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To the Reader

That the following Tracts may be the better understood, it may be convenient to premise their history.

D^r^ Wallis in his opus Arithmeticum published A.C. 1657. cap. 33. Prop. 68, reduced the fraction $\frac{\mathrm{A}}{1-\mathrm{R}}$ by perpetual division, into the series $\mathrm{A}+\mathrm{AR}+\mathrm{AR}^2+\mathrm{AR}^3+\mathrm{AR}^4+\&c$

Vicount Brounker squared the Hyperbola by this Series $\frac{1}{1\times2}+\frac{1}{3\times4}+\frac{1}{5\times6}+\frac{1}{7\times8}+\&c$. that is by this $1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\frac{1}{5}-\frac{1}{6}+\frac{1}{7}-\frac{1}{8}+\&c$ conjoyning every two terms into one. And the Quadrature was published in the Philosophical Transactions in Apr 1668.

M^r^ Mercator soon after published a demonstration of this Quadrature by the Division of D^r^ Wallis; & before the end of the year M^r^ Ia. Gregory published a Geometrical Demonstration thereof. And these books were a few months after sent by M^r^ Iohn Collins to D^r^ Barrow at Cambridge & by D^r^ Barrow comunicated to M^r^ Newton (now Sir Isaac Newton) in Iune 1669. Whereupon D^r^ Barrow mutually sent to M^r^ Collins a Tract of M^r^ Newton's entituled Analysis per æquationes numero terminorum infinitas. And this Tract M^r^ Newton in his Letter to M^r^ Oldenburg dated 24 Octob. 1676 mentioned in the following manner. Eo ipso tempore quo Mercatoris Logarithmotechnia prodijt, communicatum est per amicum D. Barrow (tunc Matheseos Professorem Cantab.) cum D. Collinio Compendium quoddam harum Serierum, in quo significaveram Arias & Longitudines Curvarum omnium, & Solidorum superficies & contenta ex datis rectis; & vice versa ex his datis rectas determinari posse, & methodum indicatam illustraveveram diversis seriebus.

M^r^ Collins in the years 1669, 1670, 1671 & 1672 gave notice of this Compendium to M^r^ Iames Gregory in Scotland, M^r^ Bertet & M^r^ Vernon then at Paris, M^r^ Alphonsus Borelli in Italy, & M^r^ Strode, M^r^ Townsend, M^r^ Oldenburg, M^r^ Dary & others in England, as appears by his Letters still extant. And M^r^ Oldenburg in a

Letter dated 14 Sept 1669, & entred in the Letter Book of the R. Society gave notice of it to M^r Francis Slusius at Liége & cited several sentences out of it. And particularly M^r Collins in a Letter to M^r David Gregory dated 11 Aug. 1676 mentions it in this manner. Paucos post menses quam editi sunt hi libri, [viz. Mercatoris Logarithmotechnia et Exercitationes Geometricæ Gregorij] missi sunt ad Barrovium Cantabrigiæ Ille autem responsem excogitatam fuisse quam ederetur Mercatoris Logarithmotechnia & generaliter omnibus figuris applicatam simulque transmisit D. Newtoni opus manuscriptum. And in a Letter to M^r Strode dated Iuly 26 1672 M^r Collins wrote thus of it. Exemplar ejus [Logarithmotechniæ] misi Barrovio Cantabrigiam, qui quasdam Newtoni chartas extemplo remisit: e quibus et alijs quæ prius ab autore cum Barrovio communicata fuerant patet illam methodum a dicto Newtono aliquot annis antea cogitatam & modo universali applicatam fuisse: ita ut ejus ope, in quavis figura curvilinea proposita, quæ una vel pluribus proprietatibus definiter, Quadratura vel Area dictæ figuræ, accurata si possibile sit, sin minus infinite vero propinqua, Evolutio vel longituto Lineæ Curvæ, Centrum gravitatis figuræ, Solida ejus rotatione genita & eorum superficies: sine ulla radicum extractione <576r> obtineri queat. Postquam intellexerat D. Gregorius hanc Methodum a D. Mercatore in Logarithmotechnia usurpatam & Hyperbolæ quadrandæ adhibitam, quamque adauxerat ipse Gregorius jam universalem redditam esse, omnibusque figuris applicatam; acri studio eandem acquisivit multumque in ea enodanda desudavit. Vterque D. Newtonus & Gregorius in animo habet hanc Methodum exornare: D. Gregorius autem D. Newtonum primum ejus inventorem anticipare haud integrum ducit. So then by the testimony of D^r Barrow this Analysis was invented two or thre years before the Logarithmotechnia of M^r Mercator came abroad. And since it gave the areas of figures accurately if it might be, or else by approximation, it included the invention of such converging series as brake of & became finite when ever the area could be found by a finite equation. How this was to be done is not described in the Compendium, but it's there said: hujus [methodi] beneficio Curvarum areé & longitudines &c. (id modo fiat) exacte et Geometrice determinantur Sed ista narrandi non est locus. M^r Newton in his Letter of 24 Octob. 1676 tells us that this was done by the method of fluxions, & in the first six Propositions of his book of Quadratures sets down how it was done & there is no other way of doing it. And therefore the Method described in those six Propositions was known to him before he wrote the said Compendium.

And whereas M^r Collins represents this Method a general one & that it proceeds without any extraction of roots, meaning that it proceeds without stopping at surds, this is also an argument that M^r Newton had the method of fluxions before he wrote the Compendium. For in his Letters of 10 Decem 1672 & 24 Octob. 1676 he gave this as a character of his general method that it stuck not at surds.

M^r Newton in his Answer to a Letter of M^r Leibnitz dated 9 Apr 1716, has told us that he hath still in his custody several Mathematical Papers written in the years 1664, 1665 & 1666 some of which happen to be dated; & that in one of them dated the 13th of Novem 1665 the direct Method of fluxions is set down in these words. PROB. An Equation being given, expressing – – – – gives the relation of p. q, r, &c. Suppose now that the equation $ax^2 - xy^2 + y\sqrt{aa-xx} = 0$ was given & the fluxions p & q were to be found, the surd quantity $\sqrt{aa-xx}$ might be extracting its root be turned into a converging series, but this is not necessary. Put $z = \sqrt{aa-xx}$, & the first equation will become $ax^2 - xy^2 + ayz$ <577r> $+ayz = 0$ & the second $zz = aa - xx$. & these give the fluxional equations $2axp - py^2 - 2xqy + aqz + ayr = 0$ & $2rz = -2px$; & by putting $\frac{-2px}{z}$, for r, & $\sqrt{aa-xx}$ for z, the first fluxional exquation becomes $2axp - py^2 - 2xqy + \frac{a^3q - aqx^3 + aypx}{\sqrt{aa-xx}} = 0$. So you have the relation of the fluxions p & q without extracting the root of $aa - xx$. And this is what M^r Collins means by saying that the method gives the areas & solid contents & surfaces of figures &c without any extraction of roots.

This Compendium is called Analysis per æquationes numero terminorum infinitas to signify that it is not merely a particular method of squaring figures by converging series, but a general method of Analysis — teaching first howe to reduce finite equations & other given Data in Problemes to converging series to Equations including converging series whenever it shall be found necessary & then how to work in such Equations as well as in finite ones untill the Problem be solved. And this we are told in the Compendium it self where its said. Et quic quid quicquid Vulgaris Analysis per æquationes ex finito terminorum numero constantes (quando id sit possibile) perficit, hæc per æquationes infinitas semper perficiat: Vt nil dubitaverim etiam nomen Analyseos etiam huic tribuere. For it teaches how to resolve finite equations & finite terms of equations into converging series whenever it shall be necessary, & how by the Method of Moments or fluxions to apply Equations both finite & infinite to the solution of all Problemes. It begins where D[r] Wallis left off, & founds the method of Quadratures upon these Rules.

