Draft letter from Sir Isaac Newton to Pierre des Maizeaux

Author: Isaac Newton

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Sir

M^r^ Leibnitz Letter of is like the rest of his Letters & all of them full of assertions accusations & railing reflexions without proving any thing. And by this character you may guess at the Author of that Libel.

He is unwilling to acknowledge himself the Aggressor, & complains that the Passage in the Acta Lipsiensia of Ianuary 1715 alleged against him, has been poisoned by a malicious interpretation of a man who would pick a quarrel. In the Introduction to the Book of Quadratures I affirmed that I found the Method of fluxions gradually in the years 1665 & 1666. And in opposition to this the said Acta in giving an Account of the said Introduction we represent that for the better undersstanding the said Introduction we must have recourse to the Differential Method cujus elementa ab Inventore Dn. Gothofredo Gulielmo Leibnitio in his Actis sunt tradita varijque usus tum ab ipso, tum a Dom. fratribus Bernoullijs tum & D^no^ Marchione Hospitalio sunt ostensi. Pro diffentijs igitur Leibnitianis Dn. Newtonus adhibet semperque adhibuit <u>fluxiones</u> quæ sint quamproxime ut fluentium augmenta æqualibus temporis particulis quam minimis genita; ijsque tum in suis Principijs Naturæ Mathematicis, tum in alijs postea editis eleganter est usus, quemadmodum et Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianæ Methodo substituit. And then they go on to explain the method & character of M^r^ Leibnitz instead of that of M^r^ Newton. But they that read & compare the Introduction to M^r^ Newton's book of Quadratures, the whole Account given thereof in the Acta Eruditorum, the Answers of D^r^ Keill & the Answer of M^r^ Leibnitz to the Doctor dated 29 Decem. 1711, will find the contrary. The matter is sufficiently stated in the Commercium Epistolicum, & I need not repeat it. In the Preface to the book of Quadratures I wrote that I found the method of fluxions gradually in the years 1665 & 1666 . The Acta gave a contrary account of the method, & M^r^ justified justified this account & thereby made it his own.

& gives a contrary Interpretation of his own, saying that the words <u>adhibet semperque adhibuit</u> imply that I used fluxions not only after I had seen his differences, but even before; as if <u>pro differentijs L . . . anis D. N . . . us adhibet semper adhibuit fluxiones</u> quemadmodum I H. Fabrius motuum progressus could signify that I used fluxions before M^r^ Leibnitz used Differences or I knew that he used them. In the Introduction to the Book of Quadratures I affirmed – – – instead of that of M^r^ Newton And M^r^ Leibnitz in his Letter of 29 Decem: 1711, has justified all this & made it his own. If he that interpreted the word adhibuit by the word substituit has poisoned this Vassage in the Acta by a malitious interpretation the crime is his that wrote the Passage.

And as for his suggesting that I might be willing to find a pretence to ascribe to my self the invention of the new calculus contrary <421r> to my knowledge owned in my Principles p. 253 in the first Edition: its well known here that this controversy was begun between him & D^r^ Keill before I knew what had been published in the Acta Leipsica.

D^{r} Barrows method of tangents was published in the year 1670. M^{r} Gregory the same year in his Letter above mentioned gave notice that from D^{r} Barrows method of Tangents compared with his own he had deduced a general method of drawing tangents without calculation. M^{r} Slusius also in the year 1672 gave notice to M^{r} Oldenburg that he had such an easy method of Tangents. M^{r} Collins thereupon gave me notice of these methods of Tangents & desired that I would send my method. Thereupon I wrote my Letter of 10 Decem 1672 wherein I < insertion from above the line > I described my met <421r> hod & < text from f 421r resumes > represented that I took it to be the same with that of Slusius & Gregory & added that it was a branch or rather a corollary of a general method which without any troublesome calculation extended not only to tangents of all sorts of Curves but also to other abstruser sorts of Problemes concerning the Curvatures, Areas, Lengths, centers of gravity of Curves &c And was not (like the method of maxima & minima of Hudde) restrained to equations free from surdes; & that I had interwoven this method with that of infinite series, meaning in the Tract which I wrote the year before, as M^{r} Collins mentioned in his Letters to Borellus & Vernon in December 1675 And M^{r} Leibnitz a year after he had received copies of these Letters & eight months after he had seen my other Letters in the hands of M^{r} Collins (amongst which I reccon my Analysis per æquationes numero terminorum infinitas [& my letter of 24 Octob. 1676 the newly arrived at London but not yet copied) sent back D^{r} Barrows method of tangents with the characteristick changed & explained how this method readily gave the method of tangents of Gregory & Slusius, & might be improved so as not to stop at fractions & surds, & faciliated quadratures, & from these characters he concluded that this method was like that which I had described in my Letters. And this is the Differential method which was mentioned in my book of Principles pag 253, 254, & is now claimed by M^{r} Leibnitz as the first inventor thereof, notwithstanding that he had notice of all this by my Letters of earlier date compared with that of Gregory & that I had written a treatise of this method & of the method of series together in the year 1671.

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

M^{r} Leibnitz saith that the Letter which I call defamatory being no sharper then that which has been published against him, I have no reason to complain. But the sharpness of the Letter lies in accusations & reflexions without any proofe < insertion from above the line > which is an unlawful way of w <421r> riting < text from f 421r resumes > , the sharpness of the Commercium lies in facts which are lawfull to be produced, such as is the fact that after M^{r} Leibnitz had received the series of Gregory twice from London, he sent it to me & published it in Germany as his own without mentioning that he <421r> had received it from London: And further, the Letter was published in a clandestine backbiting manner without the name of the author or mathematician or printer or City where it was printed. And it was dispersed two years & a quarter before we were told that the Mathematician was Iohn Bernoulli.

He saith that when he was the first time in London he was not acquainted With M^{r} Collins as some have maliciously feigned. But who has feigned this or what need there was to feign it I do not know. D^{r} Pell gave him notice of Mercators series for the Hyperbola & he might have notice of mine for the circle either in London or at Paris without being acquainted with M^{r} Collins.

He saith that he found the arithmetical Quadrature of the Circle towards the end of the year 1673 the general method arbitrary series & in the year 1676 & soon after the differential calculus in the same year & that in his Letter of 27 Aug. 1676 by the words certa Analysi he meant the differential Analysis. And am not I as good an evidence that I found the method of fluxions in the year 1665 & improved it in the year 1666. And if I should add that before the end of the year 1666 I wrote a small tract on this subject which was the grownd of that larger tract which I wrote in the year 1671, & in this smaller tract, tho I generally put letters for fluxions as D^{r} Barrow in his method of tangents put Letters for differences, yet in giving a general Rule for finding the curvature of curves I put the letter x with one prick for first fluxions & with two pricks for second fluxions:

am not I as good an evidence for this as M^{r} Leibnitz is for what he affirms; especially since both thse Tracts are still in being & ready to be produced upon occasion.

