# Papers relating to the origin of the dispute

**Author:** Isaac Newton

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## Origin of Commercium Epistolicum[1]

In the year 1671 Mr Newton was upon a designe of publishing his Theory of colours & Method of fluxions, but upon wrangling disputes arising about the theory of colours, he chose the sake of a quiet life to lay his designe aside before the Tract about the Method of fluxions was finished. There wanted that part which related to the solution of Problemes not reducible to Quadratures. In this state things rested till the year 1676, & then he composed the Book of Quadratures extracting it out of his old Papers, & in the year 1704 published this Tract & his Theory of colours, & in the Introduction of the Tract of Quadratures said that he found the method of fluxions gradually in the years 1665 & 1666, this being not so much as Dr Wallis had said nine years before (in the Preface to the first Volume of his works) without being then contradicted. And the next year in the Acta Eruditorum for Ianuary, a Paper was published without the name of the Author which is giving an account of the said Tract accuses it of plagiary in these words. Ingeniosissimus deinde Author [Newtonus] antequam ad Quadraturas Curvarum (vel potius figurarum curvilinearum) veniat præmittit brevem Isagogem. Quæ ut MELIVS INTELLIGATVR, sciendum est, cum magnitudo aliqua continuò crescit, incrementa illa momentanea appellari differentias, nempe inter magnitudinem quæ antea erat et quæ pen mutationem momentaneam est producta; atque hinc natum este Calculum integralem eique reciprocum summatorium, cujus elementa ab INVENTORE D. Godefrido Guilielmo Leibnitio in his Actis sunt tradita, varijque usus tum ab ipso tum a fratibus Bernoulijs tum a D. Marchione Hospitalio — sunt ostensi Pro diffentijs IGITVR Leibnitianis D. Newtonus adhibet semperque [pro ijsdem adhibuit Fluxiones — ijsque tum in suis Principijs Naturæ Mathematicis tum in alijs postea editis [pro differentijs Leibnitianis] eleganter est usus, QVEMADMODVM et Honoratus Fabrius in sua synopsi Geometrica, motuum progressus Cavallerianæ methodo SVBSTITVIT. Dr Menkenius who printed this defamatory Paper ought to discover the name of the author that he may either prove the accusation or be looked upon as guilty of calumny.

This accusation gave a beginning to the late controversy about the author of the Method. For when the accusation was contradicted by Dr Keill, Mr Leibnitz wrote twice to the R. S. against the Dr & in his second Letter dated 29 Decem 1711 justified the accusation saying that the Acta Lipsiensia had given every man his due & claimed a right to the Invention & represented that n**{**o**}** body had gone before him in it; & only palliated the accusation by saying that he & his friends allowed that Mr Newton attained the method by him <97v> self. And all this as much as to say that Mr Newton was not first inventor, & therefore committed a Act of Plagiary in pretending to have found the Method gradually in the years 1665 & 1666 whereby he gave himself a right to the method. For second Inventors have no right. It lay upon Mr Leibnitz therefore to prove that he was the first inventor.

Mr Leibnitz pressing the R. Society to condemn & silence Dr Keill they appointed a Committee to search out old Letters & Papers relating to this matter & report what they found in them, & then ordered the Papers & Report to be publist. And Mr Leibnitz avoided answering this Commercium Epistolicum to the day of his death: For the Book is matter of fact & uncapable of an answer, & establishes Mr Newton the first Inventor.

To avoid answering it he pretended the first year that he had not seen it nor had leasure to examin it, but had appealed to the judgment of the first rank well skilled in these things & impartial desiring him to examin it & give his judgment upon it. And his judgment dated 13 Iune 1713 was inserted into a defamatory Letter dated Iuly 29 following, & published in a flying paper without the names of the Authors or printer or city where it was printed & dispersed it over all the western parts of Europe: a back biting infamous way of proceeding which in England is punishable by the civil Magistrate. This Paper has been since translated into French & inserted into another abusive Letter & answered by Dr Keill in Iuly 1714 & no Answer has yet been given to the Doctor. In his paper Mr Newton is accused of plagiary by both the authors in a more open manner & in a higher degree then before, & therefore by the laws of all nationes the Authors ought to have proved their acusation upon pain of being deemed guilty of calumny. The Iudge pretends that when the Letters published in the Commercium were written Mr Newton did not so much as dream of his calculus of fluxions because there are no prickt letters in them; no nor when he wrote his book of Principles, these letters first appearing in the third Volume of the book of Dr Wallis many years after the differential calculus had obteined every where, & Mr Newton not knowing how to find the differences of differences long after it was familiar to others. But this impartial Iudge gave judgment contrary to the evidence which lay before him. For the Method of fluxions is taught without prickt letters in the Introduction to the very Book de Quadratura Curvarum, & prickt letters with the Rule for finding all degrees of fluxions were published in the second Volume of the works of Dr Wallis A.C. 1693, three years before any Rule for finding all degrees of differences came abroad. This candid Iudge cited Mr Bernoulli by the name of an eminent Mathematician as if Mr Bernoulli was not the author of the Iudgment & yet Mr Leibnitz in his Letters to Mr l'Abbé Conti, the Comtess of Kilmansegge & Baron Bothmar that Mr Bernoulli himself was the author.

Mr Leibnitz in a Letter to Mr Chamberlain dated from Vienna 28 Apr. 1714, wrote that he had not yet seen the Commercium Epistolicum & so could not make such an Appology as the thing required: & a little after he wrote to Mr Chamberlain that when he came to Hanover might print another Commercium & for that end desired that the Original Letters might be sent to him to be printed entire**{**ly**}** & impartially. But upon reading this Letter to the Society, it was represented <98r> that this was a reflexion upon their Committee, the the Originalls ought to be kept for justifying what had been published by the Order, & that Mr Newton was so far from publishing the Commercium himself that he did not so much as produce some ancient Letters in his own custody, & it as improper that Mr Leibnitz himself should be trusted with printing a Commercium, at least not untill the ancient Letters in his Custody should be examined & approved by them who knew the hands, & at the same time Mr Newton produced two ancient Letters which he had in custody without producing them to be published in the Commercium, the one written to him by Mr Leibnitz himself from Hanover $\frac{7}{17}$ Martij 1693, the other writter to him by Dr Wallis from Oxford Apr. 10th 1695. The first shews that Mr Leibnitz himself gave Mr Newton the preference till the beginning of the year 1693 at which time he knew nothing more of Mr Newtons Method then what he had learnt from his Letters & Papers writ in or before the year 1676 & from his book of Principles & the second (compared with the Preface to the first Volume of the Doctors works) shews what opinion the English Mathematicians had of this matter when they first heard that the Differential Method began to be celebrated in Holland as invented by Mr Leibnitz. These two Letters being examined & approved befor the Society were ordered to be laid up in their Archives & an Anwer was given to Mr Chamberlain that if Mr Leibintz had any ancient Letters relating to this matter & would send them to any friend in London to be examined before the R. Society by them who knew the hands, after they were approved he might either print them himself or have them printed in the Philosophical Transactions if he pleased: but nothing has been sent.

