AN ACCOUNT OF THE BOOK ENTITULED COMMERCIUM EPISTOLICUM COLLINII ET ALIORUM, DE ANALYSI PROMOTA

By

Isaac Newton

(Philosophical Transaction of the Royal Society of London, N°. 342.
January and February $171\frac{4}{5}$, pp. 173–224)

Edited by David R. Wilkins

2002

NOTE ON THE TEXT

This edition has been prepared from the original text in the *Philosophical Transactions of the Royal Society of London*, N^{o} 342, for the months of January and February which concluded the year 1714 in the Julian calendar then employed in England (being the first two months of the year 1715 in the Gregorian calendar then in use on the Continent of Europe).

This Account appeared anonymously, but is known to have been written by Sir Isaac Newton.

An occurrence of 'then' on p. 185 of the original text in the *Philosophical Transactions* has been corrected to 'than'.

The words 'Years 1669' on p. 204 of the original text in the *Philosophical Transactions* has been corrected to 'Year 1669'.

The following spellings, differing from modern British English, are employed in the original text: examin, entred, inclosed, least [lest], determin, untill, perswading.

David R. Wilkins
Dublin, June 2002

An Account of the Book entituled Commercium Epistolicum Collinii & aliorum, De Analysi promota; *published by order of the* Royal-Society, *in relation to the Dispute between Mr.* Leibnitz *and Dr.* Keill, *about the Right of Invention of the Method of* Fluxions, *by some call'd* the Differential Method.

[*Philosophical Transaction of the Royal Society of London*, N°. 342.
January and February $171\frac{4}{5}$, pp. 173–224.]

SEVERAL Accounts having been published abroad of this *Commercium*, all of them very imperfect: It has been thought fit to publish the Account which follows.

This *Commercium* is composed of several ancient Letters and Papers, put together in order of Time, and either copied or translated into Latin from such Originals as are described in the Title of every Letter and Paper; a numerous Committee of the Royal-Society being appointed to examin the Sincerity of the Originals, and compare therewith the Copies taken from them. It relates to a general Method of resolving finite Equations into infinite ones, and applying these Equations, both finite and infinite, to the Solution of Problems by the Method of Fluxions and Moments. We will first give an Account of that Part of the Method which consists in resolving finite Equations into infinite ones, and squaring curvilinear Figures thereby. By Infinite Equations are meant such as involve a Series of Terms converging or approaching the Truth nearer and nearer *in infinitum*, so as at length to differ from the Truth less than by any given Quantity, and if continued *in infinitum*, to leave no Difference.

Dr. *Wallis* in his *Opus Arithmeticum* published A. C. 1657: *Cap.* 33. *Prop.* 68. reduced the Fraction $\frac{A}{1-R}$ by perpetual Division into the Series

$$A + AR + AR^2 + AR^3 + AR^4 + \&c.$$

Viscount *Brounker* 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,

$$1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} + \&c.$$

conjoyning every two Terms into one. And the Quadrature was published in the *Philosophical Transactions* for *April* 1668.

Mr. *Mercator* soon after published a Demonstration of this Quadrature by the Division of Dr. *Wallis*; and soon after that Mr. *James Gregory* published a Geometrical Demonstration thereof. And these books were a few Months after sent by Mr. *John Collins* to Dr. *Barrow* at *Cambridge*, and by Dr. *Barrow* communicated to Mr. *Newton* (now Sir *Isaac Newton*) in

June 1669. Whereupon Dr. *Barrow* mutually sent to Mr. *Collins* a Tract of Mr. *Newton*'s entituled *Analysis per æquationes numero terminorum infinitas.* And this is the first Piece published in the *Commercium*, and contains a general Method of doing in all Figures, what my Lord *Brounker* and Mr. *Mercator* did in the Hyperbola alone. Mr. *Mercator* lived above ten Years longer without proceeding further than to the single Quadrature of the Hyperbola. The Progress made by Mr. *Newton* shews that he wanted not Mr. *Mercator*'s Assistance. However, for avoiding Disputes, he supposes that my Lord *Brounker* invented, and Mr. *Mercator* demonstrated, the Series for the Hyperbola some Years before they published it, and, by consequence, before he found his general Method.

The aforesaid Treatise of *Analysis* Mr. *Newton*, in his Letter to Mr. *Oldenburgh*, dated *Octob.* 24. 1676, mentions in the following Manner. *Eo ipso tempore quo* Mercatoris *Logarithmotechnia prodiit, communicatum est per amicum D.* Barrow (*tunc* Matheseos *Professorem* Cantab.) *cum D.* Collinio *Compendium quoddam harum Serierum, in quo significaveram Areas & Longitudines Curvarum omnium, & Solidorum superficies & contenta ex datis rectis; & vice versa ex his datis rectas determinari posse: & methodum indicatam illustraveram diversis seriebus.* Mr. *Collins* in the Years 1669, 1670, 1671 and 1672 gave notice of this Compendium to Mr. *James Gregory* in *Scotland*, Mr. *Bertet* and Mr. *Vernon* then at *Paris*, Mr. *Alphonsus Borelli* in *Italy*, and Mr. *Strode*, Mr. *Townsend*, Mr. *Oldenburgh*, Mr. *Dary* and others in *England*, as appears by his Letters. And Mr. *Oldenburg* in a Letter, dated *Sept.* 14. 1669. and entred in the Letter-Book of the Royal-Society, gave notice of it to Mr. *Francis Slusius* at *Liege*, and cited several Sentences out of it. And particularly Mr. *Collins* in a Letter to Mr. *James Gregory* dated *Novemb.* 25. 1669. spake thus of the Method contained in it. Barrovius *Provinciam suam publicè prælegendi remisit cuidam nomine* Newtono Cantabrigiensi, *cujus tanquam viri acutissimo ingenio præditi in Præfatione Prælectionum Opticarum, meminit: quippe antequam ederetur* Mercatoris *Logarithmotechnia, eandem methodum adinvenerat, eamque ad omnes Curvas generaliter & ad Circulum diversimode applicârat.* And in a Letter to Mr. *David Gregory* dated *August* 11. 1676. he mentions it in this manner. *Paucos post menses quam editi sunt hi Libri* [viz. *Mercatoris* Logarithmotechnia & Exercitationes Geometricæ *Gregorii*] *missi sunt ad* Barrovium Cantabrigiæ. *Ille autem responsum dedit hanc infinitarum Serierum Doctrinam à* Newtono *biennium ante excogitatam fuisse quam ederetur* Mercatoris *Logarithmotechnia & generaliter omnibus figuris applicatam, simulque transmisit D.* Newtoni *opus manuscriptum.* The last of the said two Books came out towards the End of the year 1668, and Dr. *Barrow* sent the said Compendium to Mr. *Collins* in *July* following, as appears by three of Dr. *Barrow*'s Letters. And in a Letter to Mr. *Strode*, dated *July* 26. 1672, Mr. *Collins* wrote thus of it. *Exemplar ejus* [Logarithmotechniæ] *misi* Barrovio Cantabrigiam, *qui quasdam* Newtoni *chartas extemplò remisit: E quibus & aliis quæ prius ab authore cum* Barrovio *communicata fuerant, patet illam methodum à 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 infinitè verò propinqua, Evolutio vel longitudo Lineæ Curvæ, Centrum gravitatis figuræ, Solida ejus rotatione genita & eorum superficies; sine ulla radicum extractione obtineri queant. Postquam intellexerat D.* Gregorius *hanc methodum à 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. Uterque*

D. Newtonus *&* Gregorius *in animo habet hanc methodum exornare: D.* Gregorius *autem D.* Newtonum *primum ejus inventorem anticipare haud integrum ducit.* And in another Letter written to Mr. *Oldenburgh* to be communicated to Mr. *Leibnitz*, and dated *June* 14 1676, Mr. *Collins* adds: *Hujus autem methodi ea est præstantia ut cum tam late pateat ad nullam hæreat difficultatem.* Gregorium *autem aliosque in ea fuisse opinione arbitror, ut quicquid uspiam antea de hac re innotuit, quasi dubia diluculi lux fuit si cum meridiana claritate conferatur.*

This Tract was first printed by Mr. *William Jones*, being found by him among the Papers and in the Hand-writing of Mr. *John Collins*, and collated with the Original which he afterwards borrowed of Mr. *Newton.* It contains the above-mention'd general Method of *Analysis*, teaching how to resolve finite Equations into infinite ones, and how by the method of Moments to apply Equations both finite and infinite to the Solution of all Problems. It begins where Dr. *Wallis* left off, and founds the method of Quadratures upon three Rules.

Dr. *Wallis* published his *Arithmetica infinitorum* in the Year 1655, and by the 59*th* Proposition of that Book, if the *Abscissa* of any curvilinear Figure be called x, amd m and n be Numbers, and the Ordinates erected at right Angles be $x^{\frac{m}{n}}$, the Area of the Figure shall be $\dfrac{n}{m+n}x^{\frac{m+n}{n}}$. And this is assumed by Mr. *Newton* as the first Rule upon which he founds his Quadrature of Curves. Dr. *Wallis* demonstrated this Proposition by Steps in many particular Propositions, and then collected all the Propositions into One by a Table of the Cases. Mr. *Newton* reduced all the Cases into One, by a Dignity with an indefinite Index, and at the End of his Compendium demonstrated it at once by his method of Moments, he being the first who introduced indefinite Indices of Dignities into the Operations of Analysis.

By the 108*th* Proposition of the said *Arithmetica Infinitorum*, and by several other Propositions which follow it; if the Ordinate be composed of two or more Ordinates taken with their Signes + and −, the Area shall be compos'd of two or more Areas taken with their Signes + and − respectively. And this is assumed by Mr. *Newton* as the second Rule upon which he founds his Method of Quadratures.