In the year 1684 M^{r} Leibnitz published only the elements of the Calculus differentialis & applied them to questions about tangents & maxima & minima but proceeded not to the higher Problemes. The Principia mathematica gave the first instances made publick of applying it to the higher Problemes & I understood M^{r} Leibnitz in this sense in what I said concerning the Acta Eruditorum for May 1700. But M^{r} Leibnitz observes that what was there said by him relates only to a particular artifice de maximis et minimis with which he there allowed that I was acquainted when I gave the figure of my vessel in my Principles. But this depending upon the differential method as an improvement thereof & being the artifice by which they solved the Problemes which they value themselves most upon (that of the linea celerrimi descensus & the Catenaria, & velaria) & which M^{r} Leibnitz there calls a method of the highest moment & of greatest extent, I content my self with his acknowledgment that I was the first that proved by a specimen made publick that I had that artifice.

D^{r} Barrow printed his differential method of Tangents in 1670 M^{r} Gregory from this method deduced a general method of Tangents without calculation & by his Letter of 5 Sep. 1670 gave notice thereof to M^{r} Collins. Slusius in November 1672 gave notice of the like method to M^{r} Oldenburge. I in my Letter of 10 Decem 1672 sent the like method to M^{r} Collins & added that I tooke the Methods of Slusius & Gregory to be the same with mine, & that it was but a branch or rather a Corollary of a general method which without any troublesom calculation extended to not only to Tangents but also to other abstruser sorts of Problems concerning the crookednesses, areas lengths centers of gravity of Curves &c & this without stopping at surds: & added that I had interwoven this method with that of infinite series. meaning in my Tract which was written in 1671 Copies of these Letters were sent to M^{r} Leibnitz by M^{r} D. in the Collection of Gregories Letters & Papers in Iune 1676 & M^{r} Leibnitz in Iune his Letter of 21 1677 sent nothing more back then Barrows Method of Tangents extend to the method of Slusius & to Quadratures & equations involving surds: all which he had notice of by these letters. But this is not the case between me & <421v> D^{r} Barrow. He saw my Analysis in the year 1669 & found no fault with it. And when he was publishing his Lectures I told him of my method of drawing Tangents without any computation, as I mention **{**in**}** my Letter of 10 Decem 1672. but he did not extend his method so far.

He tells us that in his Letter of 27 Aug. 1676 where he speaks of Problemes which depend not on equations nor quadratures he meant only equations of the vulgar Analysis. But the words plainly relate to the reducing of Problems generally not to vulgar equations but to infinite series & doing it in the difficulter cases by other methods then the extracting the roots of vulgar equations. In the next words he said that he solved the Probleme of De Beaune certa Analysi & this Analysis he saith was the Differential. But the Analysis in this case doth not require a differential equation. Any one may presently see that if the Abscissa of this Curve increase in Arithmetical proportion the Ordinate shall increase or decrease in Geometrical, & therefore the Ordinate hath the same relation to the Abscissa as the Logarithm to its Number. & this is all the Analysis requisite.

In my Answer dated 24 Octob. 1676 I said Inversa de tangentibus Problemata sunt in potestate aliaque illis difficiliora. And a little after Inversum hoc Problema de Tangentibus quando Tangens inter punctum contactus et axem [seu Abscissam] figuræ est datæ longitudinis non indiget his methodis. Est tamen Curva illa Mechanica cujus determinatio pendet ab Area Hyperbolæ. Ejusdem generis est etiam Problema quando pars Axis inter Tangentem et Ordinatim applicatam datur Longitudine. Sed hos casus vix numeraverim inter ludos Naturæ. Nam quando in triangulo rectangulo quod ab illa Axis parte et Tangente ac Ordinatim Applicata constituitur, relatio duorum quorumlibet laterum per æquationem quamlibet definitur, Problema solvi potest absque mea methodo Generali: Sed ubi pars axis ad punctum aliquod positione datum terminata ingreditur vinculum; res aliter se habere solet; id est, Problema indigere solet mea methodo generali.

Pag. 191. lin 21. After the words year 1676 add M^{r} Iames Gregory in a Letter to M^{r} Collins dated 5 Sept 1670 & printed in the Commercium Epistolicum pag 22 wrote thus [Barrovij Lectiones summa cum voluptate & attentione perlegi; atque omnes qui unquam hisce de rebus scripserunt infinito intervallo superasse comperio.] Ex ejusdem [Barrovij] methodis Tangentes ducendi cum quibusdam e proprijs collatis, inveni

methodum generalem et Geometricam ducendi Tangentes ad omnes Curvas sine calculo & quæ complectitur non tantum Barrovij Methodos particulares, sed et ipsius generalem Methodum Analyticam, quam habes sub finem Lectionis decimæ [A copy of this Letter was sent to M^r^ Leibnitz at Paris in the aforesaid Collection of Gregoris Letters 26 Iune 1676.] And two years after upon notice that Slusius & Gregory had Methods of drawing Tangents without calculation, M^r^ Newton wrote back to M^r^ Collins 10 Decem 1672 in these words Ex animo gaudeo D. Barrovij amici nostri reverendi Lectiones Mathematicas exteris adeo placuisse; neque parum me juvat intelligere eos [Slusium et Gregorium] in eandem mecum incidisse ducendi. Tangentes Methodum. Qualem eam esse conjiciam ex hoc exemplo percipies. And at the end of the example he added these words Hoc est unum particulare – – – – – – – ad series infinitas &c

Pag 192. lin. 10. – of Iune 13 1676, & amongst the same Letters was M^r^ Gregories Letter of 5 Decem 1670. Ib. l. 19 & that the method of Tangents of Slusius & Gregory was but a branch p. 196. lin ult. – – – general method, & M^r^ Gregory that he had deduced it from the Method of D^r^ Barrow. And thus far M^r^ Leibnitz described nothing more then what he might learn from D^r^ Barrow & M^r^ Gregory

But whereas M^r^ Newton had said

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He fell foul upon D^r^ Keill & **{**th**}**e prest the R. S. against him & chalenged me before I knew what had been published in the Acta Leipsica, & therefore he is the aggressor & ought to prove his accusation upon pain of being deemed guilty of calumny.

