Several drafts of letters of Newton to Des Maizeaux after the death of Leibniz

Author: Isaac Newton

Source: MS Add. 3968, ff. 383r-411v, Cambridge University Library, Cambridge, UK

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Sir

The author of the Letter written to Mr Leibnitz of 7 Iune 1713, did know by the first Proposition of the Book of Quadratures that Mr Newton understood second third & fourth differences & gave a true rule for finding them & by the Introduction to that Book that he practised the Method of fluxions as well without prickt letters as with 'em, & continued to put the letter o for the moment of x & that the Principia Philosophiæ were writ by the Method of Synthesis & could not have been written by that Method without a previous Analysis, & that in writing by Synthesis the Analysis is to be concealed.

Mr Leibnitz has told us that the author of this Letter was M{^{r}} Iohn Bernoulli. And as for the Letter of 29 Iuly 1713 it was certainly writ by some body who remembred what Mr Leibnits did at Paris 39 years before, as may be gathered from this sentence. <u>Modum quo Leibnitius, ad seri{e}i Nicolai Mercatoris imitationem, invenit seriem suam, ipse statim, Hugenio B. Lutetiæ agenti communicavit, qui et per Epistolam laudavit.</u>

Sir

I have run my eye over the printed Letters which you have put into my hands, & since you have the Originalls I think it is right to let them come abroad I have observed some faults may be put into the errata. Pag. 8 l 13 dele c'est a dire, commune ou superficielle. Pag. 16. l. 5. write A Londres Feb. 26, $17\frac{15}{16}$ st. vet. Pag. 17. l. 13 write en Iuillet 1714. P. 19. l 20 desquelles. P. 46. l. 11 en 1677. P. 75 lin 15 write en Iuin 1713 P. 78 l. 9 write 24, 26.

In the Letter of Mr Leibnitz to Madam la Comtesse de Kilmanségger, pag. 51, 52 he gives an Account of his finding the series for the circle like that of Mercator for the Ellipsis & then adds Cependant le mien fut assez – – – j'appris tard. If you consult the original Letters you will see that Mr Gregory found this series in the beginning of the year 1671 & sent it then to Mr Collins in a Letter dated 15 Feb $16\frac{70}{71}$ & that Mr Collins inserted a copy of this Letter into the Collection of Gregories papers sent to Mr Leibnitz in Iune 1676. And Mr Oldendenburg sent the same series to Mr Leibnitz in a letter dated Apr. 15. 1675 & Mr Leibnitz in a letter dated 20 May 1675 acknowledged the receipt of Mr Oldenburgs Letter. And after all this Mr Leibnitz sent this Series to Mr Newton in a letter dated 27 Aug 1676 & published it in Germany A.C. 1682 without mentioning that he had received it from Engla{nd} & that Gregory invented it before him.

In the same Letter pag. 34, 35 Mr Leibnitz transcribes the Scholium upon the Lemma of 2^d Book of my Principles & then adds. Ainsi, Mr Newton ne me contesta point de avoir trouvé la chose de mon chef. [But he

goes from the Question. The true Question is not, whether Mr Leibnitz found it apart, but whether <383v> he was the first Inventor. Neither I nor the Committee of the R. Society ever accused of not finding the method by himself but we have said that he was not the first inventor & in the year 1676 saw copies of my Letters of 10 Decem 1672, 13 Iune 1676 & 24 Octob 1676 {illeg} that those Letters were sufficient to make him understand that I had a very general method of which & the method of Series together I wrote a Tract in the year 1671 & founded this method upon a sentence which in] And its very true that Mr Newton neither then nor at any time since contested the point whether Mr Leibnitz invented this series by himself or not. This was not the Question but whether Mr Leinitz was the first inventor. And in this Scholium Mr Newton refers to his Letters of 13 Iune & 24 Octob 1676 by which it appears that he had the Method when he wrote those Letters & and five years before or above, In the year 1684 Mr Leibnitz published the elements of the Differential Method, & in the end of his paper added that this method extended to the solution of such Problemes as could not be resolved without either this or another method like it. And becaus he did not say whose was the other method not what he knew of it nor make any mention of his correspondence with me eight years before by means of Mr Oldenburg about these matters, as he ought to have done; I supplied the defect by writing the said Scholium. In the second Lemma of the second Book of Principles I demonstrated the Elements of this method synthetically because I had occasion to make use of them in the following Propositions, & I added the aforesaid Scholium not to give away the Lemma as Mr Leibnitz hath pretended but to satisfy the Reader that I had it before I kn

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And that in the end of the year 1671 I invented the Method of extracting fluents out of Equations involving fluxions & at the request of Dr Wallis sent it to him in September 1692, & that in the year 1676 I wrote the book of Quadratures (exccept the Introduction & Conclusion) extracting most of it out of old papers & when I had finished tenth Proposition with its Corollaries & the composure thereof was fresh in memory I wrote the Letter about it to Mr Collins dated Octob. 8. 1676 which Mr Iones found amonge his papers & published. ② And that as I invented the Method of fluxions & the Theory of Colours in the years 1665 & 1666 & in the year 1671 was upon a designe of publishing them, so for a reason mentioned in my Letter of 24 Octob 1676 I forbore {illeg} publishing them till the year 1704. ① And that when I wrote in my Letter of 24 Octob. 1676 that the general Theoremes there mentioned for squaring of Curves were founded on the Method of fluxions, I had relation to the method of finding them described in the six first Propositions of the Book of Quadratures.

③ The Introduction to the Book of Quadratures the method of fluxions is taught without pricket Letters & the letter o is used both in the Introduction & in the Book it self without destroying the advantages of the ✝ < insertion from the bottom of the page > ✝ the differention Notation, & the Notation by pricks is shorter then that by the letter d, & the Notation by pricks & the letter o together has more advantages then that by the letter d < text from f 384v resumes > & the first Proposition of the Book is a demonstration that I understood all the degrees of fluxions when I wrote it, & my saying in the Introduction that I found the Method gradually in the years 1665 & 1666, which has occasioned all this squabble, is not so much as Dr Wallis affirmed long before in the Preface to the two first Volumes of his works without being then contradicted by any body.

And that in the end of the year 1671 I invented the Method of extracting fluents out of Equations involving fluxions mentioned in my Letter of 24 Octob. 1676, & in the year 1676 I wrote the book fo Quadratures except the Introduction & Conclusion extracting most of it out of old Papers. And when I wrote in my Letter of 24 Octob 1676 that the general Theoremes there mentioned for squaring of Curves were founded on the Method of fluxions, I had relation to teh method of finding them described in the six first Propositions of the Book of Quadratures, And when I wrote to Mr Collins my Letter of 8 Novem. 1676 published by Mr Iones I had relation to the tenth Proposition of the Book of Quadratures with its Corollaries which being then newly written were fresh in memory. & in two Letters dated Aug 27 & Sep. 17 1692 I sent to Dr Wallis the method of fluxions described in the first Proposition of the book of Quadratures & the method of extracting fluents out of equations involving fluxions mentioned in my Letter of Octob 24, 1676 both which methods were printed the next year in the second Volume of the Doctors works pag. 391, 392, 393, 394, 395. And that About the same time that I invented the method of fluxions I invented also the Theory of Colours vizt in the

beginning of the year 1666 | winter between the years 1665 & 1666, & in the year 1671 I was upon a design of publishing both: but soon after laid aside my designe for a reason mentioned in my Letter of 24 Octob. 1676 & for the same reason forbore to publish them till the year 1704.

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And that these papers, where I considered only first fluxions, were composed without prickt letters, but where I considered second third & fourth fluxions (as for instance in extracting fluents out of Equations involving fluxions) I used prickt letters & that in the year 1676 I wrote the book of Quadratures (except the Introduction & Conclusion,) extracting most of it out of old papers & with relation to the tenth Proposition & its Corrollaries, (when I had newly finished them, & they very fresh in memory I wrote to Mr Collins my Letter of 8 Novem. 1676 published by Mr Iones ‡ < insertion from f 383v > ‡ And that the Tables at the end of that Proposition for squaring some Curves & comparing others with the Conic Sections were invented by the inverse method of fluxions before the year 1671 as may be understood by my Letter of 24 Octob 1676, [& most of their above four years before] < text from f 384r resumes > And that in my Letter of 24 Octob. 1676, where I said that the general Theoremes there mentioned for squaring of Curves were founded on the Method of fluxions copied from the first Proposition of the Book of Quadratures & the method of extracting fluents out of equations involving fluxions mentioned in my Letter of Octob 24 1676, both which Methods were printed the next year viz A.C. 1693 in the second volume of the Doctors works. pag. 391, 392, 393, 394, 395. [All which may suffice to justify my saying in the Introduction to the Book of Quadratures that I found this method gradually in the years 1665 & 1666, which was not so much as was said & published by Dr Wallis nine years before in the Preface to the first Volume of his works without being then contradicted.]

And that I usually put letters for fluxions where I considered only first fluxions but where I considered also second third & fourth fluxions (as for instance in extracting – – –) distinguished them by letters with one two or more pricks. And that in the year 1676 – – – – 3094. 395. And that about the same time that

Mr Leibnitz does not pretend to have found it before the year 1676 & in his Letter of 27 Aug. 1676 there are some things which satisfy me that he did not know it when he wrote that Letter

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& therefore was either Mr Iohn Bernoulli himself or copied the expression solutionem meam from one of Mr I. Bernoulli's letters.

Sir

I have perused the printed sheets which you left in my hands & beleive that the Letter of M. Leibnitz to Mr Remond which is printed in the eighth place should have been printed in the second, the contents thereof relating to the Postscript of the Letter of Mr Leibnitz to M^r l'Abbé Conti which is printed in the first place. Also the Letter of Mr Leibnitz to M^r l'Abbé Conti which was writ in answer to mine & is dated 9 April 1716 should have come before the Letter of Mr Leibnitz to Madam la Comtess de Kilmansegger dated 18 April 1716

None of the Letters being written to me I did not think it my self concerned to answer any of them till M^r l'Abbé Contj sollicited me to answer the Posctscript of his first Letter that my Answer as well as that Postscript might be shewen to the King. But when my Answer was sent to Mr Leibnits with a Letter of M^r l' Conti & he sent both open to Mr Remond at Paris with his Answer to them as he had done the Poscript before, & his Answer was sent from Mr Remond to M^r L'Abbé Conti & he gave M^r l'Abbe Conti notice that he took this Method that he might have neutral & intelligent witnesses of the dispute, I refused to an Answer to be sent to him & only wrote some Remarks upon his Answer & shewed them privately to some friends here to satisfy them that it was easy to have returned an Answer had I thought fit to let him go on with his politicks. And as soon as I heard that he was dead I caused the Letters to be printed with those Remarks least they should come out imperfectly abroad.

At the same time that these Letters were sent to Mr Remond Mr Leibnitz wrote also to Madam la Comtesse de Kilmansegger & a large Poscript to the Baron Bothmar, about these matters. And this Letter & Postscript were then shewed me, but they being not written to me I did {not} write any Answer to them; & much less do I think it proper for me now to meddle with them, Mr Leibnitz being dead.

There was also at the same time a Panegiric upon Mr I. Bernoulli & a Satyr upon Dr Keill written by an anonymous author & published {I}n the Acta eruditorum for Iuly 1716. The author haz fathered it upon Mr Iohn Bernoulli himself by calling a formula of his meam formulam pag. 34 lin. 27 & thereby has made Mr I. Bernoulli call himself eminentem Mathematicum, excelsum ingenium & virum ad abstrusa et abdita detegenda natum pag. 296 l. 13 & p. 298. l 32 & p. 301 lin 29. But its possible that the author may have copied the words meam formulam out of one of Mr I. Bernoullis Letters. Mr Nicolas <385v> Bernoulli was in England in Autumn A.C. 1712 &

Mr Leibnitz went from London to Paris in Febr. or March 1673 carrying with him the Mercators & kept a correspondence with Mr Oldenburg till Iune following for he dyd not yet understand the higher Geometry

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Mr Iames Bernoulli in the Acta eruditorum for Ianuary 1691 pag. 14, said – – – – – – & 24 Octob. 1676.

In the end of the year 1669 Mr Collins sent notice to Mr Iames Gregory that I had a general Method of series, & Mr Gregory by this notice & one of my series, being put upon searching after this Method found it after a years study. But tho he found it by himself, yet because he knew that I had it before him he never claimed a right to it.