D[r] Wallis published his Arithemtica Ininitorum – – – – – – – he calls the second Difference the Difference of Moments.

And that you may know what kind of Calculation M[r] Newton used in or before the year 1669 when he wrote this Compendium, you read the Demonstration of the first Rule above mentioned set down in the end of the Compendium.

And by the same way of working the second Rule may be also demonstrated. And if any equation – – – – fluxions of the first Area.

And as by this way of working the Ordinate may be deduced from any equation expressing the relation between the Abscissa & the Area

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H. 2 Wallis in his opus Arithmeticum published A. 1657 cap 33 Prop 68 reduced the Algebraic fraction $\frac{A}{1-R}$ by perpetual Division into the series $A + AR + AR^2 + AR^3 + AR^4 + \&c$.

3 Vicount Brunker squared the Hyperbola by this series $\frac{1}{1\times 2} + \frac{1}{3\times 4} + \frac{1}{5\times 6} + \frac{1}{7\times 8} + \&c$ that is by this $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} + \&c$, & the Quadrature was published in April 1668.

4 Mercator published a Demonstration of this Quadrature by the division of D[r]Wallis. 8 Gregory soon after published a Geometrical Demonstration thereof & months after sending these books to D[r] Barrow & M[r] Collins after

8 Newton having found a new Analysis composed of a double method the one of converging series the other of Moments; which Analysis extended to the Quadrature of all Curves, & the solving of such other Problems as were reducible to Quadratures, & gave the Ordinates Tangents & Quadratures of the Curves then called Mechanical communicated a small Tract upon this subject to D[r] Barrow then Mathematical Professor at Cambridge & D[r] Barrow communicated it to M[r] Iohn Collins in Iuly 1669. In this Tract amongst other Theorems were these two Let the Radius of a circle be 1, the sine x, & arc z
$z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{1152}x^9 + \&c$ $x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362680}z^9 - \&c$

M[r] Iames Gregory having received one of M[r] Newtons series from M[r]Collins after some consideration found the method of series in December 1670, & in the beginning of the next year 15 Feb. 1701 sent M[r] Collins some other Theorems of the same kind particularly this. Let the Radius be r, the Arc a & the tangent t
$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^9} - \&c$ These Theoremes M[r] Gregory gave M[r] Collins full liberty to communicate to whom he pleased & M[r] Collins was very free in communicating what he had received both from M[r] Newton & from M[r]Gregory.

6 M[r] Leibnitz was that year in London & published there two pieces, the one dedicated to the Royal Society the other to the Academy at Paris, & in the dedication of the first he mentions his correspondence with M[r] Oldenburg. In March 1673 he went thence to Paris & the next year in Iuly & October wrote two Letters to M[r] Oldenburg in the first of which he represented that he had a wonderfull Theoreme which gave the Area of a Circle or any Sector thereof exactly in a series of rational numbers; & in the second he described this Theoreme a little further saying that he had found the circumference of a circle in a series of very simple numbers, & that by the same method any arc whose sine was given might be found in a like series tho the proportion to the circumference be not known. His Theorem therefore which gave him the Area of a Circle or of any Sector thereof was for finding the Arc whose sine was given. If the proportion of the Arc to the

circumference was not known the Theorem or method gave him only the Arc: if it was known it gave him also the whole circumference. But the Demonstration of this Theorem he wanted & therefore had not invented it himself. For in his Letter of 12 May 1676 he desired M^{r} Oldenburg to procure the Demonstration from M^{r} Collins, meaning the method by which M^{r} Newton had invented it.

7. M^{r} Oldenburg in a letter to M^{r} Leibnitz dated 15 Apr. 1675, sent him eight of M^{r} Newtons & M^{r} Gregories series amongst which were M^{r} Newtons two series for finding the arc whose sine was given & the sine whose arc was given & M^{r} <579v> Gregories series for finding the arc whose Tangent was given. M^{r} Leibnits in his answer dated 20 May 1675 acknowledged the receipt of this Letter in these words. Literas tuas multa fruge Algebraica refertas accepi pro quibus tibi et doctissimo Collinio gratias ago. Cum nunc præter ordinarias curas Mechanicis imprimis negotijs distrahar, non potui examinare series quas misistis, ac cum meis comparare. Vbi fecero perscribm tibi sententiam mean: nam aliquot jam anni sunt quod inveni meas via quadam sic satis singulari. What his own series were is unknown to this day. For he has never yet produced any other series then those which he received in this Letter. And what he did with Gregories series for finding the Arc whose Tangent is given he has told us in the Acta Eruditorum mensis Aprilis pag 178. Iam anno 1675, saith he, compositum habebam opusculum Quadraturæ Arithmeticæ ab amicis ab illo tempore lectum &c.

8 M^{r} I. Gregory died in the latter end of the year 1675 & the next year from Paris to at the request of M^{r} Leibnitz & some others of the Academy at Paris. M^{r} Collins drew upon extracts of his Letters & the collection is still extant in the hand of M^{r} Collins with this litle Exactracts from M^{r} Gregories Letters to be lent M^{r} Leibnitz to peruse who is desired to return the same to you. These were accordingly sent to Paris a little before the 11th of August 1676 as appers by a letter of M^{r} Collins of that date, ‡ < insertion from lower down the page > ‡ & by the Answer of M^{r} Leibnitz dated 27 Aug. 1676, in which he writes Ad alia tuarum literarum venio quæ doctissimus Collinius communicare gravatus non est. Vellem adjecisset appropinquationis Gregorianæ linearis Demonstrationem. Credo tamen aliam haberi simpliciorem, etiam in infinitum euntem; quæ fiat sine ulla bisectione anguli, imo sine supposita circuli constructione; solo rectarum ductu. Vellem Gregoriana omnia conservari. Fuit enim his certe studijs promovendis aptissimus &c. By this Answer I gather that the Extracts were sent at the same time with Newtons Letter viz^{t} Iune 26th. That they were sent appears further by a Letter of M^{r} Tschurnhause from Paris to M^{r} Oldenburg dated 1 Sept 1676, which < text from f 579v resumes > & by a letter of M^{r} Tschurnhause from Paris to M^{r} Oldenburg dated 1 Sept. 1676, which ends with these words. Similia porro quæ in hac re [id est in methodo serierum Newtoni] præstitit eximius Geometra Gregorius memoranda certe sunt, et quidem optimæ famæ ipsius consulturi sunt, qui ipsius relicta Manuscripta luci publicæ ut exponantur operam navabunt. In this Collection was M^{r} Gregories Letter of 15 Feb. 1671 wherein he communicated several series to M^{r} Collins & among others the series above mentioned for finding the Arc whose Tangent was given. But M^{r} Leibnitz notwithstanding persisted in his designe of making himself the Inventor of this series.

