Extract from Bernoulli's letter of 7 June 1713, with Newton's Observations on it

Author: Isaac Newton

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Remarques sur la dispute entre Monsieur de Leibnitz et Monsieur Newton, touchant l'invention de l'Arithmetique differentiale.

Ce que bon trouve sur ce sujet dans le Iournal literaire de la Hage, et dans nos actes allemans p: 587 ne s'accorde pas en ses circonstances, et celuy qui l'a conou n'a pas êté bien instruit.

Monsieur de Liebnitz, et Monsieur Newton jamais n'ont eu des disputes entre eux sur aucune matiere; Car Monsieur Newton n'a jamais fait connoitre vouloir s attribuer à luy, et disputer à Monsieur de Leibnitz l'invention de l'Arithmetique differentiale.

Monsieur de Leibnitz n'a su, que par le rapport de quelques-uns, qui ont lu le Commercium Epistolicum imprimé à Londres il y a quelque tems, que Monsieur Newton eut part à une telle injustice, que Monsieur de Leibnitz êtant encore à Viénne, et qui n'a pas-encore vu ce traité attribue plûtôt à la mauvaise volunté de les envieux ou des gens ignorans. Aussy n'en a t-il jamais porté ses plaintes à la Societè Royale en Angleterre, comme superflu dans une cause si juste: seulement a-t-il pris occasion d'écrire à Monsieur le Secretaire de la Societé, qu'il ne doutoit point, que la Societé A Monsieur Newton même desaprouveroit entieérement un tel procedé. En sorte que la Societé n'a pas pu examiner les argumens des deux parties, et prononcer un arrêt definitif.

Voicy la verité du fait: Il y a environ quarante ans, plus ou moins, que Monsieur de Leibnitz, Oldenbourg, Newton, Collin's et autres ont eu un Commerce des lettres entre eux, dont il se trouve quelque chose d'imprimé <487v> dans la troìsiéme partie des Oeuvres Mathematiques de Wallisius. Il paroit par les lettres publieês par Wallisius que Monsieur Newton faìsoìt mystere d'une invention laquelle il a ensuite debité être l'Arithmetique differentiale, au lieu que Monsieur de Leibnitz luy communiquoit de bonne foi le fonds de cette Arithmétique, quoy qu'il ait paru du depuis, que Monsieur Newton ne l'avoit pas bien compris, sur tout ce qui regarde differentias differentiarum; Aprés cela on a trouve encore d'autres lettres ecrites entre Collins et Ses Amis, que l'on a fait imprimer à Londres avec des notes, par lesquelles on pretend prouver par des conjectures mal fondèes, et par des suppositions faus, Ses, que Monsieur Newton a inventé l'Arithmetique differentiale, et que Monsieur de Leibnitz l'a aprise de luy, non obstant que le contraire paroit clairement par les lettres de ces deux Messeurs publiées par Monsieur Wallisius.

L'auteur de ces remarques a jugé trop temerairement de choses qui me luy ont pas êté assés connues; et il a mal reussi à deviner, par quelle voye Monsieur de Leibnitz est parvenu à cette invention. Il s'est trouve de plus que Monsieur Newton n'a pas bién compris la vraye Arithmetique differentiale, lors qu'en 1687 il mit au jour Ses Principia Philosophiæ naturalis Mathematica, vu qu'il ne s'en est point servi, lorsqu'il en avoit la meilleux occasion du monde; mais a même commis des fautes capitales, tout-à-fait contraires aux principes de l'Arithmetique differentiàle, comme un Mathematicién entierement impartial a remarqué le premier. Aussy

Monsieur Newton, aprés en avoir été a verti, a corrigé ces fautes, en faisant changer quelques feilles dans la nouvelle impression, qui à paru l'année passée. Monsieur de Leibnitz avoit déja auparant publié son Arithmetique en l'annee 1684: mais Monsieur Newton n'a rien donné sur cette matiere, jusqu'à ce qu'en 1693 le 2$\underline{\text{me}}$ Tome des Oeuvres Mathematiques de Wallisius vit le <488r> jour, lorsque le systeme de Monsieur de Leibnitz êtoit déja en reputation par tout, et que partientierement les deux fréres Messieurs Iaques et Iean Bernoulli en eussent fait publiquement l'essais avec beaucoup d'aplaudissement; ce qui rendoit sans doute Monsieur Newton /quoyqu'un peu trop tard:/ Si temeraire pour vouloir y avoir part.

L'en voyoit d'abord ches Wallisius l'invention de Monsieur de Leibnitz se presenter, mais en d'autres figures et termes plus impropres. Cependant Monsieur Newton n'any alors, ny longtems aprés, eu l'audace, de troubler Monsieur de Leibnitz dans la possession de son invention Et tant que Hugenius et Wallisius, qui êtoient des Iuges impartiaux, vivoient, lesquels avount des connoissances â fond de cette affaire, il a bién jugé qu'il ne reussiroìt pas; C'est pourquoy il a attendu jusqu'à ce qu'il n'y eut plus personne de ceux, qui puissent être témoins du progrés de cette science, et qui mêmes y avoient beaucoup de part, Mais maintenant il a recours à des novices, qui ignorent ce qui s'est passe autrefois, et qui jugent selon leurs preoccupations et leurs sottes passions Un certain Novice s'est voulu mettre en reputation, en attaquant Monsieur de Leibnitz et luy adsersant une espece de defi; Mais Monsieur de Leibnitz voyant, que cet homme n'êtoit pas d'humour à se laisser detromper, il n'a pas voulu s'engager avec luy. Et il a fort bién fait, car sans cela il auroit eu un pretexte pour dire, que l'on avoit les argumens de deux parties, sur lesquels on pouvoit decider; au lieu que les Iuges pretendus n'ont que les argumens d'un coté seul.

Dans cette vuë l'on a fait imprimer le Commercium Epistolicum, dans lequel on croit avoir trouvé sur quoy se fonder, bién qu'il ne s'y trouve rién du tout <488v> qui puisse decider la dispute du veritable Inventeur de l'Arithmetique differentiale. Et Monsieur Newton a commis la foiblesse de se laisser entrai{t} sur ces fausses apparences. S'il avoit gardé le silence, il seroit demeuré participant de l'invention attendu que Monsieur de Leibnitz avoit cru sur sa parole, qu'il avoit trouvé quelque chose, qui aprochat en quelque façon de l'Arithmetique differentiale Mais presentement se trouve le contraire.

Des Gens entendus et impartiaux ont ris d'une pretension si tardive et si mal fondée; et l'on a déja fait imprimer l'opinion d'un celebre Mathematicièn fondée {h}on seulement sur le long silence, mais /: ce qui plus est./ sur les fautes de Monsieur Newton, lesquelles prouvent evidemment, qu'il n'a pas seulement compris ce qu'il pretend avoir inventé avant Monsieur de Leibnitz il y a 40 ans.

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Remarques upon the dispute between Monsr Leibnitz & Monsr Newton touching the invention of the differential Arithmetique

That which is found upon this subject in the Iournal literaire of the Hague & in our German Acts p. 587 do not agree in their circumstances, & he that conceived it was not well instructed.

Monsr Leibnitz & Monsr Newton never had any disputes between them about any matter. For Monsr Newton never made it known that he would attribute to himself & dispute with Mr Leibnitz the invention of the differential Arithmetique.

Mr Leibnitz knew not but by the report of some who had read the Commercium Epistolicum printed at London some time ago that Mr Newton had a share in such a piece of injustice, seing that Mr Leinitz was still at Vienna & had not seen the treatise attributed for the most part to the ill will of the envious or of ignorant people. So he has not yet sent his complaint to the Royal Society in England as being superfluous in a cause so just. He only took occasion to write to the Secretary of the Society that he doubted not at all but the Society & Mr Newton himself would entirely disapprove such a proceeding. So that the Society has not been able at all to examine the arguments on both sides & to pronounce a definitive sentence

See the truth of the fact. It is about 40 years more or less that Mrs Leibnitz Oldenburg Newton Collins & others had a commerce of Letters with one another.[1]

Commercium Epistolicum Collinij et aliorum prodijt anno ineunte 1713. Et post mensis sex vel septem prodijs Responsum subsequens in Germania sine nomine vel autoris vel Typographi vel Vrbis in qua impressa fuit.

29 Iulij 1713.

L . . . . us nunc Viennæ Austriæ agens ob distantiam locorum nondum vidit libellum in Anglia nuper editum &c — nec vitium paucorum genti imputari debet.

In scripto hocce maledico, dicitur Leibnitium nondum vidisse Commercium Epistolicum sed a Mathematico quodam primario postulasse ut is re examinata judicium suum proferret, & Mathematicum literis 7 Iunij datis D. Leibnitio respondisse. Vnde patet Leibnitium ipsum curasse ut hæc ederentur. Dicitur etiam in hoc scripto quod Modum quo L . . . . us invenit seriem Gregorio ascriptam ipse statim Hugenio B. Lutetiæ agenti communicavit qui et per Epistolam laudavit. Et quid L . . . . . us & Hugenius ante annos feret quadraginta Lutetiæ egissent, solo Leibnitio innotescere potuit. Ideoque L . . . . us scriptum hocce diffamatorium composuit. Nam et phrasis [illaudabili laudis amore] stylum Leibnitianum sapit. Vtrum vero Mathematicus ille primarius sit L . . . . us ipse **{illeg}**lius quisquam nondum constat.

In Diario Literario Iohnsoni pro mensibus Novembri ac Decembri anni 1713 impressum fuit hoc scriptum Gallice versum cum Epistola sequente præfixa

Pag. 2. l. 21. Fluxiones quæ differentiæ non sunt N . . . . us quandoque literis punctatis quandoque alijs notis pro lubitu designat. L . . . . us pro fluxionibus nullas habet notas. N . . . . us pro differentijs ponit rectangula sub fluxionibus & momento temporis. Hoc fecit in Analysi anno 1669 ad Collinium missæ. Hoc facit usque hodie

Ad p 2. l. 16 Quamvis Newtonus in Epistola sua ad Oldenburgum 24 Octob. 1676 data Methodum fluxionum hac sententia complexus sit: [Data æquatione fluentes quotcunque quantitates involvente fluxiones invenire et vice versa,] atque iterum hacce, [fluentem ex æquatione fluxione s involvente extrahere] & librum de hac methodo et methodo serierum anno 1671 se scripsisse testatus sit & in Epistola 10 Decem 1672 ad Collinium data universalitatem hujus methodi descripsit & in Analysi anno 166**{9}** ad Collinium missæ specimen dedit hujus calculi, tamen Iudex a L . . . . o constitutus credit Newtonum per ea tempora de Calculo fluxionum ne quidem somniasse

Ad p. 2. l. 20 Eodem argumento probare potuit Newtonum non habuisse methodum fluxionum ubi scripsit Introductionem ad Quadraturam Curvarum quia in Introductione illa non utitur literis punctatis.

