constant to himself, & still acknowledge what he acknowledged above 15 years ago, & still forbear to contradict what he forbore to contradict in those days.

In his Letter of 20 May 1675 he acknowledged the Receipt of a Letter from Mr Oldenburge dated 15 Apr. 1675 with several converging series conteined therein. And I expect from him that he still acknowledge the Receipt thereof. Many Gentlemen of Italy France & Germany (you your self being one of them) have seen the original Letters & the entrys thereof in the old Letter books of the Royal Society, & the series of Gregory is in the Letter of 15 Apr. 1675 & in Gregories original Letter dated 15 Feb. 1671.

In a Letter dated 12 May 1676 he acknowledged that he wanted the method for finding a series for the Arc whose sine was given, & by consequence <562v> that he wanted it when he wrote his Letter of 24 Octob. 1674. And I expect that he still acknowledge it.

In the Acta Eruditorum for May 1700, in answer to Mr Fatio who had said that I was the oldest inventor by many years, he acknowledged that no body, so far as he knew, had the method of fluxions or differences before me & him & that no body before me had proved by a specimen made publick that he had it. Here he allowed that I had the method before it was published or communicated by him to any body in Germany, & that the Principia Philosophiæ were a proof that I had it & the first specimen made publick of applying it to the difficult <563r> er <562v> Problemes. And I expect that he still continue to make the same acknowledgement. At that time he did not deny what Mr Fatio affirmed & nothing but want of candor can make him unconstant to himself.

In a Letter to me dated 7 March 1693 & now in the custody of the R. Society, he wrote, 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, nec res male processit &c. And what he then acknowledged he ought still to acknowledge

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To abbe Conti, Ka{illeg} p. 100

Leicester Fields, London. 26 Feb. 171$\frac{5}{6}$.

Sir

You know that the Commercium Epistolicum conteins the ancient Letters & Papers preserved in the Archives & Letter Books of the Royal Society & Library of Mr Collins relating to the dispute between Mr Leibnitz & Dr Keill & that they were collected & published by a numerous Committee of Gentlemen of severall nations appointed by the R. Society for that purpose. Mr Leibnitz has hitherto avoided returning an Answer to the same; for the Book is matter of fact & uncapable of an Answer. To avoid answering it he pretended the first year that he had not seen this Book nor had leasure to examin it, but had desired an eminent Mathematician to examin it. And the Answer of the Mathematician (or pretended Mathematician) dated 7 Iune 1713, was inserted into a defamatory Letter dated 29 Iuly following, & published in Germany without the name of the Author or Printer or City where it was printed. And the whole has been since translated into French & inserted into another abusive Letter (of the same Author as I suspect) & answered by Dr Keill in Iuly 1714, & no answer is yet given to the Doctor.

Hitherto Mr Leibnitz avoided returning an Answer to the Commercium Epistolicum by pretending that he had not seen it. And now he avoids it by telling you that the English shall not have the pleasure to see him return an Answer to their slender reasonings (as he calls them) & by endeavouring to engage me in disputes about Philosophy & about solving of Problems, both which are nothing to the Question.

As to Philosophy He colludes in the significations of words ✝ < insertion from f 565r > ✝ calling those things miracles which create no wonder & those things occult qualities whos causes are occult tho the qualities themselves be manifest, & those things the souls of men which do not animate their bodies. His Harmonia præstabilita is miraculous & contradicts the daily experience of all mankind, every man finding in himself a power of seeing with his eyes & moving his body by his will. He preferrs Hypotheses < text from f 564r resumes > Preferrs Hypotheses to Arguments of Induction drawn from experiments, accuses me of opinions which are not mine, & instead of proposing Questions to be examined by Experiments before they are admitted into Philosophy he proposes Hypotheses to be admitted & beleived before they are examined. But all this is nothing to the Commercium Epistolicum.

He complains of the Committee of the Royall Society as if they had acted partially in omitting what made against me But he fails in proving the accusation. For he instances in a Paragraph concerning my ignorance, pretending that they omitted it, & yet you will find it in the Commercium Epistolicum pag 74 lin. 10, 11, & I am not ashamed of it. He saith that he saw this Paragraph in the hands of Mr Collins when he was in London the second time, that is, in October 1676. It is in my Letter of 24 Octob. 1676, & therefore he then saw that Letter. And in that & some other Letters writ before that time I described my method of fluxions. And in the same Letter I described also two generall methods of series, one of which is now claimed from me by M^{r}Leibnitz.

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I beleive you will think it reasonable that Mr Leibnitz be constant to himself & still acknowledge what he acknowledged above 15 years ago, & & still forbear to contradict what he forbore to contradict in those days.

In his Letter of 20 May 1675 he acknowledged the Receipt of a Letter from Mr Oldenburg dated 15 Apr. 1675 with several converging series conteined therein. And I expect from him that he still acknowledge the receipt thereof. ‖ < insertion from f 565r > ‖ Many Gentlemen of Italy France & Germany (you your self being one of them) have seen the original Letters & the entries thereof in the old Letter books of the Royal Society, & the Series of Gregory is in the Letter of 15 Apr. 1675, & in Gregories original Letter dated 15 Feb. 1671.

In a Letter dated 12 May 1676 (seen by the same Gentlemen) he acknowledged that he then wanted &c < text from f 564v resumes > In a Letter dated 12 May 1676 seen by the same Gentlemen he acknowledged that he then wanted the method for finding a series for the Arc whose sine was given, & by consequence that he wanted it when he wrote his Letter of 24 Octob 1674 And I expect that he still acknowledge it.