If an Equation contein two unknown

3 For in his Answer dated 27 Aug 1676 he sent back this series to M^{r} Oldenburgh to be communicated to M^{r} Newton as his own pretending that he had found it out three years before or above.. And he endeavoured also to claim the following series from M^{r} Newton. Let $1 - \mathrm{m}$ be any number less then an unit & let the Hyperbolic Logarithm be l, & on will be $= \frac{l}{1} - \frac{l3}{1\times2} + \frac{l3}{1\times2\times3} - \frac{l4}{1\times2\times3\times4}$ &c. Let $1 + \mathrm{m}$ be any number greater then an unit & m will be $\frac{l}{1} + \frac{l3}{1\times3} + \frac{l3}{1\times2\times3} + \frac{l4}{1\times2\times3\times4} +$ &c Let p be the Radius & a the arch of a circle, & the sine of the Complement will be $1 - \frac{a^2}{1\times2\times3} + \frac{a4}{1\times2\times3\times4\times5}$ &c. These series he <580r> pretended to have found before he received M^{r} Newtons Letter of 13 Iune 1676, tho at his own request M^{r} Newton had sent him in that Letter the method of finding them.

And thus much concerning the method of converging Series.

10 In the Compendium above mentioned M^r^ Newton considered indeterminate quantities as increasing in time & from the flowing & moments of time gave the name of fluxions to the velocities wherewith quantities increased & that of moments to their parts generated in each moment of time. He exposed time by any line flowing uniformly & most commonly by the Abscissa of a Curve, & for the fluxion of time or of its exponent he put an unit & for its moment the letter o, & for the other flowing quantities he put any letters or symbols & for their fluxions any other letters or symbols, & for their moments he put their fluxions multiplied by the moment o. For fluxions are finite quantities & to make them infinitely he multiplied them by the moment o. In demonstrating Propositions he considers the moment o as indefinitely but not infinitely & performs the whole operations in finite quantities & finite figures by the Geometry of Euclide & then supposes that the moment o decreases in infinitum & vanishes. In finding out Propositions he considers the moment o as infinitely little, forbears to write it down, & works in figures infinitely little by such approximations as he thinks will make no error in the conclusion. An instance of the first way of working you have neare the end of the Compendium[1] in demonstrating the first of the three Rules upon which the Compendium is founded. A description of the second way you have four or five pages before[2] where he considers the Ordinate of a curve moving uniformly upon the Abscissa to describe the Area, & considers a point or infinitely short line as the moment of a line, & a line or infinitely narrow surface as the moment of a surface, & a surface or infinitely thin solid as the moment of a solid, & puts the lines BK(1) & AK(y) for the moments of two Surfaces, the coefficient o being understood to make these lines infinitely narrow surfaces. And by this method of moments he applies æquations both finite & infinite to the solution of Problems & describes this method to be very universal & gives it the name of Analysis.

11 If an Equation contein

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And the same is manifest also by what M^r^ Leibnitz wrote in the Acta Eruditorum Anno 1691 concerning this matter. Iam anno 1675, saith he, compositum habebam opusculum Quadratuæ Arithmeticæ ab amicis ab illo tempore lectum, sed quod materia sub manibus crescente, limare ad editionem non vacavit, postquam aliæ occupationes supervenere; præsertim com nunc prolixius exponere vulgari more quæ Analysis nostra nova paucis exhibet, non satis operæ pretium videatur. This Quadrature he composed vulgari more & began to communicate it at Paris in the year 1675. The next year he was polishing the Demonstration thereof to send it to M^r^ Oldenburg as he wrote to him in his letter dated 12 May 1676. The winter following he returned into Germany to enter upon public business & had no longer any leasure to fit it for the press, nor thought it afterwards worth his while to explain those things prolixly in the vulgar manner which his new Analysis exhibited in short. He found this new Analysis therefore after his return into Germany & by consequence not before the year 1677.

How M^r^ Newton described his method of fluxions & moments in his Analysis communicated by D^r^ Barrow to M^r^ Collins in Iuly 1669 has been shewed above D^r^ Barrow published his method of Tangents in the year 1670: M^r^ N. in his Letter dated 10 Decemb. 1672 communicated his method of Tangents to M^r^ Collins, & then added. Hoc est unum particulare . . . . reducendo eas ad series infinitas. <581v> Memini me ex occasione aliquando narrasse D. Barrovio edendis Lectionibus suis occupato, instrctum me esse hujusmodi methodo tangentes ducendi sed nescio quo diverticulo ab ea ipsi describenda fuerim avocatus. Slusij methodum tangentes ducendi brevi publice prodituram confido. Quamprimum advenerit exemplar ejus ad me transmittere ne grave ducas.

M^r^ Slusius sent his method to M^r^ Oldenburg 17 Ian 1673 & the same was soon after published in the Transactions. It proved to be the same with that of M^r^ Newton. It was founded upon three Lemmas the first of which was this Differentia duarum dignitatum ejusdem gradus applicata ad differentiam laterum, dat partes singulares gradus inferioris ex binomio laterum; ut $\frac{y^3 - x^3}{y - x} = yy + yx + xx$. That is (in the language of M^r^ Leibnitz) $\frac{dy^3}{dy} = 3yy$.

A copy of M^r^ Newton's Letter of 10 Decem. 1672 was sent to M^r^ Leibnitz by M^r^ Oldenburg amongst the papers of M^r^ Iames Gregory at the same time 20^th^ M^r^ Newton's Letter of 13 Iune 1676. And M^r^ Newton having described in these two Letters that he had a very general Analysis consisting in the method of converging series, partly in another method by which he applyed those series to the solution of almost all problems & found the tangents areas, lengths, solid contents, centers of gravity, & curvities of curves & curvilinear figures without sticking at surds or mechanical curves & that the method of Tangents of Slusius was but a branch or corollary of this method: M^{r} Leibnitz in his return home through Holland was meditating upon the improvement of the method of Slusius For in a Letter to M^{r} Oldenburg dated from Amsterdam $\frac{18}{28}$ Novem. 1676 he wrote thus. Methodus Tangentium a Slusio publicata nondum rei fastigium tenet. Potest aliquid amplius præstari in eo genere, quod maximi fore usus ad omnis generis Problemata: etiam ad meam (sine extractionibus) Æquationum ad series reductionem. Nimirum, posset brevis quædam calculari circa Tangentes Tabula, eouque continuanda donec progressio Tabulæ apparet; ut eam scilicet quisque, quousque libuerit, sine calculo continuare possit. This was the improvement of the Method of Slusius which M^{r} Leibnitz was then thinking upon & by his words Potest aliquid amplius præstari in eo genere quod maximi foret usus ad omnis generis problemata, it seems to be the only improvement which he had then in his mind for making the method of Slusius general. The improvement by the differential calculus was not yet in his mind.

In spring following he received M^{r} Newtons Letter dated 242 Octob 1676: in which M^{r}Newton mentioned the Analysis communicated by D^{r} Barrow to M^{r} Collins & also another Tract written in 1671 about converging series & about the other method by which Tangents were drawn after the method of Slusius & maxima & minima were determined & the Quadrature of Curves was made more easy &c & this without sticking at radicals. And the foundation of these operations he comprehended in this sentence exprest enigmatically. Data æquatione fluentes quotcunque quantitates involvente fluxiones invenire & vice versa. Which puts it past all dispute that he had invented the method of fluxions before that time. And if other things in that Lett{er} be considered it will appear that he had then brought it to great perfection, the Propositions in his book of Quadratures & the methods of converging series & of drawing a Curve through any number of given points being then known to them.