I wrote the Book of Quadratures in the year 1676, except – – – – – published by Mr Iames. He that compares that Letter with those Propositions & particularly with the second Corollary of the 10th Proposition will easily see that I had composed those Propositions before I wrote that Letter. The Tables at the end – – – by which those Theoremes could be invented. The Book of Quadratures was therefore composed before Mr Leibnitz understood the differental Method.

At the request of Dr Wallis I sent to him – – – nor are necessary to the Method. The Marquess published the book of infinitement petits in the year 1696, three years after the first Proposition of the book of Quadratures had been published by Dr Wallis. And therefore my Rule for finding second third & fourth fluxions was published three years before the Rule for finding second third & fourth differences came abroad.

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To the Reader

Towards the end of the year 1715 (I think about November) Mr Leibnitz writing to M^r l'Abbe Conti then at London about the systeme of Mr Nigrisoli, added a large Postscript about the invention of the Differential Method. And the Postscript was sent also to Mr Remond at Paris. The Postscript not being written to me I did not think my self concerned to meddle with it. But Mr Leibnitz declining to answer the Commercium Epistolicum or to meddle with any body but me: M^r l'Abbé le Conti at length pressed me to write an Answer to the Postscript that they might both be shewed to the King. And when my Answer was sent to Mr Leibnitz he wrote an Answer to it & sent it with his Answer open to Mr Remond at Paris & the Answer was sent by Mr Remond to M^r l'Abbe conti. This tricking management compared with the beginning of his Answer made me sensible that he was upon a design of setting aside the ancient Records published in the Commercium Epistolicum, & stripping me of my friends, & attaquing me alone with all his posse of disciples & all his interest Whereupon I refused to write an Answer to be sent to him & yet to satisfy my friends that it was easy

to have answer him had I thought fit to let him go on with his politiques, I wrote an Answer in the form of Observations & shewed them privately to some of my friends. And as soon as I heard that Mr Leibnitz was dead I caused the Letters & Observations to be printed least they should at any time hereafter come abroad imperfectly in France.

There were some other Letters writ by Mr Leibnitz, M^r l'Abbé Conti, & Mr Remond; & by M^r l'Abbé Conti left in the hands of ** Mr de M. to be published together with what I had caused to be published before. And a few days since a copy of the same was brought to me printed off except the last sheet. But they being put together in wrong order I have caused them to be reprinted in due order of time, together with a Paper

He never pretended to have found it earlier. And when he wrote his Letter of 27 Aug. 1676 he placed the perfection of Analysis not in the differential calculus as he did afterwards but in a method founded on Analytical Tables of Tangents & the combinatory Art. <u>Nihil est</u>, saith he, <u>quod norim in tota Analysi momenti majoris</u>. And a little after: <u>Ea vero non differt ab Analysi illa SVPREMA ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum</u>. And in answer to what I had said in my letter of 13 Iune 1676 concerning the universality of my method he replied: <u>id mihi non videtur</u>. In October following he came to London the second time & there met with Dr Barrows Lectures & saw my Letter of Octob. 24, 1676, & three months before received a <385v> copy of my Letter to Mr Colling of 10 Decem 1672

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② {to Des Maiseaux a Vou}{illeg}

Sir

I have perused the printed sheets which you left in my hands & beg leave to take notice that when the Postscript of Mr Leibnitz's first Letter to M^r l'Abbé Conti was shewed to me: I did not think my self concerned to meddle with it, till at length I was pressed by M^r l'Abbé Conti to answer it that the Postscript with my Answer might be shewed to the King. And when Mr Leibnitz sent it with his Answer to Mr Remond at Paris & his Answer was sent open from Paris to M^r l'Abbé Conti I refused to write any thing more to be sent to him. But yet I wrote a Paper of Remarks upon his Letter & shewed it to some friends in private to let them see how easy it was to have answered his Letter had I thought fit to let him go on with his Politiques. And whe I heard that he was dead I printed the Letters with these Remarks least they should be published imperfectly abroad. And since you are reprinting them with some other Letters written at the same time, & in some of those Letters he tells his own story at large; tho I will not write an Answer to those letters now he is dead; yet since in his Letter of 4 March 1711 when I knew not what had been printed against me Six years before in the Acta Eruditorum nor was any way concerned in this controversy, he appealed to me against Dr Keill & would contend with no body but me nor let me rest till I set pen to paper: I think I may be allowed to tell the story my self, & leave it to be compared with his Narrations.

& as to the Method of fluxions he never pretended to have found it before the year 1676. Mr Iames Gregory died in the end of the year 1675, & when the news came to Paris Mr Leibnitz desired that the Letters & Papers of Mr Gregory might be collected & a copy of them sent to Paris which was done in Iune following. He desired also that the demonstration of some of my series (meaning the method of finding them which I had sent to {illeg}{bo}{illeg}rs in the Analysis per Æquationes numero terminorum infinitas) might be procured from Mr Collins & sent to him, & promised to send back something of his own in recompence: whereupon at the request of Mr Oldenburg & Mr Collins, I wrote my Letter of 13 Iune 1676. And this Letter was sent to him at the same time with the aforesaid collection of Gregories Letters In that Collection was a copy of a Letter of Mr Gregory dated 5 Sept 1670 signifying that he had improved Dr Barrows method of Tangents into a general method of drawing Tangents without any calculation. There was also sent him in the same Collection a copy of one of my Letters to Mr Collins dated 10 Decem. 1672, in which I said that the Method of Tangents of Gregory & Slusius were a branch or corollary of my general method of Analysis, & that this method stuck not at surds, & that I had interwoven this Method with the method of Series, meaning

in a Tract which I wrote of them both in the year 1671. And by these Letters it was easy to understand that Dr Barrows method of Tangents was capable of being improved so as to give the method of Tangents <388r> of Gregory & Slusius, & that this Method was capable of further improvement so as to give my general Method of Analysis. In my Letter of 13 Iune I said: Ex his videre est quantum fines Analyseos per hujusmodi infinitas æquationes ampliantur: quippe quæ, earum beneficio, ad omnia pene dixerim problemata (si numeralia Diophanti et similia excipias) sese extendit; non tamen omnino universalis evadit nisi per ulteriores quasdam methodos eliciendi series infinitas — Sed quomodo in istis casibus procedendum sit, jam non vacat dicere ut neque alia quædam tradere quæ circa Reductionem infinitarum serienum in finitas, ubi rei narua tulerit, excogitavi. Nam parcius scribo quod hæ speculationes diu mihi fastidio esse cœperunt; adeo ut ab ijsdem jam per quinque fere annos abstinuerim. And to this Mr Leibnitz in his Letter of 27 Aug. 1676 replied: Quod dicere videmini plerasque difficulcates exceptis Problematibus Diophantæis) ad series infinitas reduci; id mihi non videtur. Sunt enim multa usque adeo mira et implexa ut neque ab And these things induce me to beleive that he wrote this Letter before he found the Differential Method.

Mr Iames Bernoulli in the Acta Eruditorum for December 1691 pag. 14, said that the Calculus of Mr Leibnitz was founded in that of Dr Barrow & differed not from it except in the notation of differentials & some compendium of operation. And the Marquess de l'Hospital in the Preface to his Analysis of infinite petits published A.C. 1696 represented that where Dr Barrow left off Mr Leibnitz proceeded, & that <451r> the improvement which he made to Dr Barrows Analysis consisted in excluding fractions & surds: but he did not then know that Mr Leibnis had notice of this improvement from me by two Letters above mentioned, dated 10 Decem. 1672 & 24 Octob. 1676. After he had notice that such an improvement was to be made, he might find it proprio Marte, but by that notice knew that I had it before him.

In the end of the year 1669 Mr Collins sent notice to Mr Iames Gregory that I had a general method of Series & Mr Gregory by this Notice & one of my Series being put upon searching after this method found it after a years study. But tho he found it proprio Marte yet because he knew that I had it before him, he never claimed a right to it.

Data æquatione fluentes quotcunque quantitates involvente, invenire fluxiones, & vice versa. This second volume came abroad in the year 1693, two years before the first Volume, & three years before the Marquess de l'Hospital published his Analysis des infinitement petits. And this is a demonstration that in those days I used prickt letters, & understood second third & fourth fluxions: & that my Rule for finding them was not only the first that was invented but also the first that was made publick. When I considered only first fluxions I seldom used letters with pricks. But when I considered also second third & fourth fluxions I distinguished them by letters with one two or more pricks & for fluents I put the fluxion either included within a square (as in the above mentioned Analysis) or with a square prefixed (as in some other papers) or with an oblique line upon it. And these notations by pricks & oblique lines are the shortest & best yet used, but were not known to the Marquess de l'Hospital when he recommended the differential notation, nor are necessary to the Method.

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Sir

I have viewed the printed papers you left in my hands. The Remarks are only upon the Letter of Mr Leibnitz to M. l'Abbé Conti dated 9 Apr. 1716. The Letters to the Princess & the Comtesse of Kilmansegger, & the Postscript of a Letter to Baron Bothmar are without an Answer; & I do not see that they need any. None of his Letters were writ to me, & I had not answered any of them had not M^r l'Abbé Conti pressed me to write an answer to the Postscript of a Letter to him that both might be shewed to the King. But when I found that Mr Leibnitz sent all the Letters open to Paris & his answer came hither from thence I declined writing any more Letters & only wrote the Remarks upon that Answer to satisfy my friends here that what he writ was easy to have been answered, if it had come hither directly. The Commercium Epistolicum notwithstanding any thing which has hitherto been said against it, remains in full force, & while that remains unshaken there is no need of writing any thing further about these matters.

Mr Leibnitz objected against it that in printing the ancient Letters & papers many things had been omitted which made for him & against me: but in offering to prove this twice by instances he failed as often. He objected also that the Committee of the R. Society strained things against him by fals interpretations, & particularly that they had maliciously interpreted the words of the Acta Eruditorum for Ianuary 1705, by saying: Sensus verborum est quod Newtonus fluxiones pro differentijs Leibnitianis substituit: but he has misinterpreted the place himself. For the Editors of the Acta in this Paragraph, call Mr Leibnitz the INVENTOR, & thence deduce this conclusion Pro differentijs IGITVR Leibnitianis Newtonus adhibet, semperque [pro differentijs illis] adhibuit fluxiones — ijsque tum in suis Principijs Naturæ Mathematicis, tum in alijs postea editis [pro differentijs illis] eleganter est usus QVEMADMODVM ET Honoratus Fabrius in sua Synopsi Geometrica motuum progressus Cavallerianæ methodo substituit. This is the interpretation which the Committee put upon the place, & the words igitur & quemadmodum & enforce it; & therefore the objection vanishes, & the Commercium Epistolicum remains untouched

It has been alledged that Mr Leibnitz found the Method by himself. But second inventors have no right. Whether Mr Leibnitz found the method by himself or not is not the Question. The Committee of the Royall Society did not enter into this Question, but on the contrary said: We take the proper Question to be, not who invented this or that Method, but who was the first inventor of the Method. And we beleive that those who have reputed Mr Leibnitz the first Inventor, knew little or nothing of his correspondence with Mr Collins & Mr Oldenbourg long before, nor of Mr Newton's having the Method above 15 years before Mr Leibnitz began to publish it in the Acta Eruditorum of Leipsic. Here the Committee treat Mr Leibnitz as second inventor & complain of him only for publishing the Method as his own without making any mention of the correspondence which he had with me & my friends about it long before he published it. By that correspondence he had notice in Iune or Iuly 1676 that I had a very general Method of Analysis, which extended to the abstruser sorts of Problems & that this Analysis proceeded without stopping at Surds, & that it was an improvement of the Method of Tangents of Gregory & Slusius, & that the method of Tangents of Gregory was an improvement of the Method of Tangents of Barrow. & that I had it when I wrote my Letter of 10 Decem 1672 & that I had interwoven this method with the method of infinite series before I wrote that Letter And in October 1676 he had further notice of these things & met with Dr Barrows Lectures in London & in his Letter of 21 Iune 1677 sent back an improvement of Dr Barrow's <389v> method of Tangents so as to give the Method of Gregory & Slusius & to proceed without stopping at surds, & then added: Arbitror quæ celare voluit Newtonus de Tangentibus ducendis ab his non abludere. Quod addit, ex hoc eodem fundamento quadraturas quoque reddi faciliores me in sententia hac confirmat. These last words relate to the notice which he had also, that my method produced Theorems for squaring of curvilinear figures by converging series which in some cases break off, & become finite; & that it produced also Theorems for comparing curvilinear figures with the Conic sections: but how it produced these Theorems he was not able to find out. He had notice also that my Analysis by the help of converging Equations was so general as to extend to almost all Problems except some numeral ones like those of Diophantus; but in his Letter of 13 Aug. 1676 he questioned its being so general & placed the perfection of Analysis in Analytical tables of tangents & the combinatory art & therefore was hitherto a stranger to the Differential Analysis. He had notice also that I had this method when I wrote my Letter of 10 Decem 1672 & that I wrote a Tract upon it in the year 1671. And tho after all these notices he might find out the method propria marte, yet by publishing it as his own eight yers after the correspondence by which he had all these notices, & doing it without mentioning that correspondence; he put me upon a necessity of mentioning it in the Book of Principles, & gave just occasion to the Committee of the R. Society to do the like.