In the Acta Eruditorum for May 1700, in answer to Mr Fatio who had said that I was the oldest inventor by many years, Mr Leibnitz acknowledged that no body so far as he knew, had the method of fluxions or differences before me & him, & that no body before me had proved by a specimen made publick that he had it. Here he allowed that I had the method before it was published or communicated by him to any Body in Germany that the Principia Philosophiæ were a proof that I had it, & the first specimen made publick of applying it to the difficulter Problemes, And I expect that he still continue to make the same acknowledgement. At that time he did not deny what Mr Fatio affirmed, & nothing but want of candor can make him unconstant to himself.

In a Letter to me dated 7 March 1693 & now in the custody of the R. S. he wrote, 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 & summas exhibent, Geometriam illam quam transcendentem appello, Analysi quodammodo subjicere nec res male processit. And what he then acknowledged he ought still to acknowledge.

In his Letter of 21 Iune 1677 writ in Answer to mine of 24 Octob 1676 wherein I had described my method partly in plain words & partly in cyphers, he said that he agreed with me that the method of tangents of Slusius was not yet made perfect, & then set down a differential method of Tangents published by Dr Barrow in the year 1670, & disguised it by a new notation, pretending that it was his own & shewed how it might be improved so as to perform those things which I had ascribed to my method, & concluded from thence that mine differed not much from his, especially since it faciliated Quadratures. And in the Acta Eruditorum for October 184, in publishing the Elements of this method he added that it extended to the difficulter Problemes which without this Method or another like it, could not be managed so easily. He understood therefore in those days that in the eyar 1676 when I wrote my said Letter I had a method which did the same things with the method which he calls differentiall, & he ought still to acknowledge it, ‖, especially < insertion from f 565r > ‖ especially now the sentences in cyphers are decyphered & other things in that Letter relating to the method are fully explained, & the Compendium mentioned therein is made publick. < text from f 564v resumes >

In his Letter of 27 Aug. 1676 he represented that he did not beleive that my Methods were so generall as I had described them in my Letter of 13 Iune preceding, & affirmed that there were many Problemes so difficult that they did not depend upon Equations nor Quadratures, such as (amongst many others) were the inverse Problemes of tangents. And by these words he acknowledged that he had not yet found the reduction of Problems to Differential Equations. And what he then acknowledged, he acknowledged again in the Acta Eruditorum for April 1691 pag. 178, & ought in candor to acknowledge still.

Dr Wallis in the Preface to the two first Volumes of his works published in April 1695, wrote that I in my two Letters written in the year 1676 had explained to Mr Leibnitz the Method (called by me the method of fluxions & by him the differential method) invented by me ten years before or above (that is, in the year 1666 or before) & in the Letters which followed between them, Mr Leibnitz had notice of this Paragraph & did not then contradict it nor found any fault with it. And I expect that he still forbeare to contradict it.

But as he has lately attaqued me with an accusation which amounts to plagiary: if he goes on to accuse me, it lies upon him by the laws of all nations to prove his accusation on pain of being accounted guilty of calumny. He hath hitherto written Letters to his correspondents full of affirmations <565r> complaints & reflexions without proving any thing. But he is the aggressor & it lies upon him to prove his charge.

I forbear to descend further into particulars. You have them in the Commercium Epistolicum & the Abstract thereof, to both which I refer you. I am

Sir

              Your most humble and most obedient Servant

                 Is. Newton

<center><566r></center>

To Conti 2{1} {illeg}

Sir

I thank you for shewing me the Postscript to the Letter of Mr Leibnitz. And for setting those matters in a true light, I will lay them before you in an historical manner in as few words as I can .

I gave an Example of the method of fluxions in my Analysis communicated by Dr Barrow to Mr Collins in the year 1669, & described the universality of it in my Letter to Mr Collins dated 10 Decem 1672 with an example thereof in drawing of Tangents, a copy of which Letter was sent to Mr Leibnitz at Paris by Mr Old. in the same Packet with My Letter of 13 Iune 1676 & therefore was received by him. In this Letter I said.

Mr Leibnitz was in London in February 1673 & after a few days went thence to Paris, & at that time & some time after he knew nothing of the higher Geometry, but some time after (suppose in the year 1674) was instructed in it at Paris by Mr Huygens as he represents in his Postscript above mentioned. In those days Mr Collins communicated to his friends at home & abroad several Series invented by me & Mr Iames Gregory. And one of these series namely that for finding any Arc whose sine is given & by consequence the whole circumference when its proportion to that arc is given, Mr Leibnitz in the year 1674 signified that he had invented such a series. And thereupon Mr Oldenburg to Mr Oldenburg sent eight series invented by & Mr Iames Gregory, to Mr Leibnitz in a letter dated 15 Apr. 1675, & Mr Leibnitz in a Letter dated 20 May 1715 acknowledged the receipt thereof, & said he would compare those series with his own but the same year communicated to his friends at Paris one of those series as his own {w}. And the next year Mr Leibnitz, upon receiving two others of those series a second time from London, desired Mr Oldenburgh (by a letter dated 12

May 1676) to procure from Mr Collins the Demonstration of those two series, meaning the method of finding them; & promised to recompence him with something of his own very different. One of the two series of which he wanted the Demonstration was that for finding the Arc whose sine was given. He pretended two yeares before to have found this series himself & now he wanted the method of finding it. The series which he sent back as a recompence was one of those which he had received from Mr Oldenburg the year before & did not then know to be his own but the same year communicated it to his friends at Paris as his own & afterwards published in the Acta Eruditorum as his own without letting the world know that he had received it from London. Vpon this request of Mr Leibnitz Mr Newton wrote his Letter of 13 Iune 1676 conteining his method of Seires illustrated with divers examples of Series. And Mr Leibnitz in his Answer dated   claimed four of the series, pretending that he had found them before he received the Letter, that is, before he had the method of finding them. And when at his request Mr Newton further explained the inverse method of series, so soon as he understood it, he replied that he had found it before as he perceived by his old papers, but had forgot it.