Sir

Sir

1 M^r^ Leibnitz is unwilling to acknowledge himself the Aggressor & complains that the Passage in the Acta Eruditorum of Ianuary 1705 [pro differentijs L . . . tianis, D. N . . . nus adhibet semperque adhibuit] has for this end been poisoned by the Author of the Remarques upon the Commercium, who says (pag. 108) sensus verborum est quod N . . . nus Fluxiones differentijs L . . . tianis substituit. This he calls a malitious interpretation of a man who would pick a quarrel, & pretends that the words adhibet semperque adhibuit relate as well to the time before I had seen M^r^ Leibnitz differences (he should have said, before the differential method was known to M^r^ Leibnit**{**s**}** as to that afterwards. But the Author of the Passage first tells his Reader that for better understanding the Introduction to the Book of Quadratures we must have recourse to the Differential method, cujus elementa ab INVENTORE Dn. G. G. L . . . tio in his Actis sunt tradita And then adds: Pro Differentijs IGITVR L . . . tianis D. N . . . tonus adhibet semperque adhibuit fluxiones — QVEMADMODVM et Honoratus Fabrius in sua Synopsi Geometrica motuum progressus Cavallerianæ methodo substicit The Author of the Passage by the word IGITVR makes the words adhibet semperque adhibuit a consequence of what went before & by the word QVEMADMODVM he makes them equipollent to substituit: M^r^ Leibnitz calls this interpretation a malitious one & interprets them in another sense which makes them no consequence of what went before & spoils the sense of quemadmodum Fabrius substituit.: The plain meaning of the words are that M. N. uses at preent & from the beginning of his having this method did always use fluxions for (or instead of) the Leibnitian Differences even as Faber substituted motions for the method of Cavellerius. And M^r^ Leibnit**{**z**}** by his Letter of 29 Decem 1711 has justified the accusation in this passage & made it his own. He chalenged me both in this Letter & in that of 4 March $161\frac{0}{1}$ to give my opinion in this matter, & now I have at length given my opinion, he takes my Letter for an answer to his challenge <422v> saying that he was unwilling to enter the lists with my forelorn hope before I appear**{**e**}**d my self, & therefore he is the aggressor.

3. M^r^ Leibnitz insinuates that my complaint of his accusing me of plagiary might be to attribute to my self the invention of the new calculus contrary to my knowledge avowed to the contrary in my book of Principles pag 253 of the first edition. And a little after he saith that in the first Edition of my Principles p. 253, 254 I allowed him the invention of the Calculus of differences independantly of my own. But he here accuses me falsly. My words were these. In literis quæ mihi cum Geometra peritissimo G. G. Leibnitio annis abhinc

decem [i.e. anno 1676] intercedebant, cum significarem me compotem esse methodi determinandi Maximas & Minimas, ducendi Tangentes & similia peragendi quæ in terminis surdis æque ac in rationalibus procederet, & literis transpositis hanc sententiam involventalibus [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire & vice versa] eandem celarem: rescripsit Vir clarissimus se quoque in ejusmodi methodum incidisse, & methodum suam communicavit a mea vix abludentem præterquam in verborum & notarum formulis. This was reprinted in the second edition of my Principles & is still allowed by me. But I never allowed that M^{r} Leibnitz was the first inventor or found this method without receiving some light from me. < insertion from above the line > M^{r} Craige is a witness that in those days I looked upon the method as mine. He received also copies of m^{r} Gregories Letter of 5 Sept in which M^{r} Gregory wrote to M^{r} Collins that from D^{r} Barrows methods of Tangents & his own he had found a general method of drawing Tangents to all Curves with <423r> out any Calculus < text from f 422v resumes > In Iuly 1676 M^{r} L. received copies of my Letters dated 10 Decem. 1672 & 13 Iune 1676. In October following he came to London & saw some others of my Letters in the hands of M^{r} Collins. < insertion from above the line > & particularly my Letter of 24 Octob. 1676 & had an opportunity of seeing my Anal <423r> ysis < text from f 422v resumes > In March following he received a copy of my Letter of 24 Octob. 1676 & after all this in a Letter dated 21 Iune 1677 he sent me back his method

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4 The Royal Society & their Committee have acted by good authority & in a regular manner & M^{r} Leibnitz has no authority to call them to account. If at his command they would have condemned D^{r} Keill without examining into the matter they would have been honest Gentlemen. But when he in his Letter of 29 Decem 1711 refused to justify his accusation & put them upon a necessity of appointing a Committee to examin old Letters & Papers relating to this matter & report their opinion thereupon that the Society might know what was fit for them to do therein; & the Society ordered the letters & papers with the Report to be published: he appealed from them to a mathematician of his own chusing & by consequence a party-man & & desired him to give his opinion upon the Commercium. And this Mathematician in his Answer dated 7 Iuly 1713 cited Iohn Bernoulli as a man different from himself in these words [quemadmodum ab eminente quodam Mathematico dudum notatum est] And this Letter wsa inserted into a defamatory Letter dated 29 Iuly 1673 & published in Germany without the name of the Author or Mathematician or Libeller or Printer or City where it was printed. & the impression dispersed by M^{r} Leibnitz & his Agents & reprinted in French in the Iournal Literair of November & December 1713 & answered by D^{r} Keill in the Iournal of Iuly & August 1714. And the Letter of the Mathematitian was reprinted in Latin in the Novelles Litterairs for 28 Decem 1675 without mentioning that it had been answered by Keill, & to give it authority, it is there said to have been written by Iohn Bernoulli & the citation above mentioned whereby the author distinguishes him self from Iohn Bernoulli is omitted. And therefore being uncertain whether this Mathematician be Iohn Bernoulli or some other man, I called him (in my Letter of 26 Feb: last) a Mathematician or pretended Mathematician. intending by those words not to detract from the skill of M^{r} Bernoulli (as M^{r} Leibnitz complains) but to touch upon the tricking, double-faced, maskerading management of the Libel.

5 M^{r} Leibnitz represents that this paper being no sharper then the Commercium Epistolicum I have no reason to complain. But the difference is that this paper has all the characters of a defamatory Libel being full of reflecting affirmations without any proof & printed & dispersed in a clandestin manner without naming the author printer or City where it was printed. M^{r} Leibnitz saith that the Mathematician whose Letter was inserted into it was nkown which is sufficient but it was not known till above two years after the Libel was dispersed And if it had been known sooner, yet the Mathematician is not answerable for the Libell unless so far as he had a hand in it: whereas the Comerc. Epist. was published fairly at London by Order of the R. S. & has nothing of sharpness in it but what is in the facts comprehended in or proved by the ancient Letters & papers. And this way of writing is allowed by all mankind.

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6. He complains of the Committee as partial, & set sup Iohn Bernoulli against them & to give the preference to this great Mathematician challenges them to solve one of his Problemes. But what solving of Problemes

has to do in this matter I do not understand. The Committee without any skill in mathematicks might search out the old Letters & papers & shew them to witnesses who knew the hands, & nothing more. then their testimony is wanted in this matter. And all Bernoullis skill in Mathematicks avails nothing against this evidence.