About November or December 1715 Mr Leibnitz in a Letter to Mr Abbe Conti wrote a large Postcript relating to these matters, railling at the Commercium Epistolicum as attaquing his candor by false interpretations & omitting what made for him or against Mr Newton, & saying that his adversaries should not have the pleasure to see him return an answer to their slender reasonings, & endeavouring to run the dispute into a squabble about universal gravity, & occult qualities & miracles & Gods being not the soul of the world but intellegentia supramundana nor having need of a sensorium, & about atoms & the nature of space & time, & about solving mathematical Problems. All which are digressions prevarications & evasions serve to no other purpose then to avoid answering the Commercium Epistolicum. [by running the dispute into a squabble about other matters.] And when he was pressed to let these digressions alone & return an Answer to the Commercium he replied in a Letter from Hannover Apr 9$^{th}$ 1716 that: M$^r$ Newton being pressed to write an Answer to this Postscript that both might be shewed to the King, wrote his Letter of Feb 26 $17^{15}_{16}$, & therein told M$^r$ Leibnitz of his endeavouring to avoid answering the Commercium Epistolicum by pretending the first year that he had not seen the Book & afterwards by running into all these digressions & saying that his adversaries should not have the pleasure to see him answer it. He pressed M$^r$ Leibnitz therefore to answer the book & told him that he was the aggressor & had brought an accusation of plagiary & by the laws of all nations was bound to prove his accusation upon pain of being deemed guilty of calumny.

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M$^r$ Leibnitz in his answer dated from Hanover 9 Apr. 1716 st. nov. pretended that he was not the aggressor nor had accused M$^r$ Newton of plagiary, & refused to answer the Commercium Epistolicum saying that to answer it from point to point would require another book as bigg at the least as that & that he must for that end enter into a great examination of many ${}_{\text{minutes}}$ | ${}^{\text{particulars}}$ passed 30 or 40 years agoe, of which he remembred but little; & that he must search his old Letters many of which were lost, besides that for the most part he had kept no minutes of his own Letters, & the others were buried in a great heap of Papers which he could not unravel but with time & patience. But he had little or no leasure for that, being engaged at present in business of a very different nature. [And in the end of his Letter he agreed with M$^r$ Newton that the Accusers ought to prove their accusations upon pain of being deemed guilty of calumny. but said that he himself was accused]

In the Elogium of M$^r$ Leibnitz published in the Acta Eruditorum for Iuly 1717, pag. 335 there are these words . Quo perspicerent intelligentes quid de tota illa controversia sentiendum sit Commercio Epistolico Anglorum, [D. Leibnitius] aliud quoddam suum idemque amplius opponere decreverat & paucis ante obitum diebus Cl. Wolfio significavit se Anglos famam ipsius lacessentes reipsa refutaturum: quamprimum enim a laboribus historicis vacaturus sit, daturum se aliquid in Analysi prorsus inexpectatum & cum inventis quæ hactenus in publicum prostant sive Newtoni sive aliorum nil quicquam affine habens. Here M$^r$ Leibnitz a few days before his death wrote that he would refute the English by a new & wonderful analytical invention of a different kind from any thing yet extant, & the Author of the Elogium thinks it was by a new Commercium Epistolicum. But what ever it was, we were to stay for it till his historical labours should be at an end. And the world was to suspend their judgments about this matter till M$^r$ Leibnitz could be at leasure to be heard. [And yet the whole series of the Letters between M$^r$ Leibnitz & M$^r$ Oldenburgh is already published so far as it relates to this matter, & it does not appear that he had any correspondence with the English in those days but by means of M$^r$ Oldenberg And the Commercium already published is plane matter of fact & uncapable of being confuted by a contrary Commercium.

It has been said that in this Commercium several things which made against M$^r$ Newton have been omitted. But M$^r$ Leibnitz to prove this produced two instances: but failed in them both.

It has been said that the Letters are interpreted falsly & maliciously: But when M$^r$ Leibnitz named an instance of this kind it proved a mistake of his own. For in his Letter of 9$^{th}$ of April 1716 he said that where the Author of the Remarks upon the Commercium Epistolicum said (pag. 108) Sensus verborum est, quod Newtonus fluxiones differentijs Leibnitianis substituit: this interpretation was a malicious one: but M$^r$ Newton in his Observations upon this Letter has shewed that M$^r$ L. himself has misinterpreted the place.

It has been said that M$^r$ Leibnitz found the differential Method by himself. And so he <99r> might not withstanding any thing in the Commercium Epistolicum to the contrary. The Committee of the R. S. say they take the proper question to be, not who invented this or that Method, but who was the first inventor of the method. D$^r$ Wallis in the Preface to the first Volume of his works said that M$^r$ Newton in his Letters of Iune 13 & Octob 24 1676 had explained to M$^r$ Leibnitz the Method found by him ten years before that time or above: but he meant nothing more then that M$^r$ Newton had given him so much light into it as made it easy for him to find it out.

Seing therefore that M$^r$ Leibnitz & his friends have deserted the Question: for putting an end to this dispute, we will only add a few observations for wiping off the aspersion u

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— In this state things rested till the year 1676, & then he composed the Book de Quadratura Curvarum extracting it out of the aforesaid Tract & other older papers & in his Letter of Octob 24 1676 cited the first Proposition of the Book verbatim in an Ænigma as the foundation of the method, & said that this foundation gave him Theorems for squaring Curvilinear figures, the invention of which is explained in the first six Propositions of the Book, & in the same letter copied the Ordinates of the Curves which in the end of the tenth Propositions are squared by the Conic Sections, & wrote a Letter to M$^r$ Collins dated 8 Novem. 1676 relating to the 7$^{th}$ 8$^{th}$ 9$^{th}$ & 10$^{th}$ Propositions of the book: all which make it sufficiently appear that the Book was then in MS.

In autumn 1690 D$^r$ Halley & M$^r$ Raphson coming to Cambridge took the Book with them to London & in the end of the year 1692 the first Proposition thereof with the solution & illustra**{**ted**}** by examples in first & second fluxions was printed almost verbatim in the second Volume of the works of D$^r$ Wallis & came abroad the next year. And at length

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Whereas a Paper was published in the Acta Eruditorum for Iuly 1761 in favour of M$^r$ Iohn Bernoulli against D$^r$ Keill, & therein M$^r$ Bernoulli is called <u>excelsum ingenium</u> & <u>vir ad abstrusa et abdita detegenda natus</u>, as if he were not the author of that Paper, & yet the author thereof ascribes it to M$^r$ Bernoulli by calling M$^r$ Bernulli's formula of an Equation <u>meam formulam</u> pag 314 And whereas in a Letter dated 13 Iune 1713 & inserted into another Letter dated 29 Iuly 1713 M$^r$ Bernoulli is cited by the name of an eminent Mathematician as if he were not the auther of that Letter & yet M$^r$ Leibnitz in several Letters has affirmed that he was the author thereof & the designe of this shuffling is to propagate an opinion that the Book of Quadratures published by Sir Isaac Newton in the year 1704 is a piece of plagiary & that M$^r$ Leibnitz was the inventor of the direct method of Fluxions & M$^r$ Bernoully the inventor of the inverse method thereof; & for compassing this designe all endeavours have been made use of to lay aside the ancient Letters & Papers published in the Commercium Epistolicum without answering them & to bring the Question to a squabble about universal gravity & occult qualities, & miracles & the Vacuum, & the sensorium of God & perfection of the world & the nature of time & space & the solving of Problems &c all which are nothing to the purpose: these are therefore to give notice that since M$^r$ Leibnitz & his friends have for above five years declined & still decline answering the Comercium Epistolicum, the friends of D$^r$ Keill will henceforward decline medling with their squabbles.