Ad l. 2. Annon vidit?

Ad l. 7. Primam a Barrovio

Ad l. 8, 9. Cum amicis ex**{c}**o**{illeg}** c**{illeg}**ta est anno 1690 Newtonus edidit Anno 1687 in Schol. ad Lem II Lib. II Princip. I

Ad l. 13, 14. Communicavit Collinio Anno 1669 & L. . . . o anno 1676.

Ad pag. 2. l. 6, 7. Calculum fluxionum ad imitationem calculi differentialis formatum fuisse fingere cœpit in Actis Eruditorum pro mense Ianuario anni 1705 Figmentum vero confirmavit in Epistola sua ad D. Sloane 29 Decem 1711 data: Et inde nata est hæc controversia.

Ad l. 4 Vox [illaudabilis] a L . . . . o solo usurpari solet.

Ad l. 9 Iudicem anonymum L . . . . us constituit, id est, vel seipsum vel amicum cui maxime potuit confidere.

Ad l. 16, 19. Mathematicus conjectatur, non probat.

Ad l. 21. N . . . . us literas punctatas adhibet pro fluxionibus quæ differentiæ non sunt. Et fluxiones quandoque quandoque alijs notis designat L . . . . us pro fluxionibus nullas habet Notas.

Ad l. 22. In Analysi ad Collinsium anno 1669 missæ quantitates et earum fluxiones designatur per literas, momenta vero per rectangula sub fluxionibus et litera o qua momentum temporis designatur. Et hujusmodi rectangulis pro momentis Newtonus usque hodie usurpat. In Scholio ad Lem II Lib. II Princip. anno 1686 conscripto demonstrantur elementa methodi fluxionum synthetica & pro fluentibus et fluxionibus ponuntur literæ majusculæ et minusculæ, & pro momentis eædem literæ minusculæ subintellecto factore o. Mathematicus vero fradulentur fingiat methodum fluxionum absque literis punctatis invenire non potuisse. Porro in Libro de Quadratura Curvarum ante annum 1676 composito pro fluxionibus quandoque litteræ punctatæ quandoque aliæ notæ adhibentur.

Ad l. 24 26, 27. Inventæ sunt Propositiones in libro illo per methodum fluxionum Sed demonstratæ sunt synthetice ut in Geometriam admitterentur. Nam Propositiones (ob certitudinem Geometriæ) non prius in Geometriam admittendæ sunt quam demonstrantur synthetice. Proinde nulla erat occasio in hoc Libro utendi calculo fluxionum.

Ad l. 29. Imo In secundo Volumine anno 1693 impresso pag 392 393. Newtonus utique anno 1692 postulante Wallisio explicationem epistolæ suæ 24 Octob ann{o} 1676 scriptæ, et extractionem fluentis ex æquatione fluxionem involventæ cum Wallisio communicavit. Et calculus differentialis nonddum invaluerat ubique

Ad p. 3. l. 2. Falso dicitur quod N . . . . us incrementum constans ipsius x nunc notat per x punctatum uno puncto. Ponitur x punctatum non pro incremento sed pro fluxione ipsius x. Et incrementum constans ipsius x usque hodie designatur per o (id est per $\dot{x}o$ seu $1 \times o$, subintellecto factore $\dot{x}$ seu 1) Et hæc Notatio magis commoda est quam ea calculi differentialis

Ad l. 3, 4 Falso etiam dicitur quod N . . . . us Regulam circa gradus ulteriores falsam dedit: Regulam dedit verissimam in Prop. I. Libri de Quadratura curvarum. Et Hanc Regulam Wallisius publicavit anno 1693 in secundo Volumine Operum suorum pag. 392.

Ad l. 4 Erravit ergo eminens ille Mathematicus nempe Bernoullius ut etiam ab alio eminente mathematico nuper demonstratum est. Cæterum hic observandum venit quod hæc Epistola nuper Gallice edita fuit tanquam a Bernoullio scripta & ut Bernoullio scripta videretur, omissa sunt verba ad Bernoullium spectantia [quemadmodum ab eminente quodam mathematico dudum notatum est.]

Ad l. 11 Vult Author noster L . . . . um Calculum differentialem in numeris primum invenisse a Moutono prius inventam; & (excogitata Analysi infinitesimalium) ad Geometriam transtulisse. Certe L . . . . us anno 1677 ubi primum incidit in methodum infinitesimalem, methodum differentialem Barrovij <491r> mutatis symbolis misisse ad Oldenburgum ut suam.

L. 15, 16, 17 Rixatur L . . . . us cum N . . . . o

L 20. Disputat L . . . . us contra Epistolam propria manu scriptam.

L. 31. Hanc mehtodum generalem N . . . . us invenit

P. 4. l. 5. Inventioni Newtonus ausam dedit. Vide Epistolas Newtoni 24 Oct. 1676 et L . . . . i 21 Iunij 1677.

P. 2. l. 22.

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Observations upon a paper entituled Remarks concerning the difference between Mr Leibnitz & Mr Newton & upon the translation of a Latine piece where this judgment is found.

The Latine piece was printed in Germany in the form of a defamatory Libel in August last without the name of any author, the Letters L—s & N—s being put for Leibnitius & Newtonus. And being void of argument & full of reflexions without any proof was judged too mean a piece to deserve any answer. But being sent to the Hague to be reprinted in your Iournal, I send you the following Observations upon it.

1 The author of the Latin piece tells us that Mr Leibnitz being at Vienna had not yet seen the Commercium Epistolicum. And this he could not know without keeping a correspondence with Mr Leibnitz. He tells us further that Mr Leibnitz not being at leisure to examin this affair himself he had referred it to the judgment of a Mathematician of the first rank & very skilful in these things, & very free from partiality. So then this paper was writ by the correspondents of Mr Leibnitz & Mr Leibnitz himself was the first mover. And therefore it must be looked upon as the ablest defence that he & his correspondents were able to make. We are indeed told that he had not yet seen the Commercium. But a Copy of it was sent to him by the Resident of the Elector of Hannover & another was sent to Lipsic for him & an answer came back thence to the Secretary of the R. S. that it was sent to him. He knew how to write to a great Mathematician who had the book And those in England who have seen the Latin Paper generally conclude from the style & humour of it that it was writ by Mr Leibnitz himself. Now this author tells us that the great Mathematician having dicussed all things gave judgment in his Letters of 7 Iune 1713 as follows

Obs. 2. The whole Latin paper is full of assertions & reflexions without any proof whereas by the laws of all nations Mr Leibnitz cannot be a witness for him self. He & his friends ought to prove what they assert. For there cannot be a greater argument of a bad cause then to affirm & reflect without being able to prove any thing.

Obs. 3. The author of the Latin paper saith that Mr Hook complained of Mr Newton about the Hypothesis of the Planets & Mr Flamsteed about the use of his Observations. And indeed Mr Hook claimed the invention of the   Proposition of the 1st book of the Principia but could never produce Demonstration & Mr Leibnitz has produced an erroneous demonstration of the same Proposition to make it his own. Whether Mr Flamsteed treated Mr Newton with candour is better known here than in Germany & whether Mr Leibnitz or Mr Tschurnhause were in the right when they fell out about an invention is not material to us in England.

<492v> infinitarum convergentium, had he but repeated the word ut in the next sentence as the sense requires, the Objection would have been at an end.

2 Obs. 5. But the great Mathematician He conjectures that Mr Newton spent his first years in cultivating the method of series without thinking of the calculus of fluxions, or reducing it to general rules. That is, he will not allow plain matter of fact. In the Analysis which Dr Barrow in the year 1669 communicated to Mr Collins the method of fluxions is described with examples of the calculus. & there is no proof that Mr Leibnitz knew any thing of the method before the year 1677.

3 But the great Mathematician brings two arguments for his conjecture 1 first saith he in all the Letters published in the Commercium Epistolicum & in all his Principia Philosophiæ the letters with pricks which Mr Newton now uses are not to be met with. And by the same argumenthe may conclude that the Ancients had no Analysis because they wrote by composition. Mr Leibnitz indeed confines his method to the symbols dx & dy so that if you take away his symbols you take away his method, Mr Newton doth not so. And whether he uses Letters with pricks or other symbols his method is still the same In his Letter of 24 Octob 1676 he represents that he had a method of extracting fluents out of equations involving their fluxions. Will Mr Leibnitz say that he had no such method unless he then used letters with pricks. If so his letters with pricks are as old at least as the year 1676 & by consequence older then the differential notes of Mr Leibnitz.

4 But its to be observed that fluxions & differences are not quantities of the same kind. Fluxions are velocities, differences are small parts of things generated by fluxion in moments of time. Fluxions are finite quantities differences are infinitely little. Mr Newton sometimes uses prickt letters sometimes other marks for fluxions, Mr Leibnitz uses no symbols for fluxions to this day. The symbols of fluxions therefore used by M^r

Newton are the oldest in the kind. These he multiplies by the letter o to make them infinitely little moments or differences.

5 His second reason is that Mr Newton understood not the differences of differences or second differences till after the writing of his Principia. For there, saith he, the constant increase of the letter x he represents not by a prickt letter as at present but by the letter o after the vulgar manner which destroys the advantages of the differential calculus. But that Mr Newton understood it many years before is manifest by his Letter dated 10 Decem 1672 wherein he represents that his method extended to Questions about the Curvature of Curves. And as for the letter o, Mr Newton used it in his Analysis communicated to Mr Collins in the year 1669, & in his book of Quadratures where he represents fluxions by prickt letters & still uses it as the best way of notation. And I recommend it to be still used in honour of Mr Fermat who made the first step towards this sort of calculus. Know therefore that Mr Newton puts a known quantity & most commonly an unit for the fluxion of time & considered as the exponent of time. the letter o or $1 \times o$ he puts for its moment, & for the moments of other flowing quantities he puts the rectangles under their fluxions & the moment o, borrowing the names of fluxions & moments from the fluxion & moments of time. And This sort of Notation Mr Newton used when he wrote his Analysis above mentioned, but Dr Barrow in his method of Tangents published A C. 1670 put the letters a & e for the differences of the Abscissas & Ordinates & m^r Leibnitz seven years after changed the letters a & e into the symbols dy & dx, & called it the differential method beginning where Dr Barrow left off as that candid Gentleman the Marquess de L'Hospital <493r> observed long since in the Preface to his Analysis. And whereas our Geometer represents that Mr Newton uses the letter o in a vulgar manner which destroys the advantages of the differential calculus the method of Mr Newton has all the advantages of Mr Leibnitz & is more universal & more Geometrical.