Mr Iames Bernoulli in the Acta Eruditorum for Decem. 1691 & the Marquess de l'Hospital in the Introduction to his Analysis represented that the differential Method was an improvement of Dr Barrow's Method of Tangents, & the Marquess represented further that the improvement lay in shewing how to proceed without stopping at surds: but the Marquess did not then know that Mr Leibnitz had notice of this improvement from me before he found it, & the Committee complained of him justly for concealing this & all the other notices.

It has been said that in the old Letters & Papers published in the Commercium Epistolicum there are no prickt letters. And indeed I seldome used prickt letters when I considered only first fluxions, as in the Introduction to the Book of Quadratures: but when I considered also second third & fourth fluxions, as in the body of the

book, I distinguished them by the number of pricks. In the year 1692, at the request of Dr Wallis I sent to him the first Proposition of the Book of quadratures with its solution & examples in first & second fluxions, copied almost verbatim from the book, I sent him also the method of extracting fluents out of Equations involving fluxions, which to the best of my memory was composed in the year 1671: & the Doctor printed them both the same year in the second Volume of his works which came abroad the next year A.C. 1693. And thence it may be understood that the Book of Quadratures was then in manuscript. In my Letter of 24 Octob. 1676 I set down the first Proposition of this Book verbatim in an Ænigma, & said that it was the foundation of the method there concealed, & that it gave me general Theorems for squaring of figures by series which sometimes break of & become finite, & how it gave me such series is explained in the first six Propositions of this Book, & I know no other method of finding them. And therefore I had the Method at that time so far as it is contened in those six Propositions. In the same Letter I copied also many Ordinates of curves from a Table in the end of the tenth Proposition & upon the 7th 8th 9th & 10th Propositions I wrote to Mr Collins my Letter of 8 Novem. 1676 printed by Mr Iones. And from all this it may be understood that the book was then in manuscript. And as the notation used in this Book is the oldest, so it is the shortest & most expedite, but was not known to the Marquess de l'Hospital when he recommended the differential Notation. If it be asked why I did not publish # < insertion from f 390v > # If it be asked why I did not publish this book sooner, it was for the same reason that I did not publish the Theory of colours sooner, & I gave the reason in my Letter of 24 Octob. 1676.

⊡ The first Proposition < insertion from f 390v > ⊡ The first Proposition of the Book of Quadratures is certainly the foundation of the method of fluxions. That Proposition was comprehended verbatim in the Ænigma by which in my Letter of Octob. 24 1676 I concealed the foundation of the Method there spoken of, & therefore that Method there spoken of was the Method of fluxions. In that Letter I said that I had written a Tract on this Method & the Method of series together five years before, but did not finish it, nor meddle any more with these things till the year 1676, being tired with them. And in my Letter of Iune 13, 1676 I wrote to the same purpose. And this is the Method which I described in my Letter of Decem 10th 1672. In my Analysis per æquationes numero terminorum infinitas, I said of the Method described in that Tract: <u>Denique ad Analyticam merito pertinere censeatur cujus beneficio Curvarum areæ et longitudines &c (id modo fiat) exacte et Geometrice determinentur. Sed ista narrandi non est locus</u>. This relates to Quadratures by Series which in some cases break off & become finite; as you may understand also by the Letter of Mr Collins to Mr Strode Iuly 26 1672. And therefore before Dr Barrow sent that Tract of Analysis to Mr Collins, that is, before Iuly 1699, I had the method of fluxions so far at the least as it is contained in the first six Propositions of the Book of Quadraturs.

The Committee of the R. S. said that I had this Method above 15 years before Mr Leibnitz began to publish it. And Mr Collins in a Letter to Mr Bertet < text from f 390v resumes > < text from f 389v resumes >

The Committee of the Royal Society said that I had this Method above fifteen years before Mr Leibnitz began to publish it; ⊙ < insertion from f 390v > ⊙ And Mr Collins in a Letter to Mr Bertet dated Feb. 21. 16$\frac{70}{71}$ said that about four years before that time I found a general Analysis for squaring all curvilinear spaces & doing what ever depends upon Quadratures. And in a Letter to Mr Strode dated 26 Iuly 1672 he said that by the Analysis per æquationes numero terminorum infinitas & other papers communicated before to Dr Barrow it appeared that I had the Method & applied it generally some years before the Doctor sent that Analysis to Mr Collins, that is some years before Iuly 1669. And in a Letter of Aug. 11 1676 to Mr David Gregory the brother of Mr Iames Gr. Mr Collins wrote that a few months after the Logarithmotechnia of Mr Mercator came abroade, [which was in Sept. 1688.] a copy of it being sent to Dr Barrow at Cambridge he wrote back that this doctrine of infinite series was invented by me about two years before the Publication of Mr Mercators Logarithmotechnia [that is about Sept. 1686] & generally applied to all Curves & then sent to him my Manuscript.. And I see no reason why the testimony of Dr Barrow grounded upon what I had communicated to him from time to time, before the Logarithmotechnia came abroad, should be questioned in this matter. Dr Wallis also who flourished in those days & was inquisitive & skilfull, & received copies of my Letters from Mr Oldenburgh in the year 1676; published the same thing in the Preface to the first Volume of his Works without being then contradicted, saying that in my Letters of Iune 13 & Octob 24 1676 I explained

to Mr Leibnitz the Method invented by me ten years before that time or above. And the testimony of these three ancient knowing & credible witnesses may suffice to excuse me for saying in the Introduction to the book of Quadratures that I found the Method by degrees in the years 1665 & 1666. < text from f 389v resumes > and Mr Collins, in his Letter to Mr Strode dated 26 Iuly 1672, said that by the Analysis per Æquationes numero terminorum infinitas & other papers communicated before to Dr Barrow it appeared that I had the Method & applied it generally <390r> some years before the Doctor sent that Analysis to Mr Collins that is, some years before Iuly 1669. And I see no reason why the testimony of Dr Barrow should be questioned in this matter. Dr Wallis also who flourished in those days & was inquisitive & skilfull & received copies of my Letters from Mr Oldenburgh in the year 1676, published the same thing [nine years before me] in the Preface to the first Volume of his Works without being then contradicted or blamed , saying that in my Letters of Iune 13 & Octob. 24 1676 I explained to Mr Leibnitz the method found by me ten years before that time or above. And the testimony of these three ancient intelligent & credible < insertion from f 390v > And the testimony of these three ancient knowing & credible witnesses may suffice to excuse me for saying in the Introduction to the Book of Quadratures that I found the method: by degrees in the years 1665 & 1666. < text from f 390r resumes >

If Mr Leibnitz could have made a good objection against the Commercium Epistolicum, he might have done it in a short Letter without writing another book as big. But this book being matter of fact & unanswerable he treated it with opprobious language & avoided answering it by several excuses, & endeavoured to lay it aside by appealing to the judgment of his friend Mr Bernoulli & by writing to his friends at Court, & by running the dispute into a squabble about a Vacuum, & Atoms, & universal gravity, & occult qualities, & Miracles, & the Sensorium of God, & the perfection of the world, & the nature of time & space, & the solving of Problemes, & the Question whether he did not find the Differential Method proprio marte: all which are nothing to the purpose. Mr Iames Gregory after a years study found the method of converging Series proprio marte, but did not claim it because he had notice from England that there was such a general method before he searched for it, & by that notice knew that he was not the first inventor. Gregorius autem, said Mr Collins, Newtonum primum ejus inventorem anticipare haud integrum ducit. The proper Question, is: Who was the first Inventor? Let it be proved that Mr Leibnitz had the Method before the year 1677, & then that he had it before he had any notice of it from England, & then let it be further proved that he had it before the date of my Letter of Decem. 10th 1672, & before the year 1671 in which I wrote a tract upon it & before Iuly 1699 &c; & by these steps if they can be made the Commercium Epistolicum will at length begin to be shaken. For the proof lies upon the friends of Mr Leib.

But whatever is done, I do not think this business of such consequence that I should meddle with it any further. Mr Leibnitz pretended that I

<391r>

Sir

I have viewed the printed papers you left in my hands. The remarks are only upon the Letter of Mr Leibnitz dated 9 Apr. 1716. The Letters to the Comtesse of Kilmansegger & the Postscript to Baron Bothmar are without an answer, nor do I see that they need any. None of the Letters were writ to me & I had not answered any of them had not Abbe Conti pressed me to write an answer to the Postscript of his Letter that both might be shewed to the King. And when Mr Leibnitz sent his Answer open to Paris & it came hither from thence I declined returning an Answer & only drew up remarks to satisfy my friends privately that what he writ was easy to be answered if he had sent his Letter hither directly.

In his Letter to the Countess of Kilmansegger he said that I formerly applauded his series for the circle, & that it was found afterwards that Gregory had found the very same series, but it was late before he knew this: but the truth is that he knew this before he sent the Series to me & ought to have let me known as much. He had before that time received it twice (if not thrice) from England as appears by authentic papers published in the Commercium Epistolicum. page 41, 47, 25.

In the same Letter he saith that in my Principles pag 253, 254 I did not dispute his having found the differential Method by himself. No more did I ever dispute Mr Iames Gregories having found the Method of converging series by himself. He had notice from England that there was such a method & spent a year in searching after it before he found it, but never laid claim to it because he knew that I had it before him. So Collins Epist ad Th. Strode: <u>D. Gregorius autem D. Newtonum primum ejus Inventorem anticipare haud integrum</u> ducit. Vide Commerc. Epist. pag. 29. Mr Leibnitz met with Dr Barrows Lectures at London in October 1676, & Mr Iames Bernulli & the Marquess de l'Hospital have represented that where the Doctor left off Mr Leibnitz proceeded, & the Marquess has told us that the improvement made by Mr Leibnitz to the Doctors methods lay in shewing how to proceed without stopping at fractions & surds. But the Marquess did not then know that Mr Leibnitz had notice of this from me by my Letter to Mr Collins of 10 Decem. 1672 (a copy of which Mr Leibnitz received at Paris in Iuly 1676,) & again by my Letter to Mr Oldenburg of 24 Octob 1676 which he saw at that time in London. When Mr Leibnits published the Elements of the Differential Method, he should have told the world what notices he had received from England about such a method before he found it. <391v> By his concealing what he knew of these notices he put me upon a necessity of making some mention of them in the Book of Principles, & particulary upon representing that I writ to him first about this method, & that I did this ten years before, that is in the year 1676 & that I then concealed it in setting down this sentence enigmatically Data æquatione fluentes quotcunque quantitates involvente invenire fluxiones et vice versa. the first part of which sentence being the first Proposition of the Book of Quadratures verbatim, may suffice to satisfy any man of candor that the Book of Quadratures was then in Manuscript. Whether Mr Leibnitz found the method by himself or not was not the Question but whether he was the first inventor.

In the Postscript of his Letter to the Count de Bothmar he cites a passage out of the Acta eruditorum for Ianuary 1705 & there pretends that the words <u>semperque adhibuit</u> are maliciously interpreted by <u>substituit</u>. For, saith he, those words seen to be put expresly to signify that I used fluxions before the publishing of his calculus. And so they might & yet be truly interpreted by <u>substituit</u>, unless they be put expresly to signify that I used fluxions for the Leibnitian differences before Mr Leibnitz invented the differential method. But saith he, between the words semper adhibuit & substituit there is this difference; the one signifies that Mr Newton always imployed his Method, the other that Pere Fabri formed his in imitation of another In the Introduction to the Book of Quadratures I said that I found the Method by degrees in the years 1665 & 1666: if he did grant that the words <u>semperque adhibuit</u> extend to all times ever since, he has absolved me from plagiary; otherwise not, & it lay upon him to prove me guilty. For his compliment that I found the method proprio marte does not absolve me from the guilt. He that claims an invention

In the beginning of the Paragraph Mr Leibniz is called the Inventor, & thence in drawn this conclusion. Pro differentijs IGITVR Leibnitianis Newtonus adhibet semperque [pro ijsdem differentijs] adhibuit fluxiones, — ijsque tum in suis Principijs Natura Mathematicis tum in alijs postea editis [pro differentijs Leibnitianis] eleganter est usus; QVEMADMODVM et Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianæ methodo substituit. This is certainly the true meaning of the words & therefore they amount to an accusation of plagiary & it lay upon Mr Leibnitz to prove the accusation.