Mr Newton neare the end of his said Letter of 13 Iune 1676, represented that <u>Analysis</u> by the help of these series, extended to allmost all problems but yet became not universall without some further methods. And in his next Letter dated Octob. 24, he said that <u>one of those Methods consisted in the extraction of a fluent out of an Equation involving its</u> fluxion: & the other <u>the Probleme</u>. The first method shews that when Mr Newton wrote these two <566v> Letters he had fluxional equations & had then carried Analysis in such equations to a very great height & by this Analysis had improved the Method of Series & made it general. The second method is that which Mr Leibnitz in his letter to you claims to himself, saying that Mr Newton indeed preceded him in the method of series, but at length he (Mr Leibnitz) found a general method for series & after this he had no more need of Mr Newton's extractions. < insertion from above the line > But this Method being Mr Newtons Mr Leibnitz has no right to any part of the method of Se <567r> ries < text from f 566v resumes >

Mr Newton in his said Letter of 24 Octob. 1676 mentioned his Analysis communicated by Dr Barrow to Mr Collins & another Tract composed in the year 1671 about Series & a method of Tangents which readily gave the method of Slusius, & stuck not at surds & extended to questions de maximis & minimis & quadratures & others & was obvious or easy to be found out & was founded in the slution of this Probleme [Data Æquatione quotcunque fluentes quantitates involvente, Fluxiones invenire & vice versa.] This is therefore the method of Fluxions whereof Mr Leibnitz now pretends to have been the first inventor calling it the differential method. This is that method which Mr Newton in his Letter of 10 Decem 1672 called his general method & said that it not only determined Tangents, but extended also to abstruser Problems concerning the Curvities, Areas Lengths Centers of gravity of Curves &c & proceeded even in Mechanical Curves & in Equations involving surds. And the things which Mr Newton in his Letter of 24 Octob 1676 cited out of his book of Quadratures shew that he had in those days carried the fluxional Analysis to a higher pitch then Mr Leibnitz & his followers have been able to carry the differential Analysis to this day.

Mr Newton's Letter of 10 Decem 1672 was sent by Mr Oldenburg to Mr Leibnitz in the same packet with his Letter of 13 Iune 1672, & therefore Mr Leibnitz received it. In October following he came from Paris to London & there Mr Collins shewed him a part of his correspondence with Mr Gregory & Mr Newton as he acknowledges in his Letter to you, & particularly he shewed him Mr Newton's Letter of 24 October just then received by Mr Oldenburg & given to Mr Collins to be copied. For the sentence concerning Mr Newton's ignorance of the dimensions of the vulgar figures except the Cissoid, is in this Letter. You may see it in the Commercium pag. 74. And therefore Mr Leibnitz has injured the Committee of the R. Society in complaining that they suppressed it. And if you please to consult the place, you will see that the construction which he puts upon it, is as injurious to Mr Newton, whose words are: <u>Sed in simplicioribus vulgoque celebratis figuris, vix aliquid relatu dignum reperi quod evasit aliorum conatus, nisi forte Longitudo Cissoidis ejusmodi censeatur.</u>

Mr Leibnitz in his Letter to you represents that if the Differential method had been hinted to Mr Newton it would have been easy for him to have found it out. And certainly it was as easy for Mr Leibnitz by the hints which he had of the fluxional method in Mr Newtons Letters above mentioned to find out that method, notwithstanding that the sentences above set down within the brackets were in ciphers. In his journey therefore from London through Holland to Hannover, he was meditating how to extend the method of Tangents & particularly that of Slusius to all sorts of Problems, & first proposed to do it by a Table of tangents as appears by his Letter to Mr Oldenburg dated from Amsterdam 18 Novem. 1676, but after his arrival at Hannover he fell into the true method of doing it; as he himself has acknowledged in the Acta Eruditorum for April 1691 pag. 178, where he saith that by new matter & other affairs coming on he was hindred from fitting his arithmetical Quadrature for the Press, & after he found his new Analysis he did not think that Quadrature worth publishing in the vulgar manner. In his Letter of 27 Aug. 1676 he wondred that Mr Newton should pretend to such general methods, & affirmed that Inverse Problemes & many others could not be reduced to equations or <567r> quadratures: but after he returned to Hannover & fell into publick business he found out how to reduce such Problems to equations & quadratures & in his Letter of 21 Iune 1677 sent back a specimen of his new method, pretending (according to his usual candor) that he had found it long before. Clarissimi Slusij Methodum Tangentium, saith he, nondum esse absolutam Newtono assentior. Et jam a multo tempore rem Tangentium generalius tractavi, scilicet per differentias Ordinatarum. Then he sets down his new method of Tangents & how it gives the method of Slusius, & adds that it is of larger extent then his former method of Tangents & shews how it proceeds (like Mr Newton's method) without taking away surds, & then adds <u>Arbitror quæ celare voluit Newtonus de Tangentibus ducendis ab his non abludere. 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.</u> Mr Newton in his Letter of 24 Octob 1676 represented that he had a general method upon which he had written a Treatis five years before, & that this was a certain method of Tangents which extended to all sorts of Problems & readily gave the method of Slusius & stuck not at surds & faciliated Quadratures. Mr Leibnitz at length finds a method of Tangents which did the same things & thence concludes it like the method which Mr Newton had described in his Letters & concealed in this sentence exprest enigmatically, Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice versa. But now he contends either that Mr Newton when he wrote those Letters had no such method, or that the Differential method was found [jam tum a multo tempore] long before the year 1677.