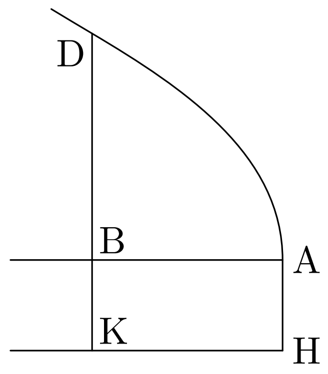
And the third Rule is to reduce Fractions and Radicals, and the affected Roots of Equations into converging Series, when the Quadrature does not otherwise succeed; and by the first and second Rules to square the Figures, whose Ordinates are the single Terms of the Series. Mr. *Newton*, in his Letter to Mr. *Oldenburgh* dated *June* 13. 1676. and communicated to Mr. *Leibnitz*, taught how to reduce any Dignity of any Binomial into a converging Series, and how by that Series to square the Curve, whose Ordinate is that Dignity. And being desired by Mr. *Leibnitz* to explain the Original of this Theoreme, he replied in his Letter dated *Octob.* 24. 1676, that a little before the Plague (which raged in *London* in the Year 1665) upon reading the *Arithmetica Infinitorum* of Dr. *Wallis*, and considering how to interpole the Series

$$x, \quad x-\tfrac{1}{3}x^3, \quad x-\tfrac{2}{3}x^3+\tfrac{1}{5}x^5, \quad x-\tfrac{3}{3}x^3+\tfrac{3}{5}x^5-\tfrac{1}{7}x^7, \ \&c.$$

he found the Area of the Circle to be

$$x-\frac{\frac{1}{2}x^3}{3}-\frac{\frac{1}{8}x^5}{5}-\frac{\frac{1}{16}x^7}{7}-\frac{\frac{5}{128}x^9}{9}-\&c.$$

And by pursuing the Method of Interpolation he found the Theoreme abovemention'd, and by means of this Theoreme he found the Reduction of Fractions and Surds into converging

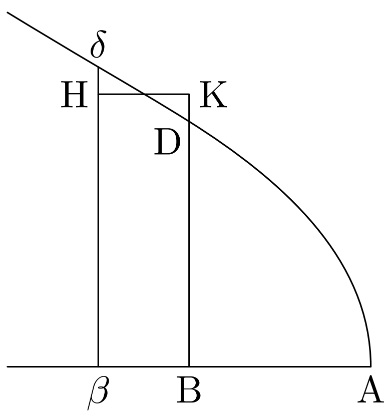


Series, by Division and Extraction of Roots; and then proceeded to the Extraction of affected Roots. And these Reductions are his third Rule.

When Mr. *Newton* had in this Compendium explained these three Rules, and illustrated them with various Examples, he laid down the idea of deducing the Area from the Ordinate, by considering the Area as a Quantity, growing or increasing by continual Flux, in proportion to the Length of the Ordinate, supposing the Abscissa to increase uniformly in proportion to Time. And from the Moments of Time he gave the Name of Moments to the momentaneous Increases, or infinitely small Parts of the Abscissa and Area, generated in Moments of Time. The Moment of a Line he called a Point, in the Sense of *Cavallerius*, tho' it be not a geometrical Point, but a Line infinitely short, and the Moment of an Area or Superficies he called a Line, in the Sense of *Cavallerius*, tho' it be not a geometrical Line, but a Superficies infinitely narrow. And when he consider'd the Ordinate as the Moment of the Area, he understood by it the Rectangles under the geometrical Ordinate and a Moment of the Abscissa, tho' that Moment be not always expressed. *Sit* ABD, saith he, *Curva quævis, & $AHKB$ rectangulum cujus latus AH vel KB est unitas. Et cogita rectam DBK uniformiter ab AH motam areas ABD & AK describere; & quod* [recta] BK (1) *sit momentum quo* [area] AK (x), *&* [recta] BD (y) *momentum quo* [area curvilinea] ABD *gradatim augetur; & quod ex momento BD perpetim dato possis, per præcedentes* [tres] *Regulas, aream ABD ipso descriptam investigare, sive cum area AK (x) momento* 1 *descripto conferre*. This is his Idea of the Work in squaring of Curves, and how he applies this to other Problems, he expresses in the next Words. *Jam qua ratione*, saith he, *superficies ABD ex momento suo perpetim dato per præcedentes* [tres] *Regulas elicitur, eâdem quælibet alia quantitas ex momento suo sic dato elicietur. Exemplo res fiet clarior.* And after some Examples he adds his Method of Regression from the Area, Arc, or solid Content, to the Abscissa; and shews how the same Method extends to Mechanical Curves, for determining their Ordinates, Tangents, Areas, Lengths, *&c.* And that by assuming any Equation expressing the Relation between the Area and Abscissa of a Curve, you may find the Ordinate by this Method. And this is the Foundation of the Method of Fluxions and Moments, which Mr. *Newton* in his Letter dated *Octob.* 24, 1676 comprehended in this Sentence. *Data æquatione quotcunque fluentes quantitates involvente, invenire Fluxiones; & vice versa.*

In this Compendium Mr. *Newton* represents the uniform Fluxion of Time, or of any Exponent of Time by an Unit; the Moment of Time or of its Exponent by the Letter o; the Fluxions of other Quantities by any other Symbols; the Moments of those Quantities by the Rectangles under those Symbols and the Letter o; and the Area of a Curve by the Ordinate inclosed in a Square, the Area being put for a Fluent and the Ordinate for its Fluxion. When he is demonstrating any Proposition he uses the Letter o for a finite Moment of Time, or of its Exponent, or of any Quantity flowing uniformly, and performs the whole Calculation by the Geometry of the Ancients in finite Figures or Schemes without any Approximation: and so soon as the Calculation is at an End, and the Equation is reduced, he supposes that the Moment o decreases *in infinitum* and vanishes. But when he is not demonstrating but only investigating a Proposition, for making Dispatch he supposes the Moment o to be infinitely little, and forbears to write it down, and uses all manner of Approximations which he conceives will produce no Error in the Conclusion. An Example of the first kind you have in the End of this Compendium, in demonstrating the first of the three Rules laid down in the Beginning of the Book. Examples of the second kind you have in the same Compendium, in finding the Length of Curve Lines *p.* 15. and in finding the Ordinates, Areas and Lengths of Mechanical Curves *p.* 18, 19. And he tells you, that by the same Method, Tangents may be drawn to mechanical Curves *p.* 19. And in his Letter of *Decemb.* 10. 1672. he adds, that Problems about the Curvature of Curves Geometrical or Mechanical are resolv'd by the same Method. Whence its manifest, that he had then extended the Method to the second and third Moments. For when the Areas of Curves are considered as Fluents (as is usual in this *Analysis*) the Ordinates express the first Fluxions, the Tangents are given by the second Fluxions, and the Curvatures by the third. And even in this *Analysis p.* 16. where Mr. *Newton* saith, *Momentum est superficies cum de solidis, & Linea cum de superficiebus, & Punctum cum de lineis agitur*, it is all one as if he had said, that when Solids are considered as Fluents, their Moments are Superficies, and the Moments of those Moments (or second Moments) are Lines, and the Moments of those Moments (or third Moments) are Points, in the Sense of *Cavallerius.* And in his *Principia Philosophiæ*, where he frequently considers Lines as Fluents described by Points, whose Velocities increase or decrease, the Velocities are the first Fluxions, and their Increase the second. And the Probleme, *Data æquatione fluentes quantitates involvente fluxiones invenire & vice versa*, extends to all the Fluxions, as is manifest by the Examples of the Solution thereof, published by Dr. *Wallis Tom.* 2. *p.* 391, 392, 396. And in *Lib.* II. *Princip. Prop.* XIV. he calls the second Difference the Difference of Moments.

Now that you may know what kind of Calculation Mr. *Newton* used in, or before the Year 1669. when he wrote this Compendium of his *Analysis*, I will here set down his Demonstration of the first Rule abovementioned.



Sit curvæ alicujus $\mathrm{A\,D}\,\delta$ *Basis* $\mathrm{A\,B} = \mathrm{x}$, *perpendiculariter applicata* $\mathrm{B\,D} = \mathrm{y}$, *& area* $\mathrm{A\,B\,D} = \mathrm{z}$, *ut prius. Item sit* $\mathrm{B}\,\beta = \mathrm{o}$, $\mathrm{B\,K} = \mathrm{v}$ *& Rectangulum* $\mathrm{B}\,\beta\,\mathrm{H\,K}$ *(*ov*) æquale spatio* $\mathrm{B}\,\beta\,\delta\mathrm{D}$. *Est ergo* $\mathrm{A}\,\beta = \mathrm{x} + \mathrm{o}$, *&* $\mathrm{A}\,\delta\,\mathrm{B} = \mathrm{z} + \mathrm{ov}$. *His præmisis, ex relatione inter* x *&* z *ad arbitrium assumpta, quæro* y *ut sequitur.*

Pro lubitu sumatur [æquatio] $\frac{2}{3}\mathrm{x}^{\frac{3}{2}} = \mathrm{z}$, *sive* $\frac{4}{9}\mathrm{x}^3 = \mathrm{zz}$. *Tum* $\mathrm{x} + \mathrm{o}$ $(\mathrm{A}\,\beta)$ *pro* x, *&* $\mathrm{z} + \mathrm{ov}$ $(\mathrm{A}\,\delta\,\beta)$ *pro* z *substitutis, prodibit* $\frac{4}{9}$ *in* $\mathrm{x}^3 + 3\mathrm{x}^2\,\mathrm{o} + 3\mathrm{xo}^2 + \mathrm{o}^3 =$ (*ex natura Curvæ*) $\mathrm{z}^2 + 2\mathrm{zov} + \mathrm{o}^2\mathrm{v}^2$. *Et sublatis* $\frac{4}{9}\mathrm{x}^3$ *& zz æqualibus, reliquisque per* o *divisis, restat* $\frac{4}{9}$ *in* $3\mathrm{x}^2 + 3\mathrm{xo} + \mathrm{o}^2 = 2\mathrm{zv} + \mathrm{ov}^2$. *Si jam supponamus* $\mathrm{B}\,\beta$ *in infinitum diminui & evanescere, sive* o *esse nihil, erunt* v *&* y *æquales, & termini per* o *multiplicati evanescent; ideoque restabit* $\frac{4}{9} \times 3\mathrm{xx} = 2\mathrm{zv}$, *sive* $\frac{2}{3}\mathrm{xx}\,(=\mathrm{zy}) = \frac{2}{3}\mathrm{x}^{\frac{3}{2}}\mathrm{y}$, *sive* $\mathrm{x}^{\frac{1}{2}}\left(=\dfrac{\mathrm{x}^2}{\mathrm{x}^{\frac{3}{2}}}\right) = \mathrm{y}$. *Quare è contra, si* $\mathrm{x}^{\frac{1}{2}} = \mathrm{y}$, *erit* $\frac{2}{3}\mathrm{x}^{\frac{3}{2}} = \mathrm{z}$.