Mr Leibnitz accused the Committee of the R. Society of partiality in omitting every thing which made for him or against me, & offered to prove this by two instances, but failed in both. He accused them also of attaquing his candor by forced interpretations & here called one of their interpretations a malicious one but has misiinterpreted the place himself. And therefore the Commercium Epistolicum remains unshaken. And untill this is answerd there will be no need of saying any thing more about this matter. The Committee did not enter into the Question <392r> whether Mr Leibnitz invented the Method proprio Marte or not. On the contrary they said that they tooke the Question to be not who invented this or that Method but who was the first inventor of the Method. And for proving that I was the first they appealed to ancient letters & papers. Mr Bernoulli answered that in those Letters & Papers there were no prickt letters: which is as much as to say that the Introduction to the book of Quadratures & the Proposition Data æquatione fluentes quotcunque quantitates involvente does not treat of the method of fluxions because there are no prick lletters in them. He said also that prict letters did not appear till the third Volume of Dr Wallis's works came abroad, & so he was mistaken in matter of fact. For the first Proposition of the Book of Quadratures with the solution examples in

first & second fluxions was published almost word for word six years before in the second Volume of the Doctors works. He said also that I did not understand second fluxions, & this is as much as to say that he had not read the first Proposition of the Book of Quadratures. So then the Commercium Epistolicum remains unshaken.

<center><393r></center>

After <u>1717 Iuly</u> to Des Maizeaux Not to Conti {Natsine} p. 111

Sir

I have viewed the printed papers you left in my hands. The Remarks are only upon the Letter of Mr Leibnitz to Mr. l'Abbé Conti dated 9 Apr. 1716. The Letters to the Comtesse of Kilmansegger, & the Postscript of a Letter to Compt Bothmar are without an Answer, & I do not see that they need any. None of his Letters were writ to me, & I had not answered any of them had not M^r l'Abbé Conti pressed me to write an Answer to the Postscript of a Letter to him that both might be shewed to the King. But when I understood that Mr Leibnitz sent all the Letters open to Paris, & only wrote the Remarks his Answer came hither from thence, I declined writing any more Letters, & only wrote the Remarks upon that Answer to satisfy my friends here that what he writ was easy to have been answered if it had come hither directly. The Commercium Epistolicum, notwithstanding any thing which has hitherto been said against it, remains in full force, & while that remains unshaken there is no need of writing any further about these matters.

Mr Leibnitz objected against it that in printing the ancient Letters & papers many things had been omitted which made for him & against me: but in offering twice to prove this by instances, he failed as often.

He objected also that the Committee of the R. Society strained things against him by fals interpretations but he never proved this in any one instanc{e} He said indeed that they had maliciously interpreted the words of the Acta Eruditorum for Ianuary 1705, by saying: <u>Sensus verborum est quod Newtonus fluxiones pro differentijs Leibnitianis substituit</u>: but he has misrepresented the place himself. For the Editors of the Acta in this Paragraph call Mr Leibnitz the INVENTOR, & thence deduce this conclusion. <u>Pro differentijs IGITVR Leibnitianis Newtonus adhibet semperque</u> [pro ijsdem] <u>adhibuit fluxiones — ijsque tum in suis Principijs Naturæ Mathematicis, tum in alijs postea editis</u> [pro differentijs illis] <u>eleganter est usus QVEMADMODVM ET Honoratus Fabrius in sua Synopsi Geometrica motuum progressus Cavallerianæ methodo substituit</u>. This is the interpretation which the Committee put upon the place, & the words <u>igitur</u> & <u>quemadmodum et</u> enforce it.

It has been represented that in the Commercium Epistolicum Mr Leibnitz is complained of as having published my Method as his own; whereas he found the Method by himself. But second inventors have no right. Whether Mr Leibnitz found the Method by himself or not is not the Question. The Committee of the Royal Society did not enter into this Question, but on the contrary said: <u>We take the proper Question to be, not who invented this or that Method but who was the first inventor of the method. And we beleive that those who have reputed Mr Leibnitz the first Inventor, knew little or nothing of his correspondence with Mr Collins & Mr Oldenbourg long before, nor of Mr Newton's having the Method above 15 years before Mr Leibnitz began to publish it in the Acta eruditorum of Leipsic</u>. Here the Committee of the R. Society treat Mr Leibnitz as second Inventor and complain of him only for publishing the Method as his own without making any mention of the correspondence which he had with me & my friends about it long before he published it, & by that concealement claiming the Method as first Inventor.

By that correspondence he had notice in Iune or Iuly 1676 that I had a very general method of Analysis which extended to the abstruser sorts <394r> of Problems in Geometry.✝ < insertion from the top of f 393v > ✝ & that the Method of Tangents of Gregory & Slusius was but a branch or rather a Corollary thereof & that it proceeded without any troublesome calculation & without sticking at surds. < text from f 394r resumes > & that I had it & had interwoven it with the Method of infinite series before I wrote my Letter of 10 Decem. 1672 & being tyred with these speculations had absteined from them five years when I wrote my Letter of 13 Iune 1676. & that The Art of Analysis by these methods became so general as to reach almost all sorts of Problems except some numeral ones like those of Diophantus.

But all this was not yet sufficient to make him understand the differential method. For in his Answer dated 13 Aug. 1676, he wrote back. <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 in the same Letter he placed the perfection of Analysis in Analytical Tables of Tangents & the combinatory art: saying of the one, <u>Nihil est quod norim in tota Analysi momenti majoris</u>; & of the other, <u>Ea vero nihil differt ab Analysi illa suprema, ad cujus intima, quantum judicare possum</u>, Cartesius <u>non pervenit. Est enim ad eam constituendam opus Alphabeto Cogitationum humanarum</u>.

Mr Iames Bernoulli in the Acta Eruditorum for December 1691 & the Marquess de l'Hospital in the Introduction to his Analysis represented that the Differential Method was an improvement of Dr Barrow's method of tangents, & the Marquess represented further that the improvement lay in shewing how to proceed without stopping at surds. But the Marquess did not then know that Mr Leibnitz had notice of this improvement from me before he found it, & the Committee complained of him justly for concealing this & all the foregoing notices.

In October 1676 Mr Leibnitz came to London & there met with Dr Barrows Lectures & with my Letter to Mr Oldenburgh dated 24 Oct. 1676, in which the above mentioned notices were repeated & he was also told that the Tract in which I interwove the two Methods was written five years before, that is, in the year 1671. And that before the plague which raged in the year 1665 forced me from the Vniversity, I had found the method of converging Series (including redactions by the binomial Rule & by division & extraction of roots both simple & affected,) so as to be able to deduce the areas of all figures & the lengths of all curve lines from their Abscissas & Ordinates & on the contrary to deduce the Abscissas & Ordinates & any other right lines from the areas & lengths of the Curves. He was told also that when Mercators Logarithmotechnia came abroad, Dr Barrow sent Mr Collins a Compendium of the Method of these Series, meaning the <u>Analysis per æquationes numero terminorum infinitas</u>. And at the same time he consulted Mr Collins to see what he could meet with in his hands of mine & Gregories Letters & Papers, & there saw a great part of our correspondence. And in his way from London to Hannover he was meditating how to improve the Method of Tangents of Slusius For in a Letter to Mr Oldenburgh from Amsterdam dated $\frac{18}{28}$ Novem. 1676 he wrote: Methodus Tangentium a Slusio publicata nondum rei fastigium tenet. Potest aliquid amplius præstari in eo genere, quod maximi foret usus ad omnis generis Problemata — Nimirum posset brevis quædam calculari circa Tangentes Tabula &c. He had not therefore yet found the right improvement, but at length he found it proprio Marte. For the next year in a Letter to Mr Oldenburgh dated 21 Iune 1677 he wrote: Clarissimi Slusij methodum tangentium nondum esse absolutam celeberrimo Newtono assentior. Et jam a multo tempore rem tangentium generalius <395r> tractavi, scilicet per differentias Ordinatarum. And then he set down Dr Barrows method of Tangents with a new notation & shewed how it might be improved so as to give the method of Tangents of Slusius, & to proceed without stopping at surds; & then added: Arbitror quæ celare voluit Newtonus de Tangentibus ducendis, ab his non abludere. Quod addit, ex hoc eodem fundamento Quadraturas quoque reddi faciliores, me in sententia hac confirmat.

<center><397r></center>

<center>**After Iuly 1717 2**</center>

Sir

I have viewed the printed papers you left in my hands. The Remarks are only upon the Letter of Mr Leibnitz to Abbé Conti dated 9 Apr. 1716. The Letters to the Comtesse of Kilmansegger & the Postscript of a Letter to Baron Bothmar are without an Answer, & I do not see that they need any. None of his Letters were writ to me, & I had not answered any of them had not M^r l'Abbé Conti pressed me to write an answer to the Postscript of his Letter that both might be shewed to the King. But when Mr Leibnitz sent his Answer to mine open to Paris & it came hither from thence I declined returning an Answer to it & only drew up the Remarks to satisfy my friends that what he writ was easy to have been answered, if it had come hither directly. The Commercium Epistolicum notwithstanding any thing which has hitherto been written against me remains in full force, & while that remains unshaken there is no need of writing any thing further.

. Mr Leibnitz indeed objected against it that in printing the ancient Letters many things had been omitted which made for him & against me: & offered to prove this by two instances, but failed in both He objected also that the Committee of the R. Society strained things against him by false interpretations & particularly that they had maliciously interpreted the words of the Acta Eruditorum for Ianuary 1705, by saying: <u>Sensus verborum est quod Newtonus fluxiones differentijs Leibnitianis SVSTITVIT</u>. but he has misinterpreted the place himself For The Editors of the Acta in this Paragraph call Mr Leibnitz the $_{Inventor}$ | ^{INVENTOR} & thence draw this conclusion Pro differentijs IGITVR Leibnitianis Newtonus adhibet semperque [pro differentijs illis] adhibuit fluxiones, ... ijsque tum in suis Principijs Naturæ Mathematicis, tum in alijs postea editis [pro differentijs Leibnitianis] eleganter est usus QVEMADMODVM ET Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianæ methodo substituit. The word <u>igitur</u> & the words quemadmodum et (the force of which Mr Leibnitz omits) enforce the interpretation here set down, which is the interpretation put upon this Paragraph in the Commercium Epistolicum. Mr I. Bernoulli objected that when the ancient Letters & Papers were first written I did not so much as dream of the method of fluxions because there are no prickt letters in them : which is much as to say that when I explained the method of fluxions in the Introduction to the Book of Quadratures I did not so much as dream of the Method of fluxions because there are no prickt letters in it, & when I set down the first Proposition of the Book of Quadratures enigmatically in my Letter of 24 Octob. 1676 as a tes{ti}mony that the method was founded in the first Proposition of this Book then in Manuscript, I did not so much as dream of the Method of fluxions because there are no prickt letters in the Ænigma. ③ Or that in my Letter of 24 Octob 1676 I did not copy from the Book of Quadratures then in Manuscript the Ordinates of curves which may be compared with the Conic Sections because there are no prickt letters in that Letter. ② Or that my Letter to Mr Collins dated 8 Novem. 1676, & published by Mr Iones had no relation to the 7th 8th 9th & 10th Propositions of the Book of Quadratures then in Manuscript because there are no prickt letters in that Letter. ① Or that my saying in my Letter of 24 Octob. 1676 that the series for squaring of Curves which in some cases breaks off & becoms <397v> finite was invented by the Method whose foundation was comprehended in the said Ænigma that is by the method founded in the first Proposition of the book of Quadratures, was not invented by the method founded in the first Proposition & in five next Propositions of the Book of Q. then in Manuscript because there are no prickt letters in that Letter. ④ Or that the Marquess de l'Hospital was mistaken in saying that the Book of Principles is full of this Calculus, because there are no prickt letters in it. Or that Mr Leibnitz himself was mistaken when he wrote to me, 7 March 1693, Mirifice ampliaveras Geometriam tuis seriebus, sed Edito Principiorum opere ostendisti patere tibi etiam quæ Analysi receptæ non subsunt. Conatus sum Ego quoque notis commodis adhibitis quæ differentias et summas exhibent Geometriam illam quam transcendentem appello Analysi quodammodo subjicere. M^r Io. Bernoulli objected further in these words: Prima vice hæ literæ punctatæ comparuerunt in tertio volumine Operum Wallisij, multis annis postquam Calculus differentialis jam ubique locorum invaluisset , – – Et apparit Newtono rectam methodum differentiandi differentialia non innotuisse longo tempore posquam alijs fuisset familiaris. But he erred in matter of fact. For in the year 1692 at the request of Dr Wallis I sent him the first Proposition of the book of Quadraturs with its solutions extending to second third & fourth fluxions &c & examples in first & second fluxions, copied almost verbatim from the book of Quadratures then in Manuscript & the Doctor that year in the second Volume of his works printed what I sent him, & the book came abroad the next year when the Differential method was but beginning to be in vogue & three years before the method of finding second third & fourth differences was published by the Marquess de l'Hospital. So then the Commercium Epistolicum stands unshaken against all Objections which have hitherto been made. Mr Leibnitz instead of answering it used his utmost endeavour to get away from it & to run the dispute into a squabble about a Vacuum & Atoms & universal gravity & Occult Qualities & miracles & the Sensorium of God & the absolute perfection of the world & the definitions of time & space & the solving of Problemes & the Question whether he did not find the method proprio Marte: all which are nothing to the purpose. If he could have made a material objection against the book he might have made it in a short Letter without writing another Book as big: but for want of such an Objection, the Book remains untoucht.