When the Differential Method began to be celebrated in Holland Dr Wallis in the Introduction to his Works printed in the year 1695, wrote that this method was the same with the Method of fluxions which Mr Newton had explained to Mr Leibnitz in his Letters written in the year 1676, & had invented then years before that time or above. Mr Leibnitz in the correspondence which followed between him & Dr Wallis did not deny this nor contend for any thing more then that he had added some things to Mr Newton's method, & that what he had added was his own.

Afterwards in the year 1699 Mr Fatio published that Mr Newton was the oldest inventor of this Calculus by many years & Mr Leibnitz the second Inventor And Mr Leibnitz in his Answer published in the Acta Eruditorum for May 1710, did not dispute it, but commended Mr Newton for his candor in his Principia Philosophiæ (pag. 258, 259) where he represented that upon signifying to Mr Leibnitz in the year 1676 that he had a method of determining Maxima & minima, drawing Tangents, & solving such like Problems which proceeded without taking away surds & concealing the method in this sentence exprest enigmatically [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice versa:] Mr Leibnitz wrote back [the next year] that he had also fallen into such a method & communicated his method scarce differing from Mr Newton's except in forms of words & chcaraters: & that the foundation of both methods was continued in the second Lemma of the second book of his Principles p 250. By commending Mr Newton for his candor in making this Representation, he acknowledged the truth of the Representation: & its now too late to dispute it. All that he can now pretend to is that he found the differential method [a multo tempore] long before the year 1677 or at least that he has added some things to Mr Newton's method & that what he has added is his own. In either case he is to prove what he pretends to.

In the same Answer to Mr Fatio, Mr Leibnitz wrote: Quam [methodum] ante Dominum Newtonum et me nullus quod sciam Geometra habuit; uti ante hunc maximi nominis Geometriam Nemo Specimine publice dato se habere probavit: ante Dominos Bernoullios et me nullus communicavit. Bernoulli is here made <567v> a party & therefore can be no judge, & Mr Newton is here acknowledged to have been the first who by giving a publick specimen proved that he had this method. When Mr Leibnitz first published his method (vizt A.C. 1684) he extended it no further then to Tangents & maxima & minima, but added that it might be extended to the difficulter problems, which could not be solved without this calculus or another like it, meaning Mr Newton's years before. But neither of them proved the extent of their method to the difficulter Problems by a publick specimen before the edition of the Principia Philosophiæ, which the Marquess de l'Hospital acknowledged to consist almost wholy of this calculus. And Mr Leibnitz himself in his Letter from Hanover to Mr Newton 17 Mach 1693 now in the custody of the R. Society, wrote thus concerning it: 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 & summas exhibeant, Geometriam illam quam transcendentem appello Analysi quodammodo subjicere: nec res male processit. His first endeavour to do this was in his thre papers published in the year 1689 de lineis Opticis, de resistentia Medij & de motuum cœlestium causis. In the end of the second of these Papers he said: Nobis nunc fundamenta Geometrica jecisse suffecerit in quibus maxima consistebat difficultas. Et fortasse attente consideranti vias quasdem novas vel certe satis antea impeditas aperuisse videbimur. Omnia autem respondent nostræ Analysi Infinitorum &c. This was the first specimen which he published of the extent of his method to the difficulter Problems. It was writ in plain words & answered to the Analysis infinitorum in imitation of Mr Newton's Principia Philosophiæ writ in the same manner. For these three Papers were nothing else then a part of the Principia Philosophiæ put into a new dress. As Mr Leibnitz by imitating Mr Newton invented the differential calculus, so he imitated him in the first specimen which he gave of the extent of this calculus.

In October 1703 Dr Wallis died, the last of the old men who corresponded with Mr Oldenburgh & Mr Collins in these matters. And hitherto Mr Leibnitz forbore to claim the precedency of invention, & contented himself with pretending that he had invented the method apart & augmented it. But heh has since begun (according to his usual candor) to contradict Dr Wallis & Mr Fatio & represent Mr Newton a Plagiary, & now refuses to make good his accusation, pretending that he will not oblige the English so far & that the Committee of the Royall Society are not legal judges. That he might not seem to have received any light into the differential method from Mr Newtons Letter of 24 Octob. 1676 he said in his Answer, A multo tempore rem tangentium generalius tractavi, scilicet per differentias Ordinatarum, I have long ago made the method of Tangents by the differences of the Ordinates become a general methods. And now he goes a step further & pretends that he made the method general not only without receiving light from Mr Newton, but even before him. He confesses that he knew nothing of the higher Geometry in the year 1673 when he went from London to Paris, & that in the year 1675 he composed his Quadratura Arithmetica in a vulgar manner before he found out his new Analysis, & that in the year 1676 he knew not how to reduce inverse Problemes of Tangents to equations or quadratures: & yet he pretends that he found out the new Analysis not only without receiving light from Mr Newton but even before him. But it lies upon him to prove his pretenses. And finding himself unable to do this, he seeks excuses & makes a clamour. But he is to know that by the laws of all nations he that accuses another publickly & doth not prove his accusation as publickly, is to be accounted guilty of calumny. And thus much in answer to the first part of his Postscript.