The first Proposition of the Book de Quadratura Curvarum with its Solution & examples in first & second fluxions was published almost verbatim by D$^r$ Wallis in the second volume of his Works A.C. 1693, (pag. 391, 392, 393, &c) being sent to the Doctor & printed off the year before; & therefore this Book was then in Manuscript. M$^r$ Ralpson has testified publickly that he & D$^r$ Halley had it in their hands at Cambridge about the year 1691 in order to bring it up to London & D$^r$ Halley remembers that this was in A.C. 1690, & thence it may be understood that this Book was MS. before the differential method began to be spread abroad by M$^r$ Iohn Bernoulli & his brother. In M$^r$ Newton's Letters of Iune 13, Octob. 24 & Novem. 8$^{th}$ 1676, there are many things relating to this Book, & particularly the first Proposition is there cited verbatim & therefore it was in MS before M$^r$ Leibnitz knew any thing of the differential Method.

In this Book are many things which had they been proposed as Problemes to be solved by others, might have puzzeled all the Mathematicians in Europe. As for instance, to reduce the integration of the following equations to the quadrature of the conic sections. $\frac{d\dot{z}z^{2n-1}}{e+fz^n+gz^{2n}} = \dot{y}$. $d\dot{z}z^{\frac{1}{2}n-1} = e\dot{y} + f\dot{y}z^n + g\dot{y}z^{2n}$ . $d\dot{z}z^{\frac{3}{2}n-1} = e\dot{y}+$ <100r> $+f\dot{y}z^n + g\dot{y}z^{2n}$ . $d\dot{z}\sqrt{e+fz^n+gz^{2n}} = z\dot{z}$ . $dz\sqrt{\frac{e+fz^n}{g+hz^n}} = z\dot{y}$ . Or to reduce the integration of the following equations to the simplest cases of quadratures. $a\dot{z}^q z^m + b\dot{z}^{q-p} z^n \dot{y}^p = c\dot{y}^q$ .

But the pedantry of proposing Problems to be solved by others is not in fashion in England.

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The Ancients had two Methods in Mathematics which they called Synthesis & Analysis or Composition & Resolution. By the method of Analysis they found their inventions & by the method of Synthesis they composed them for the publick. The Mathematicians of the last age have very much improved Analysis but stop there & think they have solved a Problem when they have only resolved it, & by this means the method of Synthesis is almost laid aside. The Propositions in the following book were invented by Analysis. But considering that the Ancients (so far as I can find) admitted nothing into Geometry before it was demonstrated by Composition I composed what I invented by Analysis to make it Geometrically authentic & fit for the publick And this is the reason why this Book was written in words at length after the manner of the Ancients without Analytical calculations. But if any man who undestands Analysis will reduce the Demonstrations of the Propositions from their composition back into Analysis (which is very easy to be done,) he will see by what method of Analysis they were invented. And By this means the Marquess de l'Hospital was able to affirm that this Book was [presque tout de ce Calcule] almost wholy of the infinitesimal Analysis. [And M$^r$ Leibnitz in a letter to me dated 17 Mar. 1693 st. vet. 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. And in the Acta Eruditorum for May 1700 he allowed that I was the first who by a specimen made publick (meaning in the Scholium upon the 35$^{th}$ Proposition of the second Book) had proved that I had the method of maxima & minima in infinitesimals. < insertion from from the end of the line > And when he himself had composed the first & second sections of the second Book of my Principles in another form of words without calculations he concluded: **{**Om <102r> nia**}** ‡] < text from f 101v resumes >

In the second Lemma of the second Book I set down the elements of this Analytic method & demonstrated the Lemma by composition. in order to make use of it in the demonstration of some following Proposition. And because M$^r$ Leibnitz had published those Elements a year & some months before without making any mention of the Correspondence which I had with him By means of M$^r$ Oldenburg ten years before that time, I added a Scholium not to give away the Lemma but to put him in mind of that correspondence. For in my Letter Letter dated 13 Iune 1676 <102r> I said that the method of converging series in conjunction with some other methods (meaning the methods of fluxions of extracting fluents & of arbitrary series) extended to almost all Questions except perhaps some numeral ones. like those of Diophantus. And in his Answer dated Aug 27 1676 he replied that he did not beleive that my methods were so general there being many Problems which could not be reduced to equations or Quadratures. And in mine dated 24 Octob. 1676, I represented that my Analysis proceeded without stopping at surds, readily gave the method of tangents of Slusius, faciliated Quadratures & gave me converging series for squaring of Curves which become finite when ever the Curve could be squared by a finite equation. & extended also to Problems which could not be reduced to Quadratures. And I said also that I had written a Treatise on this subject five years before, that is, in the year 1671, which was two years before M$^r$ Leibnitz began to study the higher Geometry. And in the foundation of this this Method I said was obvious & set it down enigmatically in this sentence: <u>Data æquatione fluentes quotcunque quantitates involvente invenire fluxiones: & vice versa</u>. And in both my Letters I said that I had then absteined from this subject five years, being tired with it before. And to put M$^r$ Leibnitz in mind of all this of what he had further received from M$^r$ Oldenburg & M$^r$ Collins in these days in relation to thes matters, was the designe of this Scholium.

For At the same time that M$^r$ Oldenburg sent my Letter of 13 Iune 1676 to M$^r$ Leibnitz, (which was Iune 26 following) he sent also (at the request of M$^r$ Leibnitz) a collection of extracts of the papers & Letters of M$^r$ Iames Gregory then deceased. And among those extracts was a Letter of M$^r$ Gregory to M$^r$ Collins dated 5 Sept 1670 in which M$^r$ Gregory represented that he had improved the method of Tangents beyond what D$^r$ Barrow had done, so as to draw Tangents to all Curves without calculation. There was also a Copy of a Letter written by me to M$^r$ Collins 10 Decem 1672 in which I wrote that the Methods of Gregory & Slusius were only Corollarium Methodi generalis quæ extendit se citra molestum ullum calculum non modo ad ducendum Tangentes ad quasvis Curvas sive Geometricas sive Mechanicas vel quomodocumque rectas lineas aliasve Curvas respicientes; verum etiam ad resolvendum alia abstrusiora Problematum genera de Curvitabus. Areis Longitudinibus, Centris gravitatum Curvarum &c. Neque ad solas restringitur æquationes illas quæ quantitatibus surdis sunt immunes. &c. These letters he received in Iuly or August 1676 & in October following coming from Paris to London he there met with D$^r$ Barrows Lectures as he had informed us ⊙ < insertion from f 102v > & in October following he procured D$^r$ Barrows Lectures as above & saw my Letter of Octob 24, [& therein had notice of my Compendium of Series which was the Analysis per series numero terminorum infinitas, [& wanted the demonstration] & consulted M$^r$ Collins to see his correspondence with Gregory & me] And all this < text from f 102r resumes > < insertion from f 102v > ⊙ And all this was enough to put him upon considering how to improv**{e}** the method of Tangents of D$^r$ Barrow as Gregory had done before, so as to draw Tangents without calculation. And then how to improve the method of Tangents of Gregory & Slusius so as to make it proceed without stopping at fractions & surds, & to extend it not only to Tangents & Maxima & Minima but also to Quadratures & other sorts of Problems so as to become such a general as I describe in my Letters of 10 Decem 1672, 13 Iune 1676 & 24 Octob. 1676. < text from f 102r resumes >