Whether Mr Leibnitz found the Method by himself or not was not the Question. The Committee of the R. Society did not Enter into this Question but on the contrary said: <u>We take the proper Question to be not who invented this or that Method but who was the first Inventor of the Method. And we beleive that those who have reputed Mr Leibnitz the first Inventor knew little or nothing of his Correspondence with Mr Collins &</u>

M<u>ͬ Oldenberg long before nor of Mͬ Newton's having the Method above 15 years before Mͬ Leibnitz began to publish it in the Acta Eruditorum of Leipsic</u>. It was the duty of Mͬ Leibnitz in point of candor, when he published the Elements of the differential method, instead of making them his own to have given the world some account of his former correspondence with Mͬ Collins & Mͬ Oldenburg in relation this matter. And the Committee complain of him for concealing this. By that correspondence he had notice Here the Committee complain of Mͬ Leibnitz for concealing his correspondence with me & my friends about this method long before he began to publish it. By that correspondence he had notice that I had a very general Method of Analysis & that this Analysis procee <398r> ded without stopping at surds & that it was an improvement of the method of tangents of Gregory & Slusius, & that the Method of Tangents of Gregory was an improvement of the Method of tangents of Barrow, & he knew that the method which in his Letter of 21 Iune 1677 he sent back to me was an improvement of Dͬ Barrows method of tangents so as to give the method of Gregory & Slusius & to proceed without stopping at surds. He had notice also that my Method gave me Theorems for squaring of curvilinear figures by converging series which in some cases brake off off & became finite & one of those Theorems was sent to him; & that it gave me also Theorems for comparing curvilinear figures with the Conic Sections, & that my Analysis by the help of Converging æquations was so general as to extend to almost all Problems except some numeral ones like those of Diophantus, & that I had this Method when I wrote my Letter of 10 Decem. 1672, & wrote a Tract upon it in 1670. And the Committee of the R. Society complained of him for concealing his knowledge of all this & making himself the first inventor by means of that concealment & putting upon the proof of what he ought to have acknowledged of his own accord **{illeg}** in point of candor. And their complaint was just

When Mͬ Leibnitz came to London the second time he met with Dͬ Barrows Lectures. And Mͬ Iames Bernoulli in the Acta Erud. for Dec. 1691 & the Marquess de l'Hospital in the Introduction to his Analysis represented that the Differential Method was an improvement of Dͬ Barrows method of Tangents, & the Marquess represented further that the improvement lay in shewing how to proceed without stopping at surds: but the Marquess did not then know that Mͬ Leibnitz had notice of this improvement from mee before he found it. & the Committe complained of him justly for concealing it. The Marquess said also that Mͬ L. [in the Iournal des Scavans du 30 Aust 1694 Leibnitz had done me justice in acknowledging that I had found the method of fluxions proprio marte, but he would not have said so if he had known that Mͬ Leibnitz had concealed the notices which I gave him of my having this method before he found it.

<div align="center"><398v></div>

But Mͬ Leibnitz pretends that the words semperque adhibuit imply that I always used the method without substituting fluxions for differences.

It has been said that Mͬ Leibnitz found the method by himself. But this is not the Question. The Committee of the R. S. did not enter into this Question, But On the contrary said

And yet in the year 1684 he published it as his own in the year 1686 without making any mention of the correspondence which had been between us about it eight years before, & thereby he put me upon a necessity of mentioning that correspondence in the Scholium upon the second Lemma of the second book of Principles, & gave just occasion to the Committee of the R. S. to take notice of his silence about that correspondence.

Mͬ Ia. Bernoulli — before he found it

Mͬ Collins in the year 1669 sent one of my series to Mͬ Ia. Gregory with notice that I had a general method of squaring figures by such series; & Mͬ Gregory after a years study found out the method proprio Marte, but never laid claim to it because by that notice he knew that I had it before him & without that notice he had not found it nor serched after it Gregorius autem Newtonum primum ejus Inventorem anticipare haud integrum ducit. So Collins to Mͬ Strode 26 Iuly 1672. Mͬ Leibnitz had much more light into the Method of fluxions but yet laid claim to it because he found it proprio Marte.

It has been further objected against the Commerc. Epist.

– – – – without writing another book as big.

He has complained indeed that the book attaques his candor in not allowing that he found the method proprio marte. But this was not the Question. The Committee of the R. Society did not enter into this question but on the contrary said: We take the proper Question to be not who invented this or that Method but who was the first inventor of the Method. And we beleive – – – – in the Acta Eruditorum of Leipsic. It was the duty – – – – – making himself the first Inventor. And their complaint was just.

When Mr Leibnitz came to London the second time – – – – of this improvement from me. Mr Leibnitz after all the notices which he had received from England concerning this Method, might find out the rest by himself: for I told him in my Letter of 24 Octob 1676 that the foundation of the Method was obvious: but his finding it after me gives him no right. And his concealing all the notices which he had from England of my having the method before him in order to give himself a right, put me upon writing the Scholium upon the second Lemma of the second Book of Principles to give the world notice of what I wrote to him about this method before he found it. So then the complaint of the Committee of the R. Society was just & the Commercium Epistolicum remains firm & untoucht.

Mr Collins sent Mr Iames Gregory one of my series with notice that I had a general method of squaring all figures all figures by such Series. And after this notice Mr Gregory spent a year in searching after this method & at length found it proprio Marte but because by the notice he knew that I had it before him he never laid claim to it. Gregorius Acri Studio, said Mr Collins, eandem acquisivit multumque in ea enodanda desudavit. – Gregorius autem Newtonum, primum ejus inventorem anticipare haud integrum ducit. Mr Leibnitz had much more light into the method of Fluxions, & after his being in England the second time had eight months time to find it before he wrote his Letter of 21 Iune 1677, & has been complained of not because after the notices which he had from England he did not find the remainder proprio Marte, but because Newtonum primum ejus inventorem anticipare integrum duxit.

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Le 9e de Decembre 1720[1] From M. Des Maizeau[2]

M^r le D^r. Clarke ayant publié en 1717 un Recueil contenant quelques Ecrite de Mr Leibniz sur les Principles de la Philosophie & de la Religion naturelle, avec ses Responses; je conseillai au sieur du sauzet, Libraire de la Haye, de les reimprimer: & il les annonça dans ses Novelles Literaires du 27 Mars 1717 p. 208. Ie lui proposas aussi d'y joindre une Traduction François des Recherches sur la Liberté de l'Homme par Mr Collins et des Rémarques de Mr Clarke sur ces Recherches.

Dans ce temps-là M^r le Chavalier Newton fit imprimer une Version Francois de deux Ecrits sur l'invention des Fluxions ou du Calcul differentiel, qu'il avoit oposes a deux Lettres de Mr Lebniz adressees a M^r l'Abbé Conti: Ces 4 petites pieces qui avoient deja eté publieè ici, me parurent tres proper a etre ajouteés aux autres. Ie ne doutai point qu'elles ne fussent bien recues dans le Pys etrangers, ou l'on n'avoit que des idées confuses de celle dispute. M^r l'Abbé Conti gouta fort ce dessein, & pour le rendre plus utile il me donna quelques Lettres de Mr Leibnitz qui avoient du rapport avec celles dout j'ai parlé, & pouvrient servir d'eclarissement; Vne entr'autres ecrit à Madam de Kielmansegg, ou il faisoit l' histoire de son demêtre avec Mr Newton; & une autre a Monsr le Comte de Bothmer qui rouloit sur la meme matiere. Il avoit deja fourni des Copies de ces mêmes Lettres a Mr Newton.

Monsr l'Abbé etant ensuit allé à Paris, au commencement de l'Annee 1718, il m'envoye par M^r l'Abbé Greco plusieurs autres Pieces de Mr Leibniz pour augmenter mon Recueil; savois, des dissertations & des Lettres a Mr Remond. Ie l'en remerciai par la Lettre, qui est imprimeé dans le II Tome pag 362 & suiv.

Aussi tot que le 4 primieres feuilles de ce second Tome furent imprimimeè, le Libraire me les envoye le 14 Iune 1718. Ie les communiquai à Mr Newton, a fin qu'il fut à tems de faire corriger les fautes qui avoient pû se glisser dans les deux escrits qui etoient de lui; & pour savoir en meme tems ce qu'il persoit sur

l'assemblage des pieces contenues dans ces quatre feuilles. En general, il n' approva pas le maniere dout on les avoit placeés: il auroit souhaite que les deux Lettres de Mr Leibniz qui le regardoient, & ses deux Responses, eussent ete misess ensemble, & que la suite n en eut éte interompuè par les Lettres à Madam de Kielmansegg & a M^r le Comte de Bothmar. Il ne parut pas meme fort content de l'impression de ces dernier Lettres, par ce qu'il y trouvoit certain facts dont il ne convenoit point, & qu'il ne jugeoit pourtant pas a propos de refuter. C'est ce qui m'a oblige d'en advertis le Lectures dans la Preface pag. LXVI.

Divers incidents ont fait trainer l'impression de se <u>Recueil</u>, qui n'a ete fini qu'au Mois de May de cette année.

Au reste, la petite Lettre du 7 de Iune 1713 inserée dans la Lettre à Madam de Kielmansegg pag. 36, & attribuée à Mr Bernoulli; avoit eté publieé sous le nom de Mr Bernoulli dans les Novellles Literairs de 28 Decembre 1715 comme je l'ai remarqué à la Marge: et il ne faut que lire ce qui la precede dans ces Nouvelles pour se convaincre que Mr Leibniz l'avoit envoyeé au Iournaliste. Mr Leibniz atribue encore cette Lettre a Mr Bernoulli dans cette qu'il a dressa à M^r l'Abbe Conti le 9 d'Avril 1716 pag 51, pour repondre à la Lettre de Mr Newton. <u>On connoit assez</u> dit il parlant de la feuille volante publieé en Latin, <u>le nom & le lieu de l'Auteur de la Lettre y insereé d'un excellent Mathematicien que j'avoit prié de dire son sentiment sur le Commercium.</u> Ainsi nous n'eumes pas le moindre souçon, M^r l'Abbé Conti & moi, que cette Lettre ne fut effectivement de lui. Cependant ayant sû avant que d'envoyer ma Preface en Hollande, que Mr Bernoulli la desavouoit; je crûs que l'equité demandoit que j'en avertis le Public, comme j'ai fait pag. XLVIII.

<401r>

{Kuy} to Maizeau

Sir

You know that when M^r l'Abbé Conti had received a Letter from Mr Leibnitz with a large Postscript against me full of accusations forreign to the Question, & the Postscript was shewed to the King, & I was pressed for an answer to be also shewed to his Majesty, & the same was afterwards sent to Mr Leibnitz: he sent it with his Answer to Paris declining to make good his charge & pretending that I was the Aggressor, & saying that he sent those Letters to Paris that he might have neutral & intelligent witnesses of what passed between us. I looked upon this as an indirect practise & forbore writing an Answer in the form of a Letter to be sent to him, & only wrote some Observations upon his Letter to satisfy my friends here that it was easy to have answered him had I thought fit to let him go on with his politicks. As soon as I heard that he was dead I caused the Letters & Observations to be printed least they should at any time come abroad imperfectly in France. You are now upon a designe of reprinting them with some other Letters written at the same time, whose Originals have been left in your hands for that purpose by M^r l'Abbé Conti for making that Controversy complete & I see no necessity of adding any thing more to what has been said, especially now Mr Leibnitz is dead.