<568r>

To Conti?

Sir

I thank you for shewing me the Postscript to the Letter of Mr Leibnitz. For setting those matters in a true light, I will describe them to you in an historical manner in as few words as I can.

<569r>

<568v>

The method of converging Series & the Method of Fluxions have great affinity with one another, so as both together to compose one universal Analysis & separately to be imperfect. I found them both in the years 1665 & 1666 by degrees. And in Iuly 1699 Dr Barrow communicated to Mr Collins a little Tract of Analysis written by me in which I founded the method of Series upon three Rules & demmonstrated the first Rule by the method of fluxions. And in the year 1671 I wrote a larger Tract upon these two methods, with a designe to have published it together with another Tract concerning Light & Colours. But finding that these matters began to entangle me in disputes I laid my designe aside being in love with a quiet life, as I represented in my Letter of 24 October 1676

In the year 1669, 1670, 1671 & 1672 Mr Collins communicated to his friends at home & abroad several series partly taken out of the said Tract of Analysis & partly sent to him by Mr Iames Gregory of Scotland. For M^r. Gregory by the help of one of my Series fell into the same Method of converging Series. And in a Letter to Mr Collins dated 10 Decem 1672 I described the universality of the Method of fluxions saying that it extended to Problemes about the Tangents, Curvities, Areas, Lengths, Centers of gravity of Curves Geometrical or Mechanical &c & proceeded without taking away surds. And I there gave an example of this method in drawing of Tangents by a Rule which proved to be the Method of Slusius. And a copy of this Letter was sent to Mr Leibnitz at Paris by Mr Oldenburg in the same Packet with my Letter of Iune 13th 1676, & therefore it came to h is hands. And these two Letters (if I mistake not) gave him the first notice of such a general method of Analysis

He was in London in Feb 1673 & there pretended to the method of Mouton, & after a few days went thence to Paris & staid there some time before he knew any thing of the higher Geometry, but at length (I think in the year 1674) he was instructed in it at Paris by Mr Hugens, as he represents in his Postscript above mentioned. And then he wrote For he began that year to write to Mr Oldenburg about the higher Geometry pretending that that he could find a series for any Arch whose sine was known. If the proportion of the Arc to the whole circumference was known it gave him the whole circumference, if the proportion was not known yet the series gave him the Arc. And this he pretended to be his own invention, but did not yet to know the Method of inventing it. For in his Letter of 12 May 1676 he desired Mr Oldenburg to procure from Mr Collins the method of finding this very series & tos end it to him. He is desired to let us know how to came by that series two years before he had the method of finding it

In a Letter dated April 15th 1675 Mr Oldenburg sent to Mr Leibnits from Mr Collins eight series invented, some of them by me, & others by Mr Gregory. And Mr Leibnitz in a Letter dated 20 May following, & still extant acknowledged the receipt thereof, & said he would compare those series with his own. At that time he did not know any of those series to be his own; & yet the same yeare he communicated one of them to his friends at Paris as his own & afterwards published it in Germany as his own without ever acknowledging that he had received it from London. This series was sent by Mr Gregory to Mr Collins the year 1671 , < insertion from from the end of the line > & a copy of Mr Greg <569r> ories <568v> Letter was sent to Mr Leibnitz in the same packet with my Letter of 13 Iune 1676 & therefore ca <569r> me to his hands. < text from f 568v resumes >

Vpon the aforesaid request of Mr Leibnitz Mr Oldenburg & Mr Collins wrote to me to communicate my method of Series to Mr Leibnitz, & I did so in my Letter of 13 Iune 1676, & therin I added that Analysis by means of this method was much enlarged so as to extend to almost all Problems <569r> except perhaps some numeral ones like those of Diophantus, but became not universal without the help of some further methods of reducing Problems to converging series which I forbore to describe, having tired my self with these studies long ago so to have absteined from them the last five years, meaning since the year 1671. And Mr Leibnitz in his Answer dated 27 Aug. 1676 replied that he did not beleive that Mr Newton's Method was so general: for (said he) there were many Problems so difficult as not to depend upon Equations or Quadratures, such as are (among many others) the inverse Problemes of Tangents. And by these words its most certain that M^r

Leibnitz had not yet found out the Differential method. In the same Answer he pretended to have found some of th{ese} series before he received them from me, that is before he had the method of finding them.

In my Answer dated 24 Octob. 1676, at the request of Mr Leibnitz I described how I found out the Method of Series a little before the Plague which happened in the year 1665. And after I had mentioned the aforesaid Tract communicated by Dr Barrow to Mr Collins, (which has been since printed from a copy found in the hand writing of Mr Collins by Mr Iones who purchased his Library) & the other Tract written upon both the methods in the year 1671: I added that the Method of fluxions readily gave the method of Tangents of Slusius & determined Maxima & Minima & Quadratures & other hard Problemes, & stuck not at Equations involving surds. & was easy to be found. And because I was not then at leasure to describe it at large I couched it in the following Probleme exprest Enigmatically An Equation involving any number of fluent quantities being given, to find their fluxions; & on the contrary. This is the first Proposition of my book of Quadratures. And in this Letter I cited so many things out of that Book as abundantly shews that that Book was then written & what was the method of fluxions which I wrote of in that Letter. And in the end of the Letter I added that Inverse Problemes of Tangents & others more difficult were in my power by the help of two methods, which methods I couched in the following words exprest enigmatically. One method consists in the extraction of a fluent quantity out of an equation involving its fluxion: the other in assuming a Series for any unknown quantity from which the rest may conveniently be deduced & in collating the homologous terms of the resulting æquation for determining the terms of the assumed series. By these two methods I had made the inverse method of Series become general five years before the writing of that letter or above. The first method shews not only that I had fluxional equations in those days, but also that I had then carried the inverse method of fluxions to a great degree of perfection. The second method is now claimed from me by Mr Leibnitz For in h is aforesaid Postscript he allows that I preceded him in the method of series, but he adds that at length he found a general method for series & after that he had no further need of my extractions. This General method he described in the Acta Eruditorum for April 1693 pag. 178. And it is the same <569v> with mine.