7 But M^r^ Leibnitz represents that the Committe have omitted things which made against me & printed every thing which could be turned against him by strained glosses, & to make this appear he produced an instance in his last Letter, but confesses now that he erred in that instance affirming that the passage was omitted in the Commerc. & he produces another instance. He saith that in one of my Letters to M^r^ Collins I owned that I could not find the second segments of Spheroids, & that the Committee have omitted this If the Committee had omitted such a Letter I think they would have done right, it being nothing to the purpose. But there was no such Letter. M^r^ Collins in a Letter to M^r^ Gregory dated 24 Decem 1670 & printed in the Commercium pag. 24, wrote that my method extended to the second segments of round solids, & I heare that the Letter in which I signified this to M^r^ Collins is still extant & conteins the dimension of the second segment of the Sphæroid, so that it makes as much for me as M^r^ Leibnitz pretends that it makes against me & yet I do not complain of the Committee for omitting it. But I complain of M^r^ Leibnitz for accusing the Committee of partiality without knowing wherein they were partial. For this rashness shews the spirit of the Gentleman.

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Sir

In my Letter of 24 Octob 1676 I represented that five years before that time, that is in the year 1671, I wrote a Book of this Method & the method of Series together, but for the sake of quiet desisted from my designe of publishing it. The Book is still in being & conteins a very plain description of those two methods. The first Proposition of the book is, Relatione quantitatum fluentium inter se data fluxionum relationem determinare. In the year 1685 M^r^ Iohn Craig desire**{**d**}** me to explain to him the elements of the differential Calculus then newly published in the Acta Eruditorum. I did so & told him that the method was mine as would appear if the Letters which past between me & M^r^ Leibnitz seven or eight years before should be published, & M^r^ Craig is still alive & remembers this. D^r^ Wallis in the Preface to the two first Volumes of his works printed in Spring, 1695, wrote that in those Letters (copies of which had been communicated to him by M^r^ Oldenburgh I had explained to M^r^ Leibnitz that Method found by me ten years before or above, that is, in the year 1666 or before. In the Account of these two Volumes published in the Acta Eruditorum the next year, this passage was taken notice of & not contradicted. D^r^ Wallis in a Letter dated 1 Decem. 1696 gave notice of this passage to M^r^ <424v> Leibnitz & in the Letters which followed between them it was not contradicted. M^r^ Fatio in the year 1699 published that I was the oldest inventor by many years & confirmed his opinion by what he had seen in my manuscripts, & M^r^ Leibnitz in his answer did not contradict him And all this was done without any encouragement from me. In the year 1704 I published (in the Introduction to my book of Quadratures) that I found the method of fluxions by degrees in the year 1665 & 1666. For this was not yet disputed. But D^r^ Wallis, (the last of the old men who had corresponded with M^r^ Oldenburg & M^r^ Collins in these matters, except M^r^ Leibnitz & my self being now dead, somebody in the Acta Eruditorum for Ianuary 1705, in giving an account of the said Introduction told his Reader that for the better understanding this Introduction we must have recourse to the Differential method cujus elementa ab INVENTORE Dn. G. G. L . . . tio in his Actis sunt tradita. And then added: Pro Differentijs IGITVR Leibnitianis Dn. Newtonus adhibet semperque adhibuit fluxiones — QVEMADMODVM et Honoratus Fabrius in sua Synopsi Geometrica motuum progressus Cavallerianæ methodo substituit. And hereby the world was told that I did not invent the method so early as has been beleived in England nor write a book upon it in the year 1671 nor mention it in my Letters of 13 Iune, & 24 Octob 1676 & 10 Decem 1672, nor was the first inventor, but do use & have always used fluxions for the Leibnitian differences even as Faber substituted motions for the method of Cavallerius. With this accusation the dispute began, & M^r^ Leibnitz hath made this accusation his own by saying in his Letter 29 Decem 1711, Frustra ad exemplum Actorum Lipsiensium [Keilius] provocat ut sua dicta excuset; in illis enim circa hanc rem quicquam detractum non reperio sed potius passim suum cuique tributum. In the same Letter he refused to contend with D^r^ Keill because the Doctor was not authorised by me, & desired that

I would give my opinion in this matter, that is, that I would either condemn D^r^ Keill & retract what D^r^ Wallis M^r^ Fatio & I had published some years before & what I had written in the year 1676 & before, or enter into the lists with him as he expresses himself in his last Letter. And by this attaque he is the Aggressor. He is very angry at the interpretation of the words adhibet semperque adhibuit by the word substituit. And yet the Author himself by the words igitur & quemadmodum has interpreted them in that manner.

M^r^ Leibnitz insinuates that my complaint of his accusing me of plagiary might be to attribute to my self the invention of the new calculus contrary to my knowledge avowed to the contrary in my Book of Principles pag 253 of the first edition. And a little after he saith that in the first Edition of my Principles p. 253, 254 I allowed him the invention of the calculus of differences independently of my own But he accuses me falsly. My words were these. In Litteris quæ mihi cum Geometra peritissimo G. G. Leibnitio annis abhinc decem intercedebant [i.e. anno 1676,] cum significarem me compotem esse methodi determinandi maximas & minimas, ducendi Tangentes, & similia peragendi quæ in terminis surdis æque ac in rationalibus procederet, & literis transpositis hanc sententiam involventibus [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice versa] eandem celarem: rescripsit vir clarissimus se quoque in ejusmodi methodum incidisse, & methodum suam communicavit a mea vix abludentem præterquam in verborum et notarum formulis. This was reprinted in the second edition of my Principles & is still allowed by me But I never allowed that M^r^ Leibnitz was the first inventor or found this method without receiving some light from me. M^r^ Craige is witness that in those days I looked upon the method as mine. In Iuly 1676 M^r^ <425r> Leibnitz received copies of my Letters dated 10 Decem. 1672 & 13 Iune 1676, & a copy of M^r^ Gregories Letter to M^r^ Collins dated 5 Sept. 1670. In October following he came to London & saw some others of my Letters in the hand of M^r^ Collins, & particularly my Letter of 24 Octob 1676 then newly arrived at London but not yet copied. In March following a copy of this Letter upon notice of his being arrived at Hannover was sent to him. And after all this, in a Letter dated 21 Iune 1677, he sent me back his method representing that it was like the method of which I had given him notice. And what he sent back was nothing more then what he had sufficient notice of from my Letters compared with M^r^ Gregories Letter & D^r^ Barrows method of tangents.

This method was published by D^r^ Barrow in the year 1670, & M^r^ Gregory the same year in his Letter above mentioned gave notice that from this method compared with his own he had deduced a general method of drawing tangents without any calculation M^r^ Slusius also in the year 1672 wrote to M^r^ Oldenburg that he had such a ready method of Tangents. And thereupon M^r^ Collins gave me notice of these methods & desired that I would send them mine & I did so in my Letter of 10 Decem 1672, representing that I took it to be same with that of Slusius & Gregory, & I added that it was a branch or rather a Corollary of a general method which without any troublesome calculation extended not only to tangents of all sorts of Curves but also to other abstruser sorts of Problemes concerning the curvatures, areas, lengths, centers of gravity of Curves &c and was not (like Hudde's method of maxima & minima) restrained to equations free from surdes; & that I had interwoven this method with that of infinite series; meaning in the Tract which I wrote the year before. And M^r^ Leibnitz a year after he had received copies of these Letters & eight months after he had seen my other Letters in the hands of M^r^ Collins (amongst which I reccon my Analysis per æquationes numero terminorum infinitas) sent back D^r^ Barrow's method of tangents with the characteristick changed & explained how this method readily gave the method of tangents of Gregory & Slusius & might be improved so as not to stop at surds & faciliated quadratures, & from these characters he concluded that this method was like that which I had described in my Letters. And this is the differential method which was mentioned in my book of Principles pag 253 & 254, & is now claimed by M^r^ Leibnitz as the first inventor thereof, notwithstanding that he had notice of all this by my Letters of earlier date & & was told that I had written a Treatise of this method & of the method of series together in the year 1671.