The grand Geometer adds that Mr Newton has given a false rule about the higher degrees of Differences. But it seems this great Artist had not skill enough in these matters to mend a press-fault. For in the Scholium at the end of the book of Quadratures where Mr Newton saith: Hæ fluxiones sunt ut termini serierum infinitarum convergentium, had he but repeated the word ut in the next sentence as the sence requires the Objection would have been at an end. And thus much in answer to the judgment of the great Mathematician [All which amounts to nothing more then this that Mr Leibnitz appeals to himself from the records published by order of the R. S. In his Letter to their Secretary dated 29 Decem 1711 he wrote that what Mr Keil wrote opposed his candor which that he should defend at that age after so many documents of his life no prudent or just man would approve of: which is as much as to say that the Royal Society were unjust unless they would admitt him to be a witness in his own cause, contrary to the laws of all nations. By]

The Author of the paper pretends that Mr Leibnitz never communicated his reasons to the Society. And so the Society did not examin the reason on both sides for giving judgment. Whereas Mr Leibnitz refused to give his reasons calling it unjust to expect that one of his age & reputation should defend his candour. That is, he insisted upon being a witness for himself, contrary to the laws of all nations. And when he would give no reasons, the Society appointed a Committee to search records & make a report thereof. And now to talk of reasons against matter of fact, & to do it without producing those reasons is very trifling. The Report of the Committe must stand till Mr Leibnitz produces his reasons against it if he has any.

The author of the paper in the next place pretends to give a true report of what passed. But begins his report with what passed in the years before.

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And now we have brought down to our own times the history of the Greek Empire with the Empires of the Saracens & Turks invaded **{itag}** king which doth according to his will

Dr Barrow printed his method of Tangents in the year 1670 &

That candid Gentleman the Marquess de l'Hospital, in the Preface to his Analysis represents that Dr Barrow stopt at fractions & surds & where Dr Barrow left off Mr Leibnitz began. His method of Tangents is the same with Dr Barrows except that he has changed the letters a & e used by Dr Barrow into the symbols dy & dx. Let the advertisments which Mr Leibnitz received in Mr Newtons Letters of 10 Decem 1672, 13 Iune 1676 & 24 Octob 1676 be added to Dr Barrows method of Tangents & you have the Differential method.

Mr Leibnits in his Letter of   Iune 1677

The Elements of his method of fluxions he described in his second

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\begin{array}{r}
52548 \\
52548 \\
6568.10 \\
\hline
584596\ 10 \\
\end{array}
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\begin{array}{r}
6999 \\
233.6 \\
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7232.6 \\
\end{array}
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<center><494r></center>

I have seen in your journal the translation of a Latin piece dated 29 Iuly 1713 & published in Germany, & the Remarks upon it. These pieces are full of assertions without proof & without the name of the author & so are of no authority.

The author of the Latine piece represents that Mr Leibnitz had not then seen the Commercium Epistolicum, & this he could not know without keeping a correspondence with Mr Leibnitz. But a copy of this book was sent to Mr Leibnitz by the Resident of the Elector of Hannover above a year ago & several other copies were then sent to Lipic one of which was for him.

That Author tells us further that Mr Leibnitz not being at leasure to examin this affair himself had referred it to the judgment of a Mathematician of the first rank very skilfull in these things & very free from partiality. So then this paper was writ by the correspondents of Mr Leibnitz & Mr Leibnitz himself desiring the judgment of the great Mathematician & sending it to his correspondent to be published was the first mover & that the credit of the Mathematician for candor & ability depends upon the credit of Mr Leibnitz. And for these reasons this paper must be looked upon as the best defence that he & his correspondents were able to make: especially if this paper be writ in the stile of Mr Leibnitz himself as some think. By his Letters against Mr Keill it appears that he is too much concerned to neglect this matter, & his appealing from a numerous Committee of the Royall Society to a nameless Mathematician of his own chusing is no better then appealing to himself. For he has wrote to the Society that it would be injustice to question his candour, that is, to deny him to be both witness & Iudge in his own cause.

Now this great Mathematician conjectures that Mr Newton spent his first years in cultivating the method of series without thinking of the {ca}lculus of fluxions or reducing it to general rules. That is he will not allow the Analysis communicated by Dr Barrow to Mr Collins in the year 1669 to be a genuine piece. And he brings too arguments for his conjecture.

First, saith he, in all the Letters published in the Commercium Epistolicum & in all the Principia Philosophiæ, the letters with pricks which Mr Newton now uses are not to be met with. But in all those Letters (except the Analysis) & in all the Principia there was no occasion to make use of the fluxional calculus. And Dr Keill

hath given a further Answer to this argument long since in his Letter dated 24 May 1711. <u>Observo ipsum Newtonum</u>, saith he, <u>sæpius mutassæ nomen et notationem calculi. In tractatu de Analysi Æquationum per series infinitas, incrementum abscissæ per literam o designat, et in principijs Philosophiæ, Fluentem quantitatem Genitam vocat ejusque incrementum Momentum appellat: illam literis majoribus A vel B, hoc minusculis a et b designat</u>. Mr Leibnitz confines his Method to the symbols dx & dy, so that if you take away his symbols you take away his method. Mr Newton doth not confine his method in such a manner. As he uses a propriety of any symbols for fluents so he uses any others for fluxions, & whether he uses letters with pricks or other symbols for fluxions his method is still the same. In his Letter of 24 Octob. 1676 he represents that he had a method of extracting Fluents out of equations involving their fluxions. Will Mr Leibnitz say that he had no such method unless he then used letters with pricks? If so, his letters with pricks must be allowed as old at least as the year 1676, & by consequence older then the differential Notes of Mr Leibnitz.

But its further to be observed that Fluxions & Differences are not quantities of the same kind. Fluxions are velocities, & Differences are small parts of things generated by fluxion in moments of time: fluxions are always finite quantities & differences are infinitely little. Mr Newton uses sometimes prickt letters sometimes other symbols for fluxions, Mr Leibnitz <494v> uses no symbols for fluxions to this day. The symbols of fluxions used by Mr Newton whether with pricks or without, are therefore the oldest in the kind. These he multiplies by the moment o to make them [2]infinitely little & puts the rectangles for moments of the Moments, & without the moment o either exprest or understood they never signify moments or differences, but are always finite quantities & signify velocities. The fluxion of time or of any exponent of time he usually represents by an unit, & the moment thereof by the letter o.

The second reason of the great Mathematician for his conjecture is that Mr Newton understood not the differences of differences till after the writing of his Principia. For there, saith he, the constant increase of the letter x he represents not by a prickt letter as at present but by the letter o after the vulgar manner, which destroys the advantages of the differential method. Here our great Mathematician commits two mistakes; one by supposing that Mr Newton represents differences by prickt letters, another by supposing that the method used in the tenth Proposition of the second Book of the Principia is Mr Newtons method of fluxions. Tis only a branch of his method of converging series. In his Letter dated 10th Decemb. 1672, where he speaks of a method whereof the method of Tangents there described is a branch or Corollary, he represents that this method (which is the method of fluxions) extended to Questions about the curvature of curves; & thence it is manifest that he then understood the second fluxions or differences of differences.

The letter o was used by Mr Newton in the manner above [3]mentioned in his Analysis communicated by Dr Barrow to Mr Collins in the year 1669, & in his Book of Quadratures & is still used by him in the very same manner. And as it is the oldest notation for moments or differences so it is the best, the method being thereby more convenient more elegant & more suitable to Geometry then by the differential notation & as universal, & does justice to the memory of M^r F{e}rmat who first a {inmadnit} brought in the use of this letter o.

To signify {din}{illeg} the summ of the ordinates or area of a Curve Mr Leibnit{z} prefixes propo{s}{illeg} the letter s to the Ordinate & Mr Newton in his Analysis communicated to Mr Collins in the year 1669, inclosed the Ordinate in a square. Mr Newtons notation of this kind is also much the oldest.

Dr Barrow published his method of Tangents in the year 1670, & [4]that very candid Gentleman the Marquess de l'Hospital, in the Preface to his Analysis, represents that Dr Barrow stopt at fractions & surds, & where Dr Barrow left off Mr Leibnitz began. His method of Tangents is the same with Dr Barrows except that he has changed his letters a & e into the symbols dx & dy, & (being admonished by Mr Newtons Letters of 10 Decem. 1672, 13 Iune 1676 & 24 Octob. 1676) [5]taught how to avoyd fractions & surds.

As to what the Author of the Latin paper saith of Mr Hook & Mr Flamsteed: Mr Hook indeed claimed one of Mr Newton's Propositions but could never produce a Demonstration thereof, Mr Leibnitz claimed it also but the Demonstration by which he claimed it is erroneous. Mr Leibnitz claimed also an Invention from M^r

Tschurnhause & who is in the right may be a question, but Mr Newton always acknowledged the use of Mr Flamsteads Observations.