In this same year in a Letter dated May 12 M$^r$ Leibnitz desired M$^r$ Oldenburg to procure from M$^r$ Collins the Demonstration of two of my series for finding the Arc of a circle whose sine is given & the sine whose Arc is given; that is, the method of finding them. And in October following coming to London he consulted M$^r$ Collins to see the Mathematical Letters & Papers which M$^r$ Collins had received from M$^r$ Iames Gregory & me. And no doubt he would desire to see the demonstration of the two series which he wanted, that is, the Analysis per series numero terminorum infinitas, which D$^r$ Barrow had sent from me to M$^r$ Collins in Iuly 1669, which Analysis consisted in reducing quantities to converging series & applying those series to the solution of Problems by the Method of Moments & Fluxions. For the direct method of fluxions described in the four first Propositions of the Book of Quadratures is in is here described under this Title: Inventio Curvarum quæ quadrari possunt And there are examples of the inverse method in finding the Areas & lengths of some Curves & demostrating the first Proposition of the Book. And the universality of the Method is described in these words. Quicquid vulgaris Analysis per æquationes ex finito terminorum numero constantes (quando id sit possibile) perficiat hæc per æquationes infinitas semper perficit: ut nil dubitaverim nomen Analyseos etiam huic tribuere — Denique ad Analysin merito pertinere censeatur cujus beneficio Curvarum areæ & longitudines &c (id modo fiat) exacte et Geometrice determinentur. Sed ista narrandi non est locus. This last is comprehended in the fift & sixt Propositions of the Book of Quadratures. And therefore the Method comprehended in the first six Propositions of the Book of Quadratures in the year 1669. And all this may suffice to justify me in <102v> publishing the second Lemma of the second Book of Principles as my own.

By measuring the quantity of water which ran out of a vessel in a given time through a given round hole in the bottom of the vessel: I found that the velocity of the water in the hole was that which a body would acquire in falling half the height of the water stagnating in the vessel. And by other experiments I found afterwards that the water accelerated after it was out of the vessel untill it arrived at a distance from the vessel equal to the diameter of the hole; & by accelerating acquired a velocity equal to that which a body would acquire in falling the whole height of the water stagnating in the vessel, or thereabouts.

In the X$^{th}$ Proposition of the second Book there was a mistake by drawing the Tangent of the Arch GH from the wrong end of the Arch. But the mistake was rectified in the second Edition. And there may have been some other mistakes occasioned by the shortness of the time in which the book was written & by its being copied by an Emanuensis who understood not what he copied; besides the Press faults. For after I had found tenn or twelve of the Propositions relating to the heavens, they were communicated to the R.S. in December 1684, & at their request that the Propositions might be printed I set upon composing this Book & sent it to the R.S. in May 1686 [ as I find entred in their Iournal Books Decem 10 1684 & May 19$^{th}$ 1686. And I was enabled to make the greater dispatch by means of the Book of Quadratures composed some years before.

Multa ex his deduci possent praxi accommodata, sed nobis nunc fundamenta Geometrica jecisse sufficerit, in quibus maxima consistebat difficultas. Et fortassis attente consideranti vias quasdam novas vel certe satis antea impeditas aperuisse videbimur. Omnia autem respondent nostræ Analysi infinitorum, (cujus elementa quædam in his Actis dedimus) communibus quoad licuit verbis hic expresso.

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M$^r$ Leibnitz has encouraged a practise of claiming a right to other mens inventions as second inventor & has claimed such a right himself to the differential method of Mouton, & offered M$^r$ Newton such a right to the infinitesimal method or method of fluxions And yet second Inventors have no right. The sole right is in the first inventor untill another man finds out the same thing. and then to take away the right of the first inventor & divide it between him & that other would be an Act of injustice. It lies upon M$^r$ Leibnitz therefore to renounce all right to the differential method of Mouton & to all other inventions whatsoever as second inventor & to forbeare to encourage this practise in others as tending to plagiary.

M$^r$ Leibnitz has claimed a right to a property of a series of numbers natural, triangular, pyramidal, triangulo-triangular &c published before by M$^r$ Paschal, & to make it his own has represented that he wondered that M$^r$ Paschal should omit it. He is therefore to renounce all right to the invention of this property.

M$^r$ Leibnitz is further desired to explain what he meant by these words in his Letter of 21 Iune 1677 : Clarissimi Slusij Methodum Tangentium nondum esse absolutam Newtono assentior: et jam a multo tempore rem Tangentium generalius tractavi scilicet per differentias Ordinataru:: Hee might meane only that he had long ago understood the method of tangents of D$^r$ Barrow or further that he had long ago changed the letters a & e used by D$^r$ Barrow into the symbols dy & dx, or further that he had long ago made it still more general so as to proceed without taking away fractions or surds. or that he had long ago made it still more general so as to extend it into Quadratures & & curvatures of Curves & into the reduction of inverse Problemes of tangents & others to converging series & to differential aquations & Quadratures [ And he is desired also to tell us whether whether by these words he intended to make himself the first or second inventor of the Differential method & if he will not explain them himself he authorises us to take them in that sence which seems most obvious to us, viz$^t$ that he had a multo tempore, long before those days found out the Differentiall method so far that he had found it above nine years before he published it. And then he is to beg M$^r$ Newton's pardon for pretending to have found the differential method long before he had found it, in order to make himself the first inventor. For its most certain that he had it not when he wrote his Letter of 27 Aug. 1676.

M$^r$ Leibnitz in his Answer to M$^r$ Fatio published in the Acta Eruditorum for May 1700, has said: Certe cum elemente calculi mea edidi anno 1684, ne constabat quidem mihi aliud de inventis Newtoni in hoc genere quam quod ipse olim significaverat in literis, posse se tangentes invenire non sublatis irrationalibus, quod Hugenius quoque se posse mihi significavit postea, etsi cæterorum istius calculi adhuc expers: sed majora multo consecutum Newtonum, viso demum libro Principiorum ejus, satis intellexi. And yet it's certain by his Letter of 27 Aug. 1676 that he did then know that M$^r$ Newton's method extended not only to the drawing of Tangents but also faciliated the Quadrature of curves. His words are <103v> Arbitror quæ celare voluit Newtonus de Tangentibus ducendis ab his non abludere Quod addit, ex hoc edem fundamento, quadraturas quoque reddi faciliores me in sententia hac confirmat, nimirum semper figuræ illæ sunt quadrabiles quæ sunt ad Æquationem Differentialem. Æquationem Differentialem voco talem qua valor ipsius dx exprimitur quæque ex alia derivata est qua valor ispius x exprimebatur. Mr Leibnitz was told also that by the same method Mr Newton determined maxima & minima without taking away irrationals & solved other problems also & had found general Theorems for squaring of Curves by converging series which brake off & became finite æquations in certain cases. And the first of the Theoremes Mr Newton sent him in that Letter with several examples thereof. And therefore Mr Leibnitz ought to beg Mr Newton's pardon for pretending that in the year 1684 when he published his method he knew nothing more of Mr Newtons inventions in this kind, then that he could draw tangents without taking away irrationalls. When he published his method he ought in candor to have told the world what light he had received into it from England, & to have let them know at that time that by the words AVT SIMILI he meant a method invented by Mr Newton & described by him in his Letter of 10 Decem 1672, 13 Iune 1676 & 24 Octob 1676 copies of which had been sent to him by Mr Oldenburg. And he ought also to have acknowledged at the same time what light he had received also from Dr Barrows method of tangents. But to conceale all this, to conceal his whole correspondence with Mr Oldenb: to conceal every thing that he had received from England & afterwards to pretend that he knew nothing more of Mr Newton's method at that time then that it extended to the drawing of Tangents without taking away surds, is a sort of behavior that wants a pardon.