<426r>

to Des Maizeaux ①

Sir

When the Postscript to M^{r} Leibnitz's first Letter to M^{r} l'Abbe Conti was shewed to me, I did not think my self concerned to meddle with it, partly because the Letter was not directed to me, & principally because I observed that his designe was to lay aside the Records in the Commercium Epistolicum & engage me in a squabble about Questions nothing to the purpose such as were: whether M^{r} Leibnitz found the Differential method proprio Marte, whether all bodies be heavy, whether I make gravity to be an occult Scholastic quality Whether I make it a Miracle Whether God be intelligentia supramundana. Whether I make him to be the Soul of the world, whether I say that he has need of a sensorium. Whether I have demonstrated a Vacuum. Whether space be the order of things coexisting & time the order of things successive. Whether God has made the world so perfect as to last from all eternity to all eternity by the mere laws of nature. Whether there be atoms, & whether any body in England can solve the problems of M^{r} Iohn Bernoulli. However at length being pressed by M^{r} l'Abbé Conti to write an Answer that the Postscript with my answer might be shewed to the King I wrote an Answer dated 26 Febr. 1716. But when my Answer was sent to M^{r} Leibnitz & he sent it with his answer to M^{r} 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; & only wrote a paper of Remarks upon his Answer & shewed them privately to some of my friends to satisfy them that it was easy to have answered his Letter had it come hither directly. And because he had sent them to be communicated to his friends at Paris, I caused them to be published as soon as I heard that he was dead, & you may reprint them with such other Letters as are fallen into your hands but I see no necessity of writing an Answer to those other Letters. The Remarks may serve for an Answer to the whole

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 it & the 7th 8th 9th & 10th Propositions with their Corollaries were fresh in memory I wrote upon them to M^{r} Collins that Letter which was dated 8 Novem 1676 & published by M^{r} Iones. 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 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 Theorems could be invented. The Book of Quadratures was therefore composed before M^{r} Leibnitz understood the Differential Method. For the Horologium oscillatorium of M^{r} Huygens was published in April 1673, & M^{r} Leibnitz began a little after <427r> to study to h igher Geometry, & never pretended to have had the differential Method before the year 1676; & when he wrote his Letter of 27 Aug. 1676, he placed the perfection of Analysis not in the differential Calculus, as he did after he found it, but in another Method founded on Analytical Tables of Tangents & the Combinatory Art. Nihil est, saith he, quod norim in tota Analysi momenti majoris. And a little after: Ea vero non differt ab Analysi illa SVPREMA ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum.

M^{r} Iames Gregory died in the end of the year 1675 & at the request of M^{r} Leibnitz his Letters were collected & the Collection sent to Paris in Iune following & among them were copies of M^{r} Gregories Letter of 5 Sept 1670 & mine of 10 Decem 1672, & by those Letters M^{r} Leibnitz had notice that M^{r} Barrow's method of Tangents was capable of improvement so as to give the method of Tangents of Gregory & that the method of Tangents of Gregory & Slusius was capable of improvement so as to give my general Method of Analysis, & that this Analysis proceeded without sticking at surds, & that I had interwoven it with the method of Series, viz^{t} in a Tract which I wrote upon this subject in the year 1671. M^{r} Leibnitz wrote also to M^{r} Oldenburg for the Demonstration of some of my Series, that is for the Method of finding them & promised him a Reward & told him that M^{r} Collins could help him to it, & therefore he had heard that M^{r} Collins had my Method, that is my Analysis per serires numero terminorum infinitas. For I had sent my Method to M^{r} Collins in no other paper then that. M^{r} Collins instead of sending what M^{r} Leibnitz desired, joyned with Oldenburg in solliciting me to answer M^{r} Leibnitz's Letter. And thereupon I wrote my Letter of 13 Iune 1676, & after I had described

my method of Series & some other Methods (meaning principally the Method of fluxions) became so general as to extend to almost all Problemes except perhaps some numeral ones like those of Diophantus. And M^r^ Leibnitz in his Letter of 27 Aug. 1676 replied that he did not belive that the Method was so general, there being many Problems & particularly the inverse Problems of Tangents not reducible to equations. And this is another argument that he had not yet found the differential method.

In October following he came to London & there met with D^r^ Barrows Lectures & saw my Letter of 24 Octob. 1676 & therin had fresh notice of the said method & of my Compendium of series sent by D^r^ Barrow to M^r^ Collins in the year 1669, under the title of Analysis per series &c. & consulting M^r^ Collins, saw in his hands several of mine & Gregories Letters especially those relating to series; & in his way home from London was meditating how to improve the Method of Tangents of Slusius as appears by his Letter to M^r^ Oldenburg dated from Amsterdam $\frac{18}{28}$ Novem. 1676. And the next year in a Letter to M^r^ Oldenburg dated 21 Iune, he sent us his new Method with this Introduction: Clarissimi Slusij methodum tangentium nondum esse absolutam celeberrimo Newtono assentior. And in describing this Method he abbreviated D^r^ Barrows Method of Tangents & shewed how it might be improved so as to give the method of Slusius & to proceed in equations involving Surds, & then subjoyned: Arbitror quæ celare voluit Newtonus de tangentibus ducendis ab his non abludere: Quod addit, ex eodem fundamento quadraturas reddi faciliores me in sententia hac confirmat. And after seven years he published the Elements of this Method as his own without mentioning the correspondence which he had formerly had with the English about these matters He mentioned indeed a methodus similis; but whose that Method was & what he knew of it he did not say, as he should have done. And this <459r> his silence put me upon a necessity of writing the Scholium upon the second Lemma of the second Book of Principles, least it should be thought that I borrowed that Lemma from M^r^ Leibnitz. In my Letter of 24 Octob. 1676, when I had been speaking of the method of fluxions I added: Fundamentum harum operationum, satis obvium quidem, quoniam non possum explicationem ejus prosequi, sic potius celavi 6accdæ13eff7i3l9n404qrr459t12vx. And in the said Scholium I opened the ænigma, saying that it conteined the sentence Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice versa. For I looked upon this as a sufficient security without entring into a wrangle: but M^r^ Leibnitz was of another opinion.[1]

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In the end of the year 1669 M^r^ Collins sent notice to M^r^ Iames Gregory that I had a general method of Series & M^r^ 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.