He came to London the second time in October 1676 & before he went from thence into Holland Mr Collins shewed him a part of his correspondence with Mr Gregory & me as he tells you in his Postscript. And there he saw my aforesaid Reply or Letter of 24 October 1676 just then received by Mr Oldenburg & put into the hands of Mr Collins, I suppose to be copied. For he tells you that in the papers which Mr Collins shewed him he observed that I acknowledged my ignorance in many things & said among other things that I had found nothing about the dimension of the celebrated Curves besides the dimension of the Cissoid. And this passage is in the said Letter of 24 Octob 1676. Whether Mr Collins did not at the same time shew him my Analysis may be doubted. For that Analysis was the fountain of all the correspondence about Series. He complains that all this passage about my pretended ignorance was omitted by the Committee of the R. Society, & gives this as an instance of the partiality of the Committee in not printing the Letters entire. But he accuses them injuriously for the passage is printed entire in the Commercium Epistolicum pag. 74. My Letter of 10 Decem. 1672 is printed entire The Letters of Mr Leibnitz dated 15 Iuly 1674, 26 Octob 1674, 12 Iuly 1675 28 Decem 1675, 27 Aug. 1676, 18 Novem 1676, 21 Iune 1677 & 12 Iuly 1677 & those of Mr Newton dated 13 Iune 1676 & 24 Octob 1676 have been printed entire by Dr Wallis without any dispute arising upon them & they that please to compare them with the excerpta taken out of them by the Committee of the R. Society will find that nothing of moment to the controversy has been omitted by them. And that of Mr Oldenburg dated 15 Apr. 1675 & those of Mr Leibnitz extant in his own hand writing & dated 20 May 1675 & 12 May 1676 have been collated with the extracts in the presence of many Gentlemen of Germany Italy France & England you your self being one of them. And these being all the Letters of Moment (for those of Mr Collins written in the years 1669, 1670, 1671 & 1672 were only to show how free he was in communicating the Series which he had received from me & Mr Gregory) it is evident that the complaint of Mr Leibnitz against the Committee of the R. Society is frivolous & injurious.

Mr Leibnitz in his aforesaid Postscript saith that it would have been easy for me to have found out the differential method if it had been hinted to me. And this is an acknowledgment that it was easy for him to find it out by the light which I gave him into it. For in my Letter of 24 Octob. 1676 I hinted the method to

him very plainly & said it was obvious. I gave him so much light into it that Dr Wallis in the Preface to the two first Volumes of his works printed above 20 years ago said that in my two Letters written in the year 1676 I explained to Mr Leibnitz that Method found by me ten years before that year or above. And Mr Leibnitz in the Letters which followed thereupon between him & Doctor Wallis did not deny what the Doctor had affirmed, nor blame him for it nor made the least dispute about it, but said only that he had added to my method as Des Cartes had added to the Analysis of Vieta.

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To Conti? {about Desmaiaux}

Sir

It requiring some time to write Letters receive answers & do what else you & I were discoursing this morning before the publishing of the Letters of Mr Leibnitz : I beg the favour to signify to your Bookseller that if he pleases to deferr publishing them till Lady next I will give him twelve Guineas as a recompense for the loss of his time. I am

        Your humble Servant

          Is. Newton.

<center><571r></center>

Sir

The more I consider the Postscript of Mr Leibnitz the less I think it deserves an answer. For it is nothing but a piece of railery from the beginning to the end. He saith that it doth not appear that I had the infinitesimal Characteristique & Analysis before him, but he is to prove that he had it before me. For he has accused me of plagiary before the R. S. & by the laws of all nations he is guilty of calumny if he doth not prove his accusation. He appeals from the judgment of the Committee of the R. S. to the judgment of Mr Bernoulli But N Bernoulli claims a share with Mr Leibnitz in the infinitesimal method, Mr Bernoulli hath erred & Dr Wallis above 20 years ago gave a contrary judgment. & Dr Keill hath shewed that. He saith that it was easy for me to have found the Method before him if it had been notified to me: & Dr Wallis said above 20 years ago that I explained it to Mr Leibnitz in the year 1676. He saith that the Committee of the R. Society have attacked his candor by misrepresentations, & that he will not answer their little reasons: but thethey that read the Papers printed by the Committee with their Observations upon them will find that the whole is matter of fact which admits of no answer. He complains that in falling upon series they go from the fact, but the Question is whether Mr Leibnitz or I be guilty of Plagiary & what they say about series is very apposite to decide that Question He insists upon his own candor & endeavours to make himself a witness in his own c & they speak to the credit of the witness. On the other hand he himself is guilty of what he complains of in others. For he goes from the fact both when he falls foul upon my Philosophy, & when he sends mathematical Problems to try who was the best Mathematician 45 or 50 years ago when he knew nothing of Geometry. He saith that I invented series before him but at length he found a general method after which he had no further need of my extractions. But this general method is mine. It is mentioned in my Letters of 13 Iune & 24 Octob. 1676. He complains that the Comittee of the R. S. did not print the Letters entire as Dr Wallis did with his consent: but it would have been impertinent to print what did not relate to the matter in hand. He saith that when he came to London the second time (which was in Octob. 1676) he saw in the hands of Mr Collins a part of his Commerce with me & Gregory, & there he observed that I acknowledged my ignorance in many things & particularly that I said that I had found nothing about the dimension of the celebrated Curvi-lineaars besides that of the Cissoid: & that the Committee had supprest all this. He alledges this as an instance that they had acted partially in omitting things which made against me. But he injures them. For you will find all this printed in the Commercium Epistolicum pag 74, & I am not ashamed of it. It is in my Letter of 24 Octob. 1676 & therefore he saw that Letter in the hands of Mr Collins before he left London. And he might at the