Vel generaliter, Si $\dfrac{\mathrm{n}}{\mathrm{m}+\mathrm{n}} \times \mathrm{ax}^{\frac{\mathrm{m}+\mathrm{n}}{\mathrm{n}}} = \mathrm{z}$; *sive ponendo* $\dfrac{\mathrm{na}}{\mathrm{m}+\mathrm{n}} = \mathrm{c}$, *&* $\mathrm{m} + \mathrm{n} = \mathrm{p}$. *Si* $\mathrm{cx}^{\frac{\mathrm{p}}{\mathrm{n}}} = \mathrm{z}$, *sive* $\mathrm{c}^{\mathrm{n}}\mathrm{x}^{\mathrm{p}} = \mathrm{z}^{\mathrm{n}}$*: Tum* $\mathrm{x} + \mathrm{o}$ *pro* x, *&* $\mathrm{z} + \mathrm{ov}$ *sive* (*quod perinde est*) $\mathrm{z} + \mathrm{oy}$ *pro* z *substitutis, prodit* c^{n} *in* $\mathrm{x}^{\mathrm{p}} + \mathrm{pox}^{\mathrm{p}-1}$ &c. $= \mathrm{z}^{\mathrm{n}} + \mathrm{noyz}^{\mathrm{n}-1}$ &c. *reliquis nempe* [Serierum] *terminis qui tandem evanescerent, omissis. Jam sublatis* $\mathrm{c}^{\mathrm{p}}\mathrm{x}^{\mathrm{p}}$ *&* z^{n} *æqualibus, reliquisque per* o *divisis, restat* $\mathrm{c}^{\mathrm{n}}\mathrm{px}^{\mathrm{p}-1} = \mathrm{nyz}^{\mathrm{n}-1}\left(=\dfrac{\mathrm{nyz}^{\mathrm{n}}}{\mathrm{z}} = \dfrac{\mathrm{nyc}^{\mathrm{n}}\mathrm{x}^{\mathrm{p}}}{\mathrm{cx}^{\frac{\mathrm{p}}{\mathrm{n}}}}\right)$ *sive dividendo per* $\mathrm{c}^{\mathrm{n}}\mathrm{x}^{\mathrm{p}}$, *erit* $\mathrm{px}^{-1} = \dfrac{\mathrm{ny}}{\mathrm{cx}^{\frac{\mathrm{p}}{\mathrm{n}}}}$ *sive* $\mathrm{pcx}^{\frac{\mathrm{p}-\mathrm{n}}{\mathrm{n}}} = \mathrm{ny}$; *vel restituendo* $\dfrac{\mathrm{na}}{\mathrm{m}+\mathrm{n}}$ *pro* c *&* $\mathrm{m} + \mathrm{n}$ *pro* p, *hoc est* m *pro* $\mathrm{p} - \mathrm{n}$, *&* na *pro* pc, *fiet* $\mathrm{ax}^{\frac{\mathrm{m}}{\mathrm{n}}} = \mathrm{y}$. *Quare è contra, si* $\mathrm{x}^{\frac{\mathrm{m}}{\mathrm{n}}} = \mathrm{y}$ *erit* $\dfrac{\mathrm{n}}{\mathrm{m}+\mathrm{n}}\mathrm{ax}^{\frac{\mathrm{m}+\mathrm{n}}{\mathrm{n}}} = \mathrm{z}$. Q. E. D.

By the same way of working the second Rule may be also demonstrated. And if any Equation whatever be assumed expressing the Relation between the Abscissa and Area of a Curve, the Ordinate may be found in the same manner, as is mentioned in the next Words of the *Analysis.* And if this Ordinate drawn into an Unit be put for the Area of a new Curve, the Ordinate of this new Curve may be found by the same Method: And so on perpetually. And these Ordinates represent the first, second, third, fourth and following Fluxions of the first Area.

This was Mr. *Newton*'s way of working on those Days, when he wrote this Compendium of his *Analysis.* And the same Way of working he used in his Book of Quadratures, and still uses to this Day.

Among the Examples with which he illustrates the Method of Series and Moments set down in this Compendium, are these. Let the Radius of a Circle be 1, and the Arc z, and the Sine x, the Equations for finding the Arc whose Sine is given, and the Sine whose Arc is given, will be

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&\text{c.}$$
$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \&\text{c.}$$

Mr. *Collins* gave Mr. *Gregory* notice of this Method in Autumn 1669, and Mr. *Gregory*, by the Help of one of Mr. *Newton*'s Series, after a Year's Study, found the Method in *December* 1670; and two Months after, in a Letter dated *Feb.* 15. 1671. sent several Theorems, found thereby, to Mr. *Collins*, with leave to communicate them freely. And Mr. *Collins* was very free in communicating what he had received both from Mr. *Newton* and from Mr. *Gregory*, as appears by his Letters printed in the *Commercium.* Amongst the Series which Mr. *Gregory* sent in the said Letter, were these two. Let the Radius of a Circle be r, the Arc a, and the Tangent t, the Equations for finding the Arc whose Tangent is given, and the Tangent whose Arc is given, will be these.

$$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \text{\&c.}$$
$$t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \text{\&c.}$$

In this Year (1671) Mr. *Leibnitz* published two Tracts at *London*, the One dedicated to the Royal-Society, the Other dedicated to the Academy of Sciences at *Paris*; and in the Dedication of the First he mentioned his Correspondence with Mr. *Oldenburgh.*

In *February* $167\frac{2}{3}$ meeting Dr. *Pell* at Mr. *Boyle*'s, he pretended to the differential Method of *Mouton.* And notwithstanding that he was shewn by Dr. *Pell* that it was *Mouton*'s Method, he persisted in maintaining it to be his own Invention, by reason that he had found it himself without knowing what *Mouton* had done before, and had much improved it.

When one of Mr. *Newton*'s Series was sent to Mr. *Gregory*, he tried to deduce it from his own Series combined together, as he mentions in his Letter dated *December* 19. 1670. And by some such Method Mr. *Leibnitz*, before he left *London*, seems to have found the Sum of a Series of Fractions decreasing *in Infinitum*, whose Numerator is a given Number, and Denominators are triangular or pyramidal or triangulo-triangular Numbers, *&c.*. See the Mystery! From the Series

$$\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \textit{\&c.}$$

subduct all the Terms but the first (*viz.*

$$\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} \ \textit{\&c.})$$

and there will remain

$$1 = 1 - \frac{1}{2} + \frac{1}{2} - \frac{1}{3} + \frac{1}{3} - \frac{1}{4} + \frac{1}{4} - \frac{1}{5} + \frac{1}{5} \ \textit{\&c.}$$
$$= \frac{1}{1 \times 2} + \frac{1}{2 \times 3} + \frac{1}{3 \times 4} + \frac{1}{4 \times 5} + \textit{\&c.}$$

And from this Series take all the Terms but the first, and there will remain

$$\frac{1}{2} = \frac{2}{1 \times 2 \times 3} + \frac{2}{2 \times 3 \times 4} + \frac{2}{3 \times 4 \times 5} + \frac{2}{4 \times 5 \times 6} + \textit{\&c.}$$

And from the first Series take all the Terms but the two first, and there will remain

$$\frac{3}{2} = \frac{2}{1 \times 3} + \frac{2}{2 \times 4} + \frac{2}{3 \times 5} + \frac{2}{4 \times 6} + \textit{\&c.}$$

In the End of *February* or beginning of *March* $167\frac{2}{3}$. Mr. *Leibnitz* went from *London* to *Paris*, and continuing his Correspondence with Mr. *Oldenburg* and Mr. *Collins*, wrote in

July 1674. that he had a wonderful Theoreme, which gave the Area of a Circle or any Sector thereof exactly in a Series of rational Numbers; and in *October* following, that he had found the Circumference of a Circle in a Series of very simple Numbers, and that by the same Method (so he calls the said Theoreme) any Arc whose Sine was given might be found in a like Series, though the Proportion to the whole Circumference be not known. This Theoreme therefore was for finding any Sector or Arc whose Sine was given. If the Proportion of the Arc to the whole Circumference was not known, the Theoreme or Method gave him only the Arc; if it was known it gave him also the whole Circumference: and therefore it was the first of Mr. *Newton*'s two Theoremes above mention'd. But the Demonstration of this Theoreme Mr. *Leibnitz* wanted. For in his Letter of *May* 12. 1676. he desired Mr. *Oldenburgh* to procure the Demonstration from Mr. *Collins*, meaning the Method by which Mr. *Newton* had invented it.

In a Letter compos'd by Mr. *Collins* and dated *April* 15. 1675. Mr. *Oldenburgh* sent to Mr. *Leibnitz* Eight of Mr. *Newton*'s and Mr. *Gregory*'s Series, amongst which were Mr. *Newton*'s two Series above-mention'd for finding the Arc whose Sine is given, and the Sine whose Arc is given; and Mr. *Gregory*'s two Series above mentioned for finding the Arc whose Tangent is given, and the Tangent whose Arc is given. And Mr. *Leibnitz* in his Answer, dated *May* 20. 1675. acknowledged the Receipt of this Letter in these Words. *Literas tuas multa fruge Algebraica refertas accepi, pro quibus tibi & doctissimo* Collinio *gratias ago. Cum nunc præter ordinarias curas Mechanicis imprimis negotiis distrahar, non potui examinare Series quas misistis ac cum meis comparare. Ubi fecero perscribam tibi sententiam meam: nam aliquot jam anni sunt quod inveni meas via quadam sic satis singulari.*

But yet Mr. *Leibnitz* never took any further notice of his having received these Series, nor how his own differed from them, nor ever produced any other Series than those which he received from Mr. *Oldenburgh*, or numerical Series deduced from them in particular Cases. And what he did with Mr. *Gregory*'s Series for finding the Arc whose Tangent is given, he has told us in the *Acta Eruditorum mensis Aprilis* 1691. pag. 178. *Jam anno* 1675, saith he, *compositium habebam opusculum Quadraturæ Arithmeticæ ab amicis ab illo tempore lectum*, &c. By a Theoreme for transmuting of Figures, like those of Dr. *Barrow* and Mr. *Gregory*, he had now found a Demonstration of this Series, and this was the Subject of his *Opusculum.* But he still wanted a Demonstration of the rest: and meeting with a Pretence to ask for what he wanted, he wrote to Mr. *Oldenburgh* the following Letter, dated at *Paris May* 12. 1676.

Cum Georgius Mohr *Danus nobis attulerit communicatam sibi à Doctissimo* Collinio *vestro expressionem rationis inter arcum & sinum per infinitas Series sequentes; positio sinu* x, *arcu* z, *radio* 1,

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&c.$$
$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \&c.$$

Hæc, INQUAM, cum nobis attulerit ille, quæ mihi valde ingeniosa videntur, & posterior imprimis Series elegantiam quandam singularem habeat: ideo rem gratam mihi feceris, Vir clarissime, si demonstrationem transmiseris. Habebis vicissim mea ab his longe diversa circa hanc rem meditata, de quibus jam aliquot abhinc annis ad te perscripsisse credo, demonstratione tamen non addita, quam nunc polio. Oro ut Clarissimo Collinio *multam a me salutem dicas: is facile tibi materiam suppeditabit satisfaciendi desiderio meo.* Here, by the word *INQUAM*, one would think that he had never seen these two Series before, and that his *diversa circa hanc rem meditata* were something else than one of the Series which he had received from Mr. *Oldenburgh* the Year before, and a Demonstration thereof which he was now polishing, to make the Present an acceptable Recompence for Mr. *Newton*'s Method.