This Author in the next place complains of the Committee of the R. Society for representing that Mr Leibnitz had from Mr Iames Gregory the series for finding the Arc of a circle by the Tangent given, that is, he represents that the Letter of Mr Gregory, Mr Collins, Mr Oldenburg & Mr Leibnits examined & approved by a numerous Committee <495r> of the Royal Society were fourged. The Letter of Mr Gregory dated 15 Feb. $167\frac{0}{1}$ is still extant su{b}{illeg} in his own hand-writing & conteins this series with several others then sent to Mr Collins. That of Mr Oldenburg dated 15 Apr. 1675 is extant in the Letterbook of the Royal Society left by Mr Oldenburg in their Archives & conteins this series with several others then sent from Mr Collins by Mr Oldenburg to Mr Leibnitz at Paris. The answer of Mr Leibnitz dated from Paris May 20th 1675 was found in the same Letter-book; & the original Letter in the hand-writing of Mr Leibnitz was also found in the Archives of the R. Society & conteins his acknowledgment of the Receipt of Mr Oldenburghs Letter above mentioned. It begins thus. Literas tuas multa fruge Algebraica refertas accepi pro quibus tibi et doctissimo Collinio gratias ago. Cum nunc præter ordinarias curas Mechanicis imprimis negotijs distrahar, non potui examinare series quas misistis, ac cum meis comparare. By these words its plain that Mr Leibnitz at this time knew none of the series then sent him to be his own, tho before the end of the year he communicated to his friends at Paris one of those series then sent him as his own, vizt that of Gregory then dead, & by vertue of that communication has ever since claimed it as his own. The collection of the papers of Mr Gregory made by Mr Collins after the death of that Gentleman, is still extant in the hand-writing of Mr Collins, & at the request of Mr Leibnitz was sent to Paris in Iune 1676, & conteins a copy of the aforesaid Letter of Mr Gregory. But upon the death of that Gentleman Mr Leibnitz pretended in his Letter dated 28 Decem. 1675 that he had communicated it at Paris above two years before & that it was the series whereof he had wrote before to Mr Oldenburg, that is, in his Letter of 15 Iuly & 24 Octob. 1674. And under this pretence he sent it back to Mr Oldenburg as his own in his Letter dated 27 Aug. 1676. And yet the Series which he wrote of in his said two Letters dated 15 Iuly & 24 Octob 1674 was not this Series for finding the arc by the tangent but a Theoreme or Method for finding the Arc by the sine. This Theoreme or Series Mr Collins had received from Mr Newton in Iuly 1669 & communicated it soon after to his friends very freely. Mr Leibnitz was in London in the years 1671, 1672 & the beginning of 1673 & having met with this series either in London or soon after in France pretended in his said Letters of 15 Iuly & 24 Octob 1674 to have found it himself, & yet in his Letter dated 12 May 1676 desired Mr Oldenburg to procure from Mr Collins the Demonstration thereof, that is, the method of finding it. And when he had received the method with some of Mr Newtons series, he pretended to have found three of those series before, tho he did not yet understand the method of finding them. For in his Letter of 27 Aug. 1676, he wrote back for a further explication of the method. Mr Newton therefore in his Letter of [6] 24 Aug. 1676 explained it further & added another method of the same kind, & Mr Leibnitz in his Letter dated 21 Iune 1677 still desired a further explication, but so soon as he understood it, he wrote in his Letter dated 12 Iuly 1677 that he found by his old papers, that he had used one of those methods before. And by the same power {illeg} faculté of invention d'{illeg}, when he had newly found the Differential method (which he might do by the help of Gregories & Barrows methods of Tangents & Newtons Letters) he wrote back: Clarissimi Slusij methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium generalius tractavi, scilicet per differentias Ordinatarum. And yet its very certain that he had but newly found it. For in his Letter dated 27 Aug. 1676, he wrote: 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. Quæ etiam Cartesius in potestate non esse fassus est These words are a Demonstration that he did not then understand the Differential method. He was then composing & polishing his Quadrature of the Circle vulgari more, & left of that way of writing as soon as he found the Differential method. I pass by his claiming the Inventions of Mouton & Paschal & a considerable part of Mr Newton's Principia Philosophiæ.

Our Author tells us further that Mr Leibnitz published in the Acta Eruditorum a general method for finding the Ordinates of transcendent Curve not by <495v> extraction of roots but deduced from a profounder foundation of the differential calculus, by which the business of Series was brought to a greater degree of perfection. But Mr Newton many years before (vizt in his Letter to Mr Oldenburg dated 24 Octob. 1676, communicated the very same method in this sentence. <u>Altera</u> [methodus consistit] tantum in assumptione seriei pro quantitate qualibet incognita ex qua cætera commode derivari possint, & in collatione terminorum homologorum æquationis resultantis ad eruendos terminos assumptæ seriei. Mr Leibnitz therefore has no title to any part of the method of converging series.

Our Author tells us further that Mr Leibnitz was the first who used the exponential calculus while Mr Newton knew nothing thereof. Certainly Mr Newton was one of the first who introduced into Analysis fractions radicals & negative quantities for the indices or exponents of dignities & thereby very much enlarged Analysis & laid the i**{illeg}** foundation of making it universal. In his Letter dated 24 Octob. 1676 he mentioned such Exponents of Dignities & thereupon Mr Leibnitz in his Answer dated 21 Iune 1677, proposed indeterminate exponents of Dignities. And this seems to have been the Original of the Exponential Calculus. But such a calculus has hitherto been of no use. a **{illeg}**la**{r}**t s'a**{n}** trouvé n'a**{illeg}**in ana**{mar}**age

Our Author tells us also that the English & Scotch, Wallis, Hook, Newton & Gregory junior, acknowledged 36 years ago the series for finding the arc of a circle by the Tangent to be the invention of Mr Leibnitz That is, he complains of Mr Oldenburg for not letting the English & Scotch know that he had sent this series with several others to Mr Leibnitz in April 1675. Tis sufficient that the Letters between Mr Oldenburg & Mr Leibnitz were left by Mr Oldenburg in the Letter-book of the R. Society & that the Original Letter of Mr Leibnitz is still extant in his own hand-writing.

And thus much concerning the printed paper.

In the Remarks its represented that Mr Leibnitz never communicated his reasons to the Royal Society of England & so the So**{ciet}**y hath not examined the reasons on both sides for giving judgment. But the truth is, Mr Leibnitz refused to give any reasons at all, representing it injustice to expect that he should defend his candor, detracting from the candor of Mr Keil & pressing the R. Society to give judgment prononcer without hearing reasons: & the Committee of the R. Society grounded their Report not upon plausible & slippery reasons but upon the matter of fact conteined in the Letters & Papers found in the Adversaria of the R. Society & in those of Mr Collins & in the Acta Eruditorum; & published those Letters & Papers in the Commercium Epistolicum that all the world might see the grownd & justice of their Report. And those Records are sufficiently plain to any man that considers them impartially.

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I have seen in your Iournal Literaire the translation of a Latin piece dated 29 Iuly 1713 & published in Germany & the Remarks upon it These pieces are full of assertions without any proof & without the name of the Author & so are of no credit or authority.

The Author of the Latine piece represents that Mr Leibnitz had not then seen the Commercium Epistolicum: & this he could not know without keeping a correspondence with Mr Leibnitz. Certainly a Copy of this book was sent to him by the Resident of the Elector of Hannover above a year ago, & several other Copies were sent to Leipsic one of which was for him, & he knew how to write to a friend who had a copy

For this Author tells us further that Mr Leibnitz not being at leasure to examin this affair himself had referred it to the judgment of a Mathematician of the first rank very skilfull in these things & very free from partiality. So then this paper was writ by the Correspondents of Mr Leibnitz, & Mr Leibnitz himself was the first mover, & the credit of the Mathematician for candor & ability depends upon the credit of Mr Leibnitz. And for these reasons this paper must be looked upon as the best defence that he & his correspondents were able to make, especially if the Latin paper be writ in the style of Mr Leibnitz himself as some think. By his Letters against Mr Keil it appears that he is too much concerned to neglect this matter, & his appealing from the Report of a numerous Committee of the Royal Society to a nameless Mathematician of his own chusing is no better then appealing to himself He has told the R. Society that it would be injustice to question his candor that is, to deny him to be a witness in his own cause, & now he is for makeing himself also the Iudge.

Now the great Mathematician conjectures that Mr Newton spent his first years in cultivating the method of series without thinking of the calculus of fluxions or reducing it to general Rules. That is, he will not allow the Analysis communicated by Dr Barrow to Mr Collins in the year 1669 to be a genuine piece. And he brings too Arguments for his conjecture

First, saith he, in all the Letters published in the Commercium Epistolicum, & in all the Principia Philosophiæ, the Letters with pricks which Mr Newton now uses are not to be met with. But in all those Letters except the Analysis, & in all the Principia there was no occasion to make use of the fluxional calculus. And Dr Keill hath given a further Answer to this argument long since in his Letter dated 24 May 1711. Observo ipsum Newtonum, saith he, sæpius mutasse nomen et notationem calculi. In Tractatu de Analysi Æquationum per series infinitas, incrementum Abscissæ per literam o designat, et in Principijs Philosophiæ, Fluentem quantitatem Genitam vocat, ejusque incrementum Momentum appellat: illam literis majoribus A vel B, hoc minusculis a et b designat. I may add that in one & the same book, the Book of Quadratures he sometimes uses prickt letters sometimes not. For in the Introduction to that book where he describes his Method of fluxions & illustrates it with examples he makes no use of prickt letters. Mr Leibnitz confines his method to the symbols dx & dy, so that if you take away his symbols you take away the characteristick of his method. Mr Newton doth not so. Whether he uses Letters with pricks or other symbols for fluxions, his method is still the same. In his Letter of 24 Octob. 1676, he represents that he had a method of extracting fluents out of Equations involving their fluxions. Will Mr Leibnitz say that he had no such method unless he then <496v> used letters with pricks? If so, his letters with pricks are as old at least as the year 1676, & by consequence older then the differential notes of Mr Leibnitz.

But its to be observed that Fluxions & Differences are not quantities of the same kind. Fluxions are velocities: Differences are small parts of things generated by fluxion in moments of time. Fluxions are always finite quantities: Differences are infinitely little. Mr Newton uses sometimes prickt letters, sometimes other symbols for fluxions: Mr Leibnitz uses no symbols for fluxions to this day. The symbols of fluxions used by Mr Newton whether with pricks or without are therefor{e} the oldest in the kind. These he multiplies by the moment o to make them infinitely little, & puts the rectangles for moments of fluent quantities: & without the moment o either exprest or understood the prickt letters never signify moments or differences but are always finite quantities & signify velocities. The fluxion of time or of any exponent of time he usually represents by an unit & the moment thereof by the Letter o which is equipollent to the rectangle $1 \times o$. In his Analysis above mentioned he represents fluents by the areas of curves, fluxions by their ordinates & moments by the rectangles under the ordinates & the moment of the common Abscissa. And these rectangles he uses instead of the Indivisibles of Cavallerius, & thereby makes his method Geometrical. For in Geometry there are no Indivisibles. When he is demonstrating any Proposition he always writes down the moment o & takes it in the sense of the vulgar for an indefinitely small part of time & performs the whole calculation in finite figures by the Geometry of the Ancients without any approximation, & so soon as the calculation is ended & the Equation reduced he supposes the moment o to decrease in infinitum & vanish. Examples of this you have in the end of his Analysis & in the first Proposition of his Book of Quadratures. But when he is not demonstrating but only investigating a Proposition, he supposes the moment o to be infinitely small & usually for making dispatch neglects to write it down & proceeds in the calculation by any approximations which he thinks will create no error in the conclusion. But this last way (to which the Differential method is

equipollent) is not Geometrical. For Geometry admits not of approximations nor of lines & figures infinitely little.