And since in his Letter to Dr Sloan dated Decem 29 1711 he has told us that his friends know how he came by the Differential Method, but is silent about it pretending that Mr Keil being haud satis exercitatus artis Inveniendi arbite**{r}** he [Mr Leibnitz] is not bound to teach him & yet his way of coming by it is in question: it lies upon him in point of candor openly & plainly & without any further hesitation to tell the world how he came by this method

And since in the same Letter he has told us that he had this method above nine years before he published it, & it follows from thence that he had it in the year 1675 or before: it lies upon him to prove that he had it before he wrote his Letter to Mr Oldenburg dated Aug 27, 1676 wherein he affirmed that Problemes of the Inverse Method of tangents & many others could not be reduced to infinite series nor to Equations or Quadratures. Or rather it lies upon him in point of candor to tell us what he means by pretending to have found the method before he had found it. For its most certain by that affirmation that when he wrote that Letter, he had it not.

And whereas in his Letter of Iune 29 1677 he wrote: Clarissimi Slusij methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium longe generalius tractavi; scilicet per differentias Ordinatarum: Which is as much as to say that he had long ago improved the method of Tangent beyond what Slusius had done, And made it non general so as to include that method as a particular branch thereof, & yet in his Letter to Mr Oldenburg dated $\frac{18}{28}$ Novemb. 1676 which was but half a year before, he was contriving to improve the method of Slusius & make it more general & extend it to all sorts of Problems, not by the Differential method but by by means of a Table of Tangents. it lies upon him in point of candor to explain to us what he meant in Iune following when he was but newly fallen into the Differential method, to pretend <104r> that he had thereby improved the method of tangents beyond that of Slusius , jam multo tempore long before those days, & to satisfy that he did it either by accident or inadvertancy or with some other designe then to rival Mr Newton & to make us beleive that he had it before Mr Newton explained it to him in his Letter of 13 Iune & 24 Octob 1676, & before he received a copy of Mr Newton's Letter of 10 Decem 1672 whereby it was further explained.

Mr Leibnitz in his Answer to Mr Fatio — sent him in that Letter with several examples thereof. And therefore Mr Leibnitz is to tell us what he meant by concealing all this when he published his Differentiall method, & telling the world afterwards that when he published it he knew nothing more of Mr Newton's inventions in this kind then that he could draw Tangents without taking away surds. All that he then published of the Differential method was the manner of drawing tangents & determining maxima & minima without taking away fractions & surds. He knew that Mr Newton's method would do all this & ought in candor to have acknowledged what he knew. He added in general that his method extended to other Problemes which were not to be resolved without his method AVT SIMILI. If he knew any thing of such another method it lies upon him to tell us why he did not do Mr Newton justice by acknowledging whose was the method & what he had learnt from England concerning it.

When Mr Leibnitz first published his Differential Method he ought in candor to have acknowledged what he knew of Mr Newtons method for doing the same things. All that he then explained of his own method was how to draw tangents & determin maxima & minima without taking aways fractions & surds. He knew that Mr Newtons method would do all this & therefore ought in candor to have acknowledged it. After he had thus far explained his own method he added that this method extended to the most difficult & notable problems which were scarce to be resolved without the Differential calculus AVT SIMILI, or another like it. What he meant by the words AVT SIMILI it was impossible for the Germans to understand without an interpreter. He ought to have done Mr Newton justice in plain intelligible language & told them that the proposed method which he there published extended to such difficult Problemes as were not to be resolved without his calculus differentialis or another calculus of Mr Newton of which he had received some notice by his correspondence with Mr Oldenburg & which he took to be like his own. But on the contrary in his Answer to Mr Fatio published in the Acta Eruditorum for May 1700 he has said: Certe cum elementa . . . . . satis intellexi. [It lies upon him therefore in point of candor to tell us why in the year 1684 when he first published his method, he concealed his knowledge of what had been communi <104v> cated to him from England of the same kind before the method was known to him; & why in the year 1700 in his answer to Mr Fatio he denyed his knowledge of almost all that he had received from England: & why he now denys what he then acknowledged & contradicts himself, telling us that it doth not appear by the book of Principles that Mr Newton knew any thing of this method.] When by his correspondence with Mr Oldenburg he received the first notices of Mr Newtons method he acknowledged in part saying: 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 hoc confirmat, nimirum semper figuræ illa sunt quadrabiles quæ sunt ad æquationem differentialem. When he published his method he conceled all this tho he knew it. as is manifest by the words AVT SIMILI. For he understood his own words tho they were not intelligible to others. When Mr Fatio taxed him he denyed that he knew any thing more of Mr N's method before the publishing of his Principles, whereby he understood that it was of much greater extent. And now he denys that the Principles make any discovery of the method. It lies upon him therefore to give an account of his candor in all this management, & at length to acknoledge publickly, that before he wrote his Letter of 21 Iune 1677 he did know that Mr Newton had a method not only for drawing of tangents & determining maxima & minima without taking away surds but also faciliating the quadrature of curves & determining their lengths & curvatures, for squaring Curves by series which break off & become finite in certain cases, for solving inverse Problemes of Tangents & other more difficult, for For comparing the areas of Curves with those of the Conic Sections, by which his method of series became so general as to extend to almost all Problemes except perhaps some numeral ones like those of Diophantus

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|---|---|
| 47,35. | 68 56,59 |
| 335 | 413 |
| 1645 | 3304 |

**Observations upon the Proposal**

Lin 1. very scarce. Obs. They are very scarce only in London, & sixty Tonns of new farthings would be enough for that city.

Lin 3. necessary. Obs. It never was though{t}necessare to coyn to much. In coyning the last copper money one hundred Tons per an in five years made such a clamour as gave occasion to the Parliament to stop that coynage for a year &c Six hundred Tons were then found sufficient for all England,

l. 6 for private advantage only. Obs. The Officers of the Mint have always opposed the proposals made for private advantage, & in their Reports upon them represented that the next copper money should be coyned as neare as could be to the intrinsic value ( that there might be no more temptation to counterfeit them was necessary, that they be well coyned to make it difficult to counterfeit them, that there should be no more coyned then was necessary for the uses of the nation for fear of clamours that the coynage should be upon account so that if any thing were to be got by it it might go to the king. And that it be put into a standing method

We have shewed that M^r Leibnitz the ende of the year 1676 in returning home from France by England & Holland was meditating how to improve the method of Slusius & extend it to all sorts of Problems & for this end proposed a general Table of Tangents, & therefore had not yet found out the true improvement: but about half a year after, viz^t Iune 21 1677 when he was newly fallen upon the true improvement, wrote back: Clarissimi Slusij methodum Tangentium nondum esse absolutam celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium generalius tractavi scilicet per differentias Ordinatarum. Which is as much as to say that he had this improvement long ago. It lies upon him in point of candor to make us understand that he pretended this antiquity of his invention with some other designe then to rival & supplant M^r Newton & make us beleive that he had the Differential method before M^r Newton explained it to him in his Letters of 13 Iune & 24 Octob 1676 & before M^r Oldenburg sent him a copy of M^r Newtons Letter of 10 Decem 1672 concerning it

<105r>

In Mathematical Sciences the Ancients had two Methods which they called Synthesis & Analysis or Composition & Resolution. By the Method of Analysis they found their inventions & by the Method of Synthesis they composed them for the publick. The Mathematicians of the last age have very much improved Analysis, but stop there & think they have solved a Problem when they have only resolved it, & by this means the method of Synthesis is almost laid aside. The Propositions in the following Book were invented by Analysis: but considering that the Ancients (so far as I can find) admitted nothing into Geometry before it was (for the greater certainty) demonstrated by composition, I composed what I invented by Analysis, to make it Geometrically authentic & fit for the publick. And this is the reason why this Book was written in words at length after the manner of the Ancients without Analytical Symbols & Calculations. But if any man who understands Analysis, will reduce the Demonstrations of the Propositions from their composition back into Analysis (which is easy to be done) he will see by what method of Analysis the Propositions were invented. And by this means the Marquess de l'Hospital was able to affirm that this Book was [presque tout de ce calcule] almost whole of the Infinitesimal Analysis.