Upon the Receipt of this Letter Mr. *Oldenburg* and Mr. *Collins* wrote pressingly to Mr. *Newton*, desiring that he himself would describe his own Method, to be communicated to Mr. *Leibnitz*. Whereupon Mr. *Newton* wrote his Letter, dated *June* 13. 1676, describing therein the Method of Series, as he had done before in the Compendium above-mentioned; but with this Difference: Here he described at large the Reduction of the Dignity of a Binomial into a Series, and only touched on the Reduction by Division and Extraction of affected Roots: There he described at large the Reduction of Fractions and Radicals into Series by Division and Extraction of Roots, and only set down the two first Terms of the Series into which the Dignity of a Binomial might be reduced. And among the Examples in this Letter, there were Series for finding the Number whose Logarithm is given, and for finding the Versed Sine whose Arc is given: This Letter was sent to *Paris, June* 26. 1676. together with a MS. drawn up by Mr. *Collins*, containing Extracts of Mr. *James Gregory*'s Letters.

For Mr. *Gregory* died near the End of the Year 1675; and Mr. *Collins*, at the Request of Mr. *Leibnitz* and some other of the Academy of Sciences, draw up Extracts of his Letters, and the Collection is still extant in the Hand Writing of Mr. *Collins* with this Title; *Extracts of Mr.* Gregory*'s Letters, to be lent to Mr.* Leibnitz *to peruse, who is desired to return the same to you.* And that they were sent is affirmed by Mr. *Collins* in his Letter to Mr. *David Gregory* the Brother of the Deceas'd, dated *August* 11. 1676. and appears further by the Answers of Mr. *Leibnitz*, and Mr. *Tschurnhause* concerning them.

The Answer of Mr. *Leibnitz* directed to Mr. *Oldenburgh* and dated *August* 27. 1676, begins thus; *Literæ tuæ die* Julii 26. *datæ plura ac memorabiliora circa rem Analyticam continent quam multa volumina spissa de his rebus edita. Quare tibi pariter ac clarissimis viris* Newtono *ac* Collinio *gratias ago, qui nos participes tot meditationum egregiarum esse voluistis.* And towards the End of the Letter, after he had done with the Contents of Mr. *Newton*'s Letter, he proceeds thus. *Ad alia tuarum Literarum venio quæ doctissimus* Collinius *communicare gravatus non est. Vellem adjecisset appropinquationis* Gregorianæ *linearis demonstrationem. Fuit enim his certe studiis promovendis aptissimus.* And the Answer of Mr. *Tschurnhause*, dated *Sept.* 1. 1676, after he had done with Mr. *Newton*'s Letter about Series, concludes thus. *Similia porro quæ in hac re præstitit eximius ille Geometra* Gregorius *memoranda certe sunt. Et quidam optimè famæ ipsius consulturi, qui ipsius relicta Manuscripta luci publicæ ut exponantur operam navabunt.* In the first Part of this Letter, where Mr. *Tschurnhause* speaks of Mr. *Newton*'s Series, he saith that he looked over them cursorily, to see if he could find the Series of Mr. *Leibnitz* for squaring the Circle or Hyperbola. If he had searched for it in the Extracts of *Gregory*'s Letters he might have found it in the Letter of *Febr.* 15. 1671. above-mentioned. For the MS. of those Extracts with that Letter therein is still extant in the Hand-Writing of Mr. *Collins*.

And tho' Mr. *Leibnitz* had now received this Series twice from Mr. *Oldenburgh*, yet in his Letter of *August* 27. 1676. he sent it back to him by way of Recompence for Mr. *Newton*'s Method, pretending that he had communicated it to his Friends at *Paris* three Years before or above; that is, two Years before he received it in Mr. *Oldenburgh*'s Letter of *April* 15. 1675; at which Time he did not know it to be his own, as appears by his Answer of *May* 20. 1675 above-mentioned. He might receive this Series at *London*, and communicate it to his Friends at *Paris* above three Years before he sent it back to Mr. *Oldenburg*: but it doth not appear that he had the Demonstration thereof so early. When he found the Demonstration, then he compos'd it in his *Opusculum*, and communicated that also to his Friends; and he himself has told us that this was in the Year 1675. However, it lies upon him to prove that he had this Series before he received it from Mr. *Oldenburgh.* For in his Answer to Mr. *Oldenburgh* he did not know any of the Series then sent him to be his own; and concealed from the Gentlemen at *Paris* his having received it from Mr. *Oldenburgh* with several other Series, and his having seen a Copy of the Letter in which Mr. *Gregory* had sent it to Mr. *Collins* in the Beginning of the Year 1671.

In the same Letter of *August* 27. 1676, after Mr. *Leibnitz* had described his Quadrature of the Circle and Equilateral Hyperbola, he added: *Vicissim ex seriebus regressuum pro Hyperbola hanc inveni. Si sit numerus aliquis unitate minor* $1-\mathrm{m}$, *ejusque logarithmus Hyperbolicus* l. *Erit*

$$\mathrm{m}=\frac{\mathrm{l}}{1}-\frac{\mathrm{l}^2}{1\times2}+\frac{\mathrm{l}^3}{1\times2\times3}-\frac{\mathrm{l}^4}{1\times2\times3\times4}+\&\mathrm{c}.$$

Si numerus sit major unitate, ut $1+\mathrm{n}$, *tunc pro eo inveniendo mihi etiam prodiit Regula quæ in* Newtoni *Epistola expressa est: scilicet erit*

$$\mathrm{m}=\frac{\mathrm{l}}{1}+\frac{\mathrm{l}^2}{1\times2}+\frac{\mathrm{l}^3}{1\times2\times3}+\frac{\mathrm{l}^4}{1\times2\times3\times4}+\&\mathrm{c}.$$

—— *Quod regressum ex arcubus attinet, incideram ego directe in Regulam quæ ex dato arcu sinum complementi exhibet. Nempe sinus complementi*

$$=1-\frac{\mathrm{a}^2}{1\times2}+\frac{\mathrm{a}^4}{1\times2\times3\times4}-\&\mathrm{c}.$$

Sed postea quoque deprehendi ex ea illam nobis communicatam pro invenendo sinu recto, qui est

$$\frac{\mathrm{a}}{1}-\frac{\mathrm{a}^3}{1\times2\times3}+\frac{\mathrm{a}^5}{1\times2\times3\times4\times5}-\&\mathrm{c}.$$

posse demonstrari. Thus Mr. *Leibnitz* put in his Claim for the Co-invention of these four Series, tho' the Method of finding them was sent him at his own Request, and he did not yet understand it. For in this same Letter of *August* 27 1676. he desired Mr. *Newton* to explain it further. His Words are. *Sed desideraverim ut Clarissimus* Newtonus *nonnulla quoque amplius explicet; ut originem Theorematis quod initio ponit: Item modum quo quantitates* p, q, r, *in suis Operationibus invenit: Ac denique quomodo in Methodo regressuum se gerat, ut cum ex Logarithmo quærit Numerum. Neque enim explicat quomodo id ex methodo sua derivetur.* He pretended to have found two Series for the Number whose Logarithm was given, and yet in the same Letter desired Mr. *Newton* to explain to him the Method of finding those very two Series.

When Mr. *Newton* had received this Letter, he wrote back that all the said four Series had been communicated by him to Mr. *Leibnitz*; the two first being one and the same Series in which the Letter *l* was put for the Logarithm with its Sign + or −; and the third being the Excess of the Radius above the versed Sine, for which a Series had been sent to him. Whereupon Mr. *Leibnitz* desisted from his Claim. Mr. *Newton* also in the same Letter dated *Octob.* 24. 1676. further explained his Methods of Regression, as Mr. *Leibnitz* had desired. And Mr. *Leibnitz* in his Letter of *June* 21. 1677. desired a further Explication: but soon after, upon reading Mr. *Newton*'s Letter a second time, wrote back *July* 12. 1677. that he now understood what he wanted; and found by his old Papers that he had formerly used one of Mr. *Newton*'s Methods of Regression, but in the Example which he had then by chance made use of, there being produced nothing elegant, he had, out of his usual Impatience, neglected to use it any further. He had therefore several direct Series, and by consequence a Method of finding them, before he invented and forgot the inverse Method. And if he had searched his old Papers diligently, he might have found this Method also there; but having forgot his own Methods he wrote for Mr. *Newton*'s.

When Mr. *Newton* in his Letter dated *June* 13. 1676. had explained his Method of Series, he added: *Ex his videre est quantum fines Analyseos per hujusmodi infinitas æquationes ampliantur: quippe quæ earum beneficio ad omnia pene dixerim problemata* (*si numeralia* Diophanti *& similia excipias*) *sese extendit. Non tamen omnino universalis evadit, nisi per ulteriores quasdam Methodos eliciendi Series infinitas. Sunt enim quædam Problemata in quibus non licet ad Series infinitas per Divisionem vel Extractionem radicum simplicium affectarumve pervenire. Sed quomodo in istis casibus procedendum sit jam non vacat dicere; ut neque alia quædam tradere, quæ circa Reductionem infinitarum Serierum in finitas, ubi rei natura tulerit, excogitavi. Nam parcius scribo, quod hæ speculationes diu mihi fastidio esse cœperunt; adeo ut ab iisdem jam per quinque fere annos abstinuerim.* To this Mr. *Leibnitz* in his Letter of *August* 27. 1676. answered: *Quod dicere videmini plerasque difficultates* (*exceptis Problematibus* Diophantæis) *ad series Infinitas reduci; id mihi non videtur. Sunt enim multa usque adeo mira & implexa ut neque ab æquationibus pendeant neque ex Quadraturis. Qualia sunt* (*ex multis aliis*) *Problemata methodi Tangentium inversæ.* And Mr. *Newton* in his Letter of *Octob.* 24. 1676, replied: *Ubi dixi omnia pene Problemata solubilia existere: volui de iis præsertim intelligi circa quæ Mathematici se hactenus occuparunt, vel saltem in quibus Ratiocinia Mathematica locum aliquem obtinere possunt. Nam alia sane adeo perplexis conditionibus implicata excogitare liceat, ut non satis comprehendere valeamus: & multo minus tantarum computationum onus sustinere quod ista requirerent. Attamen ne nimium dixisse videar, inversa de Tangentibus Problemata sunt in potestate, aliaque illis difficiliora. Ad quæ solvenda usus sum duplici methodo, una concinniori, altera generaliori. Utramque visum est impræsentia literis transpositis consignare, ne propter alios idem obtinentes, institutum in aliquibus mutare coreger.* 5 a cc d æ 10 e ff h, &c. id est, *Una 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 serieri.* By Mr. *Newton*'s two Letters, its certain that he had then (or rather above five Years before) found out the Reduction of Problems to fluxional Equations and converging Series: and by the Answer of Mr. *Leibnitz* to the first of those Letters, its as certain that he had not then found out the Reduction of Problems either to differential Equations or to converging Series.