After the receipt of this Letter M^{r} Leibnitz wrote back in a letter dated 21 Iunij 1677: Clarissime Slusij methodum Tangentium nondum esse absolutam Celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium generalius tractavi scilicet per differentias Ordinatarum. — Hinc nominando in posterum dy differentiam duarum proximarum y &c. Here M^{r} Leibnitz began first to propose his differential method: & there is not the large evidence that he knew it before this year. He affirms indeed jam a multo tempore rem tangentium generalius tractavi scilicet per differentias Ordinanatarum: but he is not a witness in his own case. A Iudge would be very unjust & act contrary to the laws of all nations <582r> who should admit any man to be a witness in his own case. And therefore it lies upon M^{r} Leibnitz to prove that he found out this method before the receipt of M^{r} Newton's Letters. And if he cannot prove this, the Question, Who was the first Inventor of the Method, is decided

D^{r} Barrow in his method of Tangents drawing two Ordinates indefinitely neare one another puts the letter a for the difference of the Abscissas & the letter e for the difference of the Ordinates & for drawing the tangents gives these three Rules. 1 Inter computandum, saith he, omnes abjicio terminos in quibus ipsarum a vel e potestas habetur vel in quibus ipsæ ducuntur in se. Etenim isti termini nihil valebunt. 2 Post æquationem constitutam omnes abjicio terminos literis constantes quantitates notas seu determinatas significantibus aut in quibus non habentur a vel e. Etenim illi termini semper ad unam æquationis partem adducti nihil adæquabunt. 3 Pro a Ordinatam & pro e subtangentem substituo. Hinc demum subtangentis quantitas dignoscetur. M^{r} Leibnitz in his Letter of 21 Iune 1677 abovementioned has followed this method exactly excepting that he has changed the letters a & e of D^{r} Barrow into dy & dx. For in the Example which he then gives he draws two parallel lines & sets all the terms below the under line in which dx & dy are (severally or joyntly) of more then one dimension & all the terms above the upper line in which dy & dx are wanting & for the reasons given by D^{r} Barrow makes all these terms vanish. And by the terms in which dy & dx are but of one dimension which he sets between the lines he determines the proportion of the subtangent to the ordinate. Well therefore did the Marquess de L' Hospital observe that where M^{r} Barrow left off M^{r} Leibnitz began: for their methods of Tangents are exactly the same

But Mr Leibnitz observes that the conclusion is coincident with the Rule of Slusius & shews how that Rule presently occurrs to any one who understands this method. And in the next place — — — — — — — — — — In the next place Mr Leibnitz shews how this method of tangents may be improved so as to procead in more unknown quantities then two & not to stick at radicalls. And then in relation to what Mr Newton had told him of these improvements he adds. Arbitror quæ celare voluit Newtonus de tangentibus ducendis, ab his non abludere. Quod addit, Ex hoc eodem fundamento quadraturas quoque reddi faciliores, me in hac sententia confirmat; nimirum semper figuræ illæ sunt Quadrabiles quæ sunt ad æquationes differentialem. By which words its manifest that Mr Leibnitz at this time understood that Mr Newton had a method which would do all these things & that his method was either the same with Dr Barrows method of Tangents improved & made general or another like it.

At length, vizt in November 1684, Mr Leibnitz published the Elements of his differential method in the Acta Eruditorum & illustrated it with examples of drawing Tangents & determining maxima & minima, & then added: Et hæc quidem initia sunt tantum Geometriæ cujusdam multo sublimoris ad difficillima et pulcherrima quæque etiam mistæ Matheseos Problemata pertingentis, quæ sine calculo nostro differentiali, AUT SIMILI non temere quisquam pari facilitate tractabit. The words AVT SIMILI plainly relate to Mr Newtons method.

And in the Acta Eruditorum of Iune 1686 pag 297 he added Malo autem dx et similia adhibere quam literas pro illis quia istud dx est modificatio quædam ipsius x &c. He knew that in this method he might have used better with Dr Barrow but chose rather to use the new symbols dx & dy, tho there is nothing which can be done by these symbols but may be done by letters with more brevity.

When Mr Newton wrote his Analysis he used letters for fluxions & the rectangles under those Letters & the moment o for moments. Mr Leibnitz has no symbols for fluxions & <582v> to this day & therefore al Mr Newton's symbols for fluxions are

And the true Church into the first Temple with seven Candlesticks & the second Temple with two Candlesticks, the Lamb with seven eyes & seven horns in the first Temple. & the son of Man with two eyes & two Leggs on mount Sion, the 144000 who in the persecution of the remnant of the womans seed nor raised by the Dragon receive the mark of God in their foreheads & remain with the Lamb on mount Sion & worship in the first Temple & the

And the true Church becomes divided into the first Temple with seven golden Candlesticks where the Dragon makes war upon the rem of the womans seed & into the second Temple with two Candlesticks of olive tree where the Gentiles composing the body of the Beast worship in the

The Empire becomes divided into the Dragon who being returned back from the woman begins to make war upon the remnant of her seed left by her in the court of the first Temple, & the Beast who now rises out of the Sea & whose people are the Gentiles to whom the outward Court of the second Temple is given. And the church at the same time becomes divided into two false churches

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Argumentum

The Letters & Papers till the year 1676 inclusively shew that Mr Newton had a general method of solving Problems by deducing them to equations finite or infinite, whether those equations include moments (the exponents of fluxions) or do not include them, & by deducing fluents & their moments from one another by means of those equations.

The Analysis printed in the beginning of this collection shews that he had such a general method in the year 1669. And by the Letters & Papers which follow, it appears that in the year 1671, at the desire of his friends he composed a larger Treatise on the same method (p. 27. l. 10, 27 & p. 71. l. 4, 26) that it was very general & easy without sticking at surds or mechanical curves & extended to the finding tangents, areas lengths centers of gravity & curvatures of Curves &c (p. 27, 30, 85) that in Problemes reducible to quadratures it proceeded by the Propositions since printed in the book of Quadratures, which Propositions are there founded upon the method of fluents (p. 72, 74, 76) that it extended to the extracting of fluents out of equations involving their fluxions & proceeded in difficulter cases by assuming the terms of a series & determining them by the conditions of the Probleme (p. 86) that it determined the species of the curve by the length thereof (p 24) & extended to inverse Problemes of Tangents & others more difficult, & was so general as to reach almost all Problemes except numeral ones like those of Diophantus (p. 55, 85, 86). And all this was known to M^r^ Newton before M^r^ Leibnitz began to write of the method as appears by the dates of their Letters.

For in the year 1673 M^r^ Leibnitz was upon another differential method (p. 32) In May 1676 he desired M^r^ Oldenburg to procure him the method of infinite series (p. 45) & in his Letter of Aug. 27^th^ 1676 he wrote that he did not beleive M^r^ Newton's method to be so general as M^r^ Newton had described it. For, said he, there are many Problemes & particularly the inverse Problemes of Tangents which cannot be reduced to equations or quadratures (p. 65) which words make it evident that M^r^ Leibnitz had not yet the method of differential equations. And in the year 1675, he communicated to his friends at Paris a Tract written in a vulgar manner about a series which he received from M^r^ Oldenburg & continued to polish in the year 1676 with intention to have it <584r> published, but it swelling in bulk he left off polishing it after other business came upon him, & afterwards finding out the differential Analysis he did not think it worth publishing because written in a vulgar manner p. 42, 45. In all these Letters & Papers there appears nothing of his knowing the Differential method before the year 1677. It is first mentioned by him in his Letter of Iune 21^th^ 1677, & there he began the description of it with these words: Hinc nominando IN POSTERVM dy differentiam duarum proximarum y &c p. 88.

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— And in the mean time I take the liberty to acquaint him, that by taxing the Royal Society with injustice in giving sentence against him without hearing both parties, he has transgressed one of their Statutes which makes it expulsion to defame them.