In the second Lemma of the second Book of these Principles, I set down the Elements of the Analytic Method & demonstrated the Lemma by Composition in order to make use of it in the Demonstration of of some following Propositions. And because M^r Leibnitz had published those elements a year & some months before without making any mention of the Correspondence which I had with him by means of M^r Oldenburg ten years before that time, I added a Scholium not to give away the Lemma, but to put him in mind of that Correspondence. in order to his making a publick acknowledgment thereof before he proceeded to claim that Lemma from me. For in my Letter dated Iune 13^th 1676 I said that the Method of converging series in conjunction with some other methods (meaning the Methods of Fluxions & Arbitrary Series) extended to almost all Questions except perhaps some numeral ones like those of Diophantus. And in his Answer dated Aug. 27 1676, he replied that he did not beleive that my method were so general, there being many Problemes which could not be reduced to Equations or Quadratures. And in mine dated 24 Octob. 1676 I represented that my Analysis proceeded without stopping at surds, & readily gave the method of Tangents of Slusius & faciliated Quadratures & extended also to Problems which could not be reduced to Quadratures & gave me converging series for squaring of Curves which become finite whenever the Curve can be squared by a finite equation. And I said also that I had written a Treatise on this subject five years before that time, that is in the year 1671: which was two years before M^r Leibnitz began to study the higher Geometry. And the foundation of this method I said was obvious, & wrote it down enigmatically in this sentence: Data æquatione fluente quotcunque quantitates involvente invenire fluxiones, & vice versa. And in both my Letters I said that I had joyned this method & the method of Series together & that I had then absteined from this subject five years, being tyred with it before. When this Letter of 24 Octob arrived at London M^r Leibnitz was there the second time & saw it, & procured D^r Barrows Lectures, wherein was his method of Tangents invented above 12 years before, & M$^{r}$ Leibnitz in his way to Hanover was considering how to make the Method of Tangents of Slusius general by a Table of <105v> Tangents as I find by a Letter of his to M$^{r}$ Oldenburg dated at Amsterdam Novem 18 1676. st vet. And when M$^{r}$ Oldenburg heard that M$^{r}$ Leibnitz was arrived at Hanover, which was in March following, he sent to him a copy of my last Letter. And M$^{r}$ Leibnitz in his Answer dated 21 Iune 1677 sent back D$^{r}$ Barrows Method of Tangents under the differential notation & how this method might be improved so as to give the method of Tangents of Slusius; & then how it might be further improved so as to proceed without taking away fractions & surds: & then added Arbitror quæ celare voluit Newtonus, ab his non abludere. Quod addit, ex hoc eodem fundamento, nimirum semper figuræ illæ sunt quadrabiles, quæ sunt ad æquationem differentialem. And to put M$^{r}$ Leibnitz in mind of all this was the meaning of the Scholium above mentioned.

At the same time that M$^{r}$ Oldenburg sent my Letter of 13 Iune 1676 to M$^{r}$ Leibnitz (which was the 26$^{th}$ day of the same Iune) he sent also (at the request of M$^{r}$ Leibnitz) a collection of extracts of the Letters & papers of M$^{r}$ Gregory to M$^{r}$ Collins dated 5 Sept. 1670 in which M$^{r}$ Gregory represented that he had improved the Method of Tangents beyond what D$^{r}$ Barrow had done, so as to draw Tangents to all Curves without Calculation. There was also a Copy of a Letter written by me to M$^{r}$ Collins 10 Decem 1672 in which I wrote that the methods of Tangents of Gregory & Slusius were only Corollarium Methodi generalis quæ extendit se citra molestum ullum calculum non modo ad ducendum Tangentes ad quasvis Curvas sive Geometricas sive Mechanicas, vel quomodocunque rectas lineas aliasve curvas respicientes; verum etiam ad resolvendum alia abstrusiora Problematum genera de Curvitatibus, Areis, Longitudinibus, Centris gravitatum Curvarum &c. Neque (quemadmodum Huddenij Methodus de Maximis et Minimis) ad solas restringitur æquationes illas quæ quantitatibus surdis sunt immunes Hanc methodum intertextui alteri isti, qua Æquationum Exegesin instituo, reducendo eas as Series infinitas. These Letters M$^{r}$ Leibnitz received in Iuly 1676. And in October following he procured D$^{r}$ Barrows Lectures at London as above & saw my Letter of Octob 24. And all this was enough to put him upon considering how to improve the Method of Tangents of D$^{r}$ Barrow as Gregory had done before so as to draw Tangents without calculation; and how to improve the Method of Tangents of Gregory & Slusius so as to make it proceed without stopping at fractions & surds, & to extend it not only to Tangents & Maxima & Minima, but also to Quadratures & other sorts of Problemes, so as to become such a general Method as I described in my Letters of 10 Decem. 1672, 13$^{th}$ Iune 1676 & 24 Octob 1676. And after all this light received from England (besides what he saw in the hands of M$^{r}$ Collins when he was last in London) I had great reason by the Scholium above mentioned to put him in mind of his correspondence with M$^{r}$ Oldenburg & M$^{r}$ Collins.

By measuring the quantity of water which runns out of a vessel in a given time through a given round hole in the bottom of the vessel I found that the velocity of the water in the hole was that which a body would acquire in falling half the height of the water stagnating in the vessel. And by other experiments I found afterwards that the water accelerated after it was out of the vessel untill it arrived at a distance from the vessel equal to the diameter of the hole, & by accelerating acquired a velocity equal to that which a body would acquire in falling the whole height of the water stagnating in the vessel or thereabouts. That the streame might not accelerate by its weight, it ran out horizontally, & that its diameter at several distances from the hole might be measured with <106r> more exactness it was above half an inch thick.

In the tenth Proposition of the second Book there was a mistake in the first edition by drawing the Tangent of the Arch GH from the wrong end of the Arch which caused an error in the conclusion: but in the second Edition I rectified the mistake And there may have been some other mistakes occasioned by the shortness of the time in which the book was written & by its being copied by an Emanuensis who understood not what he copied; besides the press faults. For I wrote it in 17 or 18 months, beginning in the end of December 1684 & sending it to the R. Society in May 1686: excepting that about ten or twelve of the Propositions were composed before, viz$^{t}$ the 1$^{st}$ & 11$^{th}$ in December 1679, the 6$^{th}$ 7$^{th}$ 8$^{th}$ 9$^{th}$ 10$^{th}$ 12$^{th}$, 13$^{th}$ 17$^{th}$ Lib. I & the 1, 2, 3 & 4 Lib. II, in Iune & Iuly 1684.