And the same is manifest also by what Mr. *Leibnitz* wrote in the *Acta Eruditorum, Anno* 1691, concerning this Matter. *Jam anno* 1675, saith he, *compositum habebam opus-*

culum Quadraturæ Arithmeticæ ab amicis ab illo tempore lectum, sed quod, materia sub manibus crescente, limare ad Editionem non vacavit, postquam aliæ occupationes supervenere; præsertim cum nunc prolixius exponere vulgari more quæ Analysis nostra paucis exhibet, non satis operæ pretium videatur. This Quadrature composed *vulgari more* he began to communicate at *Paris* in the Year 1675. The next Year he was polishing the Demonstration thereof, to send it to Mr. *Oldenburgh* in Recompense for Mr. *Newton*'s Method, as he wrote to him *May* 12. 1676; and accordingly in his Letter of *August* 27. 1676. he sent it composed and polished *vulgari more.* The Winter following he returned into *Germany*, by *England* and *Holland*, to enter upon publick Business, and had no longer any Leisure 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 out this new *Analysis* therefore after his Return into *Germany*, and by consequence not before the Year 1677.

The same is further manifest by the following Consideration. Dr. *Barrow* published his *Method of Tangents* in the Year 1670. Mr. *Newton* in his Letter dated *December* 10. 1672. communicated his *Method of Tangents* to Mr. *Collins*, and added: *Hoc est unum particulare vel Corollarium potius 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 Gravitatis Curvarum, &c. Neque* (*quemadmodum* Huddenii *methodus de* Maximis *&* Minimis) *ad solas restringitur æquationes illas, quæ quantitatibus surdis sunt immunes. Hanc methodum intertextui alteri isti qua Æquationum Exegisin instituo, reducendo eas ad series infinitas.* Mr. *Slusius* sent his Method of Tangents to Mr. *Oldenburgh Jan.* 17, $167\frac{2}{3}$, and the same was soon after published in the *Transactions.* It proved to be the same with that of Mr. *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 Notation of Mr. *Leibnitz* $\frac{dy^3}{dy} = 3yy$. A Copy of Mr. *Newton*'s Letter of *Decemb.* 10. 1672 was sent to Mr. *Leibnitz* by Mr. *Oldenburgh* amongst the Papers of Mr. *James Gregory*, at the same time with Mr. *Newton*'s Letter of *June* 13. 1676. And Mr. *Newton* having described in these two Letters that he had a very general *Analysis*, consisting partly of the Method of converging Series, partly of another Method, by which he applied those Series to the Solution of almost all Problems (except perhaps some numeral ones like those of *Diophantus*) and found the Tangents, Areas, Lengths, solid Contents, Centers of Gravity, and Curvities of Curves, and curvilinear Figures Geometrical or Mechanical, without sticking at Surds; and that the Method of Tangents of *Slusius* was but a Branch or Corollary of this other Method: Mr. *Leibnitz* in his returning Home through *Holland*, was meditating upon the Improvement of the Method of *Slusius.* For in a Letter to Mr. *Oldenburgh*, dated from *Amsterdam Nov.* $\frac{18}{28}$ 1676, he wrote thus. *Methodus Tangentium à* Slusio *publicata nondum rei fastigium tenet. Potest aliquid amplius præstari in eo genere quod maximi foret usus ad omnis generis Problemata: etiam ad meam* (*sine extractionibus*) *Æquationum ad series reductionem. Nimirum posset brevis quædam calculari circa Tangentes Tabula, eousque continuanda donec progressio Tabulæ apparet; ut eam scilicet quisque quousque libuerit sine calculo continuare possit.* This was the Improvement of the

Method of *Slusius* into a general Method, which Mr. *Leibnitz* was then thinking upon, and 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 extending the Method to all sorts of Problems. The Improvement by the differential Calculus was not yet in his Mind, but must be referred to the next Year.

Mr. *Newton* in his next Letter, dated *Octob.* 24. 1676, mentioned the *Analysis* communicated by Dr. *Barrow* to Mr. *Collins* in the Year 1669, and also another Tract written in 1671. about converging Series, and about the other Method by which Tangents were drawn after the Method of *Slusius*, and *Maxima* and *Minima* were determined, and the Quadrature of Curves was made more easy, and this without sticking at Radicals, and by which Series were invented which brake off and gave the Quadrature of Curves in finite Equations when it might be. And the Foundation of these Operations he comprehended in this Sentence exprest enigmatically as above. *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 Letter be considered, it will appear that he had then brought it to great Perfection, and made it exceeding general; the Propositions in his Book of Quadratures, and the Methods of converging Series and 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 into converging Series by the binomial Theoreme, and by the Extraction of Fluents out of Equations involving or not involving their Fluxions. And when Finite Equations are wanting, he deduces converging Series from the Conditions of the Probleme, by assuming the Terms of the Series gradually, and determining them by those Conditions. And when Fluents are to be derived from Fluxions, and 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 Mr. *Newton* had in those Days made his Method of Fluxions much more universal than the Differential Method of Mr. *Leibnitz* is at present.

This Letter of Mr. *Newton*'s, dated *Octob.* 24. 1676, came to the Hands of Mr. *Leibnitz* in the End of the Winter or Beginning of the Spring following; and Mr. *Leibnitz* soon after *viz.* in a Letter dated *June* 21. 1677, wrote back: *Clarisimi* Slusii *methodum Tangentium nondum esse absolutam Celeberrimo* Newtono *assentior. Et jam à multo tempore rem Tangentium generalius tractavi, scilicet per differentias Ordinatarum.* —— *Hinc nominando*, in posterum, dy *differentiam duarum proximarum* y *&c.* Here Mr. *Leibnitz* began first to propose his Differential Method, and there is not the least Evidence that he knew it before the Receipt of Mr. *Newton*'s last Letter. He saith indeed, *Jam à multo tempore rem Tangentium generalius tractavi, scilicet per differentias Ordinatarum*: and so he affirmed in other Letters, that he had invented several converging Series direct and inverse before he had the Method of inventing them; and 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 Judge would be very unjust, and act contrary to the Laws of all Nations, who should admit any Man to be a Witness in his own Cause. And therefore it lies upon Mr. *Leibnitz* to prove that he found out this Method long before the Receipt of Mr. *Newton*'s Letters. And if he cannot prove this, the Question, Who was the first Inventor of the Method, is decided.

The Marquiss *De l'Hospital* (a Person of very great Candour) in the Preface to his Book *De Analysi quantitatum infinitè parvarum*, published *A. C.* 1696. tell us, 'that a little after the

Publication of the Method of Tangents of *Des Cartes*, Mr. *Fermat* found also a Method, which *Des Cartes* himself at length allowed to be, for the most part, more simple than his own. But it was not yet so simple as Mr. *Barrow* afterwards made it, by considering more nearly the nature of Polygons, which offers naturally to the Mind a little Triangle, compos'd of a Particle of the Curve lying between two Ordinates infinitely near one another, and of the Difference of these two Ordinates, and of that of the two correspondent *Abscissa*'s. And this Triangle is like that which ought to be made by the Tangent, the Ordinate, and the Sub-tangent: so that by one simple Analogy, this last Method saves all the Calculation which was requisite either in the Method of *Des Cartes*, or in this same Method before. Mr. *Barrow* stopt not here, he invented also a sort of Calculation proper for this Method. But it was necessary in this as well as in that of *Des Cartes*, to take away Fractions and Radicals for making it useful. Upon the Defect of this Calculus, that of the celebrated Mr. *Leibnitz* was introduced, and this learned Geometer began where Mr. *Barrow* and others left off. This his *Calculus* led into Regions hitherto unknown, and there made Discoveries which astonished the most able Mathematicians of *Europe*,' &c. Thus far the Marquiss. He had not seen Mr. *Newton*'s *Analysis*, nor his Letters of *Decem.* 10. 1672. *June* 13. 1676, and *Octob.* 24. 1676: and so not knowing that Mr. *Newton* had done all this and signified it to Mr. *Leibnitz*, he reckoned that Mr. *Leibnitz* began where Mr. *Barrow* left off, and by teaching how to apply Mr. *Barrow*'s Method without sticking at Fractions and Surds, had enlarged the Method wonderfully. And Mr. *James Bernoulli*, in the *Acta Eruditorum* of *January* 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 à Domino* Leibnitio *inventum, ignorare vix poterit; utpote qui in priori illo fundatus est, & nisi forte in Differentialium notatione & operationis aliquo compendio ab eo non differt.*

Now Dr. *Barrow*, in his Method of Tangents, draws two Ordinates indefinitely near to one another, and puts the Letter a for the Difference of the Ordinates, and the Letter e for the Difference of the *Abscissa*'s, and for drawing the Tangent gives these Three Rules. 1. *Inter computandum*, saith he, *omnes abjicio terminos in quibus ipsarum* a *vel* e *potestas habeatur, 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 nihilum adæquabunt.* 3. *Pro* a *Ordinatam, & pro* e *Subtangentem substituo. Hinc demum Subtangentis quantitas dignoscetur.* Thus far Dr. *Barrow*.

And Mr. *Leibnitz* in his Letter of *June* 21. 1677 above-mentioned, wherein he first began to propose his Differential Method, has followed this Method of Tangents exactly, excepting that he has changed the Letters a and e of Dr. *Barrow* into dx and dy. For in the Example which he there gives, he draws two parallel Lines and sets all the Terms below the under Line, in which dx and dy are (severally or jointly) of more than one Dimension, and all the Terms above the upper Line, in which dx and dy are wanting, and for the Reasons given by Dr. *Barrow*, makes all these Terms vanish. And by the Terms in which dx and dy are but of one Dimension, and which he sets between the two Lines, he determines the Proportion of the Subtangent to the Ordinate. Well therefore did the Marquiss *de l'Hospital* observe that where Dr. *Barrow* left off Mr. *Leibnitz* began: for their Methods of Tangents are exactly the same.

But Mr. *Leibnitz* adds this Improvement of the Method, that the Conclusion of this

Calculus is coincident with the Rule of *Slusius*, and shews how that Rule presently occurs to any one who understands this Method. For Mr. *Newton* had represented in his Letters, that this Rule was a Corollary of his general Method.

And whereas Mr. *Newton* had said that his Method in drawing of Tangents, and determining *Maxima* and *Minima*, &c. proceeded without sticking at Surds: Mr. *Leibnitz* in the next Place, shews how this Method of Tangents may be improved so as not to stick at Surds or Fractions, and then 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 æquationem differentialem.* By which Words, compared with the preceding Calculation, its manifest that Mr. *Leibnitz* at this time understood that Mr. *Newton* had a Method which would do all these things, and had been examining whether Dr. *Barrow*'s Differential Method of Tangents might not be extended to the same Performances.