The Philosophy which M^r^ Newton in his Principles & Opticks has pursued is experimental, & it is not the business of experimental Philosophy to teach the causes of things any further then they can be proved by Experiments. We are not to fill this Philo sophy with opinions which cannot be proved by phænomena. In this Philosophy Hypotheses have no place unles as conjectures or Questions proposed to be examined by experiments. For this reason M^r^ Newton in his Optiques distinguished those things which remained uncertain & which he therefore proposed in the end of his Optiques in the form of Quæres. For this reason, in the Preface to his Principles, when he had mentioned the motions of the Planets Comets Moon & Sea as deduced in this book from gravity, he added: <u>Vtinam cætera Naturæ Phænomena ex Principijs Mechanicis eodem argumentandi genere derivare liceret. Nam multa ne movent ut nonnihil suspicer ea omnia ex viribus quibusdam pendere posse quibus corporum particulæ per causes nondum cognitas vel in se mutuo impelluntur & secundum figuras regulares cohærent, vel ab invicem fugantur & recedunt: quibus viribus ignotis Philosophi hactenus Naturam frustra tentarunt.</u> And in the end of this book in the second Edition, he said that he forbore to describe the effects of this [electrical] attraction for want of a sufficient number of experiments to determin the laws of its acting. And for the same reason he is silent about the cause of gravity, there occurring no experiments or phænomena by which he might prove what was the cause thereof: And this he hath abundantly declared in his Principles neare the beginning thereof in these words: <u>Virium causes et sedes Physicas jam non expendo</u>. And a little after: <u>Voces attractionis, impulsus vel propensionis cujuscunque in centrum indifferenter & pro se mutuo promiscus usurpo, has vires non physice sed mathematice tantum considerando. Vnde caveat Lector ne per hujusmodi voces cogitet me speciem vel modum actionis, causamve aut rationem physicam alicubi definire, vel centris (quæ sunt puncta Mathematica) vires vere et physice tribuere, si forte aut centraa trahere aut vires centrorum esse dicero.</u> And in the end of his Opticks: <u>Qua causa efficiente hæ attractiones</u> [sc. gravitas, visque magnetica et electrica aliæque] <u>peragantur, hic non inquiro. Quam ego attractionem appello fieri sane potest ut ea efficiatur impulsu vel alio aliquo modo nobis incognito. Hanc vocem attractionis ita hic accipi velim ut in universum solummodo vim aliquam significare intelligatur qua corpora ad se mutuo tendant, cuicunque demum causæ attribuenda sit illa vis. Nam ex phænomenis Naturæ illud nos prius idoctos oportet quænam corpora seinvicem attrahant, et quænam sint leges et proprietates istius attractionis, quam in id inquirere par sit quanam efficiente causa peragatur attractio</u> And a little after he mentions the same attractions as forces which by phænomena appear to have a being in nature, tho their causes be not yet known, & distinguishes them from occult qualities which are supposed to flow from the specific forms of things. And in the Scholium at the end of his Principles, after he had mentioned the properties of gravity, he added: Rationem vero <585v> harum gravitatis proprietatem ex phænomenis nondum potui deducere et hypothesis non fingo. Quicquid enim ex phænomenis non deducitur Hypothesis vocanda est, et Hypotheses seu Metaphysicæ seu Physicæ seu qualitatum occultarum seu Mechanicæ, in philosophia experimentali locum non habent. — Satis est quod Gravitas revera existat & agat secundum leges a nobis expositas, & ad corporum cœlestium et maris nostri motus omnes sufficiat. And after all this one would wonder that M^r Newton should be reflected upon for not explaining the cause of gravity by an Hypothesis, as if it were a crime to content himself with certainties & let uncertainties alone. 2 Whether the cause of gravity be mechanical or not Mechanical he hath no where affirmed And yet the Editors of the Acta Eruditorum[a][3] 2 have accused him of denying that the cause of gravity is mechanical. He hath no where said that Gravity is essential to matter or an occult quality or a miracle, & yet M^r Leibnitz[b][4] hath accused him of making gravity a natural or essential property of bodies & an occult quality & a miracle.

It is true that these two Gentlemen differ very much in Philosophy. The one proceeds upon the evidence arising from Experiments & Phænomena, & stops where such evidence is wanting the other is taken up with Hypotheses, & propounds them not to be exaimined by Experiments, but to be beleived without examination. The one, for want of experiments to decide the Question, doth not affirm whether the cause of gravity be mechanical or not mechanical: the other, that it is a perpetual miracle if it be not mechanical. The one, by way of inquiry, attributes it to the will of the creator that the least particles of matter are hard: the other attributes the hardness of matter to conspiring motions, & calls it a perpetual miracle if the cause of this hardness be other than mechanical. The one doth not affirm that animal motion in man is purely mechanical: the other teaches that it is purely mechanical; the soul or mind (according to the Hypothesis of an Harmonia præstabilita) never acting upon the body, so as to alter or influence its motions. The one teaches that God (the God in whom we live & move & have our being) is Omnipresent: not as the soul of the world: the other that he is not the soul of the world but INTELLIGENTIA SVPRAMUNDANA an Intelligence above the bounds of the world: Whence it seems to follow that he cannot do any thing within the world unless by an incredible miracle. The one teaches that Philosophers are to argue from Phænomena & Experiments to the causes thereof, & thence to the causes of those causes, & so on till we come to the first cause: the other that all the actions of God are miracles, & all the laws imprest on nature by the will of God are perpetual miracles & occult qualities, & therefore not to be considered by Philosophers. But why must it go for a miracle < insertion from above the line > if the first cause of things & the governor of the Vniverse hath inter <586r> medled with the world since the first creation, & so < text from f 585v resumes > hath any thing to do with the world? < insertion from f 586r > Why must God be removed out of the bounds of the Vniverse? Why must the body of a man be a mere Machin acting without the influence of his mind & nothing but matter be left within those bounds <585v> Why must the laws of nature <586r> if derived from the power of God, be called miracles & occult qualities, that is to say, wonders & absurdities? < text from f 585v resumes > And why must all the arguments for a God taken from the Phænomena of Nature, be exploded by hard-names? For certainly Philosophers are rather to argue without railing, thon to rail without arguing.

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And if the body of a man be not a meer machine acting without the influence of his mind; if God be not removed out of the bounds of the Vniverse; if the first cause of things & the Author of the Vnivers may have sometimes & in some places intermedled with the World since the first creation; if he created all things for certain reasons & with certain designes: why must the laws of Nature if derived from the will of God be called miracles & occult qualities, that is to say, wonders & absurdities? & why must all the arguments for a Deity taken from Phænomena be exploded by hard names? For certainly Philosophers are to argue without railing & not to rail without arguing. They are to examin Opinions by Phænomena & not to publish & press opinions to be believed without examination. The Editors of the Acta Eruditorum have indeed accused M^r Newton of publishing an Hypothesis in the end of his Principles about the actions of a very subtile spirit. And if he had done so, yet its more excusable to propose one Hypothesis amongst many things proved then to propose nothing but Hypotheses. But M^r Newton did not propose it by way of an Hypothesis but in order to an inquiry as by his words may appear to any unprejudiced person. After he had shewed the laws, power & effects of Gravity without medling with the cause thereof & from thence deduced all the motions of the great bodies in the system of the Vniverse, he added in a few words his suspicions about another sort of attraction between the small parts of bodies upon which many Phæneoman might depend, & for want of a sufficient number of experiments left the enquiry to others who might hereafter have time & skill enough to pursue it, & to give them some light into the enquiry mentioned two or three of the principal phænomena which might arise from the actiosn of the Spiprit or Agent by which this Attraction is performed. He has told his friends that there are sufficient Phænomena to ground an inquiry upon but not yet sufficient to determin the laws of this attraction.