<106v>

**{In} this is the account how long Sir I. was writing his principia**

<107r>

Veteres duplici methodo tractabant res Geometricas, Analysi scilicet et Synthesi seu Resolutioneet compositione ex Pappo liquet. Per Analysin investibant Propositiones suas et per Synthesin demonstrabant inventas ut in Geometriam admitterentur. Laus enim Geometriæ in ejus certitudine consistit, ideoque nihil in ipsam prius admitti debet quam reddatur certissimum. Hæc certitudo oritur ex demonstrationibus et Veterum Demonstrationes omnes erant syntheticæ. Algebra nihil aliud est quam Arithmetica ad res Geometricas applicata, et ejus operationes complexæ sunt & erroribus nimis obnoxiæ & a solis Algebræ peritis legi possunt. Propositiones autem in Geometria sic proponi debent ut a plurimis legantur, et mentem claritate sic maxime afficiant, ideoque synthetice demonstrandæ sunt. Vtilis est Analysis ad veritates inveniendas, sed certitudo inventi examinari debet per compositionem Demonstrationis & quam fieri potest perspicua clara & omnibus manifesta reddi: præsertim cum Proportio quæ non demonstratur synthetice, ex mente Veterum non demonstratur, ideoque in Geometriam admitti non debet. Problemato quoque quorum constructiones innotescunt tantum per Analysin, nondum soluta sunt sed tantum resol**{u}**ta, nec prius soluta dici debent quam eorum Constructiones demonstrentur synthetice.

His de causis Propositiones in Libris Principiorum quas inveni per Analysin demonstravi per Synthesin, si forte Prop. XLV Libri primis & Prop. X Lib II excipiantur. Mathematicis autem hujus sæculi, qui fere toti versantur in Algebra, genus hocce syntheticum scribendi minus placet, seu quod nimis prolixum videatur & methodo veterum nimis affine, seu quod rationem inveniendi minus patefaciat Et certe minori cum labore potuissem scribere Analyticè quàm ea componere quæ Analytice inveneram: sed propositum non erat Analysin docere. Scribebam ad Philosophos Elementis Geometriæ imbutos & Philosophiæ naturalis fundamenta Geometrice demonstrata ponebam. Et inventa Geometrica quæ ad Astronomiam et Philosophiam non spectabant, vel penitus præteribam, vel leviter tantum attingebam. Cum autem de Analysi disputetur qua usus sum, visum est hanc paucis exponere.

<109r>

1 Geometræ Veteres quæsita investigabant Analysin, inventa demonstrabant per Synthesin, demonstrata edebant ut in Geometriam reciperentur. Resoluta non statim recipiebantur in Geometriam: opus erat solutione per compositionem demonstrationum. Nam Geometriæ vis et laus omnis in certitudine rerum, certitudo in demonstrationibus luculenten compositis constabat. In hac scientia non tam brevitati scribendi quam certitudim rerum consulendum est. Ideoque in sequenti Tractatu Propositiones per Analysin inventas demonstravi synthetice.

2. Geometria Veterum versabatur quidem circa magnitudines: sed Propositiones de magnitudinibus nonunquam demonstrabantur mediante motu locali: ut cum triangulorum æqualitas in Propositione quarta libri primi elementorum Euclidis demonstraretur transferendo trangulum alterutrum in locum alterius. Sed et genesis magnitudinum per motum continuum recepta fuit in Geometria: ut cum linea recta duceretur in lineam rectam ad generandam aream, & area duceretur in lineam rectam ad generandum solidum. Si recta quæ in aliam ducitur datæ sit longitudinis generabitur area parallelogramma. Si longitudo ejus lege aliqua certa continuo mutetur generabitur area curvilinea: Si magnitudo areæ in rectam ductæ continuo mutetur generabitur solidum superficie curva terminatum. Si tempora, vires, motus et velocitates motuum exponantur per lineas areas , solida vel angulos, tractari etiam possunt hæ quantitates in Geometria.

Quantitates continuo fluxu crescentes vocamus fluentes & velocitates crescendi vocamus fluxiones, & incrementa momentanea vocamus momenta, et methodum qua tractamus ejusmodi quantitates vocamus methodum fluxionum et momentorum: estque hæc methodus vel synthetica vel analytica.

Methodus synthetica fluxionum et momentorum in Tractatu sequente passim occurrit, et ejus elementa posui in Lemmatibus undecim primis Libri primi & Lemmate secundo Libri secundi.

Methodus analyticæ specimina occurrunt in Prop XLV & Schol Prop. XCII Lib. I & Prop. X & XIV Lib. II. et præterea describitur in Scholio ad Lem. II & Lib. II. Sed et ex Demonstrationibus compositis Analysis qua Propositiones inventæ fuerunt, addisci potest regrediendo. [Et præterea describitur in Scholio ad Lem. II Lib: II. [Tractatum de hac Analysi ex chartis antea editis desumptam, Libro Principiorum subjunxi.]

Scopus Libri Principiorum non fuit ut methodos mathematicas edocerem, non ut difficilia omnia ad magnitudines motus & vires spectantia eruerem; sed ut ea tantum tractarem quæ ad Philosophiam naturalem et apprime ad motus cœlorum spectarent: ideoque quæ ad hunc finem parum conducerent, vel penitus omisi, vel leviter tantum attigi, omissis demonstrationibus.

In Libris duobus primis vires generaliter tractavi, easque si in centrum aliquod seu immotum seu mobile tendunt, centripetas (nomine generali) vocavi, non inquirendo in causas vel species virium, sed earum quantitates determinationes & effectus tantum considerando. In Libro tertio quamprimum didici vires — quibus Planetæ in orbibus suis retinentur, recedando a Planetis in quorum centra vires illæ tendunt, decrescere in duplicata ratione distantiarum a centris, & vim qua Luna retinetur in Orbe suo circum Terram, descendendo ad superficiem Terræ æqualem evadere vi gravitatis nostræ, adeoque vel gravitatem esse vel <109v> vim gravitatis duplicare: cœpi gravitatem tractare ut vim qua corpora cœlestia in orbibus suis retineantur. Et in eo versatur Liber iste tertius, ut Gravitatis proprietates, vires, directiones & effectus edoceat.

Planetas in orbibus fere concentricis & Cometas in orbibus valde excentricis circum Solem revolvi, Chaldæi olim crediderunt, Et hanc philosophiam Pythagorei in Græciam invexerunt. Sed et Lunam gravem esse in Terram, & stellas graves esse in se mutuo, et corpora omnia in vacuo æquali cum velocitate in Terram cadere, adeoque gravia esse pro quantitate materiæ in singulis notum fuit Veteribus. Defectu demonstrationum hæc philosophia intermissa fuit eandemque non inveni sed vi demonstrationum in lucem tantum revocare conatus sum. Sed et Præcessionem Æquinoxiorum, & fluxum & refluxum maris et motus inæquales Lunæ et orbes Cometarum & perturbationem orbis Saturni per gravitatem ejus in Iovem ad ijsdem Principijs consequi, et quæ ab his Principijs consequuntur Cum Phænomenis probe congruere, hic ostensum est. Causam gravitatis ex phænomenis nondum didici.