In *November* 1684 Mr. *Leibnitz* published the Elements of this Differential Method in the *Acta Eruditorum*, and illustrated it with Examples of drawing Tangents and determining *Maxima* and *Minima*, and then added: *Et hæc quidem initia sunt Geometriæ cujusdam multo sublimioris, ad difficillima & pulcherrima quæque etiam mistæ* Matheseos *Problemata pertingentis, quæ sine calculo differentiali* AUT SIMILI *non temere quisquam pari facilitate tractabit.* The words AUT SIMILI plainly relate to Mr. *Newton*'s Method. And the whole Sentence contains nothing more than what Mr. *Newton* had affirmed of his general Method in his Letters of 1672 and 1676.

And in the *Acta Eruditorum* of *June* 1686, *pag.* 297. Mr. *Leibnitz* added: *Malo autem* dx *& 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 Letters with Dr. *Barrow*, but chose rather to use the new Symbols *dx* and *dy*, though there is nothing which can be done by these Symbols, but what may be done by single Letters with more brevity.

The next Year Mr. *Newton*'s *Principia Philosophiæ* came abroad, a Book full of such Problemes as Mr. *Leibnitz* had called *difficillima & pulcherrima etiam mistæ Matheseos problemata, 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*; composed almost wholly of this Calculus. And Mr. *Leibnitz* himself in a Letter to Mr. *Newton*, dated from *Hannover, March* $\frac{7}{17}$ 1693, and still extant in his own Hand-writing, and upon a late Occasion communicated to the Royal Society, acknowledged the same thing 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 & summas exhibeant, Geometriam illam quam Transcendentem appello Analysi quodammodo subjicere, nec res male processit*; And again in his Answer to Mr. *Fatio*, printed in the *Acta Eruditorum* of *May* 1700. *pag.* 203. *lin.* 21. he acknowledged the same thing. In the second Lemma of the second Book of these *Principles*, the Elements of this Calculus are demonstrated synthetically, and at the End of the Lemma there is a Scholium in these Words. *In Literis quæ mihi cum Geometra peritissimo* G. G. Leibnitio *annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi Maximas & Minimas, ducendi Tangentes & similia peragendi, quæ in terminis surdis æque ac in rationalibus procederet; & literis transpositis hanc sententiam involventibus* [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice versa] *eandem celarem: rescripsit Vir clarissimus se quoque in ejusmodi methodum incidisse, & methodum suam communicavit à mea vix abludentem præterquam in verborum & notarum formulis. Utriusque fundamentum continetur in hoc Lemmate.* In those Letters, and in another dated *Decem.* 10. 1672, a Copy of which, at that time, was sent to Mr. *Leibnitz* by Mr. *Oldenburgh*, as is mentioned above, Mr. *Newton* had so far explained his Method, that it was not difficult for Mr. *Leibnitz*, by the Help of Dr. *Barrow*'s Method of Tangents, to collect it from those Letters. And its certain, by the Arguments above-mentioned, that he did not know it before the writing of those Letters.

Dr. *Wallis* had received Copies of Mr. *Newton*'s two Letters of *June* 13. and *Octob.* 24. 1676 from Mr. *Oldenburgh*, and published several things out of them in his *Algebra*, printed in *English* 1683, and in Latin 1693; and soon after had Intimation from *Holland* to print the Letters entire, because Mr. *Newton*'s Notions of Fluxions passed there with Applause by the Name of the Differential Method of Mr. *Leibnitz*. And thereupon he took notice of this Matter in the Preface to the first Volume of his Works published *A. C.* 1695. And in a Letter* to Mr. *Leibnitz* dated *Decemb.* 1. 1696. he gave the Account of it. *Cum Præfationis (præfigendæ) postremum folium erat sub prælo, ejusque typos jam posuerant Typothetæ; me monuit amicus quidam (harum rerum gnarus) qui peregre fuerat, tum talem methodum in Belgio prædicari, tum illam cum* Newtoni *methodo Fluxionum quasi coincidere. Quod fecit ut (translatis typis jam positis) id monitum interseruerim.* And in a Letter dated *April* 10. 1695, and lately communicated to the Royal-Society, he wrote thus about it. *I wish you would print the two large Letters of* June *and* August [he means *June* and *October*] 1676. *I had intimation from* Holland*, as desired there by your Friends, that somewhat of that kind were done; becuse your Notions (of Fluxions) pass there with great Applause by the name of* Leibnitz*'s* Calculus Differentialis. *I had this intimation when all but part of the Preface to this Volume was printed off; so that I could only insert (while the Press stay'd) that short Intimation thereof which you there find. You are not so kind to your Reputation (and that of the Nation) as you might be, when you let things of worth lye by you so long, till others carry away the Reputation that is due to you. I have endeavoured to do you Justice in that Point, and am now sorry that I did not print those two Letters* verbatim.

The short intimation of this Matter, which Dr. *Wallis* inserted into the said Preface, was in these words. *In secundo Volumine (inter alia) habetur* Newtoni *Methodus de Fluxionibus (ut ille loquitur) consimilis naturæ cum* Leibnitii *(ut hic loquitur) Calculo Differentiali (quod qui utramque methodum contulerit satis animadvertat, ut ut sub loquendi formulis diversis) quam ego descripsi (Algebræ* cap. 91 &c. *præsertim* cap 95) *ex binis* Newtoni *Literis, aut earum alteris*, Junii 13. & Octob. 24. 1676 *ad* Oldenburgum *datis, cum* Leibnitio *tum communicandis (iisdem fere verbis, saltem leviter mutatis, quæ in illis literis habentur,) ubi* METHODUM HANC *LEIBNITIO* EXPONIT, *tum ante* DECEM ANNOS *nedum plures* [id est, anno 1666 vel 1665] *ab ipso excogitatam. Quod moneo, nequis causetur de hoc Calculo Differentiali nihil à nobis dictum esse.*

Hereupon the Editors of the *Acta Lipsiensia*, the next Year in *June*, in the Style of Mr. *Leibnitz*, in giving an Account of these two first Volumes of Dr. *Wallis*, took notice of this Clause of the Doctor's Preface, and complained, not of his saying that Mr. *Newton* in his two Letters above-mentioned explained to Mr. *Leibnitz* the Method of Fluxions found

* Extat hæc Epistola in tertio volumine operum *Wallisii*.

by him Ten Years before or above; but that while the Doctor mentioned the Differential Calculus, and said that he did it *nequis causetur de calculo differentiali nihil ab ipso dictum fuisse*, he did not tell the Reader that Mr. *Leibnitz* had this Calculus at that time when those Letters passed between him and Mr. *Newton*, by means of Mr. *Oldenburgh*. And in several Letters which followed hereupon, between Mr. *Leibnitz* and Dr. *Wallis*, concerning this Matter, Mr. *Leibnitz* denied not that Mr. *Newton* had the Method Ten Years before the writing of those Letters, as Dr. *Wallis* had affirmed; pretended not that he himself had the Method so early; brought no Proof that he had it before the year 1677; no other proof besides the Concession of Mr. *Newton* that he had it so early; affirmed not that he had it earlier; commended Mr. *Newton* for his Candour in this Matter; allowed that the Methods agreed in the main, and said that he therefore used to call them by the common Name of his *Infinitesimal Analysis*; represented, that as the Methods of *Vieta* and *Cartes* were called by the common Name of *Analysis Speciosa*, and yet differed in some things; so perhaps the Methods of Mr. *Newton* and himself might differ in some things, and challenged to himself only those things wherein, as he conceived, they might differ, naming the Notation, the differential Equations and the Exponential Equations. But in his Letter of *June* 21. 1677 he reckon'd differential Equations common to Mr. *Newton* and himself.

This was the State of the Dispute between Dr. *Wallis* and Mr. *Leibnitz* at that time. And Four years after, when Mr. *Fatio* suggested that Mr. *Leibnitz*, the second Inventor of this Calculus, might borrow something from Mr. *Newton*, the oldest Inventor by many Years: Mr. *Leibnitz* in his Answer, published in the *Acta Eruditorum* of *May* 1700, allowed that Mr. *Newton* had found the Method apart, and did not deny that Mr. *Newton* was the oldest Inventor by many Years, nor asserted any thing more to himself, than that he also had found the Method apart, or without the Assistance of Mr. *Newton*, and pretended that when he first published it, he knew not that Mr. *Newton* had found any thing more of it than the Method of Tangents. And in making this Defence he added: *Quam* [methodum] *ante Dominum* Newtonum & Me *nullus quod sciam Geometra habuit; uti ante hunc maximi nominis Geometram NEMO specimine publice dato se habere probavit, ante Dominos* Bernoullios *&* Me *nullus communicavit.* Hitherto therefore Mr. *Leibnitz* did not pretend to be the first Inventor. He did not begin to put in such a Claim till after the Death of Dr. *Wallis*, the last of the old Men who were acquainted with what had passed between the *English* and Mr. *Leibnitz* Forty Years ago. The Doctor died in *October A. C.* 1703, and Mr. *Leibnitz* began not to put in this new Claim before *January* 1705.

Mr. *Newton* published his *Treatise of Quadratures* in the Year 1704. This Treatise was written long before, many things being cited out of it in his Letters of *Octob.* 24 and *Novemb.* 8. 1676. It relates to the Method of Fluxions, and that it might not be taken for a new Piece, Mr. *Newton* repeated what Dr. *Wallis* had published Nine Years before without being then contradicted, namely, that this Method was invented by Degrees in the Years 1665 and 1666. Hereupon the Editors of the *Acta Lipsiensia* in *January* 1705. in the Style of Mr. *Leibnitz*, in giving an Account of this Book, represented that Mr. *Leibnitz* was the first Inventor of the Method, and that Mr. *Newton* had substituted Fluxions for Differences. And this Accusation gave a Beginning to this present Controversy.