The second reason of the great Mathematician for his conjecture is that Mr Newton understood not the differences of differences till after the writing of his Principia. For there saith he the constant increase of the letter x he represents not by a prickt letter as at present but by the letter o after the vulgar manner which destroys the advantages of the differential method. But here our great Mathematician commits two mistakes: one in supposing that Mr Newton represents Differences by prickt letters, the other by supposing that the method used in the Scholium the 10th Proposition of the second Book of the Principia, is Mr Newtons method of Fluxions. Tis only a branch of his method of converging series. The elements of his method of fluxions he described in the second Lemma of the second book of his Principles & subjoyned this Scholium. In literis quæ mihi cum Geometra peritissimo G. G. Leibnitio annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi maximas et minimas, ducendi Tangentes et similia peragendi quæ in terminis surdis æque ac in rationalibus procederet, & literis transpositis hanc sententiam involventalibus [Data æquatione quotcunque Fluentes quantitates involvente, Fluxiones invenire & vice versa] eandem celarem; rescripsit Vir Clarissimus se quoque in ejusmodi methodum incidisse & methodum suam communicavit a mea vix abludentem præterquam in verborum et notarum formulis. Vtriusque fundamentum continetur in hoc Lemmate. The Letter here referred unto is that of 24 Octob. 1676 printed by Dr Wallis. In this Letter Mr Newton distinguishes between the Method of infinite series & that of fluxions & represents that he had writ a treatise of both these methods five years <497r> before & that the method of fluxions readily gives the method of Tangents of Slusius & sticks not at equations affected with radicals. And in a Letter to Mr Collins dated 10 Decem 1672 Mr Newton writing of the method whereof the Method of tangents of Slusius is a branch or Corollary & which sticks not at surds, & which by consequence was the Method of fluxions, represented it a very general method reaching to the abstruser sorts of Problemes & among others to the determining of the Curvature of Curves, a Probleme which requires the consideration of the second fluxions. And therefore he had then extended the method to the second fluxions or fluxions of fluxions. And it is further observable that Mr Newton in the 2^d Book of his Principles makes frequent mention of the increase of the velocities wherewith lines are described. The lines are the fluents, the velocities their fluxions & the increase of the velocities the fluxions of their fluxions or second fluxions. And particularly in demonstrating the 14th Proposition of the second Book of his Principles, he has these words. Est igitur differentia momentorum, id est momentum differentiæ arearum &c Where differentia arearum is the first difference & momentum differentiæ is the second difference of the areas. So then Mr Newton, when he wrote his Principles of Philosophy & a great many years before, had extended his method to the consideration of the second fluxions of quantities. And indeed, to say that he then understood not second fluxions is all one as to say that he understood not how to consider motion as a quantity increasing & decreasing.

And whereas the great Mathematician represents that Mr Newton uses the Letter o in the vulgar manner which destroys the advantages of the Differential method: he uses it, & has used it ever since the writing of his Analysis, in such a manner as makes his method more beautiful more Geometrical & more advantageous then the Differential, & (by joyning the methods of series & fluxions together) much more universal.

The Differential method is nothing else then the method of Tangents published by Mr Gregory in the year 1668 & by Dr Barrow in the year 1670, disguised by changing Dr Barrows symbols a & e into dy & dx, improved by the instructions which Mr Leibnitz received by the Letters of Mr Newton, & taken from them all by pretending that Mr Leibnitz found it long before he did. For in his Letter dated 21 Iuly 1677 he pretended to have found it jam a multo tempore, & yet he had not found it the year before. For in his Letter dated 27 Aug. 1676 he wrote that there were many Problems which could not be reduced to Equations or Quadratures such as were those of the inverse method of Tangents & many others. This method without the use of the Letter o is not demonstrative, without the method of series is not universal, nor has any advantages which are not to be found in the method of Fluxions, nor has Mr Leibnitz added any thing to it of his own besides a new name & a new notation. And thus much in answer to the great Mathematician.

As to what the Author of the Latine paper saith of Mr Flamsteed & Mr Hook: Mr Newton always acknowledged the use of Mr Flamsteeds Observations; Mr Hook could never produce a Demonstration of the Proposition claimed by him tho often asked to produce one; Mr Leibnitz pretended to the same Proposition by an erroneous Demonstration; & whether he or Mr Tschurnhause were in the right about an erroneous Proposition claimed by them both, I leave to be examined.

This Author in the next place complains of the Committee of the R. Society for representing that Mr Leibnitz had from Mr Iames Gregory the series for finding the Arc of a circle by the Tangent, that is, he confesses that he has no other way of defending Mr Leibnitz then by laying aside the records relating to this matter examined & approved by a numerous Committee of the R. Society & still ready to be produced

The same Author in the next place ascribes a certain general method of series to Mr Leibnitz, tho this method was found many years before by Mr Newton as <497v> appears by his Letter of 24 Octob. 1676. And in the next place he magnifies an Invention called the Exponential Calculus, without considering that Mr Leibnitz had the hint from Mr Newton & that this Calculus hath hitherto been of no use.

Our Author tells us also that the English & Scotch, Wallis, Hook, Newton, Gregory junior, acknowledged 36 years ago that the series for findiing the Arc of a circle by the Tangent was the invention of Mr Leibnitz & that Mr Hugens commended it. But he should have complained of Mr Oldenburg & Mr Leibnitz for not letting the English & Scotch & Mr Hugens know that this series with many others was sent by Mr Oldenburg to Mr Leibnitz in April 1675; And that a Collection of Gregories Papers with this series in it, was also sent by him to Mr Leibnitz the next year.

In the Remarks it is represented that Mr Leibnitz never communicated his reasons to the R. Society of England, & so the Society has not examined the reasons on both sides for giving judgment. And upon this pretence, the Author of the Remarks gives a judgment contrary to that of the Committee of the R. Society. But the truth is, Mr Leibnitz absolutely refused to give any reasons, calling it injustice to expect that he should defend his candor. And the Committee of the R. Society grownded their Report upon ancient & unquestionable Records, & published the Records, that the justice of their Report might appear to the world. But the Author of the Remarks has laid aside the Records of the first seven years which make for Mr Newton & begins his Report with the years 1676 & 1677, & thereby confesses that he has no way to defend Mr Leibnitz but by laying aside the Records which make against him.

In the Remarks its said further that Mr Newton did not speak of this matter till after the death of Mr Hugens & Dr Wallis who were well informed & able to judge thereof: Which implyes that Mr Newton began this dispute. Whereas Mr Leibnitz began it nine years ago by giving an abusive reflecting account of Mr Newton book De Quadratura Figurarum, & Dr Keil retorted the charge upon Mr Leibnitz before Mr Newton knew what Mr Leibnitz had done. As for Mr Hugens he never was well informed about this matter nor doth it appear that he gave any judgment about it. And as for Dr Wallis, he gave his judgment against Mr Leibnitz 19 years ago in his Preface to the first Volume of his Mathematical works published A.C. 1695. For there he saith that Mr Newton in his Letters of Iune 13 & Octob. 24 1676 methodum hanc [de Fluxionibus] Leibnitio exponit, tum ante decem annos nedum plures ab ipso excogitatam, explained to Mr Leibnits the method of Fluxions invented by him ten years before or above, that is, in the year 1666 or 1665. And in a Letter dated from Oxford Apr. 20 1695 & extant in the Archives of the R. Society Dr Wallis represented that he had intimation from Holland that Mr Newton's Papers relating to the Method of Fluxions should be printed because his notions of fluxions passed there with great applause under the name of the Differential method. And tho Mr Newton has in this matter neglected his reputation abroad, yet in the second book of his Principles written 28 years ago, he mentioned the method of Fluxions as known to him in the year 1676, & Mr Leibnitz has hitherto allowed it without going about to make it appear that the Differential method was known to him before the year 1677.

But before Mr Leibnitz & his correspondents or some of them have composed & published in Germany the Latin Paper without a name whereby they defame Mr Newton accuse the committee of the R. Society of partiality, affirm & deny things without proof, & endeavour to set aside Records & bring things to a wrangle: I intend to give you hereafter a fuller account of these matters out of the Records themselves.

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I have seen in your journal the translation of a Latin piece dated 29 Iuly 1713 & published in Germany & the Remarks upon it. These pieces are full of assertions without any proof & without the name of the author & so are of no authority.

The author of the Latine piece represents that Mr Leibnitz had not then seen the Commercium Epistolicum: & this he could not know without keeping a correspondence with Mr Leibnits. But a copy of this book was sent to him by the Resident of the Elector of Hannover above a year ago & several other copies were sent to Lipsic, one of which was for him. The Author tells us further that Mr Leibnitz not being at Leasure to examin this affair himself had referred it to the judgment of a Mathematician of the first rank very skillfull in these things & very free from partiality. So then this paper was writ by the correspondents of Mr Leibnitz, & Mr Leibnitz himself was the first moover & credit of the Mathematician for candor & ability depends upon the credit of Mr Leibnitz : & for these reasons this paper must be looked upon as the best defence that he & his correspondents were able to make, especially if this paper be writ in the style of Mr Leibnitz himself as some think. By his Letters against Mr Keil it appears that he is too much concerned to neglect this matter: & his appealing from the Report of a numerous Committe of the R. Society to a nameless Mathematician of his own chusing is no better then appealing to himself. For he has told the Society that it would be injustice to question his candor, that is, to deny him to be a witness in his own cause.

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First, saith he, in all the Letters published in the Commercium Epistolicum & in all the Principia Philosophiæ, the letters with pricks which Mr Newton now uses are not to be met with. But in all those Letters (except the Analysis) & in all the Principia, there was no occasion to make use of the fluxional calculus. And Dr Keil hath given a further answer to this argument long since in his Letter dated 24 May 1711. <u>Observo ipsum Newtonum</u>, saith he, <u>sæpius mutasse nomen & notationem calculi. In tractatu de Analysi Æquationum per series infinitas, incrementum Abscissæ per litteram o designat, et in Principijs Philosophiæ, Fluentem quantitatem Genitam vocat ejusque incrementum Momentum appellat: illam literis majoribus A vel B, hoc minusculis a et b designat.</u> Mr Newton doth not so. And whether he uses Letters with pricks or other symbols for fluxions his method is still the same. In his Letter of 24 Octob. 1676 he represents that he had a method of extracting fluents out of Equations involving their fluxions. Will Mr Leibnitz say that he had no such method unless he then used letters with pricks? If so, his Letters with pricks are as old at least as the year 1676, & by consequence older then the differential Notes of Mr Leibnitz.

But its to be observed that fluxions & Differences are not quantities of the same kind. Fluxions are velocities & Differences are small parts of things generated by fluxion in moments of time: fluxions are always finite quantities & differences are infinitely little. Mr Newton uses sometimes prickt letters sometimes other symbols for fluxions: Mr Leibnitz uses no symbols for fluxions to this day. The symbols of fluxions used by Mr Newton, whether with pricks or without, <498v> therefore the oldest in the kind. These he multiplies by the moment o to make them infinitely little & puts the rectangles for moments & without the moment o either exprest or understood the prickt letters never signify moments or differences, but are always finite quantities & signify velocities. The fluxion of time or of any exponent of time he usually represents by an unit & the moment thereof by the letter o.

The second reason of the great Mathematician for his conjecture is that Mr Newton understood not the differences of differences till after the writing of his Principia. For there, saith he, the constant increase of the letter x he represents not by a prickt letter as at present but by the letter o after the vulgar manner which destroys the advantages of the differential method. Here our great Mathematician commits two mistakes: one by supposing that Mr Newton represents differences by prickt letters, another by supposing that the method used in the 10th Proposition of the second book of the Principia is Mr Newtons method of fluxions. Tis only a branch of his method of converging series. In his Letter dated 10 Decem 1672 where he speaks of a method whereof the method of Tangents there described is a branch or Corollary, he represents that this method (which is the method of fluxions) extended to Questions about the curvature of curves; & thence it is manifest that he then understood the second fluxions or differences of Differences.