When he wrote his Letter of 27 Aug. 1676 he placed the perfection of Analysis not in the Differential Calculus as he did afterwards but in a Method founded on Analytical Tables of Tangents & the Combinatory Art. <u>Nihil est</u>, saith he, <u>quod norim in tota Analysi momenti majoris</u>. And a little after: <u>Ea vero non differt ab Analysi illa SVPREMA ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum.</u> When he was in London the second time (which was in October 1676,) he met with Dr Barrow's Lectures & saw my Letter of October 1676,) he met with Dr Barrow's Lectures & saw my Letter of Octob. 24, 1676, & three months before received a copy of my Letter to Mr Collins dated 10 Decem. 1672. Mr Iames Bernoulli in the Acta Eruditorum for Ianuary 1691 pag. 14 said that the Calculus of Mr Leibnitz was founded in that of Dr Barrow & differed not from it except in the notation of Differentials & some compendium of operation. And the Marquess de l'Hospital in the Preface to his Analysis of infinite petits published A.C. 1696 represented that the improvement which Mr Leibnitz made to Dr Barrows Analysis consisted in excluding fractions & surds: but he did not then know that Mr Leibnitz had notice of this improvement from me by my two Letters above-mentioned, dated 10 Decem. 1672 & 24 Octob. 1676.

I wrote the Book of quadratures in the year 1676, except the Introduction & Conclusion; extracting most of it out of old Papers, & when I had <402r> finished it & the 7th 8th 9th & 10th Propositions with their Corollaries were fresh in memory, I wrote upon them to Mr Collins that Letter which was dated 8 Novem. 1676 & published by Mr Iones. The Tables at the end of the 10th Proposition for squaring of some Curves & comparing others with the Conic Sections, were invented by the inverse method of fluxions before the year 1671 as may be understood by my Letter of 24 Octob. 1676. And in the same Letter where. I represented that the general Theoremes there mentioned for squaring of Curves were invented by the Method of fluxions, I meant the direct & inverse method described in the first six Propositions of the Book of Quadratures. for I know of no other Method by which those Theoremes could be invented. At the request of Dr Wallis I sent to him in two Letters dated 27 Aug. & 17 Sept. 1692, the first Proposition of the Book of Quadratures copied almost verbatim from the Book & the Method of extracting fluents out of equations involving fluxions mentioned in my Letter of 24 Octob. 1676 & copied from an older Paper, & the Doctor printed them both the same year (vizt A.C. 1692) in the second Volume of his works, pag. 391, 392, 393, 394, 395, 396, subjoyning an explication of the method of fluxions direct & inverse comprehended in the sentence,

<403r>

pag. 1. lin. 47 – sections. And in my said Letter to Mr Oldenburg of 24 Octob 1676 I set down the first Proposition of this Book with its invese verbatim in this sentence <u>Data æquation quotcunque fluentes quantitas involvente invenire fluxiones & vice versa</u> & represented it the foundation of the method then spoken of. & then I added the fundamento conatus sum etiam reddere speculationes de quadratura Curvarum simpliciores; pervenique ad Theoremata quædam generalia. And ... I set down a Theorem for squaring Curves whose ordinates were dignities of Binomials & illustrated it with examples & then added that I had other such like Theorems for Trinomials. And all this implies the knowledge of the method of fluxions so far as it is conteined in the first six Propositions of the Book of Quadratures. In the same Letter I said also At quando hujusmodi Curva aliqua non potest Geometrice vel saltem cum alijs Figuris simplicissimis quibuscum potest comparari — et dudum retuli. And then I set down the Ordinates of the Curves in the latter part of the Table for comparing Curves with the Conic Sections set down in the 10th Proposition of this book which is a proof that I had that Table some years before I wrote that Letter. And in my Letter to Mr Collins dated 8 Novem. 1676

Pag. lin 11

About three years after the writing of these Letters by the help of this method of Quadratures I found the Demonstration of Keplers Propositions that the Planets revolve in Ellipses describing with a Radius drawn to the sun in the lower focus of the Ellipsis, areas proportional to the lines And in the year 1686 I set down the Elements of the Method of Fluxions & moments, & demonstra{ted} them synthetically in the second Lemma of the second Book of Principles in order to make use of the Lemma in demonstrating some following Propositions. And because Mr Leibnitz had published those Elements in the Acta Eruditorum two years before without taking any notice of the corresondence which had passed between him & me by means of Mr Oldenburg ten years before: I added a Scholium to that Lemma not to give away the Lemma but to put Mr Leibnitz in mind of making a candid & publick acknowledgment of that correspondence And in demonstrating the XIV Prop. of that second Book I considered the differentia momentorum arearum or momentum differentiæ, & therefore when I wrote this Book I was no stranger to second moments or differences. ‡ < insertion from the top of f 403v > ‡ After this Book came abroad, Mr Leibnitz acknowledged that I w**{illeg}** it by the**{illeg}** < text from f 403r resumes > After this, in the year 1691 the Book of Quadratures was in the hands of Dr Hally & Mr Ralphson at Cambridge as one of them attested in print before his death & the other still attests. And in the year 1692 – – – – – the best [but the Book of Quadratures continued in MS till the year 1704. [In the meane time Dr Wallis in the Preface to the first Volume of his works (which came abroad after the second & was published in April 1695) inserted the following Paragraph. Quæ in**{illeg}** secundo volumine habentur – – – – – – ab ipso excogitatam. And so soon as this vo{lume} came abroad he sent to me the following Letter]

This year I received from Mr Leibnitz the following Letter dated 17 Mart. 1693 st. n. Quantum tibi scientiam rerum mathematicarum totiusque naturæ scientiam debere arbitrem occasione data etiam publice sum professus. Mirifice ampliaveras Geometriam tuis seriebus, sed edito Principiorum opere ostendisti patere Tibi etiam quæ analysi receptæ non subsunt. Conatus sum ego quoque notis commodis adhibitis quæ differentias & summas exhibent, Geometriam illam quam Transcendentem appello, Analysi quodammodo subjicere, nec res male processit. &c Here Mr Leibnitz acknowledged himself convinced by the Book of Principles that I had this Method & gave the preference therein to me

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to Des Maizea{u} {an Me Copy}

Sir

You know that when M^r l'Abbé Conti had received a Letter from Mr Leibnitz with a large Postscript against me full of accusations ⊙⊙ forreign to the question & the Postscrip was shewed to the King, & I was pressed to write an answer to be also shewed to the King, & the same was afterwards sent to Mr Leibnitz he sent it with his Answer to Paris declining to make good his charge & pretending that I was the Aggressor & saying that he sent those Letters to Paris that he might have impartial witnesses of what passed between us! I looked upon this as an indirect practise & forbore writing an Answer in the form of a Letter to be sent to him & only wrote some Observations upon his Letter to satisfy my friends here that it was easy to have answer him, had I thought fit to let him go on with his polyticks. As soon as I heard that he was dead I caused the Letters & Observations to be printed least they should at any time hereafter come abroad imperfectly in France. You are now upon a designe of reprinting them with some other Letters whose originals have been left in your hands for that purpose by M^r L'Abbé Conti, but I see no reason why I should meddle with this dispute any furth**{er}**, especially now Mr Leibnitz is dead. For I have always indoustriously avoided disputes.

② I wrote the Book of Quadratures in the year 1676, except the Introduction & Conclusion; extracting most of it out of old Papers, when I had finished the seventh eighth ninth & tenth Propositions with their Corollaries & they were fresh in memory, I wrote upon them to Mr Collins that Letter which was dated 8 November 1676 & published by Mr Iones. The Tables at the end of the tenth Proposition for squaring of some Curves & comparing others with the Conic Sections, were invented by the Inverse Method of Fluxions before the year 1671 as may be understood by my Letter of 24 Octob. 1676. And in the same Letter where I represented that the general Theoremes there mentioned for squaring of Curves were invented by the Method of fluxions, I meant the direct & inverse Method described in the first six Propositions of the Book of Quadratures: for I know of no other Method by which those Theoremes could be invented. At the request of Dr Wallis I sent to him, in two Letters dated 27 Aug. & 17 Sept 1692, the first Proposition of the Book of Quadratures copied almost verbatim from the Book, & the Method of extracting fluents out of Equations involving fluxions mentioned in my Letter of 24 Octob 1676 & copied from an older Paper, & the Doctor printed them both the same year (vizt A.C. 1692) in the second Volume of his works pag. 391, 392, 393, 394, 395, 396, subjoyning an explication of the method of **{fl}**uxions direct & inverse comprehended in the sentence <u>Data</u> <405r> <u>æquatione fluentes quotcunque quantitates involvente, invenire fluxiones, et vice versa</u>. This Volume came abroad in the year 1693 two years before the first Volume. And this is a demonstration that in those days I used prickt letters & understood second & third fluxions. When I considered only first fluxions I seldome used letters with pricks, but when I considered also second third & fourth fluxions I distinguished them by Letters with one two three or more pricks. And for fluents I put the fluxion either included in a square or with a square prefixed or with an oblique line upon it. **✝** < insertion from f 404v > **✝** And these notations are the best yet extant, but were not known to the Marquess de l'Hospital when he recommended the differential notation, nor are necessary to the Method. < text from f 405r resumes >

③ By the inverse method of fluxions I found in the year 1677 the Demonstration of Keplers Astronomical Proposition vizt that the Planets move in Ellipses, which is the Eleventh Propopsition of the Book of Principles; & in the year 1683 at the importunity of Dr Hally I resumed the consideration thereof & added some more Propositions about the motions of the heavenly bodies which were by him communicated to the

R. Society & entred in their Letter-Book the winter following, & upon their request that those things might be published, I wrote the Book of Principles in the years 1684, 1685, 1686, & in writing it, made much use of the method of fluxions direct & inverse, but did not set down the calculations in the Book it self because the book was written by the method of Composition, as all Geometry ought to be. [And ever since I wrote that Book I have been forgetting the Methods by which I wrote it.]

If it be asked why I did not publish the Book of Quadratures before the year 1704, I answer; For the same reason that I did not publish the Theory of colours before that year. I found that Theory in the beginning of the year 1666, & in the year 1671 was upon a designe of publishing it together with the Method of Series & Fluxions, but the next year for a reason given in my Letter of 24 Octob. 1676 I first suspended & then left off my designe of publishing them till the year 1704. And for the same reason I I am very averse fro meddling with these matters any further.

✝ < insertion from f 404v > ✝ ① When Mr Leibnitz wrote his Letter of 27 Aug. 1676 he placed the perfection of Analysis in a Method founded on Analytical Tables of Tangents & the Combinatory Art. <u>Nihil est</u>, saith he, <u>quod morim in tota Analysi momenti majoris</u>. And a little after: <u>Ea vero non differt ab Analysi illa SVPREMA ad cujus intima, Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum</u>. Would he have said this if he had then understood the differential calculus? When he was in London the second time < text from f 405r resumes > When Mr Leibnitz was in London the second time (which was in October 1676,) he met with Dr Barrows Lectures & saw my Letter of 24 Octob. 1676 & three months before received a copy of my Letter to Mr Collins dated 10 Decem 1672, Mr Iames Bernoulli in the Acta Eruditorum for Ianuary 1691 pag 14 said that the Calculus of Mr Leibnitz was founded in that of Dr Barrow & differed not from it except in the notation of differentials & some compendium of operation. And the Marquess de l'Hospital in the Preface to his Analysis des infinitement petits published A.C. 1696 represented that the improvement which Mr Leibnitz made to Dr Barrows Analysis consisted in excluding fractions & surds, but he did not then know that Mr Leibnitz had notice of this improvement from me by my two Letters above mentioned..

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I have read over the printed Letters which you have put into my hands, & since Seignior l'Abby Conti found means to engage me in writing part of them, & Mr Leibnitz in his Letter to Madam la Comtess de Kilmansegger pag. 34 & 35 has told his own story at large I give you leave to publish also what I take to be matter of fact.