Qui leges et effectus Virium electricarum pari successu et certitudine eruerit, philosophiam multum promovebit, etsi forte causam harum Virium ignoraverit. Phænomena primo observanda sunt dein horum causæ proximæ, & postea causæ causarum eruendæ; ac tandem a causis causarum per phænomena stabilitis, ad earum effectus, argumentando a priori, descendere licebit. Philosophia naturalis non in opinionibus Metaphysicis, sed in Principijs proprijs fundanda est; & hæc

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Consessus Arbitrorum a Regia Sociètate constitutus Commercij subsequentis Epistolici exemplasia tantum pauca Anno 1712 imprimi curavit et ad mathematicas mitti qui soli de his rebus judicare possent. Cum vero D. Leibnitius huic Libro minime responderet sed Quæstionem desereret & ad Quæstiones Methaphysicas iliasque ad hanc rem nihil spectantes sophistice confugoret rixando, et ejus amici quidam adhuc rixentur, visum est hunc librum una cum ejus Recensione quæ in Tansactionibus

Et cum

In scribendis Philosophiæ Principijs Mathematicis Newtonus Libro hocce de quadraturis pluronum est usus , ideoque eundem Librio Principiorum subjungi voluit. Investigavit utique Propositiones in Libro Principiorum per Analysin, investigatas demonstravit per Synthesin pro lege Veterum qui Propositiones suas non prius in Geometriam admittebant quam demonstratæ essent synthetics. Analysis hodierna nihil aliud est quam Arithetica in specibus. Hæc Arithmetica ad res Geometricas applicare potest, et Propositiones sic inventæ sunt Arithmetice inventæ. Demonstrari debent Synthetice more veterum et tum demun pro Geometricis haberi.

D. Fatio de Duillier incidit in hanc Methodum anno 1687 ed visis postea Newtoni MSS antiquis ille **{is}** anno 1699 in Tractatu de solido minimæ resistentiæ testimonium pro Newtono exhibuit his verbis: Newtonum tamen primum ac pluribus annis vetustissimum hujus calculi inventorum ipse verum evidentia coactus agnosco. A quo utrum quicquam mutuatus sit Leibnitius secundus ejus inventor, malo eorum quam meum sit judicium quibus visæ fuerent Newtoni Litteræ alijque ejusdem manuscripti Codices.

At quamvis Liber Principiorum synthetice scriptus sit, tamen Analysis per Synthesin elucet, et plerunque erui potest regrediendo a Synthesi ad Analysin & quærendo Analysin aqua Synthesis derivate fuit. Hac retione Marchi <u>Hospitalius</u> intellexit librum Principiorum <u>fere totum esse de hocce</u> [momentorum] <u>calculo.</u> Et Leibnitius in Epistola ad Newtonum 7 Mar. 1693 data scipsit:<u>Mirifice ampliaveras Geometriam tuis seriebus</u>

sed edito Principiorum opere ostendisti patere Tibi quæ Analysi recepte 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.

<112v>

## Ad Lectorem.

## Annotationes. Vel Schol. in Prop. V.

✝ NB Pag. 41 Hoc, Wallisius prius affirmavit in Præfatione ad Volumen primum Operum suorum. Et idem sic ostenditur Newtonus literis ad Wallisium datis Aug 27 & Sep. 17 1692 mittebat Propositiones duas hujus Libri, primam et quintam una cum Propositione Extrahendi radiem ex Æquatione fluxionem radicis involvente; et Wallisius hæc edidit anno 1693 in secundo Volumine operum suorum, Et ibi notavit has tres Propositiones, in Epistola Newtoni ad Oldenbergium 24 Octob. 1676 data, describi. In eadem Epistola Propositio Prima hujus Libri dicitur esse fundamentum Methodi generalis de qua Newtonus anno 1671 tractatum scripserat . Propositio secunda extat in Analysi per Series sub finem et pendet a Propositione prima ideoque Propositiones duæ Primæ hujus Libri Newtono innotuere anno 1669 quo utique Barrovius hanc Analysin ad Collinium misit. Sed et Propositio quinta eodem anno Newtono innotuit. Nam in Analysi illa dicitur, quod illuius beneficio curvarum areæ et longitudines &c (id modo fiat) exacte et Geometrice determinantur. Et hoc fit per Propositionem illam quintam. Propositio autem tertia et quarta sunt tantum exempla Propositionis secundæ ut in hoc libro dicatur. Ideoque Methodus Fluxionum quatenus habetur in Propositionibus quinque primis Libri de Quadraturis Newtono innotuit anno 1669. Denique Collinius in Epistola ad Tho Strode 26 Iulij 1672 data, scripsit quod mense Septembri anni 1668 Mercator Logarithmotechniam suam edidit quod ex Analysi per series et chartis alijs quæ olim a Newtono cum Barrovio communicatæ fuerant pateret illam methodum a dicto Newtono aliquot annis antea excogitatam & modo universali applicatam fuisse: ita ut ejus ope in quavis figura Curvilinea proposita quæ una vel pluribus proprietatibus definitur, Quadratura vel Area dictæ Figuræ accurata si possibile sit, sin minus infinite vero propinqua — obtineri queat. Hoc fit per Propositionem illam quintam. Ideoque methodus fluxionum quatenus habetur in Proposionibus quinque primis Libri de Quadraturis Newtono innotuit annis aliquot antequam prodiret Mercatoris Logarithmetechnia testibus Barrovio et Collinio, id est anno 1666 aut antea, ut Wallisius affirmavit.

Corollarium secundum Propositionis decimæ habetur in Epistola Newtoni ad Collinium Novem. 8 1676. data et a Ionesio in Analysi per Quantitatum Series, Fluxiones ac Di{f}ferentias edita. Et Ordinatæ Curvarum quæ in Tabula ultima in Scholio ad Prop. X habentur, recitantur eodem ordine & ijsdem literis in Epistola Newtoni ad Oldenburgium Octob. 24 1676. ⊙ < insertion from the bottom of the page > ⊙ et ibi Tabula illa Catalogus Theorematum dicitur pro comparatione Curvarum cum Conicis Sectionibus dudum conditus id est diu ante annum 1676, et propterea anno 1671 aut antea. Inde vero colligitur Propositionem illam deciman Newtono innotuisse anno 1671. Extractus utique fuit hic liber circa annum 1676 ex libro antiquiore quem Newtonus scripsit anno 1671. < text from f 112v resumes > Inde vero colligitur Propositionem illam decimam Newtono innotuisse anno 1671 Extractus utique fuit hic Liber circa annum 1676 ex Libro antiquiore quem Newtonus scripsit anno 1671.

Beneficio hujus methodi didici anno 1664 vires quibus Planetæ primarij retinentur in orbibus circa Solem esse in ratione duplicata distantiarum mediocrium a sole inverse et vim qua Luna retinetur in Orbe cirum Terram esse in eadem fere ratione ad gravitatem in superficie Terræ. Deinde anno 1679 ad finem vergente inveni demonstrationem Hypotheseos Kepleri quod Planetæ primarij revolvuntur in Ellipsibus Solem in foco inferiore habentibus, & radijs ad Solem ductis areas describunt temporibus proportionales Tandem anno 1685 et parte anni 1686 beneficio hujus methodi & subsidio libri de Quadraturis scripsi libros duos primos Principiorum mathematicorum Philosophiæ. Et propterea <112r> Librum de Quadraturis subjunxi Libro Principiorum.

Anno 1666 incidi in Theoriam colorum, et anno 1671 parabam Tractatum de hac re, aliumque de methodo serierum & fluxionum ut in lucem ederentur. Sed subortæ mox disputationes aliquæ me a consilio deterruerunt usquead annum 1704.

Interea D. Leibnitius in methodum momentorum incidit anno 1677.

Fatum et necessitas non ent causa sufficiens nisi per productionem Entis omnipræsentis intelligentes, volentis & actiones suas eligentis. Hujusmodi ens est Natura illa sepientissima quæ nihil facit frustra quamque omnes ex phænomenis prædicant, [& quæ rectius fons naturæ quam natura ipsa dici deberet.

cum ipse prius in Actis Eruditorum pro mense anni 1712 pro Leibnitio contra Newtono scripsisset

Et ab eo tempore Newtonum aggressus est propositis novis disputationibus

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