The Letter o was used by Mr Newton in the manner above described in his Analysis communicated by Mr Barrow to Mr Collins in the year 1669 & in his book of Quadratures, & is still used by him in the very same manner. And thus much in answer to the great Mathematician.

As to what the Author of the Latin paper saith of Mr Hook & Mr Flamsteed. Mr Hook indeed claimed one of Mr Newtons Propositions but could never produce a demonstration: Mr Leibnitz claimed it also but the Demonstration by which he claimed it is erroneous. Mr Leibnitz also claimed an invention from Mr Tschurnhause But Mr Newton always acknowledged the use of Mr Flamsteeds observations.

Our Author in the next place complains of the Committee of the R. Society for representing that Mr Leibnitz had from Mr Iames Gregory the series for finding the Arc of a circle by the tangent; that is, he confesses that he has no other way of defending Mr Leibnitz then by presenting that the Letters of Mrs Gregory Collins Oldenburg & Leibnitz examined & approved by a numerous Committee of the R. Society were fourged. The Letter of Mr Gregory dated 15 Feb. 167$\frac{0}{1}$ is still extant in his own hand-writing & conteins this series with several others then sent to Mr Collins. That of Mr Oldenburg dated 15 Apr. 1675 is extant in the Letter-book of the R. Society left by Mr Oldenburge in their Archives & conteins this series with several others then sent by Mr Oldenburgh from Mr Collins to Mr Leibnitz at Paris. The answer of Mr Leibnitz dated from Paris May 20th 1675 was found in the same letter-book; & the original letter in the hand-writing of Mr Leibnitz was also found in the Archives of the R. Society & conteins his acknowledgment of the receipt of Mr Oldenburgs Letter above mentioned . It begins thus. <u>Litteras tuas multa fruge Algebraica refertas accepi pro quibus tibi et doctissimo Collinio gratias ago. Cum nunc præter ordinarias curas Mechanicis imprimis negotijs distrahar, non potui examinare Series quas misistis ac cum meis comparare.</u> By these words its plain that Mr Leibnitz at this time knew distinguished none of the series then sent him to be his own, tho before the end of the year he communicated to his friends at Paris one of them as his own, vizt that of Gregory then dead, & by vertue of that communication has ever since claimed it as his own. The collection of the papers of Mr Gregory made by Mr Collins after the death of that Gentleman, is still extant in the hand-writing of Mr Collins & at the request of Mr Leibnits was sent to Paris in Iune 1676, & conteins <499r> a copy of the aforesaid Letter of Mr Gregory. But upon {the d}eath of that Gentleman Mr Leibnitz {p}retended in his Letter dated 28 Decem 1675 that he h{ad co}mmunicated it at Paris above two years before, & that it was the ser{ies} which he had wrote of before to Mr Oldenburg, that is in his Letters {of} 15 Iuly & 24 Octob. 1674. And under this pretence he sent it back to Mr Oldenburg as his own in his Letter dated 27 Aug. 1676. And yet the series which he wrote of in his said two Letters dated 15 Iuly & 24 Octob 1674 was not this series for finding the arc by the tangent, but a Theorem or Method for finding the arc by the sine. This Theorem or series Mr Collins had received from Mr Newton in Iuly 1669, & communicated it soon after to his friends very freely. Mr Leibnitz was in London in the years 1671, 1672 & the beginning of 1673, & having met with this series either in London or soon after in France pretended in his said Letters of 15 Iuly & 24 Octob. 1674 to have found it himself, & yet in his Letter dated 12 May 1676 desired Mr Oldenburge to procure from Mr Collins the Demonstration thereof, that is, the method of finding it. And when he had received the method with some of Mr Newtons series he pretended to have found three of those series before, tho he did not yet understand the method of

finding them. For in his Letter of 27 Aug. 1676 he wrote back for a further explication of the method. Mr Newton therefore in his Letter of 24 Octob. 1676 explained it further & added another method of the same kind, & Mr Leibnitz in his Letter dated 21 Iune 1677 still desired a further explication, but so soon as he understood it, he wrote in his Letter dated 12 Iuly 1677 that he found by his old papers that he had used one of those methods before. And by the same power of invention when he had newly found the differential method, (which he might do by the help of Gregories & Barrows methods of Tangents & Newtons Letters of 10 Dec. 1672, 13 Iune 1676 & 24 Octob 1676) he wrote back <u>Clarissimi Slusij methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium generalius tractavi; scilicet per differentias Ordinatarum</u>. And yet its very certain that he had but newly found it. For in his Letter dated 27 Aug 1676 he wrote: <u>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; quæ etiam Cartesius in potestate non esse fassus est</u>. These words are a demonstration that he did not then understand the differential method. He was then composing & polishing the Quadrature of the circle vulgari more & left off that way of writing as soon as he found the differential method. I pass by his claiming the inventions of Mouton & Paschal, & a considerable part of Mr Newtons Principia Philosophiæ.

Our Author tells us further that Mr Leibnitz published in the Acta Eruditorum a general method for finding the Ordinates of transcendent Curves not by extraction of roots but deduced from a profounder foundation of the differential calculus, by which the business of series was brought to a greater degree of perfection. But Mr Newton many years before (vizt in his Letter to Mr Oldenburge dated 24 Octob. 1676) communicated the very same method in this sentence. <u>Altera</u> [methodus consistit] <u>in assumptione seriei pro quantitate qualibet incognita ex qua cætera commode derivari possint & in collatione terminorum homologorum æquationis resultantis ad eruendos terminos assumptæ seriei</u>. Mr Leibnitz therefore has no title to any part of the method of converging series.

<499v> it {unive}rsal. In his Letter dated 24 Octob. 1676 he mentioned such exponents {of D}ignities, & thereupon Mr Leibnitz in his Answer dated 21 Iune 1677 propose{d in}determinate Exponents of Dignities. And This seems to have been the Original of the exp{on}ential calculus: but such a calculus has hitherto been of no use.

Our author tells us that the English & Scotch, Wallis, Hook, Newton & Gregory junior, acknowledged 36 years ago the series for finding the arc of a circle by the Tangent to be the invention of Leibnitz. That is, he complains of Mr Oldenburg for not letting the English & Scotch know that he had communicated this series with several others to Mr Leibnitz in April 1675. Tis sufficient that the Letters between Mr Oldenburg & Mr Leibnitz were left by Mr Oldenburg in the Letter-book of the R. Society & that the Original Letter of Mr Leibnitz is still extant in his own handwriting. I may add that that Mr Leibnitz in his Letter dated 21 Iune 1677 acknowledged that Mr Newton had a method like the differential. And thus much concerning the printed paper.

In the Remarks it's represented that Mr Leibnitz never communicated his Reasons to the Royal Society of England & so the Society has not examined the reasons on both sides for giving judgment. But the truth is Mr Leibnits refused to give any reasons at all, calling it injustice to expect that he should defend his candour detracting from the candor of Mr Keil & pressing the R Society to give judgment without hearing reasons; & the Committee of the R. Society grounded their Report not upon plausible reasons but upon the matter of fact conteined in the Letters & Papers found in the Adversaria of the R. Society & in those of Mr Collins , & published those Letters & Papers in the Commercium Epistolicum that all the world might see the grownd of their Report. And those records are sufficiently plain to any man that consider authentic considers them impartially.

And they have this further weight that Mr Leibnitz is known to be of a temper too loquacious, & vainglorious to be admitted a witness in his own cause.

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Sir

I have seen in your journal the piece sent you from Germany conteining remarks upon the difference between Mr Leibnitz & Mr Newton with a translation of a Latine piece dated 29 Iuly 1713 & published in Germany These pieces are full of assertions without proof without the name of the author & so are of no authority.

The author of the Remarks pretends that Mr Leibnitz has not yet seen the Commercium Epistolicum, & therefore he keeps a correspondence with Mr Leibnitz But a copy thereof was sent to Mr Leibnitz above a year ago by the Resident of the Elector of Hanover & several other copies being sent to Leipsick, notice was sent back that one of them was sent to him from thence.

This nameless Author pretends that Mr Leibnits never communicated his reasons to the R. Society of England & so the Society has not examined the reasons on both sides for giving judgment. When the secretary of the R. Society sent to Mr Leibnitz a Letter of Dr Keil conteining such reasons against him as were unanswerable, Mr Leibnitz refused to answer them or produce any reasons for himself & cried out that Mr Keil impugned his candour, which that at such an age & after so many documents of his life he should defend, would be injustice to expect That is, he told the Society that they would be unjust unless they admitted him to be a witness for himself, contrary to the laws of all nations. Whereupon the Society whose motto is <u>Nullius in verba</u> appointed a Committee to examin Records & the Report of the Committee being grounded upon questionable Records is as valid as the Records themselves, For the Records are plain & evident, & the pretended reasons of Mr Leibnitz are not to be regarded till he produces them.

The said Author proceeds to make a Report contrary to that of the R. Society, as if the Report of a nameless person could be of any validity. He begins his report with the Letters between Mrs Leibnitz Oldenburgh Newton & drops the Analysis communicated by Dr Barrow to Mr Collins some years before in which Mr Newtons method of fluxions is plainly described & therefore his report is partial. Let the Records themselves published in the Commercium Epistolicum be consulted.

The author complains of Mr Newton's long silence but before he complained of this he ought to have shewed that Mr Newton began this dispute It's almost 40 years since he left of corresponding in Mathematicks & almost twenty since he left of these studies. Mr Leibnitz by what he printed against Mr Newton in the Acta Eruditorum in Ianuary Anno 1707, begun this dispute, Mr Keil answered Mr Leibnitz & Mr Leibnitz wrote his first Letter against Mr Keil before Mr Newton knew what was printed against him in the Acta Eruditorum as is here well known. And Mr Newton must be allowed to give his friends leave to repell injuries.

The Author of the Latine piece dated 29 Iuly 1713 represents that Mr Leibnitz had not then seen the Commercium Epistolicum & therefore he also keeps a correspondence with Mr Leibnitz. He tells us further that Mr Leibnitz not being at leasure to examin this affair himself, had referred it to the judgment of a Mathematician of the first rank very skilful in these things & very free from partiality. So then this paper was writ by the correspondents of Mr Leibnits, & Mr Leibnitz himself was the first mover: & therefore it must be looked upon as the ablest defence that he & his correspondents could make, especially if this paper be writ in the style of Mr Leibnitz himself as some think. By his Letters against Mr Keill it appears that he is much

Now this great Mathematician conjectures that Mr Newton <500v> spent his first years in cultivating the method of series without thinking of the calculus of fluxions or reducing it to general Rules. That is, he will not allow the Analysis communicated by Dr Barrow to Mr Collins in the year 1669 to be a genuine piece.