same time see my Analysis which Dr Barrow in the year 1669 communicated to Mr Collins, & in which my method of moments & fluxions was described.

After this he falls foul upon my Philosophy, that is upon the Philosophy of the ancient Phenicians & Greeks as if they had introduced miracles & occult qualities. & <571v> tells you that he has proved to Mr Bayle that the word Miracles signifies not only wonders but also constant events, such as by reason of their constancy create no wonder. He tells you also that God cannot be in the world without animating the world tho a mans soul according to his Philosophy doth not animate his body. He accuses me as if I said that God had a sensorium in a literal sense. He pretends that all places not filled with tangible bodies may be filled with an intangible corporeal fluid, that its the fault of the workman & not of the Watch that it will at length cease, & that it would be Gods fault if the world should ever want an amendmente He commends Experimentall Philosophy & yet adheres to such Hypotheses as can never be proved by experiments & brings his hypotheses as arguments against things proved from experiments by the argument of Induction & thereby endeavours to overthrow that Philosophy, & to set up in its room a heap of precarious Hypotheses which are nothing better then a Romance.

But I beg leave to acquaint you that its almost 40 years since I left of writing Letters about Mathematicks & Philosophy & twenty years since I left of those studies. And therefore I cannot now suffer my self to be engaged in disputes of this kind; especially since they are nothing to the Question in hand, about the infinitesimal method, For understanding this Question more fully I must referr you to the Commercium Epistolicum it self & to the Account given of it in the Phil. Transactions & the Answer of Dr Keill to the Libel published in Germany & Holland against the Committee of the R. S.

He accuses me & by consequence the ancient Phenicians & Greeks as if they had introduced miracles & the occult qualities of the Schoolmen into Philosophy. And to make this appear he tells us that he has proved to Mr Bayle that the word Miracles, that is wonders, includes the laws imprest by God upon nature tho by their constant acting they create no wonder, & that the words occult qualities signify qualities which are not occult but whose causes be occult tho qualities be very manifest. He saith that God must be Intelligentia supramundana because if he were in the world he would be the soul of the world that is he would animate the world, & yet according to his Philosophy of an Harmonia præstabilita the soul of a man doth not animate his body He accuses me as if I affirmed that God hath a Sensorium in a litteral sense. He saith that I have not demonstrated a vacuum nor universal gravity nor Atomes. But he denyes Conclusions without shewing the fault of the Premisses, & means that the Argument of Induction from Experiments upon which experimental Philosophy is grounded is not a good one. For I never attempted to demonstrate any thing universally in natural Philosophy by any stronger argument then that of Induction from Experiments, And as for Atomes I never attempted to demonstrate them by this Argument, but put them amongst a set of Quæres. He saith that Space is the order of coexistences & time the order of successive existences: I suppose he meanes that space is the order of coexistences in space & time the order of successive existences in time, or that space is space in space & time is time in time. He insinuates that it is the fault of the workman & not of the materials that a Watch will at length cease to go & in like manner that it would be Gods fault if the world should ever decay & want an amendment. And by the same way of arguing a man may say that it would be Gods fault if matter doth not think He applauds Experimental Philosophy but recommends Hypotheses to be admitted into Philosophy in order to be examined by experiments: whereas he should propose not Hypotheses to be admitted but Questions to be examined & decided by <572r> experiments before they are admitted into Philosophy. † † And whilst he applauds experimental Philosophy & cries out against miracles, he introduces an Hypothesis of an Harmonia præstabilita which [is contrary to the daily experience of all Mankind &] cannot be true without an incredible miracle, & which is contrary to the daily experience of all mankind For all men find by experience that they can move their bodies by their will, & that they see & hear & feel by means of their bodies. [He is of opinion that space void of all tangible body may be full of a corporeal intangible fluid whereas the Ancients beleived that all things intangible were incorporeal. I understand tangibility not in a mathematical but in a physical sense, such a tangibility as by some resistance can affect the sense of touching.] He glories in the number of disciples, but you know that he has spent his life of making them by a general correspondence whilst I leave truth to shift for it self. For its almost 40 years since I left of all correspondence about Mathematicks & Philosophy & about 20 since I left of these studies. And for that reason I hope you will pardon me if I am averse from being engaged in disputes of this kind.