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At the end of the Remarks – – – – – – – – top of his skill at that time.

The Book of Quadratures was written in the year 1676 being for the most part extracted out of a Tract which I wrote in the year 1671 but did not finish & out of some other older Papers. The first Proposition Data æquatione quotcunque fluentes quantitates involvente invenire fluxiones is comprehended verbatim in the AEnigma which I set down in my Letter of the 24^th^ of October 1676. In that Letter I set down a Series for squaring of figures which in some cases breaks of & becomes finite, & illustrated it with examples & said that I found that & some others of the same kind by the Method comprised in that Enigma that is by the Method of fluxions; & how I found them I explained in the first six Propositions of the book of Quadratures & I do not know of any other method whereby they could be found; & therefore when I wrote that Letter I understood the Method of fluxions so far as it is comprehended in those six Propositions. When I had finished the book & the 7^th^ 8^th^ 9^th^ & 10^th^ Propositions were fresh in memory I wrote upon them to M^r^ I. Collins that Letter which was dated 8 Novem. 1676, & printed by M^r^ Iones. The Theorems at the end of the tenth Proposition for compoaring curvilinear figures with the Conic Sections were known to me when I wrote the said Letter of 24 Octob 1676, the Ordinates of the Figures being there set down. They were copied from the Tract which I wrote in the year 1671. To understand these two Letters & how to find those Theoremes

requires skill in the Method of fluxions so far as it is comprehended in all the first ten Propositions of the Book. And the Book conteins but one Proposition more.

The first Proposition of this Book with the solution & examples were printed almost verbatim in the second Volume of the works of D^{r} Wallis A.C. 1692 & came abroad the next year. And thus the Rule for finding second third & fourth fluxions was published some years before the Rule for finding second third & fourth differences, & was long before in manuscript. In the Introduction to this Book the method of fluxions is taught without the use of prickt letters; for I seldome used prickt letters when I considered only first fluxions: but when I considered also second third & fourth fluxions I distinguished them by the number of pricks. And this notation is not only the oldest but is also the most expedite tho it was not known to the Marquess de l'Hospital when he recommended the differential Notation.

M^{r} Collins in his Letter to M^{r} Berte dated 26 Iuly 1672 wrote thus of what I communicated to D^{r} Barrow in the year 1669 & before. Mense Septembri 1668 – – – – haud integrum ducit. Gregory found the Method of series proprio Marte but did not claim it because he knew that he was not the first inventor. By the testimony of Barrow grounded upon papers communicated to him from time to time, M^{r} Collins in this Letter wrote that it appeared that I had the Method conteined in the Analysis per æquationes numero terminorum infinitas some years before the Doctor sent this Analysis to part of this Method is to square figures accurately when it may be done or else by perpetuall approximation: & this requires skill in the method of fluxions so far at least as it is conteined in the first six Propositions of the Book of Quadratures as was said above. Nothing has been said to prove that M^{r} <429r> Leibnitz had the method before he came to London the second time. Then he met with D^{r} Barrows Lectures & the Marquess de l'Hospital has said that where the Doctor left off M^{r} Leibnitz proceeded, & that the improvement which he made to the Doctors methods consisted in shewing how to exclude fractions & radicals. But the Marquess did not know that by my Letters of 10 Decem 1672 & 24 Octob 1676 he had notice from me that this improvement was to be $_{\text{done.}}$ | $^{\text{made.}}$ He might afterwards find it proprio Marte, but by that notice knew that I had it before him. For in his letter of 21 Iune 1677 wherein he first communicate his Method he acknowledged that I knew it when I wrote my Letter of 24 Octob. 1676. And a copy of my Letter of 10 Decem 1672 was sent to him (amongst Gregories papers) in Iune preceding. And in the Scholium upon the second Lemma of the second Book of Principles I put him in mind of this in a friendly manner And yet in his Letter of 29 Decem 1711 he claimed a right to the method as Inventor & justified what was published in the Acta Eruditorum for Ianuary 1705, where he is called the Inventor, & from his being so is drawn this Conclusion: Pro differentijs igitur Leibnitianis Newtonus adhibet, semperque [pro ijsdem] adhibuit, fluxiones — ijsque tum in Principijs Naturæ Mahtematicis, tum in alijs postea editis [pro differentijs illis] eleganter est usus, quemadmodum et Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianæ methodo substituit.

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At the end of the Remarks after the words du même genre de celles que nous venons de marquer, add. When M^{r} Leibnitz wrote his Letter of 27 Aug. 1676 he placed the perfection of Analysis in another method composed of Analytical Tables of Tangents & the combinatory Art. Nihil est, said he, quod norim in TOTA Analysi momenti majoris. And a little after: Ea vero nihil differt ab Analysi illa SVPREMA ad cujus intima Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum. This was the top of his skill at that time.

<430r>

At the end of the Remarks after the words du même genre de celles que nous venons de marquer, adde. When M^{r} Leibnitz wrote his Lettre of 27 Aug. 1676 he placed the perfection of Analysis in another method composed of Analytical Tables of Tangents & the Combinatory Art. Nihil est, said he, quod norim in tota Analysi momenti majoris. And a little after: Ea vero nihil differt ab Analysi illa SVPREMA ad cujus intima quantum judicare possum Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum. This was the top of his skill at that time.

I wrote the Book of Quadratures in the year 1676, (except the Introduction & the Scholium at the end of it) extracting most of it out of old papers. And when I had finished it & the 7th 8th 9th & 10th Propositions were fresh in memory I wrote upon them to Mr Collins that Letter which was dated 8 Novem. 1676 & published by Mr Iones The Table in the end of the 10th Proposition for comparing Curvilinear Areas with the Conic Sections is mentioned in my Letter of 24 Octob. 1676 & the very words of the first Proposition of the Book are also there set down enigmatically; and the contents of these two Letters require skill in the whole book to understand them.

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 & also the Method of extracting fluents out of equations involving fluxions mentioned in my Letter of 24 Octob. 1676 & copied from an older paper; & an explication of the method direct & inverse comprehended in the sentence Data æquatione quotcunque fluentes quantitates involvente invenire fluxiones, & vice versa: & the Doctor printed them all the same year in the second Volume of his works which was then in the Press & came abroad the next year (A.C. 1693) two years before the first Volume was printed off. And thus the Rule for finding second third & fourth fluxions was published some years before the Rule for finding second third & fourth differences & was long before in manuscript. In the Introduction to this Book the Method of fluxions is explained without the use of prickt letters. For I seldome used prickt letters when I considered only first fluxions But when I considered also second third & fourth fluxions, I distinguished them by the number of pricks. And this Notation is the most expedite & fit for use, but was not known to the Marquess de l'Hospital when he recommended the differential Notation.