And he brings two arguments for his conjecture. First saith he in all the Letters published in the Commercium Epistolicum & in all his Principia Philosophiæ the letters with pricks which Mr Newton now uses are not to be met with. But Dr Keill hath answered this argument long since in his Letter dated 24 May 1711. <u>Observo ipsum Newtonum</u> saith he <u>sæpius mutasse nomen & notationem calculi. In tractatu de Analysi Æquationum per series infinitas, incrementum Abscissæ per literam o designat. Et in Principijs Philosophiæ, Fluentem, quantitatem Genitam vocat, ejusque incrementum momentum appellat: illam literis majoribus A vel B, hoc minusculis a et b designat.</u> Mr Leibnitz confines his Method to the symbols dx & dy, so that if you take away his symbols you take away his method. Mr Newton doth not so. And whether he uses Letters with pricks or other symbols for fluxion his method is still the same. In his Letter of 24 Octob. 1676 he represents that he had a method of extracting fluents out of Equations involving their fluxions. Will Mr Leibnitz say that he had no such method unless he then used letters with pricks? If so, his letters with pricks are as old at least as the year 1676 & by consequence older then the differential Notes of Mr Leibnitz.

But its to be observed that fluxions & differences are not quantities of the same kind. Fluxions are velocities & differences are small parts of things generated by fluxion in moments of time. Fluxions are always finite quantities differences are infinitely little. Mr Newton sometimes uses prickt letters sometimes other letters or symbols for fluxions: Mr Leibnitz uses no symbols for fluxions to this day. The symbols of fluxions therefore used by Mr Newton are the oldest in the kind. These he multiplies by the Letter o to make them infinitely little moments or differences, & without the letter o either exprest or understood they never signify differences but are always finite quantities & signify velocities.

The second reason of our Mathematician for his conjecture is that Mr Newton understood not the differences of differences till after the writing of his Principia. For there, saith he, the constant increase of the letter x he represents not by a prickt letter as at present but by the letter o after the vulgar manner which destroys the advantages of the differential method. Here our great Mathematician commits two mistakes: one by supposing that Mr Newton represents differences by prickt letters; another by supposing that the method used in the Principia is Mr Newtons method of fluxions. Tis only a branch of his method of converging series, a method of resolving quantities into converging series & applying those series to the solution of Problemes without considering fluxions. In his Letter dated 10 Decem 1672 where he is speaking of a method whereof the method of Tangents there described is a branch or corollary, he represents that this method (which is the method of fluxions) extended to Questions about the Curvature of curves, & thence its manifest that he then understood the second fluxions or differences of differences. In the end of his book of Quadratures

And as for the letter o in the method of fluxions Mr Newton used it in his Analysis communicated to Mr Collins by Dr Barrow in the year 1669, & in his book of Quadratures where he represents fluxions by prickt letters, & he still used it in the same sence. And as it is the oldest notation so it is the best, the method thereby being more convenient more elegant & more Geometrical then the Differential & as universal. Dr Barrow in his method of Tangents published A.C. 1670, put the letters a & e for the differences of the Abscissas & Ordinates, & Mr Leibnits seven years after changed these letters a & e into the symbols dx & dy & gave the method a new name; beginning where <501r> Dr Barrow left off as that candid Gentleman the Marquess de L'Hospital observed long since in the Preface to his Analysis. And this was the original of the differential method, Mr Leibnitz learning by the Letters of Mr Newton how to improve the differential method of Dr Barrow. Mr Leibnitz in his Letter to Mr Oldenburgh dated 27 Aug. 1676 wrote: <u>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.</u> And these words make it demonstratively certain that Mr Leibnitz did not then understand the differential method. And yet in his Letter dated 21 Iune 1677 he wrote <u>Clarissime Slusij methodum tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem tangentium generalius tractavi, scilicet per differentias Ordinatarum.</u> He had newly found the differential method: & to make it his own pretended that he had found it jam a multo tempore But if he would have us beleive that he found it before the year 1677 it lies upon him to prove it. For its against the law of all nations to allow any many to be a witness in his own cause. And thus much in Answer to the great Mathematician

As to what the Author of the Latin paper saith of Mr Hook He indeed claimed one of Mr Newtons Propositions but could never produce a demonstration. Mr Leibnitz has claimed it also but the Demonstration by which he endeavoured to make it his own is erroneous. And if Mr Flamsteed denied Mr Newton the use of his Observations, Mr Tschurnhause denied Mr Leibnitz an invention

Our Author in the next place complains of the Committee of the Royal Society for representing that Mr Leibnitz had from Mr Iames Gregory the Series for finding the arc of a Circle by the tangent that is, he represents that the letters of Gregory Collins Oldenburg & Leibnitz examined & approved by the Committee & published in the Commercium were fourged. The Letter of Mr Gregory dated 15 Feb 167$\frac{0}{1}$ is still extant in his own hand writing & conteins this Series with several others then sent to Mr Collins. That of Mr Oldenburg dated 15 Aprril 1675 is extant in the Letterbook left by Mr Oldenburg in the Archives of the R. Society & conteins this series with several others then sent from Mr Collins by Mr Oldenburg to Mr Leibnitz The Answer of Mr Leibnitz dated from Paris 20 May 1675 was found in the Archives of the R. S. kept by their Secretaries & is still extant in the very hand writing of Mr Leibnitz & conteins his acknowlegment of the receipt of Mr Oldenburgs letter above mentioned with the series conteined therein. The Collection of the papers of Mr Gregory by Mr Collins after the death of that Gentleman is still extant in the handwriting of Mr Collins & at the request of Mr Leibnitz was sent to Paris in Iune 1676 & conteins a copy of the aforesaid Letter of Mr Gregory. But upon the death of Mr Gregory Mr Leibnitz to make this series his own pretended in his Letter dated 28 Decem 1675 that he had communicated it at Paris above two years before & that it was the series which he had wrote of to Mr Oldenburg before, that is in his Letters of 15 Iuly & 24 Octob 1674 & under this pretence he sent it back to Mr Oldenburg as his own in his Letter dated 27 Aug. 1676. Whereas the series which he wrote of in his said <501v> two Letters dated 15 Iuly & 24 Octob 1674 was not this series but another for finding the Arc of a circle by the sine. This series Mr Collins had received from Mr Newton in Iuly 1669 & communicated it soon after to some of his friends. Mr Leibnitz was in London in the years 1671 1672 & 1673, & having met with this series pretended in his said Letters of 15 Iuly & 24 Octob 1674 to have found it himself & yet in his Letter dated 12 May 1676 desired Mr Oldenburg to procure from Mr Collins the Demonstration thereof, that is, the method of finding it. And when he had received the method with some of Mr Newtons series, he pretended to have found some of those series before, though he did not yet understand the method of finding them. For in his Letter of 27 Aug 1676 he wrote back for a further explication of the method. And when Mr Newton in his Letter of 24 Octob. 1676 had explained a double method & added another method of regression Mr Leibnits wrote back in his Letter dated 12 Iuly 1677 for a further explication. {or} so soon as he understood it he wrote back that he found by his old papers that he had used one of those methods before. And by the same spirit, of invention when by the help of Newtons Letters & Gregories & Barrows methods of Tangents he had newly found the differential method, to make that method his own, he wrote back Et jam a multo tempore rem tangentium generalius tractavi scilicet per differentias Ordinatarum. And yet its very certain that he had but newly found it. For in his Letter dated

But our Author tells us that the English & Scotch (vizt Wallis, Hook, Newton & Gregory junior,) acknowledged for above 36 years together the Series for finding the arc of a circle by the Tangent to be the invention of Leibnits. Hoos is dead long since & medled not with these matters. Wallis & Gregory lived at Oxford & Newton at Cambridge and first at Edinburgh & then at Oxford & had not opportunity of knowing what passed between Oldenburg & Leibnitz. Gregory in a treatise published 1684, complained that nothing of his Vnkles adversaria relating to this method of series came to his hands except a few examples destitute of the method of finding them. Oldenburg died soon after he had sent Gregories series to Leibnitz & when Leibnitz sent him word that he had found this series some years before, there was no reason for Oldenburg to fall out with his friend & countriman about it. It was no bodies interest but Gregories to trouble themselves about it, & the matter might have lain still in the dark had not Mr Leibnitz himself put the R. Society upon appointing a Committe to search their Archives & those of Mr Collins about what related to the correspondence between Oldenburg Collins & Leibnitz. And since Mr Leibnitz & his correspondents question the credit of the Letters found upon this search the R. Society have ordered affadavits to be taken

thereof before a publick Notary & are ready to shew the Originals & affadavits to any publick ministers of the Empire France or Holland at the request of Mr Leibnitz or his friends.

Our Author tells us that Leibnitz shewed the series for the arc by the Tangent to Hugenius who applauded the same & that Mr Newton also acknowledged the method by which he found it to be a new one but he does not tell us that Mr Leibnitz let either Mr Hygens or Mr Newton know that he had received this series from Mr Oldenburg. And his method is only a Proposition for transmuting of Figures & deserves not to be called a method of series.

Our Author adds that Mr Leibnitz published in the Acta Leipsica a general method for finding the Ordinates of transcendent curves not by extraction of roots but deduced from a profounder foundation of the differential Calculus, by which the business of series was brought to a greater degree of perfection. But Mr Newton many years before, (viz in his Letter dated 24 Octob. 1676) communicated the same method in this sentence. <u>Altera [methodus consistit] in assumptione seriei pro quantitate qualibet incognita ex qua cætera commode derivari possint, & in collatione terminorum homologorum æquationis resultantis ad eruendos terminos assumptæ seriei</u>. Mr Leibnitz therefore has no title to any part of the method of converging series.

Our author tells us further that Mr Leibnitz was the first who used the exponential calculus. Certainly Mr Newton was the first who used fractions radicals & negative quantities for the indices of Dignities & thereby very much enlarged Analysis & laid the foundation of making it universal. In his Letter dated 24 Octob. 1676 he mentioned such indices of Dignities & thereupon Mr Leibnitz in his Answer dated 21 Iune 1677 proposed æquations with indeterminate dignities, But such æquations have hitherto been of no use.

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The Author of the Remarks makes a Report in opposition to the Report of the Committe of the R. Society, but begins his Report with what passed in the year 1676 whereas he should have begunn it with what passed seven years before. What he has omitted we may take occasion to supply hereafter. He saith further that Mr Newton did not speak of this matter till after the death of Mr Huygens & Dr Wallis who were well informed & able to judg thereof: whereas neither of them had seen the Analysis & Letters published since their death, & Mr Newton claimed the method in his Principia Philosophiæ, & Dr Wallis in the Præface to the first Volume of his Mathematical works published A.C. 1695 saith that Mr Newton in his Letters of Iune 13 & Octob 24 1676, <u>methodum hanc</u> [de Fluxionibus] <u>Leibnitio exponit, tum ante decem annos, nedum plures, ab ipso excogitatam</u>.