He sends you also Mathematical Problemes to be solved by the English Mathematicians. And all this is nothing else then an amusement to avoid proving his accusation against me & returning a fair answer to the matter of fact which has been published by order of the R. Society. If he pleases to return such answer, I desire that he will be constant to himself & continue to acknowledg whatever he acknowledged above 15 years ago & not contradict what he did not contradict in those days; or else to forbeare boasting of his candor, By his Letter of   he acknowledged the receipt of Mr Oldenburgs Letter of   & expect that he continue to acknowledge it still. By his Letter of   he acknowledged that he {had} not then the method of finding a series for the arc whose sine was given, & I expect that he acknowledg it still.

He complains that in falling upon series they go from the fact: & yet he himself goes from the fact both in falling upon my Philosophy & in sending a Probleme to try who was the best Mathematician 45 or 50 years ago at which time he understood nothing of Geometry The Question is about Mr Leibnitz's candor & mine & if he claimed one of my Series as invented by himself & afterwards wrote to Mr Oldenburg to procure & send to him the method of inventing it; if he received from London a series invented by Mr Gregory & afterwards published it as his own: the world by these instances may judge of his candor in being silent 20 years ago when Dr Wallis told him that I had by my Letters in the year 1676 explained to him the method of fluxions found by me ten years before or above & now pretending that he was the first inventor. He saith that I invented Series before him

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Dr Wallis died in October 1703, the last of the old men who knew what had passed between Mr Leibnitz & me by means of Mr Oldenburg. And afterwards I was accused in the Acta Eruditorum & before the R. Society as a plagiary who had taken the method from Mr Leibnitz. And when the R. Society caused the ancient Letters & papers extant in their Archives & Letter Books & in the Library of Mr Collins to be published, all which are unanswerable matters of fact; instead of answering the same in a fair manner, & proving his accusation of plagiary a defamatory Libel dated 29 Iuly 1713 was published against me in Germany without the name of the Author or publisher or City where it was published, & dispersed over Germany France & Italy, & the Libel it self represents that Mr Leibnitz set it on foot. And instead of proving his accusation he goes on to write defamatory wrangling Letters.

In the Latter part of his Postscript he falls foul upon my Philosophy as if I (& by consequence the ancient Phenicians & Greeks) introduced Miracles & occult qualities. And to make this appear he gives the name of miracles or wonders to the laws imprest by God upon nature tho by reason of their constant working they create no wonder, & that of occult qualities to qualities which are not occult but whose causes are occult tho the qualities themselves be very manifest.

<center><573r></center>

Sir

The more I consider the Postscript of Mr Leibnitz the less I think it deserves an answer. For it is nothing but a piece of railery from the beginning to the end. He saith that it doth not appear that I had the Infinitesimal Characteristique & Calculus before him; but he is to prove that he had it before me. For he has accused me of plagiary & by the Laws of all nations he is guilty of calumny if he doth not prove his accusation. He appeals from the judgment of the Royal Society to the judgment of his friend Mr Bernoulli: But Dr Wallis gave a contrary judgment above 20 years ago, without being contradicted till of late, & Dr Keill hath proved that Mr Bernoulli hath erred. Mr Leibnitz saith that it was easy for me tohave found the Method before him if I had been advised of it: And Dr Wallis published above 20 years ago that I explained it to Mr Leibnitz in the year 1676. He saith that the Committee of the Royal Society have attaqued his Candor by misrepresentations, & that he will not answer their little reasons: but they that read the Papers printed by the Committee with their Observations upon them, will find that the whole is matter of fact which admits of no answer. He complains that in falling upon series they go from the fact, & yet he himself goes from the fact both in falling upon my Philosophy & in sending a Probleme to try who was the best Mathematician 45 or 50 years ago, at which

time he understood nothing of Geometry. The Question is about the candor of Mr Leibnitz & me . And if he claimed one of my Series as invented by himself, & afterwards wrote to Mr Oldenburg to procure & send to him the Method of inventing it; if he received from London a Series invented by Mr Gregory, & afterwards published it as his own: the world by these instances may judge of his candor in pretending now to be the first inventor of the Infinitesimal method, which he did not pretend to till of late. For 21 years ago when Dr Wallis had published that I by my Letters in the year 1676 had explained it to Mr Leibnitz the infinitesimal method found by me ten years before or above & given him notice hereof he found no fault with what the Doctor had said nor pretended to any thing more then that he had added some things to my Method He saith that I invented Series before him, but at length he found a general method after which he had no further need of my extractions: But this general Method is mine. It is mentioned in my Letters of 13 Iune & 24 Octob. 1676. He complains that the Committee of the Royal Society did not print the Letters entire as Dr Wallis did with his consent: & yet it would have been impertinent to print such parts of the Letters as did not relate to the matter in hand. He saith that when he came to London the second time (which was in October 1676) he saw in the hands of <573v> Mr Collins a part of his Commerce with me & Mr Gregory, & there he observed that I acknowledged my ignorance in many things & particularly that I said that I had found nothing about the dimension of the Curvilinear figures besides that of the Cissoid, & that the Committee had supprest all this. He alledges this as an instance that they had acted partially in omitting things which made against me. But he injures them. For you will find all this printed in the Commercium Epistolicum pag. 74, & I am not ashamed of it. It is in my Letter of 24 Octob. 1676, & therefore he saw that Letter in the hands of Mr Collins before he left London. And he might at the same time see my Analysis which Dr Barrow in the year 1669 communicated to Mr Collins, & in which my method of moments & fluxions was described before Mr Leibnitz knew any thing of Geometry.

[1] Acta Erudit. pro Novem. 1684