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Sir

Vpon reading the Letters which you are printing I see no need of saying any thing further about that matter. When Mr Collins sent one of my series to Mr Iames Gregory with notice that I had a genera**{**l**}** method of squaring curvilinear figures by such series, Mr Gregory after a years study found out the method proprio Marte but yet never claimed a right to it because he knew that he was not the first Inventor.

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Corrigenda

Pag 8 lin 13 dele c'est a dire, commune ou superficiell. Pag. 16. l. 5 scribe A Londres Feb. 26, $17\frac{15}{16}$ st. vet. Pag. 17. l. 13 scribe en Iuillet 1714. Pag 19 l. 20 desquelles. Pag. 46. l. 11 en 1677. P. 75 lin 15 scribe Iuin 1713. Pag. 78 l. 9 scribe 24, 26. Pag 8 lin 6 Post Nature adde et qui n'ont rien de marveilleux. Pag. 87 lin 6 & 9 dele designoient & scribe enveloppoient. Pag 88 lin 2, Adde, And that I usually put letters without pricks for fluxions where I considered only first fluxions, but where I considered also second, third, & fourth fluxions &c (as for instance in extracting fluents out of equations involving fluxions) I distinguished them by letters with one two three four or more pricks; a notation which is more convenient then that of Mr Leibnitz tho not necessary to the method. And that when Mr Leibnitz by his Letter of 12 May 1676 had put me upon resuming the consideration of these things I wrote the Book of Quadratures (escept the Introduction & Conclusion) extracting most of it out of old papers; & when I had newly finished the tenth Proposition with its Corollaries, & they were fresh in memory I wrote upon them that Letter to Mr Collins which was dated 8 Novem. 1676 & published by Mr Iones; & 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, & that in the same Letter where I said that the general Theoremes there mentioned for squaring of Curves were founded on the method of fluxions, I had relation to the six first Propositions of the book of Quadratures: In writing the book of Principle I made much use of this method direct & inverse but did not set down the calculations because the book was writ by the method of Composition as all Geometry ought to be. And At the request of Dr Wallis when he was printing the 2d Volume of his works I sent him in two Letters dated 27 Aug & 17 Sept: 1692 the first Proposition of the Book of Quadratures copied almost verbatim illustrated with examples in first & second

fluxions, & the method of extracting fluents out of equations involving fluxions mentioned in my Letter of Octob. 24, 1676, & the Doctor printed them the same year (viz^{t} A.C. 1692) in the second Volume of his works pag 391, 392, 393, 394, 395, & this was above two years before the Doctor heard any thing of the Differential method of M^{r} Leibnitz. <432v>

Pag. 93. After venons de marquer adde In that Letter he did not duly deny that the difficulter Problemes could be reduced to converging series, & that inverse Problems of tangents & many others could be reduced to æquations but also mentioned a method by Analytical Tables & the Combinatory Art as the top of his skill in Analysis at that time. Nihil est, – – – to be perfected

By the inverse Method of fluxions I found in the year 1677 the Demonstration of Keplers Proposition which is the eleventh Proposition of the Book of Quadratures & in the year 1683 at the importunity of D^{r} Hally I resumed the consideration thereof & added some more Propositions about the motions of the heavenly bodies which were communicated by him to the R. Society & entred in their Books the winter following & upon their request that those things might be published I wrote the Book of Principles < insertion from above the line > in the years 1684, 1685, <433r> 1686 < text from f 432v resumes > & 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 by disuse been forgetting the Methods by which I wrote it.

Pag 93. After the words venons de marquer, add. < insertion from above the line > When he wrote < text from f 432v resumes > that Letter he placed the perfection of Analysis in another method founded on Analytical Tables of Tangents & the Combinatory Art. Nihil est, saith he, quod norim in tota Analysi momenti majoris And a little after: Ea vero nihil differt ab Analysi illa SVPREMA ad cujus intima, quantum judicare possum, Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum.

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– as their Ancestors did before they corrupted themselves. For the first of the moral Precepts of the Noachides was to worship no other Gods then one.

Methods by which I wrote it.

When M^{r} Leibnitz wrote his Letter dated 27 Aug. 1676, he placed the perfection of Analysis not in the Differential Method but in another Method founded on Analytical Tables of Tangents & the Combinatory Art. Nihil est, saith he, – – – – – cogitationum humanarum. When he was in London the second time viz^{t} in October 1676 he met with D^{r} Barrows Lectures, & M^{r} Iames Bernoulli in a Paper printed in the Acta Eruditorum for Ianuary 1691 wrote thus: Qui calculum Barrovianum (quem decennio ante [i.e. ante editionem Elementorum Calculi Leibnitiani] in Lectionibus suis Geometricis adumbravit auctor, cujusque specimina sunt tota illa Propositionum inibi contentarum farrago) intellexerit, alterum a Dn. L. inventum ignorare vix poterit; utpote qui in priori illo fundatus est, et nisi in differentialium notatione & operationis aliquo compendio ab eo non differt. The Marquess de l'Hospital, in the Preface to the Analysis des Infinitement Petits published A.C. 1696, hath told us that the improve which M^{r} Leibnitz made to D^{r} Barrows Methods consisted in excluding fractions & surds from the calculation: but he did not then know that I had given M^{r} Leibnitz notice of this improvement in my Letters of 10 Decem 1672 & 24 Octob. 1676.

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When the Marquess de l'Hospital wrote his book de infinite Petites & affirmed that the Differential notation was more convenient then mine, he meant my Notation in the second Lemma of the second Book of Principles: for he had then seen no other. That notation I have not used in Calculations: I used it only in demonstrating that Lemma synthetically. The method of extracting fluents out of Equations involving

fluxions I invented in the end of the year 1671 or beginning of the year following & at the request of D^{r} Wallis sent it to him Septem. 17 1692 & he inserted it into the second Volume of his works which were printed the next year & came abroad two years after. And here I used prickt letters. In the year 1676 I wrote the Book of Quadratures except the Introduction & Conclusion which were written many years after I extracted most of it out of old Papers. And after I had finished the tenth Proposition with its Corollaries I wrote to M^{r} Collins my Letter of Octob. 8 1676 published by M^{r} Iones. This Letter has a particular relation to the second Corollary of that Proposition & conteins things which were fresh in memory when I had newly composed that Proposition &

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Corrigenda.