Mr Leibnitz was in London in the beginning of the year 1673 & went thence in or about March to Paris carrying Mercators Logarithmotechnia along with him. At Paris he kept a correspondence with Mr Oldenburg about Arithmetical matters till Iune being not yet acquainted with the higher Geometry. [The Horologium oscillatorium of Mr Huygens was published in April 1673 & Mr Leibnitz] but about that time he began to study it being entred into it by Mr Huygens, & begining with his Horologium oscillatorium which was published in April 1673. And the next year he renewed his correspondence with Mr Oldenburg by two letters dated 15 Iuly & 26 Octob. representing that as the Lord Brounker & Mr Mercator had found an infinite series of rational numbers equal to the area of the Hyperbola so he had done the like for the circle & added that by the same method the arch of a circle might be found whose sine was given tho the proportion of the arch to the whole circumference was not known. Thereupon Mr Oldenburg in letter dated 15 Apr. 1675 sent to him from Mr Collins several series invented by me & Gregory four of which were for finding the Arc whose sine or tangent was given & the sine or tangent whose Arc was given. And Mr Leibnitz in his Answer dated 20 May 1675. acknowledged the receipt of this Letter. And the next year in a Letter dated May 12 he commended the two series above mentioned for finding the arc whose sine was given & the sine whose arc was given as very ingenious especially the latter which had a singular elegance & desired Mr Oldenburg to procure from Mr Collins the Demonstration thereof, that is the Method of finding it. And in recompence for the same, he promised to send his own meditations upon the same subject, the demonstration whereof he was

then polishing for that purpose. But Mr Oldenburg & Mr Collins wrote earnestly to me to send to him my method & after much sollicitation I wrote my Letter of 13 Iune 1676

Mr Gregory died in the end of the year 1675, & at the request of Mr Leibnitz Mr Collins collected his Letters & Papers & Mr Oldenburg sent the Collection to Paris at the same time with my Letter. In this Collection was a Copy of a Letter of Mr Gregory to Mr Collins dated 15 Feb. $16\frac{70}{71}$ in which the series above mentioned for finding any arc of a circle whose tangent is given. There was also a copy of my Letter to Mr Collins dated 10 Decem. 1672 in which I represented that the method of tangents of Slusius & Gregory seemed to be the same with mine & that it was but a branch or corollary of my general method which extended to the abstruser sorts of Problems & stuck not at surds & which I had interwoven with my method of infinite Series, meaning in a Tract which I had written upon this subject the year before vizt A.C. 1671. In the same Collection was also a Letter of Mr Gregory to Mr Collins dated 5 Sept. 1670 in which Mr Gregory represented that his method of <406v> Tangents was deduced from that of Dr Barrow.

Mr Leibnitz wrote back to Mr Leibnitz Aug. 27. 1676 & in recompence for my method of Series sent me the Series of Gregory for finding the Arc whose tangent was given, without letting me know that he had received it twice from England. And I looked upon it as invented by Mr Leibnitz till some of the Committee appointed by the R. Society to search their Archives found the original Letters by which it appeared that the series was Gregories. And by this Letter dated Aug. 27 & one of Mr Tschurnhause dated 1 Sept 1676, it appears that the aforesaid Collection came to their hands.

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I have read over the printed Letters which you have put into my hands. Mr Leibnitz in his Letter to Madam la Comtesse de Kilmansegger pag 34 & 35 transcribes the Scholium upon the 2^d Lemma of the second Book of my Principles & then adds: <u>Ainsi, Mr Newton ne me contesta point d'avoir trouvé le chose de mon chef</u>. And by this argument he has constantly pretended that in this Scholium I granted him the method. But whether he found the method by himself or not was not the Question. The true Question was whether he was the first inventor or not, & this Question he has constantly endeavoured to avoid chusing rather to put the issue upon the Question whether he invented it by himself or not, as if it must be his if he invented it by himself though he was not the first inventor. He has often allowed that I invented the method by my self: but he never would allow that I was the first inventor. And the designe of that Scholium was to prove that I was the first that discovered that I had the Method. And it proves it by this argument, that I wrote of the Method to Mr Leibnitz before he wrote of it to me in the year 1677. I represented in my Letters that I had written a Tract of it in the year 1671 Mr Leibnitz never did pretend that he had it before the year 1676. And there are very strong arguments to prove that he had it not when he wrote his Letter of 27 Aug. 1676. For in that Letter he mentions a method by Analytical Tables & the Combinatory Art & saith of it <u>Nihil est quod norim in tota Analysi momenti majoris – – – Ea vero nihil differt ab Analysi illa suprema ad cujus intima quantum judicare possum Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum</u>. This was therefore at that time the top of his skill in Analysis. And in a Letter to Mr Oldenburg dated at Amsterdam Novem. $\frac{18}{28}$ 1676 he wrote that by this method the Method of Tangents of Slusius was to be perfected. But after he had found the Differential Method he changed his language & wrote in his Letter of 21 Iune 1677 that it was the Differential Calculus [was the method] by which the Method of Tangents of Slusius was to be perfected. In his journey therefore from

[Now of the Committee of the R. S. have represented that besides my two Letters dated 13 Iune & 24 Octob. 1676 Mr Leibnitz received also the same year in Iuly a copy of my Letter to Mr Collins dated 10 Decem. 1672, & that in this Letter the Method was sufficiently described to any intelligent person,] London to Hannover, when he had newly received my Letter of 13 Iun 1676 & a copy of my Letter of 10 Decem 1672 & of Gregories of 5 Sept 1670 & seen my Letter of 24 Octob 1676 & what he seems to have been upon the enquiry after the Method by which the method of Tangents of Gregory & Slusius might be perfected, & to have found it in the end of that year or beginning of the <407r> year following. But whether he found the method by himself or not is a question which doth not concern me. He knew by my Letter of 24 Octob. 1676

that I had the method five years before that time or above & it appears by his own confession that he had it not before the year 1676, & by his Letter of 27 August 1676 that he had it not when he wrote that Letter.

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Epistola septimo Iunij 1713 data, quæ in Epistola Dⁿⁱ Leibnitij ad D^{nam} Kilmansegg Gallice versa legitur, impressa fuit in Belgio in <u>Novelles Litterairs</u> 28 Decem. 1715, pag. 414, cum hoc titulo: <u>Lettre de M. Iean Bernoulli de Bále du 7 de Iuin 1713</u>. Epistolæ D. Leibnitij ad Dominam Kilmansegg & D. Baronem Bothmar missæ lucem non viderunt nisi post annos quatuor, & nihil amplius continent ad D. I. Bernouli spectans quam quod in Novelles Literairs impressum fuerat. ✝ < insertion from the bottom of the page > ✝ Mense Septembri vel Octobri anni 1716 D. Des-Maiseaux accepit a D. Leibnitio Exemplar disputationis inter ipsum et D. Clark. Et mensibus Aprili & Iulio anni 1718 Accepit etiam a D. l'Abbé Conti , epistolas D. Leibnitij ad Dominam Kilmanseg, et D. Baronem Bothmar, aliasque nonnullas eo fine ut Epistolarum et scriptorum D. Leibnitij Collectionem aliquam in Hollandia imprimi curaret. Hæc omnia me latebant donec schedæ aliquot hujus Collectionis impressæ et ex Hollandia missæ sunt et mihi ostensæ. Id quod fiebat Mense Iulio vel Augusto anni 1718. Et in his schedis impressæ extabant Epistolæ illæ duæ ad D: Kilmanseg & D. Bothmar missæ. Anno vero sequente accepi a te Epistolam D. I Bernoulli datam 5 Iulij 1719 & post alios novem menses Collectio illa lucem vidit. Amicis m

Amicis meis dico me fidem habere D. Bernoulli in prædicta Epistola 5 Iulij 1719 data asserenti se non scripsisse Epistolam 7 Iunij 1713 datam. Et quamvis aliqui eorum mihi nondum credant tamen ijs author non sum ut mihi contradicant.

Vide Epistolam D. Leibnitij ad D. Maiseaux 27 Aug. 1716, Et Epistolam D. Des Maizeaux ad D. l'Abbe Conti 21 Aug. 1718, Has vide in secundo Collectionis Tomo pag 356 & 362. < text from f 408r resumes > D^{nus} Des-Maizeaux easdem a D. L'Abbé Conti mense Martio vel Aprili Anni 1718 accepit eo fine ut Epistolarum Dⁿⁱ Leibnitij Collectionem aliquam in Hollandia imprimi curaret me prorsus consilium eoruum nesciente donec schedæ aliqut hujus collectionis impressæ sunt & ex Hollandia missæ mense Iulio ejusdem anni & mihi ostensæ In his schedis Epistolæ illæ duæ extabant expressæ. Et Anno sequente, accepi a te Epistolam D. I. Bernoulli datam 5 Iulij 1719, & post alios novem menses Collectio illa lucem vidit. Amicis meis dico me fidem habere D^{no} Bernoulli in prædicta Epistola 5 Iulij 1719 data asserenti se non scripsisse Epistolam 7 Iun. 1713 datam. Et quanquam aliqui eorum mihi nondum credant, tamen ijs author non sum ut mihi contradicant.

Pictam tui effigiem jam tandem accepi

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Has{illeg} duas Epistolas D. Desmaizeaux accepit a D Abbate de Comitibus circa mensem Maium Anni 1717. Et anno sequente circa Mensem Iulium accepit etiam ab eodem Abate Epistolas alias D. Leibnitij eo fine ut Epistolarum et scriptorum ejus collectionem aliquam in Hollandia imprimi curavit.

M^rLeibnitz in his life time sent the dispute bewteen him & Dr Clark to Mr Desmaiseaux as you may perceive by his Letter to M. desmaizeaux dated 21 Aug. 1716 in order to their being printed in Holland. When I heard that Mr Leibnitz was dead I caused what had passed between him & me to be printed at the end of Ralphsons book because copies thereof had been dispersed by Mr Leibnitz. And While this was doing Mr Desmaiseaus consulted with D. Abby Conti about reprinting the same in Holland together with other Letters of Mr Leibnitz & for that end he then received from Abbe Conti the Letters to Madam Kilmanseg & B. Bothmar. And in Iune or Iuly following he received from Abbe Conti then at Paris some other Letters to be added to the former which were then in the Press. Hæc omnia me latebant.

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The letter of 7 Iune 1713 inserted into the Letter of Mr Leibnitz to Madam Kilmanseg, was printed in Holland in the Novelles Literaris 28 Decem. 1715 pag 414 with this Title: <u>Lettre de M. Iean Bernoulli de Bále du 7 de Iuin 1713</u>. The Letters to Madam Kilmanseg & Baron Bothmar did not come abroad till above four years

after, & contein nothing more concerning the Letter of 7 Iune 1713 then what had been printed in Holland. M. DesMaizeaux received copies of them from M. L'Abbé Conti in Spring 1718 in order to print a collection of some remains of Mr Leibnitz And I knew nothing of the designe of printing them untill they were shewed me in print which was about a year before I received from you M I. Bernoulli's Letter of 5 Iuly 1719. And tho they did not come abroad till may I tell my friends that I acquiesce in Mr I. Bernoulli's Declaration that he was not the author of the Letter of 7 Iune, 1713, And tho some of them do not yet beleive me, yet I have encouraged none of them to contradict me.

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Bernulli {illeg} 1717 {illeg} to {illeg}

Epistola septimo Iunij 1713 data, quæ in Epistola D^{ni} Leibnitij ad D^{nam} Kilmanseg Gallice versa legitur, impressa fuit in Belgio in Novelles Literairs 28 Decem. 1715 pag 414, cum hoc Titulo: Lettre de M. Iean Bernoulli de Bâle du 7 de Iuin 1713. Epistolæ D. Leibnitij ad D^{nam} Kilmanseg & D. Barronem Bothmar missæ lucem non viderunt nisi post annos quatuor, & nihil amplius continent ad D. I. Bernoulli spectans quam quod in Novelles Literairs impressum fuerat.

Mense Septembri vel Octobri anni 1716 D. Des-Maizeaux ✝[3] accepit a D. Leibnitio Exemplar Disputationis inter ipsum et D. Clark. Et proximo vere is accepit etiam a D. Abbate de Comitibus, epistolas D. Leibnitij ad Abbatem illum, ad D^{nam} Kilmanseg, & ad D. Comitam de Bothmar scriptas Et anno proximo accepit etiam alias. ab eodem Accepit autem eo fine ut Epistolarum et Scriptorum D. Leibnitij Collectionem aliquam in Hollandi{a} imprimi curaret. Et Hæc omnia me latebant donec schedæ aliquot hujus Collectionis impressæ sunt et ex Hollandia missæ & mihi ostensæ. Id quod fiebat mense Iulio anni 1718. Et in his schedis impressæ extabant Epistolæ illæ duæ ad D: Kilmanseg & D. Bothmar, . Anno vero sequente accepi a te Epistolam D. I. Bernoulli datam 5 Iulij 1719; quo tempore Collectio fere tota impressa fuit: Et post alios novem menses eadem lucem vidit.