And why must the Philosophy of M^r^ Newton be exploded as miraculous & absurd because he has taken time to consider whether all the Phænomena in Nature can be solved by mere mechanical causes & the solutions proved by experiments & for want of such a proof has not yet declared his opinion that they can.

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He hath suggested a suspicion that there is a subtile Spirit or Agent latent in bodies by which Electrical Attraction & many other phænomena may be performed, & proposed this matter not to be beleived without proof but to be inquired into by experiments: but the Editors of the Acta Eruditorum tell us that if this Agent be not the subtile matter of teh Cartesians it will be looked upon as a trifle. [These Gentlemen are so much accustomed to Hypotheses that we must not enquire into the properties & effects of the Agent by which electrical attraction is performed, & of that by which light is emitted reflected & refracted, & of that by which sensation is performed &c & inform our selves whether they be not one & the same agent; unless we have first explained by an hypothesis what this Agent may be. We must not pursue experimental Philosophy by experiments untill we founded it upon hypotheses] And by these indirect practises they would have it beleived that M^r^ Newton was unable to find the infinitesimal method. / And must experimental Philosophy be rendred uncertain by filling it with Opinions not yet proved by any experiments?] If M^r^ Leibnitz never found but a new experiment in all his wlife for proving any thing; If M^r^ Newton has by so great multitude of new Experiments discovered many things about light & colours & settled a new Theory thereof never to be shaken; If M^r^ Newton by the infinitesimal Analysis applyed to Geometry & Mechanicks, has setled the Theory of the Heavens, & M^r^ Leibnitz in his Tentamen de Motuum Cœlestium causis has endeavoured to imitate him, but without success for want of skill in this Analysis. If M^r^ Leibnitz has pretended that he had the infinitesimal Analysis before he had it in order to step before M^r^ Newton; If he has done the like in several other inventions. If he has concealed from the world what he received from M^r^ Oldenburg concerning this Method & concerning the series of Gregory in order to make them his own; if when he had possessed Geromany with an opinion that they were his own he accused M^r^ Newton of plagiary in order to make the same opinion be received in England, but has not produced any one good argument for proving his pretense; if for want of arguments he has insisting upon his own candor as if it were injustice to question it & in the very same Letter questioning the candor of M^r^ Newton, & by doing so has endeavoured to make himself a witness in his own cause contrary to the laws of all nations; if after he had accused M^r^ Keill & moved the R. Society to make him recant & was put upon proving the accusation, he declined to do it if for want of arguments against

And must Experimental Philosophy be exploded as miraculous & absurd because it asserts nothing more then can be proved by experiments, & we cannot yet prove by experiments that all the Phænomena in Nature can be solved by mere mechanical causes? Certainly these things deserve to be better considered.

So then M^r^ Leibnitz

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M^r^ Gregory being told by M^r^ Collins that I had a general method of series & having received one of my Series from M^r^ Collins, after a year's study found out the method. M^r^ Leibnitz had more light into the method of fluxions & the method was much easier to be found out. And perceiving by any Letter**{s}** that it readily gave the method of tangents of Slusius, he was in his journey from London through Holland into Germany studying how to make the method of Tangents of Slusius extend to all Problems as I find by a letter of his dated from Amsterdam. And at length in a Letter dated from Hannover 21 Iune 1677 he wrote back Clarissimi Slusij methodum tangentium nondum esse absolutam celeberrimo Newtono assentior. Et jam a multo tempor**{e}** rem tangentium longe generalius tractavi. And then subjoyned a method of tangents published by D^r^ Barrow in the year 1670, but disguised it by a new notation to make it his own, & shewed how this method readily gave the Rule of Slusius & might be improved so as not to stick at surds. And from these characters concluded that he took my method to be like it, especially since both of them faciliated Quadratures.

In the year 1684 (M^r^ Oldenburg & M^r^ Collins being then dead) he published this method so far as it related to tangents & maxima & minima & added that it extended to the abstruser Problems of Geometry such as could not be solved without this method or another like it, meaning my method, but did not yet shew how to apply it to such abstruser Problems.

In the year 1687 my book of Principles came abroad, which was full of such Problems as (according to M^r^ Leibnitz) could not be resolved without the differential method or another like it. And M^r^ Leibnitz in a Letter to me dated from Hanover 17 March 1693 & still extant, acknowledged the same thing of this book. And in the Acta Eruditorum for May 1700 p. 306. l. 89 said further of this Book, that no man before M^r^ Newton had by a specimen made publick, proved that he had this method. This book was therefore by the acknowledgement of M^r^ Leibnitz himself in those days, the first specimen made publick of applying this method to the difficulter Problemes of Geometry. And the next specimen was that of three papers published by M^r^ Leibnitz in the the year 1689 concerning Opticks, the resistance of Mediums, & the systeme of the heavens: All which were nothing else then Propositions taken from my book of Principles & put into a new form of words & intermixt with some physical Hypotheses & claimed by M^r^ Leibnitz as invented by himself before my book came abroad. And its very remarkable that M^r^ Leib. to make my Principal Proposition his own, adapted to it an erroneous demonstration by which it was impossible to invent it.

Hitherto the Differential Method had made no noise, but the next year it was taken notice of by M^r^ Iames Bernoulli, & from that time to be celebrated more & more in Germany France & Holland while the method of fluxions was celebrated in England. In this state things continued till the death of D^r^ Wallis which happened in October 1703 And now **{**atte**}{illeg}** upon any saying in the Introduction to the book of Quadratures that I found the method of fluxions gradually in the years 1665 & 1666, I was traduced in the Acta Eruditorum as a lying plagiary,

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What he saith about Philosophy is foreign to the Question & therefore I shall be very short upon it. He denys conclusions without telling the fault of the premisses. His arguments against me are founded upon metaphysical & precarious hypotheses & therefore do not affect me: for I meddle only with experimental Philosophy. He changes the signification of the words Miracles & Occult qualities that he may use them in railing at universal gravity. For Miracles are so called not because they are the actions of God but because they happen seldom & by happening seldom create wonder. If they happened constantly they would not be wonders. And occult qualities are decried not because their causes are unknown, but because the Schoolmen beleived that those things which were unknown to their Master Aristotel, could never be known. He insinuates that I ascribe to God a sensorium in a literal sense, which is a fiction He presents that God must be Intelligentia supramundana least he should be the soul of the world & by the same way of reasoning a man may prove that the soule of a man cannot be in his head least it should be the soul of the Images, of Objects formed in the sensorium. He represents that God has made this world so perfect that it can last eternally without needing any amendments because God was able to make it, so, & by the same way of arguing a man may prove that matter can think. He pleads for Hypothetical philosophy because there may happen experiments to decide which of the Hypoteses are true, & yet almost all his Philosophy consists in metaphysical Hyposeses such as never were never can be decided by experiments, one of them (that of the Harmonia præstabilita) is contrary to the daily experience of all mankind. For every man finds in himself a power of moving his body by his will it is because he has spent all his life in corresponding with men of all nations for propagating his opinions whilst I have rested & left truth to shift for it self Hypotheses may be propended by way of Questions to be examined by experiments: but when they are grounded as Opinions to be beleived without examination, they turn Philosophy into a Romance. He boasts of the number of his disciples, that is of his having spent all his life in keeping a correspondance with men of all nations, to make disciples whilst I keep no such correspondence but leave truth to shift for it self.