But since Mr Leibnitz began these disputes, & detracts from the candor of those who oppose him & in opposition to them represents it unjust to question his candor, making himself a witness in his own cause contrary to the laws of all nations, & appeals from the Report of a large Committee of the R. Society, to the judgment of a nameless Mathematician chosen by himself, which is the same thing as to make himself a Iudge as well as a witness in his own cause; & since his correspondents endeavour to set aside the consideration of the ancient Letters & Papers & bring matters to a wrangle: I desire you to print (in French) the Letter of Mr Iames Gregory dated 15 Febr. 1671 to the words secundum vulgaris Algebræ præcepta, a copy of which Letter was sent to Paris in Iune 1676 to be communicated to Mr Leibnitz. I desire you to print also the two Letters of Mr Leibnitz dated 15 Iuly & 26 Octob 1674 concerning a Theoreme or method for finding the Sector or Arc of a Circle whose sine is given; & Mr Oldenburgs Letter of 15 Apr. 1675 wherein he sent several series to Mr Leibnitz amongst which was the series of Gregory; & the Answer of Mr Leibnitz dated 20 May 1675 wherein he acknowledged the receipt of those series; & the latter part of his Letter dated 28 Decem. 1675 beginning with the words, <u>Habebis & a me instrumentum &c</u>. All which five Letters were left entered in the Letter books of the R. Society by Mr Oldenburg. Then print the letter of Mr Leibnitz dated 12 May 1676 which is still extant in his own hand writing, & that part of his Letter of 27 August 1676 which begins with these words, Sit QAIF Sector duabus rectis &c, & ends with these maximeque afficiens mentem. And then leave it to the Reader to make his judgment upon those Letters concerning the pretence of Mr Leibnitz to the series of Mr Newton for finding the Arc by the sine & to that of Mr Gregory for finding the

Arc by the Tangent & to some other series sent to him by Mr Newton. After which the Reader will be better able to make a judgment of his pretence to the original invention of the method of moments or differences.
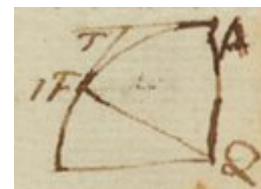

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To the R

The author of the Remarks makes a Report in opposition to the Committee of the R. S. but begins his Report with what passed in the year 1676 whereas he should have begunn it with what passed seven years before. He saith further that Mr Newton did not speak of this matter till after the death of Mr Huygens & Dr Wallis who were well informed & able to judge thereof. Whereas Mr Hugens was not fully informed, & Dr Wallis in the Preface to the first Volume of his mathematical works published A.C. 1695 saith that Mr Newton in his Letters of Iune 13 & Octob 24 1676, methodum hanc [de Fluxionibus] Leibnitio exponit, tum ante decem annos nedum plures, ab ipso excogitatam. And Mr Newton in his Principia Philosophiæ written 28 years ago, spake of this matter representing that he had

But since Mr Leibnitz began these disputes & dectracts from the candor of those who opposes him & in opposition to them represents it unjust to question his candor making himself a witness in his own cause contrary to the laws of all nations & appeales from the Report of a large Committee of the R. Society to the judgment of a nameless Mathematician of his own chusing, which is the same thing as to make himself a Iudge as well as a Witness in his own cause, & since his correspondents endeavour to set aside the consideration of the ancient Letters & Papers & bring matters to a wrangle, I desire you to print the Letter of Mr Iames Gregory dated 15 Febr. 1671 to the words secundum vulgaris Algebræ præcepta, a copy of which Letter was sent to Paris in Iune 1676 to be communicated to Mr Leibnitz. I desire you to print also the two Letters of Mr Leibnitz dated 15 Iuly & 26 Octob. 1674 concerning a Theoreme or Method of finding the sector or arc whose sine is given. And Mr Oldenburgs Letter of 15 Apr. 1675 wherin several series were sent to M^r L: & the Answer of Mr Leibnitz dated 20 May 1675 wherein he acknowledged the receipt of those series & the latter part of his Letter dated 28 Decemb. 1675 beginning with the words Habebis & a me Instrumentum &c. All which five Letters were left entered in the Letter books of the R. Society by Mr Oldenburg. Then print the letter of Mr Leibnitz dated 12 May 1676 which is still extant in his own hand

writing & that part of his Letter of 27 Aug. 1676 which beginns with these words Sit QAIF Sector, duabus rectis &c & ends with these maximeque afficiens mentem. And then leave it to the Reader to make his judgment upon those Letters concerning the pretence of Mr Leibnitz to the series of Mr Newton for finding the Arc by the sine & to that of Mr Gregory for finding the Arc by the tangent, & to some other series sent to him by Mr Newton After which the Reader will be better able to make a judgment of his pretence to the original invention of the method of moments & differences.
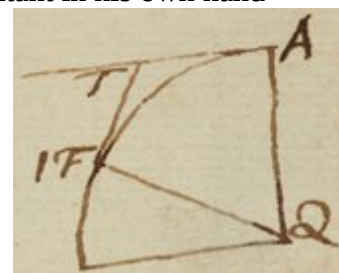
written to Mr Leibnits concerning the method of fluxions ten years before that time But Mr Leibnitz did not pretend to the differential method till the year after the receipt of Mr Newtons Letters. And Mr Newton also in the second Book of his Principles written 28 years ago claimed the method of fluxions as known to him in the year 1676 & Mr Leibnitz has hitherto allowed that claim without being able to make it apper that the Differential method was known to him before the year 1677. Dr Wallis also in a letter dated from Oxford Apr. 20 1695 & extant in the Archives of the R. Society represented that he had intimation from Holland that Mr Newtons Letters or papers relating to the Method of Fluxions should be printed because his notions of Fluxions passed there with great applause under the name of the Differential method, & thereupon he complained that Mr Newton in neglecting this matter was not so kind to his reputation as he ought to be.

But because Mr Leibnitz & his correspondents have published in Germany a Paper whereby they endeavour endeavour to set aside the ancient Records defame Mr Newton & the Committee of the R. S. & misrepresent

the whole affair. I intend to take an occasion of giving you an Account of these matters out of records themselves. But because Mr Leibnitz by his correspondents have published somewhere in Germany a Paper without a name whereby they endeavour to defame the Committee of the R. Society, & Mr Newton, to & to set aside Records & bring the matter to a squabble & make it a dispute between England & Germany (all which are very dishonest practises) I intend to give you hereafter a fuller account of these matters out of the Records themselves.

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Mr Newton in a Letter written to Mr Collins 10 Decemb. 1672, that is, some weeks before Slusius sent his Method of Tangents into England, described the same method of tangents as a Corollary or branch of his general method – – – – extended it to the consideration of the second fluxions.

And in his Letter to Mr Oldenburg dated 24 Octob. 1676 he represented that five years before, viz A.C. 1671, he wrote a treatise of the method of infinite series & of another method which readily gave the method of Tangents of Slusius & stuck not at surds & which was founded in this sentence. Data æquatione quotcunque fluentes quantitates involvente, invenire fluxiones & vice versa. Which sentence relating to the 2^d 3^d & following fluxions as well as to the first: it must be allowed that in the year 1671 he had extended his method to all those fluxions, especially since the method is one & the same in them all. The method which being applied to the first equation gives a new equation involving the first fluxions, if applied to this new equation will give another new one involving the second fluxions, & so on perpetually.

The sentence Data æquatione quotcunque fluentes quantitates involvente invenire fluxiones & vice versa, being the foundation of the method upon which he wrote in the year 1671 & relating to the 2^d 3^d & following fluxions as well as to the first, & being one & the same method in them all, it must be allowed that his method in the year 1671 extended to all the fluxions. For after the very same manner that this method being applied to any æquation gives a new æquation involving the first fluxions of the fluents, if it be applied to this new one it gives another new one involving their second fluxions & so on perpetually

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I had almost forgot to observe that the author of the Remarks complains of the Royal Society for giving judgment without hearing both sides, & thereupon represents their sentence voyd. Mr Leibnitz complained to them against me, proposing that they should make me publickly retract what I had written. And could they with any colour of justice condemn me without examining the matter? But Mr Leibnitz thought the matter so clear as to need no examination & therefore demanded justice against me without submitting the matter to the examination of the R. Society. And was it either just or decent for Mr Leibnitz to make himself Iudge in his own cause & order the R. Society to put his sentence in execution. But Mr Leibnitz wrote to the Society that I impugned his candor which that he at so great an age & after so many documents of his life should defend, with an Apology & contend as it were before a tribunal with a novice & one unaccquainted with things done formerly no man prudent or just would approve of. And have not I as much a right to complain of Mr Leibnitz before the R. Society as Mr Leibnitz has to complain of me? Or have they not an equal authority over us both? Or if they could not condemn Mr Leibnitz without a hearing, could it be just in Mr Leibnitz to write to them to condemn me without a hearing? Certainly Mr Leibnitz in complaining to the R. Society against me gave them authority to examin the matter between us, & obliged himself to give his reasons against me for justifying his accusation & upon declining to justify his accusation made himself liable to be condemned of calumny. However the R. Society have only appointed a Committee to search out old records & to give their opinion to the Society upon them & ordered the Records & Report upon them to be published, & they that compare the Report with the Records will find them agree. But Mr Leibnitz still declines to enter into the merits of the cause, & pretends that he has not yet seen the Commercium Epistolicum & that he being not at leasure has writ to give his judgment upon the matter & sent the judgment of the Mathematician dated 7 Iune 1713 to a nameless correspondent to be published. And his correspondent has published it in Germany together with a railing scurrilous letter dated 29 Iuly 1713, without setting any man's name to it. And while Mr Leibnitz instead of a sober defence has set on foot the writing of this paper & either writ it

himself & sent it to his correspondent to be published or knows the names of them who writ it, he has made himself answerable for the whole untill he shall discover the names of his Accomplices.

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I had almost forgotten to observe that the Author of the Remarks complains of the Committee of the R. S. for giving judgment without hearing both parties. And have I not as much reason to complain that Mr Leibnitz desired the R. Society to condemn me without a hearing. If he thought matters so plaine against me as to need no examination, I think them plainer against him. If By complaining to the R. S. against me & pressing his complaint by a second letter he gave them authority to appoint a Committee to examin the matter between us & obliged himself to produce his reasons for justifying his accusation least it should go for a calumny.

[1] see Keill to Newton May 25 1714

[2] **{*}**

[3] **{*}**

[4] **{*}**

[5] **{*}**

[6] **{*}**