Amicis meis dico me fidem habere D. Bernoulli, in prædicta Epistolis Iulij data, asserenti, se non scripsisse Epistolam 7 Iunij 1713 datam. Et quamvis aliqui eorum mihi nondum credant; tamen ijs author non sum ut mihi contradicant.

Mense Augusto et Septembri anni 1716 D. Leibnitius disputationem inter ipsum et D. Clark misit ad D. Des-Maizeaus

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The design of publishing M. Des-Maizeaux's collection was set on foot by Mr Leibnis a little before his death & carried on by Mr Abbé Conti a little after For this end Mr Leibnitz sent Mr Leibnitz a copy of the dispute between himself & Dr Clark as

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le Mathematicien ou pretendu Mathematicien] l'auteur ou le pretendu auteur]

Gratiæ meæ multis nominibus tibi debentur; tum quod mediante R. Patre Reynau & ejus amico applicasti te ad D. Cancellarium Galliæ & prælongo Memoriali causam Bibliopolæ Montela{illeg} ei explicuisti & ejus auctoritate obtinuisti ut Bibliopola flecteretur: tum quod negotium, cum Bibliopola ad finem perduxisti & longe plura Libri exemplaria pro me obtinuisti quam expectabam tum etiam quod magnum illum laborem in te suscipere digneris conferendi Librum cum correctionibus missis, in quo faciendo spero & precor ut proprio utaris judicio & libere corrigas quæcunque tibi in correctionibus missis corrigenda videntur. Nam D. Maivreus omnia judicio tuo submittit. Pactum tuum cum Bibliopola ratum habeo & vice duodecim librarum sterlingarum dabo illi viginti. Et nummus solvetur erit ubicunque volueris. Epistolam inclusam D. Cancellario

tradas precor una cum gratijs meis quamplurimus favoribus quibus me decoravit. Gratias etiam meas reddas precor Patri Reynau ut et ejus amico cujus mediatione negotium delatum est ad D. Cancellarium, & D. Abbati Daguesseau D. Cancellarij fratri quod causæ meæ coram D. Cancellario faveret.

Ex Epistola tua ad D. Moivreum data intelligo quod D. Bernoullio displicet me ipsum vocasse [the Mathematician or pretended Mathematician] quamvis his verbis nihil aliud intellexi erim quam quod esset [the Author or pretended Author] Ipsi etiam displicet me illum vocasse hominem novum & rerum anteactarum parum peritum, ut Leibnitius Keilio objecit id est hominem qui post tempora Oldenburgij floruit et rerum ante mortem ejus int Leibnitium et Anglos gestarum scientiam non habuit nisi ex scriptus antiquis. Sed ipsi maxime displicet quod de ipso tanquam equite erratico locutus essem: cum tamen non contra Bernoullium, ibi locatus sim sed contra Leibnitium Is enim felicitatem suam in disciplis Iactavit Epistolam meam ad Abbatem de Comitibus vocavit une espece de Cartel, dixit se nolle in arenam descendere cum militibus in eis emissarijs, (sit enim vocavit Concessum a Regia Societate constitutum & D. Cotes,), sed cum ipse tandem apparerem, satisfactionem mihi reddere se paratus esse. Et ad hæc ejus verba alludens, non culpavi D. Bernoullium quod anno 1697 Problema suum edidisset, sed in Leibnitium sic scripsi. D. Leibnitius Consessum a Regia Societate constitutum accusat: quod Epistolas antiquas non totas edidissent quo Commercium utique mole multum auctum fuisset, sed cum respondere debuisset, quæritur librum prægrandem esse et Responsum molis non minoris requirere. Et sic Epistolæ et chartæ antiquæ [ex mente Leibnitij] rejiciendæ sunt et Quæstio in rixam deducenda est circa Philosophiam & {B}res alias; & Mathematicus qui in Epistola ad Leibnitium 7 Iun 1713 nomen suum celaverat ut judex æquus haberi posset, jam velum deponere debet [secundum Leibnitium] & in hac rixa non amplius judic{em} sed militem agere & per Leibnitium Mathematicos Anglos provocare: quasi duellum vel forte prælium inter milites meos emissarios & exercitum discipulorum ejus in quorum numero se felicem prædicat, methodus esset magis idonea veritatem determinandi quam provocatio ad scripta antiqua et authentica; & Mathesis imposterum factis nobilibus equitum erraticorum vice Demonstrationum implenda sit. Non voco Bernoullium equitem erraticum, sed culpo Leibnitium quod is quod is discipulos suos jactaret tanquam milites Equites , & seposita Quæstione de Methodo differentiali, proponeret Quæstiones novas Metaphysicas & Physicas & Problemata mathematica de quibus belligerandum esset rixando.

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In literis ad D. Conti, vocandi Bernoullium Mathematicum vel fictum mathe intellexi Iudicem vel fictum Iudicem & Bernoullius ipse scripsit se Iudicium 13 Iunij 1713 datum < insertion from f 411r > non < text from f 410v resumes > dedisse. Vocavi ipsum hominem novum & rerum anteactarum parum peritum eodem quo leibnitius Keilium sic vocavit: id est, ut significarem illum post tempora Oldenburgij floruisse, & rerum eo vivente actarum nihil scrivisse nisi per epistolas antiquas. Non vocavi ipsum equitem errabundum, sed culpavi Leibnitium quod Bernoullium tanquam equitem errabundum tractaret. In Epistola sua prima ad Abbatem de Comitibus, Commercio Epist respondere recusavit Questionem de primo inventore deseruit, quæstiones novas non paucas proposuit ad rem nihil spectantes, vizt de gravitate, occultis qualitatibus miraculis vacuo, spatio, perfectione {mutidi}, & Problemate Bernoullij ex actis Eruditorum desumpto & jactabat se discipulis felicem esse. Et postquam Abbas de Comitibus qui Mediatoris officium in se susceperat, responsum a me extortum missesset ad Leibnitium, Leinitius epistolam Abbatis vocabat une espece de cartel de la part de M. Newton. Et addidit se nolle in arenam descendere cum militibus meis emissarijs sed cum ipse apparerem se paratum esse mihi satisfactionem dare. Ob hoc duellum Leibnitium ridebam comparando disputationes de tot quæstionibus ad rem nihil spectantibus inter nostros discipulos cum duello gladiatorio ad veritatem determinandam minime idoneo

Editionem Librorum semper dedi vel Bibliopolæ vel ei qui editionem curabat

Pactum cum Bibliopola quovis nunquam feci, sed beneficium editionis

Pactum tuum cum Bibliopola ratum habeo, et pro libris duodecim circiter pulchre tectis quos in animo habeo amicis apud vos donare, dabo insuper Bibliopolæ libros octo Sterlingas et nummus solvetur quandocunque volueris. Reliquos libros reservatos amicis tuis distribuas precor.

In literis meis ad Abbatem de Comitibus, non vocavi Bernoullium Equitem erranticum, sed Leibnitium culpavi qui Bernoullium ut equitem errantem jactabat,

D. Des Maiseaux commercium cum Leibnitio olim habuit & literas inter Leibnitium et Clarkium in primo collectinoum Tomo impressas accepit a Leibnitio ut easdem in lucem mitteret, & mihi ignotus erat priusquam Literas inter Leibnitium et me in prima parte Tomi secundi ad usque paginam octogesimam octavam impressas mihi ostendit, et in secunde parte quæ priore major est edidit Collectionem chartarum Leibnitij quæ ad me nil spectant. Et ex his demonstratur eum amicum fuisse Leibnitij Nam tota Collectio ex Actis nobilibus Leibnitij componitur. Præfationem quo me defendit, moleste habui tum quod non esset mathematicus Controversiam non satis intelligeret sed aliorum argumenta tantum recitaret, tum quod amicus esset Leibnitij & eo nonum minime idoneus qui me defenderet.

D. Leibnitius Problema Bernoullij ex Actis Eruditorum desumptum ad determinandam veritatem alterius generis bis proposuit, primo contra N. Fatium anno 1700 deinde contra Consessum a Regia Societate constitutum, anno 1715 quasi hi nec judices nec testes esse possent nisi Problemata Bernoullij possent solvere, et] anno 1715 quasi testimonia Barrovij, Collinij, Wallisij, Fatij aliumque ex scriptis antiquis desumpta pro nihilo habenda essent et veritas pro victorias in solutione problematum, & disputationibus metaphysicis determinanda esset.

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Pactum tuum cum Bibliopola ratum habeo & ad libras duodecim sterlingas addam alias octo nummumque mittam quandcunque volueris. D. Moivreus tibi mittet catalogum nominum plus minus quindecim amicorum meorum. Et expensas compingendi exemplaria librorum ijs. donanda ex illis octo libris sterlingis desumi possunt. Exemplaria alia sex in schedis solutio ad me mitti cupio & reliqua exemplaria pro lubitu tuo distribuenda

In schedula Iudicis Mathematici accusor plagiarij . Leibnitius hunc judicem in schedulo illo Gallice edita Bernoullium esse dixit. Injuriæ fuit publica & satisfactio debuit esse publica. Satisfationem non quærebam. Bernoullius sponte obtulit. & offerendo licentiam mihi dedit epistolam ejus in lucem emittendi. Quæritur quod Ostendi tantum amico una & altere privatim.

D. Leibnitius Problema Bernoullij ex Actis Eruditorum desumptum ad determinandam veritatem alterius generis bis proposuit, primo contra N. Fatium Anno 1700 deinde anno 1715 contra Concessum a Regia Societate constitutum, quasi testimonia Barrovij, Collinij, Wallisij, Fatij, aliaque ex scriptis antiquis desumpta pro nihilo habenda essent, et veritas non per testimonia & argumenta sed per victorias in solutione Problematum et in disputationibus metaphysicis determinanda esset, ut fit in rixis militum quarum decisio ad pugnas referri solet.

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ea lege ut libros viginti vel viginti quinque tegat ornate pro donis.

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to you 12^{Li} already promised to the Bothmars I shall add 8^{Li} more to bee paid you when you think fit, Mr De Moivre will send you a List of about 15 Names, I desire that the Charges of the bindings may be taken out of the 8^{L}, as for the rest of the Copies in sheets I desire to have 6 for my self, and that you would dispose of the rest of the Papres as you thincke fit

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I understand by your letter to Mr Moiver that Mr Bernoulli complains of me for calling him a Mathematician or pretended Mathematician & homo novus & rerum anteactarum parum peritus & a Knight errant. And yet by the first I meant only to call him the author or pretended author of the judgment published in the flying paper & said to be written by a great mathematician And this was in his favour: For he has since declared that he was not the author. If he was the Iudge then the Iudge was in a conspiracy with Mr L. to be carried on against Dr Keill & the Committee of the R. S. by concealing the name of the judge, for two years together: And by calling Mr Bernoulli the pretended Iudge I absolved Mr B. from being in such a conspiracy And by

the second I meant that he was risen up since the days of Mr Oldenburg & unacquainted with the transactions of those days, as Mr Leibnitz objected against Dr Keill. And by the third I meant only to correct Mr Leibnitz for referring the matter in question to a duell or battel instead of deciding it by reason & old records, & making Mr Bernoulli at one & the same time an impartial judge between the two parties & a partiman in whose name he sent a challenge to the English Mathematicians. Mr Bernoulli's Problem was published in Leipsic Acts for May 1697 p. 211. Mr Leibnitz in the year 1700 challenged Mr Fatio to solve this Probleme. It reflects upon Mr Leibnitz (not upon Mr Bernoulli) for appealing from the evidence which Mr Fatio offered to a Mathematicall Duell, & thereby treating Mr Bernoulli as a Hero or Don Quixot in Mathematicks. And the same thing Mr Leibnitz repeated by challenging the English Mathematicians to solve the same Probleme

I understand by your letter to Mr Moivre that Mr Bernoulli complains of me for calling Mathematician or pretended mathematician, Homo novus & rerum anteactarum parum peritus, & a Knight errant. And yet by the first nothing more was meant then to call him the author or pretended author of the judgement published in the flying paper & ascribed to a great mathematician, by the second nothing more then that he was risen up since the days of Mr Oldenberg & unacquainted with the transactions of those days, as Mr Leibnitz had objected against Dr Keill & as for the third, but against Mr Leibnitz

[1] Bernoulli **{chiavous}** **{the}** letter to M Des Maizeau

[2] From M. Des Maizeau

[3] ✝ Vide Epistolam D. Leibnitij ad D. Des-Maizeaux 27 Aug. 1716, et Epistolam D. Des-Maizeaux ad D. Abbatem de Combitus 21 Aug. 1718. Has vide in secundo Collectionis Tomo pag. 356 & 362.