For Mr. *Keill*, in an Epistle published in the *Philosophical Transactions* for *Sept.* and *Octob.* 1708, retorted the Accusation, saying: *Fluxionum Arithmeticam sine omni dubio primus invenit D.* Newtonus*, ut cuilibet ejus Epistolas à* Wallisio *editas legenti facile constabit.*

Eadem tamen Arithmetica postea mutatis nomine & notationis modo à Domino Leibnitio *in* Actis Eruditorum *edita est.*

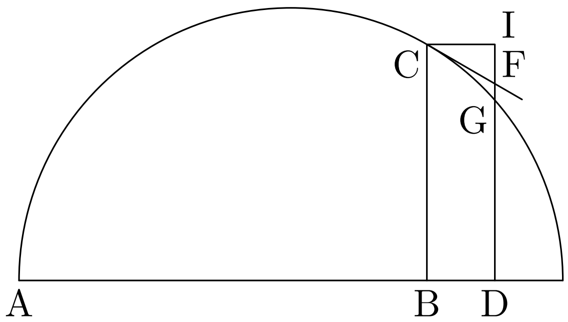
Before Mr. Newton saw what had been published in the *Acta Leipsica*, he express'd himself offended at the printing of this Paragraph of Mr. *Keill*'s Letter, least it should create a Controversy. And Mr. *Leibnitz*, understanding it in a stronger Sense than Mr. *Keill* intended it, complain'd of it as a Calumny, in a Letter to Dr. *Sloane* dated *March* 4. 1711 *N.S.* and moved that the Royal-Society would cause Mr. *Keill* to make a publick Recantation. Mr. *Keill* chose rather to explain and defend what he had written; and Mr. *Newton*, upon being shewed the Accusation in the *Acta Lipsica*, gave him leave to do so. And Mr. *Leibnitz* in a second Letter to Dr. *Sloane*, dated *Decem.* 29. 1711, instead of making good his Accusation, as he was bound to do that it might not be deem'd a Calumny, insisted only upon his own Candour, as if it would be Injustice to question it; and refus'd to tell how he came by the Method; and said that the *Acta Lipsica* had given every man his due, and that he had concealed the Invention above Nine Years, (he should have said Seven Years) that No body might pretend (he means that Mr. *Newton* might not pretend) to have been before him in it; and called Mr. *Keill* a Novice unacquainted with things past, and one that acted without Authority from Mr. *Newton*, and a clamorous Man who deserved to be silenced, and desired that Mr. *Newton* himself would give his Opinion in the Matter. He knew that Mr. *Keill* affirmed nothing more than what Dr. *Wallis* had published thirteen Years before, without being then contradicted. He knew that Mr. *Newton* had given his Opinion in this matter in the Introduction to his Book of *Quadratures*, published before this Controversy began: but Dr. *Wallis* was dead; the Mathematicians which remained in *England* were Novices; Mr. *Leibnitz* may Question any Man's Candour without Injustice, and Mr. *Newton* must now retract what he had published or not be quiet.

The Royal-Society therefore, having as much Authority over Mr. *Leibnitz* as over Mr. *Keill*, and being now twice pressed by Mr. *Leibnitz* to interpose, and seeing no reason to condemn or censure Mr. *Keill* without enquiring into the matter; and that neither Mr. *Newton* nor Mr. *Leibnitz*) (the only Persons alive who knew or remembred any thing of what had passed in these matters Forty Years ago) could be Witnesses for or against Mr. *Keill*; appointed a numerous Committee to search old Letters and Papers, and report their Opinion upon what they found; and ordered the Letters and Papers, with the Report of their Committee to be published. And by these Letters and Papers it appear'd to them, that Mr. *Newton* had the Method in or before the Year 1669, and it did not appear to them that Mr. *Leibnitz* had it before the year 1677. For making himself the first Inventor of the Differential Method, he has represented that Mr. *Newton* at first used the Letter o in the vulgar manner for the given Increment of x, which destroys the Advantages of the Differential Method; but after the writing of his Principles, changed o into $\dot{x}$, substituting $\dot{x}$ for dx. It lies upon him to prove that Mr. *Newton* ever changed o into $\dot{x}$, or used $\dot{x}$ for dx, or left off the Use of the Letter o. Mr. *Newton* used the Letter o in his *Analysis* written in or before the Year 1669, and in his Book of *Quadratures*, and in his *Principia Philosophiæ*, and still uses it in the very same Sense as at first. In his Book of Quadratures he used it in conjunction with the Symbol $\dot{x}$, and therefore did not use that Symbol in its Room. These symbols o and $\dot{x}$ are put in for things of a different kind. The one is a Moment, the other a Fluxion or Velocity as has been explained above. When the Letter x is put for a Quantity which flows uniformly, the Symbol $\dot{x}$ is an Unit, and the Letter o a Moment, and $\dot{x}o$ and dx signify the same Moment. Prickt Letters never signify Moments, unless when they are multiplied by the Moment o either exprest or understood to make them infinitely little, and then the Rectangles are put for Moments.

Mr. *Newton* doth not place his Method in Forms of Symbols, nor confine himself to any particular Sort of Symbols for Fluents and Fluxions. Where he puts the Areas of Curves for Fluents, he frequently puts the Ordinates for Fluxions, and denotes the Fluxions by the Symbols of the Ordinates, as in his *Analysis*. Where he puts Lines for Fluents, he puts any Symbols for the Velocities of the Points which describe the Lines, that is, for the first Fluxions; and any other Symbols for the Increase of those Velocities, that is, for the second Fluxions, as is frequently done in his *Principia Philosophiæ*. And where he puts the Letters x, y, z for Fluents, he denotes their Fluxions, either by other Letters as p, q, r; or by the same Letters in other Forms as X, Y, Z or $\dot{x}$, $\dot{y}$, $\dot{z}$; or by any Lines as DE, FG, HI, considered as their Exponents. And this is evident by his Book of *Quadratures*, where he represents Fluxions by prickt Letters in the first Proposition, by Ordinates of Curves in the last Proposition, and by other Symbols, in explaining the Method and illustrating it with Examples, in the Introduction. Mr. *Leibnitz* hath no Symbols of Fluxions in his Method, and therefore Mr. *Newton*'s Symbols of Fluxions are the oldest in the kind. Mr. *Leibnitz* began to use the Symbols of Moments or Differences dx, dy, dz in the Year 1677. Mr. *Newton* represented Moments by the Rectangles under the Fluxions and the Moment o, when he wrote his *Analysis*, which was at least Forty Six Years ago. Mr. *Leibnitz* has used the Symbols $\int x$, $\int y$, $\int z$ for the Sums of Ordinates ever since the Year 1686; Mr. *Newton* represented the same thing in his *Analysis*, by inscribing the Ordinate in a Square or Rectangle. All Mr. *Newton*'s Symbols are the oldest in their several Kinds by many Years.

And whereas it has been represented that the use of the Letter o is vulgar, and destroys the Advantages of the Differential Method: on the contrary, the Method of Fluxions, as used by Mr. *Newton*, has all the Advantages of the Differential, and some others. It is more elegant, because in his Calculus there is but one infinitely little Quantity represented by a Symbol, the Symbol o. We have no Ideas of infinitely little Quantities, and therefore Mr. *Newton* introduced Fluxions into his Method, that it might proceed by finite Quantities as much as possible. It is more Natural and Geometrical, because founded upon the *primæ quantitatum nascentium rationes*, which have a Being in Geometry, whilst *Indivisibles*, upon which the Differential Method is founded, have no Being either in Geometry or in Nature. There are *rationes primæ quantitatum nascentium*, but not *quantitates primæ nascentes*. Nature generates Quantities by continual Flux or Increase; and the ancient Geometers admitted such a Generation of Areas and Solids, when they drew one Line into another by local Motion to generate an Area, and the Area into a Line by local Motion to generate a Solid. But the summing up of Indivisibles to compose an Area or Solid was never yet admitted into Geometry. Mr. *Newton*'s Method is also of greater Use and Certainty, being adapted either to the ready finding out of a Proposition by such Approximations as will create no Error in the Conclusion, or to the demonstrating it exactly: Mr. *Leibnitz*'s is only for finding it out. When the Work succeeds not in finite Equations Mr. *Newton* has recourse to converging Series, and thereby his Method becomes incomparably more universal than that of Mr. *Leibnitz*, which is confin'd to finite Equations: for he has no Share in the Method of infinite Series. Some Years after the Method of Series was invented, Mr. *Leibnitz* invented a Proposition for transmuting curvilinear Figures into other curvilinear Figures of equal Areas, in order to square them by converging Series: but the Methods of squaring those other Figures by such Series were not his. By the help of the new *Analysis* Mr. *Newton* found out most of the Propositions in his *Principia Philosophiæ*: but because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically, that the Systeme of the Heavens might be founded upon good Geometry. And this makes it now difficult for unskilful Men to see the Analysis by which those Propositions were found out.

It has been represented that Mr. *Newton*, in the Scholium at the End of his Book of Quadratures, has put the third, fourth, and fifth Terms of a converging Series respectively equal to the second, third, and fourth Differences of the first Term, and therefore did not then understand the Method of second, third and fourth Differences. But in the first Proposition of that Book he shewed how to find the first, second, third and following Fluxions *in infinitum*; and therefore when he wrote that Book, which was before the Year 1676, he did understand the Method of all the Fluxions, and by consequence of all the Differences. And if he did not understand it when he added that Scholium to the End of the Book, which was in the Year 1704, it must have been because he had then forgot it. And so the Question is only whether he had forgot the Method of second and third Differences before the Year 1704.



In the Tenth Proposition of the second Book of his *Principia Philosophiæ*, in describing some of the Uses of the Terms of a converging Series for solving of Problemes, he tells us that if the first Term of the Series represents the Ordinate B C of any Curve Line A C G, and C B D I be a Parallelogram infinitely narrow, whose Side D I cuts the Curve in G and its Tangent C F in F, the second Term of the Series will represent the Line I F, and the third Term the Line F G. Now the Line F G is but half the second Difference of the Ordinate: and therefore Mr. *Newton* when he wrote his *Principia*, put the Third Term of the Series equal to half of the second Difference of the first Term, and by consequence had not then forgotten the Method of second Differences.

In writing that Book, he had frequent occasion to consider the Increase or Decrease of the Velocities with which Quantities are generated, and argues right about it. That Increase or Decrease is the second Fluxion of the Quantity, and therefore he had not then forgotten the Method of second Fluxions.

In the Year 1692, Mr. *Newton*, at the Request of Dr. *Wallis*, sent to him a Copy of the first Proposition of the Book of Quadratures, with Examples thereof in first, second and third Fluxions: as you may see in the second Volume of the Doctor's Works, *pag.* 391, 392, 393 and 396. And therefore he had not then forgotten the Method of second Fluxions.

Nor is it likely, that in the Year 1704 when he added the aforesaid Scholium to the End of the Book of Quadratures, he had forgotten not only the first Proposition of that Book, but also the last Proposition upon which that Scholium was written. If the Word [*ut*], which in that Scholium may have been accidentally omitted between the Words [*erit*] and [*ejus*,] be restor'd, that Scholium will agree with the two Propositions and with the rest of his Writings, and the Objection will vanish.

Thus much concerning the Nature and History of these Methods, it will not be amiss to make some Observations thereupon.

In the *Commercium Epistolicum*, mention is made of three Tracts written by Mr. *Leibnitz*, after a Copy of Mr. *Newton*'s *Principia Philosophiæ* had been sent to *Hannover* for him, and after he had seen an Account of that Book published in the *Acta Eruditorum* for *January* and *February* 1689. And in those Tracts the principal Propositions of that Book are composed in a new manner, and claimed by Mr. *Leibnitz* as if he had found them himself before the publishing of the said Book. But Mr. *Leibnitz* cannot be a Witness in his own Cause. It lies upon him either to prove that he found them before Mr. *Newton*, or to quit his claim.