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finding how to deduce the Method of Slusius from the Differential method of Tangents published in the year 1670 by D^r^ Barrow; he wrote back (21 Iune 1677) Clarissimi Slusij Methodum Tangentium nondum esse absolutam Celerrimo Newtono assentior. Et jam a multo tempore rem tangentium longe generalius tractavi, scilicet per differentias Ordinatarum. And then he set down D^r^ Barrows method of Tangents as his own, & shewed how this method readily gave the Method of Slusius & might be improved so as not to stick at surds. And from these circumstances concluded that he took my Method to be like it, especially since both of them faciliated Quadratures.

In the year 1684 (M^r^ Oldenburg & M^r^ Collins being both dead) M^r^ Leibnitz published this method so far as it related to Tangents & Maxima & Minima & added that it extended to the abstruser Problems of Geometry, such as could not be solved without this method or another like it, meaning my method; but did not yet shew how to apply it such abstruser Problems

In the year 1687 my Book of Principles came abroad which was full of such Problems as (according to M^r^ Leibnitz) could not be resolved without the Differential method or another like it. And M^r^ Leibnitz in a Letter to me dated from Hannover 17 March 1693 & still extant, gave the same testimony of this book in these words: Mirifice ampliaveras Geometriam tuis seriebus, sed edito Principiorum opere ostendisti patere tibi etiam quæ Analysi receptæ non subsunt. Conatus sum Ego quoque, notis commodis adhibitis quæ Differentias et Summas exhibeant, Geometriam illam quam Transcendentem appello Analysi quodammodo subjicere: nec res male processit. And in the Acta Eruditorum for May 1700 pag. 306, lin. 89 he said further of this Book, that no man before me had by a specimen made publick, proved that he had this method. This Book was therefore by the acknowledgement of M^r^ Leibnitz himself the first specimen made publick of applying this method to the difficulter Problemes of Geometry

And the next Specimen was that of three Papers published by M^r^ Leibnitz in the year 1689 concerning Opticks, the resistance of Mediums & the systeme of the heavens. All which were nothing else then Propositions taken from my Book of Principles & put into a new form of words & intermixt with some Physical Hypotheses & claimed by M^r^ Leibnitz as invented by himself long before my book came abroad. And its very remarkable that M^r^ Leibnitz to make my principal Proposition his own, adapted to it an erroneous Demonstration by which it was impossible to invent it.

Hitherto the Differential Method had made no noise, but the next year it was taken notice of by M^r^ Iames Bernoulli who published an Example of this Calculation in the Acta Eruditorum for May 1690. And from that time the Method began to be celebrated more & more in Germany France & Holland while the Method of fluxions was celebrated in England my Book of Quadratures being handed about among my friends.. And this made M^r^ Leibnitz, after he had said in the Acta Eruditorum of May 1700 that no man before me had proved by a published specimen that he had the method, subjoyn that no man before the Bernoullis & himself had communicated the method I first published the difficulter Problemes resolved: they afterwards published the resolution of the difficulter Problemes. For as the Ancients invented their Propositions by Analysis & then compounded them, & for preserving the certainty <591r> of Geometry, which is the glory of this science, admitted no Propositions into it till they were demonstrated by composition: so I first invented the Propositions in the Book of Principles by Analysis & then demonstrated them by composition that they might be admitted into Geometry. And tho this Book was written by Composition (as all things in Geometry ought to be) yet the Analysis of moments shines through the Composition so clearly that the Marquis de L'Hospital wrote that this Book was <u>presque tout de ce calcule</u>, & M^r^ Leibnitz himself that it was a proof that I had this Analysis, & the first public proof which any man gave that he had it.

Dr Wallis died in October 1703, the last of the old men who knew what had passed between Mr Leibnitz & me by means of Mr Oldenburg. And afterwards I was accused in the Acta Eruditorum & before the Royal Society as a Plagiary who had substituted Fluxions for Differences & thereby taken the Method from Mr Leibnitz. And when the Royal Society caused the ancient Letters & Papers remaining in their Archives & Letter Books & in the Library of Mr Collins to be published all which are unanswerable matter of fact: instead of answering the same in a fair manner, & proving his accusation of plagiary against me, a defamatory Libel was published against me in Germany without the name of the Author or Publisher or City where it was published, & dispersed over Germany France & Italy. & the Libel it self represents that Mr Leibnitz set it on foot.

In the latter part of his Postscript he departs from the Question & falls foul upon my Philosophy as if I (and by consequence the ancient Phenicians & Greeks) introduced Miracles & occult qualities. And to make this appear he gives the name of Miracles or Wonders to the laws imprest by God upon Nature tho by reason of their constant working they create no Wonder; & that of occult qualities to qualities which are not occult but whose causes are occult tho the qualities themselves be very manifest. He said that God is Intelligentia supramundana because if he were in the world he would be the soul of the world, that is, he would animate the world, & yet according to his Philosophy (that of an Harmonia præstabilita) the soul of a man doth not animate his body. He accuses me as if I affirmed that God hath a Sensorium in a litteral sense. He saith that I have not demonstrated a Vacuum nor universal gravity. but he denies Conclusions without shewing the fault of the Premisses, & seems to mean that the argument of Induction from experiments upon which experimental Philosophy is founded is not a demonstration & therefore ought to be respected. He saith also that I have not proved Atomes: but I have not affirmed them but place them among a set of Queries. He saith that Space is the order of coexistences & time the order of successive existences: I suppose he meanes that space is the order of coexistences in space, & time the order of successive existences in time, or that space is space in space & time is time in time. He calls the world Gods Watch, & insinuates that it is the fault of the workman & not of the materials **{if}** a Watch will at length cease to go, & in like manner that it would be Gods fault if his Watch should ever decay & want an amendments. And by the same way of arguing a man may say that it would be Gods fault if matter doth not think. He applauds experimental Philosophy, but recommends Hypotheses to be admitted into Philosophy in order to <589r> be examined by experiments: whereas almost all his Hypotheses are uncapable of such an examination, & he should recommend not Hypoteses to be admitted & beleived before examination, but Questions to be examined & decided by experiments before they are admitted into Philosophy & proposed to be beleived. And whilst he applauds experimental Philosophy & exclaims against Miracles, he introduces an Hypothesis of Harmonia præstabilita which cannot be true without an incredible Miracle, & is contrary to this daily experience of all mankind. For all men find by experience that they can move their bodies by their will, & that they see heare & feel by means of their bodies. And if, notwithstanding all this, he glories in the number of disciples, you know what his disciples are in England & that he has spent his life in keeping a general correspondence for making disciples, whilst I leave truth to shift for it self. For its about 40 years since I left of all correspondence by Letters about Mathematicks & Philosophy, & about 20 since I left off these studies And for that reason I hope you will pardon me if I have been averse from writing this Letter, & continue averse from being engaged in disputes of this kind which make nothing to the Question in hand.

He sends you also Mathematical Problemes to be solved by the English Mathematicians. And all this I look upon as nothing else then an amusement to avoid proving his accusation against me & returning a fair answer to the matter of fact which has been published by order of the Royall Society.[5]

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The next year Mr Newtons Principia Philosophiæ came abroad, a Book full of the difficulter Problemes quæ sine calculo Differentiali aut simili non temere quisquam pari facilitate tractabit. And the Marquess de L'Hospital has represented this book presque tout de ce calcul; to consist almost wholly of this calculus. In the second Lemma of the second book, the elements of this calculus are demonstrated synthetically & at the end of the Lemma there is a Scholium in these words. In literis quæ mihi cum Geometra peritissimo G. G. Leibnitsio annis abhinc decem . . . . Vtriusque fundamentum continetur in hoc Lemmate. This was written in the year 1686.