Pag. 8. lin. 6. After Nature add et qui n'ont rien de marvelleux Pag 8 lin. 13 omit c'est a dire, commune ou superficiell. Pag. 12. l. 23. dele par ordre de la Societé Royale Pag. 16. lin. 5 write the date of the Letter A Londres Feb. 26, 17$\frac{15}{16}$ st. vet. Pag. 17. l. 13 write en Iuillet 1714. Pag 19 l. 20 desquelles. Pag. 46 lin 11 write en 1677. Pag 75 lin 15 write Iuin 1713. Pag. 78 lin 9 write 24, 26. Pag. 87 lin 6 & 9 omit designoient & write enveloppoient. Ib. l. 12 write dans mes Lettres du 10 Decem 1672 & 24 Octobre 1676. Pag. 88. lin. 2. add. And that I usually In using the method of fluxions I commonly put letters without pricks for fluxions where I considered only first fluxions, but where I considered also second third & fourth fluxions &c (as for instance in extracting fluents out of equations involving fluxions) I distinguished them by letters with one two or more pricks, a Notation which I reccon more convenient then that of M^{r} Leibnitz tho not necessary to the method. As I found the methods of Series & Fluxions in the year 1665 so I found the Theory of Colours in the beginning of the next year & in the y ear 1671 was upon a design of publishing them all; but for a reason mentioned in my Letter of 24 Octob. 1676 laid aside that designe till the year 1704. However, when M^{r} Leibnitz by his Letter of 12 May 1676 had put me upon resuming the consideration of the methods of Series & Fluxions I wrote the book of Quadratures (excepting the Introduction & Conclusion) extracting most of it out of old papers; & when I had newly finished the tenth Proposition with it's Corollaries & they were fresh in memory I wrote upon them that Letter to M^{r} Collins which was dated 8 Novem. 1676 & published by M^{r} Iones. The Tables at the end of that 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 generall Theoremes there mentioned for squaring of Curves were founded on the Method of fluxions, I meant the method described in the first six Propositions of the Book of Quadratures. For I know of no other method by which those Theorems could be invented ② At the request of D^{r} Wallis, when he was printing the second Volume of his works, 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 Octob. 24. 1676 & copied from an old Paper: & the Doctor printed them both the same year (viz^{t} A.C. 1692) in that Volume of his works, pag. 391, 392, 393, 394, 395, 396, together with an explication of the method of fluxions direct & inverse comprehended in the sentence Data æquatione fluentes quotcunque quantitates involvente invenire fluxiones & vice versa. And this Volume came abroad in the year 1693 two years before the first Volume was printed off. ① By the inverse method of fluxions I found in the year 1677 the Demonstration of Kepler's Astronomical Proposition which is the eleventh Proposition of the first Book of Principles, & in the year 1683 at the importunity of D^{r} Halley 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 Books the winter following, & upon their request that things might be published, I wrote the Book of Principles in <435r> 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.

Pag. After the words venons de marquer, add. When he wrote that Letter he placed the perfection of Analysis in another method founded on Analytical Tables of Tangents & the Combinatory Art. Nihil est, saith he, quod norim in tota Analysi momenti majoris. And a little after: Ea vero nihil differt ab Analysi illa

SVPREMA ad cujus intima, quantum judicare possum, Cartesius non pervenit. Est enim ad eam constituendam opus Alphabeto cogitationum humanarum.

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In the second part of his Postscript he tells you that if all bodies be heavy gravity must be a scholastick occult quality & a miracle. notwithstanding that it may be supposed to act constantly by a certain law imprest by God upon the nature of things; that is to say it must a miracle tho it be no miracle. For Miracles are so called because they happen seldome & for that reason create wonder. All qualities are occult whose causes are not known, & M^{r} Leibnitz has not yet told us the cause of Gravity. But a Scholastick occult quality is that whose cause in our opinion cannot be found out because it was unknown to Aristotle, & no body can go beyond him. M^{r} Newton holds no such opinion, but leaves it to every man to find out the cause of gravity if he can.

But M^{r} Leibnitz insinuates that gravity must be caused by the action or impulse of some bodies or subtile matter & the matter which causes gravity cannot gravitate it self. He goes upon the Hypothesis of the materialists viz that all the phænomena in nature are caused by mere matter & motion & man himself is a mere machine His body is not actuated by any mind but moves by mere mechanism. And his zeale for this precarious hypothesis makes him rail at M^{r} Newton's universal gravity. He denys none of M^{r} Newtons experiments. He denys not this third Rule of Philosophy. And yet from the Experiments & that Rule universal gravity necessarily follows. But he denys the conclusion. And indeed he has a very good faculty at denying conclusions. That third Rule is the Rule of Induction. And without it no Proposition can become general in Naturall Philosophy. Without it we cannot affirm that all bodies are impenetrable. And the argument by Induction for universal gravity is as strong as the argument for universal impenetrability. Yet Arguments from Induction are not Demonstrations. They are only to take place till some experimental exception can be found. And if M^{r} Leibnitz out of zeale for the Hypothesis of the Materialists will except his subtile matter, the exception will do M^{r} Newton's Philosophy no harm. And by theh same liberty any body else may except the Impenetrability of the particles of his subtile matter.

He saith that God is Intelligentia supramundana because he is not the soul of the world & has no need of a Sensorium: as if the soul of a man would be the soul of the pictures of visible objects made in the sensorium if it were in the place where they are made, or as if any man (except the Anthopomorphites ever feigned that God had a Sensiorium in a litteral sence. But what he means by banishing God out of the world wants an explication Doth he mean that God is beyond all space: a being that's nusquam, And is he angry at M^{r} Newton for saying that God is every where. & that he is not far from every one of us: for in him we live & move & have our being.

He saith that he is astonished that M^{r} Newton should beleive that God hath made the world so ill that that like a watch it would at length cease to go without the extraordinary hand of God; & that this is to have very narrow Ideas of Gods wisdom & power. And by the same Argument any man may affirm that God was able to endow matter with an active & self moving principle, & enable it to think, & therefore has done it because he is wise & good, & that God created the world from all Eternity & made it a being absolutely perfect because he was able to do so, & that to deny all this is to have narrow ideas of Gods power & wisdome & goodness.

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He commends experimental Philosophy, but adds that when experiments are wanting, it is allowed to imagin Hypotheses, & expect till new experiments shall determin which of them are true. & upon this account thinks his philosophy may be justified. But he should consider that Hypotheses are nothing more then imaginations, conjectures, & suspicions & ought not to be propounded as Truths or Opinions nor admitted into Philosophy as such untill they are verified & established by experiments. And if you consider his Philosophy you will find that it consists generally in such Hypotheses as can never be established by experiments: Such as are That God is intelligentia supramundana, that the bodies of animals are moved not by the mind or will of the animal but mechanically by an Harmonia præstabilita that all the Phænomena in Nature are purely mechanical. That the world is so perfect that it can last for ever without running into disorder, that the Planets revolve in Vortices, That God has never intermedled with the frame of things since the first creation.

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It's not impossible but that an exception may be found in time. But a mere hypothesis or supposition of an exception is no exception. The exception out to be experimental. The meaning of Conclusion made by Induction is that they are to be looked upon as general till some reall exception appeare. And in this sense gravity is to be looked upon as universal . To make an exception upon a mere Hypothesis is to feign an exception. It is to reject the argument for Induction, & turn Philosophy into a heap of Hypotheses, which are no better then a chimerical Romance.

[1] **{illeg}** p. 2 × 6 Duplicate of this passage in the Supplement **{**to the**}** Remarks