In the year 1689 M^r Leibnitz published in the Acta Lepsica: these papers with relation to this book pretending to have found the same things about Opticks, about the resistance of Mediums & about the motions of the Planets. And henceforward the Differencial method began to be taken notice of abroad. In the first of these papers intituled De Lineis Opticis Re writes: A me ut obiter hic dicam, methodo serierum promovendæ præter transformationem irrationalium linearum in rationales symmetras — excogitata est ratio pro curvis transcendentes datis ubi ne extractio quidem locum habet. Assumo enim seriem arbitrariam, eamque ex legibus Problematis tractando obtineo ejus coefficientes. But this was invented by M^r Newton many years before being set down in his Letter of 24 Octob. 1676 [in these words Altera [methodus consistit] tantum in assumptione] M^r Leibnitz in his Letter of 27 Aug. 1676 had written Quod dicere videmini plerasque difficultates (exceptis problematibus Diophantæis) ad series infinitas reduci, id mihi non videtur. Sunt enim multa usque adeo mira et implexa ut neque ab æquationibus pendeant neque ex quadraturis Qualia sunt (ex multis alijs) Problemata mathodi tangentium inversæ. M^r Newton in his Letter of Octob. 24 1676 replied. Inversa de tangentibus Problemata sunt in potestate, aliaque illis difficiliora. Ad quæ solvenda usus sum duplici methodo: una concinniori, altera generaliori. Vna methodus consistit in extractione fluentis quantitatis ex æquatione simul involvente fluxionem ejus: altera tantum in assumptione seriei pro quantitate qualibet incognita ex qua cætera commode derivari possunt & in collatione terminorum homologorum æquationis resultantis ad eruendos terminos assumptæ seriei. By the words of M^r Leibnitz its manifest that he had not the method at that time, & by those of M^r Newton, that he had it.

In the second paper intituled Schediasma de Resistentia Medij M^r Leibnitz tought to do by Logarithms what M^r Newton had taught to do by the area of the Hyperbola; & represented that he had for the most part found out those things twelve years before while he was yet at Paris, that is, before he had the Differential method. And in the end of this paper (as if M^r Newton from whom he copied had done nothing) he adds Nobis nunc fundamenta Geometrica jecisse suffecerit, in quibus maxima consistebat difficultas. Et fortasse attente consideranti vias quasdem novas vel certe antea impeditas aperuisse videbimur. Omnia autem respondent nostræ Analysi infinitorum hoc est calculo summarum & differentiarum (cujus elementa quædam in his Actis dedimus) communibus quoad licuit verbis hic expresso. In the sixt Article of this Schediasma M^r Leibnitz endeavoured to make a step beyond what M^r Newton had done, viz^t in determining the Curve line which a Projectile describes with a resistance in a duplicate ratio of the velocity. But in doing this he has erred.

In the Paper entituled Tentamen de motuum cœlestium causis, M^r Leibnitz layd down several Hypotheses & definitions in the first Eleven Articles. The 12^th Article is true in concentric circles false in all other figures. The 15^th is false. The 19^th is the Principal Proposition of M^r Newton. M^r Leibnitz in attempting to demonstrat**{e}** it commited two errors which correct one another, & by adapting to it this erroneous demonstration he pretended to have found it himself. M^r Leibnit**{z}**

This he said, not knowing that the Method was communicated to M^r Collins in the year 1669 in the above mentioned Compendium. & in maintaining it **{**prenect**}** that M^r Newton should declare his opinion refused to contend with any man but M^r Newton as if all others were novices.

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& according in his Letter of 27 Aug. 1676 sent it composed & polished vulgari more.

The same is further manifest by the following consideration. D^r Barrow published his method of Tangents in the year 1670. M^r Newton in his Letter dated 10 Decem 1672 communicated his method of Tangents to M^r Collins & then added. Hoc est unum particulare . . . . reducendo eas ad series infinitas. M^r Slusius sent his method to M^r Oldenburg 17 Ian 1673 & the same . . . . .) $\frac{\mathrm{dy}^3}{\mathrm{dy}} = 3\mathrm{yy}$. A copy of M^r Newtons Letter of 10 Decem 1672 was sent . . . . The improvement by the differential calculus was not yet in his mind.

In spring following . . . . that he had then brought it to perfection, the Propositions in his book of Quadratures & the methods of converging series & of drawing a Curve line through any number of given points being then known to him For when the method of fluxions proceeds not in finite equations he reduces the equations to converging series, & when finite equations are wanting he deduces converging series from the **{**con-**}** & when fluents are to be derived from fluxions & the law of the fluxions is wanting he finds that law quam proxime by drawing a curve line through any number of given points.

After the receipt of this Letter, M^r Leibnitz . . . . Who was the first Inventor of the method is decided.

conditions of the Probleme by assuming the terms of the Series gradually & determining them by those conditions & when fluents are to be derived from fluxions & the law of the fluxions is wanting, he finds that law quam proxime by drawing a Parabolick line through any number of given points. [And by these improvements M^r Newton had in those days made the his method of fluxions much more universal then the differential method of M^r Leibnitz is at present.]

This Letter of M^r Newton dated 24 Octob 1676 came not to the hands of M^r Leibnitz till the end of the winter following or beginning of the spring & M^r Leibnitz soon after viz^t in a Letter dated 21 Iune 1677 wrote back: Clarissimi . . . . before the receipt of M^r Newton's last Letter. He affirms indeed, jam a multo tempore rem tangentium generalius tractavi scilicet per differentias Ordinatarum, & so he affirmed in other Letters that he had invented several converging series direct & inverse before he had the methods of finding them & had forgot an inverse method of series before he knew what use to make of it: but no man is a witness in his own cause. A Iudge . . . decided.

And M^r Iames Bernoulli in the Acta Eruditorum of Ianuary 1691 pag 14 writes thus. Qui calculum Barrovianum, quem in Lectionibus suis Geometricis adumbravit Auctor, cujusque specimina sunt tota illa Propositionum inibi contentarum farrago) intellexerit [calculum] alterum a D^no Leibnitio inventum ignorare vix poterit; utpote qui in priori illo fundatus est, & nisi forte in differentialium notatione & operationis aliquo compendio ab eo non differt. [And afterwards in the Acta of Iune 1691 pag 290 he speaks thus of the Compendium by which the methods differ. Cæterum in his Problematibus omnibus . . . . . . maxime commendat.

And M^r Iames Bernoulli in the Acta Eruditorum of Ianuary Iune 1691 pag. 14 & 290] Now D^r Barrow in his Method of Tangents draws two Ordinates indefinitely neare one another & puts . . . . . . exactly the same [But M^r Leibnitz adds this improvement of the method that the conclusion of the calculus is coincident with the Rule of Slusius & shews how that Rule presently occurrs to any one who understands this method. For M^r Newton had represented in his Letters that this was one character his general method.

And whereas M^r Newton had said that his method in drawing of tangents, determining maxima & minima, &c proceeded without sticking at surds: M^r Leibnitz in the next place

By saying ante Dominos Bernoullios et me nullus [methodum] communicavit, he did no**{**t**}** know that the above mentioned compendium of the metho**{**d**}** communicated to M^r Collins in the year 1669 was still extant & **{**w**}**ould be produced **{**&**}** appear to be genuine. And if this Compendium had been published a little sooner by M^r Iones, it might have prevented

[1] p 19

[2] p 14, 15

[3] Anno 1714 mense Martio p. 142

[4] In Tractatu De Bonitate Dei & in Epistolis ad D. Hartsoeker, & alibi

[5] Written in