In the last of those three Tracts, the 20*th* Proposition (which is the chief of Mr. *Newton*'s Propositions) is made a Corollary of the 19*th* Proposition, and the 19*th* Proposition has an erroneous Demonstration adapted to it. It lies upon him either to satisfy the World that the Demonstration is not erroneous, or to acknowledge that he did not find that and the 20*th* Proposition thereby, but tried to adapt a Demonstration to Mr. *Newton*'s Proposition to make it his own. For he represents in his 20*th* Proposition that he knew not how Mr. *Newton* came by it, and by consequence that he found it himself without the Assistance of Mr. *Newton*.

By the Errors in the 15*th* and 19*th* Proposition of the third Tract, Dr. *Keill* hath shewed that when Mr. *Leibnitz* wrote these three Tracts, he did not well understand the Ways of working in second Differences. And this if further manifest by the 10*th*, 11*th*, and 12*th* Propositions of this third Tract. For these he lays down as the Foundation of his infinitesimal Analysis in arguing about centrifugal Forces, and proposes the first of them with relation to the Center of Curvity of the Orb, but uses this Proposition in the two next, with Relation to the Center of Circulation. And by confounding these two Centers with one another in the fundamental Propositions upon which he grounds this *Calculus*, he erred in the Superstructure, and for want of Skill in second and third Differences, was not able to extricate himself from the Errors. And this is further confirmed by the sixth Article of the second Tract. For that Article is erroneous, and the Error arises from his not knowing how to argue well about second and third Differences. When therefore he wrote those Tracts he was but a Learner, and this he ought in candour to acknowledge.

It seems therefore that as he learnt the Differential Method by means of Mr. *Newton*'s aforesaid three Letters compared with Dr. *Barrow*'s Method of Tangents; so Ten Years after, when Mr. *Newton*'s *Principia Philosophiæ* came abroad, he improved his Knowledge in these Matters, by trying to extend this Method to the Principal Propositions in that Book, and by this means composed the said three Tracts. For the Propositions contained in them (Errors and Trifles excepted) are Mr. *Newton*'s (or easy Corollaries from them) being published by him in other Forms of Words before. And yet Mr. *Leibnitz* published them as invented by himself long before they were published by Mr. *Newton*. For in the End of the first Tract, he represents that he invented them all before Mr. *Newton*'s *Principia Philosophiæ* came abroad, and some of them before he left *Paris*, that is before *October* 1676. And the second Tract he concludes with these Words: *Multa ex his deduci possent praxi accommodata, sed nobis nunc fundamenta Geometica jecisse suffecerit, in quibus maxima consistebat difficultas. Et fortassis* *attente consideranti vias quasdam novas satis 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.* He pretends here that the *Fundamenta Geometrica in quibus maxima consistebat difficultas* were first laid by himself in this very Tract, and that he himself had in this very Tract opened *vias quasdam novas satis antea impeditas.* And yet Mr. *Newton*'s *Principia Philosophiæ* came abroad almost two Years before, and gave occasion to the Writing of this Tract, and was written *communibus quoad licuit verbis*, and contains all these Principles and all these new Ways. And Mr. *Leibnitz*, when he published that Tract, knew all this, and therefore ought then to have acknowledged that Mr. *Newton* was the first who laid the *Fundamenta Geometrica in quibus maxima consistebat Difficultas*, and opened the *via novas satis antea impeditas.* In his Answer to Mr. *Fatio* he acknowledged all this, saying *Quam* [methodum] *ante Dominum* Newtonum *& me nullus quod sciam Geometra habuit; uti ante hunc maximi nominis Geometram, NEMO SPECIMINE PUBLICE DATO, se habere PROBAVIT.* And what he then acknowledged he ought in Candour and Honour to acknowledge still upon all Occasions.

Mr. *Leibnitz* in his Letter of *May* 28. 1697, wrote thus to Dr. *Wallis. Methodum Fluxionum profundissimi* Newtoni *cognatam esse methodo meæ differentiali non tantum animadverti postquam opus ejus* [Principiorum scilicet] *& tuum prodiit; sed etiam professus sum in Actis Eruditorum, & alias quoque monui. Id enim candori meo convenire judicavi, non minus quam ipsius merito. Itaque communi nomine designare soleo Analyseos infinitesimalis; quæ latius quam Tetragonistica patet. Interim quemadmodum &* Vietæa *&* Cartesiana *methodus Analyseos speciosæ nomine venit; discrimina tamen nonnulla supersunt: ita fortasse &* Newtoniana *&* Mea *differunt in nonnullis.* Here also Mr. *Leibnitz* allows that when Mr. *Newton*'s Principles of Philosophy came abroad, he understood thereby the Affinity that there was between the Methods, and therefore called them both by the common Name of the infinitesimal Method, and thought himself bound in candour to acknowledge this Affinity: and there is still the same Obligation upon him in point of Candour. And besides this Acknowledgment, he here gives the Preference to Mr. *Newton*'s Method in Antiquity. For he represents that as the vulgar Analysis in *Species* was invented by *Vieta*, and augmented by *Cartes*, which made some Differences between their Methods: so Mr. *Newton*'s Method and his own might differ in some things. And then he goes on to enumerate the Differences by which he had improved Mr. *Newton*'s Method as we mentioned above. And this Subordination of his Method to Mr. *Newton*'s, which he then acknowledged to Dr. *Wallis*, he ought still to acknowledge.

In enumerating the Differences or Improvements which he had added to Mr. *Newton*'s Method; he names in the second Place Differential Equations: but the Letters with passed between them in the Year 1676, do show that Mr. *Newton* had such Equations at that time, and Mr. *Leibnitz* had them not. He names in the third Place Exponential Equations; but these Equations are owing to his Correspondence with the *English.* Dr. *Wallis*, in the Interpolation of Series, considered Fract and Negative Indices of Dignities. Mr. *Newton* introduced into his Analytical Computations, the Fract, Surd, Negative and Indefinitive Indices of Dignities; and in his Letter of *October* 24. 1676, represented to Mr. *Leibnitz* that his Method extended to the Resolution of affected Equations involving Dignities whose Indices were Fract or Surd. Mr. *Leibnitz* in his Answer dated *June* 21. 1677, mutually desired Mr. *Newton* to tell him what he thought of the Resolution of Equations involving Dignities whose indices were undetermined, such as were these $x^y + y^x = xy$, $x^x + y^y = x + y$. And these equations he now calls Exponential, and represents to the World that he was the first Inventor thereof, and magnifies the Invention as a great Discovery. But he has not yet made a publick Acknowledgment of the Light which Mr. *Newton* gave him into it, nor produced any one Instance of the use that he has been able to make of it where the Indices of Dignities are Fluents. And since he has not yet rejected it with his usual Impatience for want of such an Instance, we have reason to expect that he will at length explain its Usefulness to the World.

Mr. *Newton* in his Letter of *October* 24. 1676 wrote that he had two Methods of resolving the Inverse Problems of Tangents, and such like difficult ones; one of which consisted *in assuming a Series for any unknown Quantity from which all the rest might conveniently be deduced, and in collating the homologous Terms of the resulting Equation, for determining the Terms of the assumed Series.* Mr. *Leibnitz* many Years after published this Method as his own, claiming to himself the first Invention thereof. It remains that he either renounce his Claim publickly, or prove that he invented it before Mr. *Newton* wrote his said Letter.

It lies upon him also to make a publick Acknowledgment of his Receipt of Mr. *Oldenburgh*'s Letter of *April* 15. 1675, wherein several converging Series for squaring of Curves, and particularly that of Mr. *James Gregory* for finding the Arc by the given Tangent, and thereby squaring the Circle, were communicated to him. He acknowledged it privately in his Letter to Mr. *Oldenburg* dated *May* 20. 1675 still extant in his own Hand-writing, and by Mr. *Oldenburg* left entred in the Letter-Book of the *Royal-Society.* But he has not yet acknowledged it publickly, as he ought to have done when he published that Series as his own.

It lies on him also to make a publick Acknowledgment of his having received the Extracts of Mr. *James Gregory*'s letters, which, at his own Request, were sent to him at *Paris* in *June* 1676 by Mr. *Oldenburgh* to peruse: amongst which was Mr. *James Gregory*'s Letter of *Feb.* 15. 1671, concerning that Series, and Mr. *Newton*'s Letter of *December* 10. 1672 concerning the Method of Fluxions.

And whereas in his Letter of *Decem.* 28. 1675 he wrote to Mr. *Oldenburgh*, that he had communicated that Series above two Years before to his Friends at *Paris*, and had written to him sometimes about it; and in his Letter of *May* 12. 1676 said to Mr. *Oldenburgh* that he had written to him about that Series some Years before; and in his Letter to Mr. *Oldenburgh* dated *Aug.* 27. 1676, that he had communicated that Series to his Friends above three Years before; that is, upon his first coming from *London* to *Paris*: He is desired to tell us how it came to pass, that when he received Mr. *Oldenburgh*'s Letter of *Apr.* 15. 1675 he did not know that Series to be his own.

In his Letters of *July* 15. and *Octob.* 26. 1674, he tells us of but one Series for the circumference of a Circle, and saith that the Method which gave him this Series, gave him also a Series for any Arc whose Sine was given, tho' the Proportion of the Arc to the whole Circumference be not known. This Method therefore, by the given Sine of 30 Degrees, gave him a Series for the whole Circumference. If he had also a Series for the whole Circumference deduced from the Tangent of 45 Degrees, he is desired to tell the World what Method he had in those Days, which could give him both those Series. For the Method by the Transmutation of Figures will not do it. He is desired also to tell us why in his said Letters he did not mention more Quadratures of the Circle than one.

And if in the Year 1674 he had the Demonstration of Series for finding any Arc whose

Sine is given, he is desired to tell the World what it was; and why in his Letter of *May* 12. 1676 he desired Mr. *Oldenburgh* to procure from Mr. *Collins* the demonstration of Mr. *Newton*'s Series for doing the same thing; and wherein his own Series differed from Mr. *Newton*'s. For upon all these Considerations there is a Suspicion that Mr. *Newton*'s Series for finding the Arc whose Sine is given, was communicated to him in *England*; and that in the Year 1673 he began to communicate it as his own to some of his Friends at *Paris*, and the next Year wrote of it as his own in his Letters to Mr. *Oldenburgh*, in order to get the Demonstration or Method of finding such Series. But the year following, when Mr. *Oldenburgh* sent him this Series and the Series of Mr. *Gregory* and Six other Series, he dropt his Pretence to this Series for want of a Demonstration, and took time to consider the Series sent him, and to compare them with his own, as if his Series were others different from those sent him. And when he found a Demonstration of *Gregory*'s Series by a Transmutation of Figures, he began to communicate it as his own to his Friends at *Paris*, as he represents in the *Acta Eruditorum* for *April* 1691. *pag.* 178, saying; *Jam Anno* 1675 *compositum habebam opusculum Quadraturæ Arithmeticæ ab Amicis ab illo tempore lectum*, &c. But the Letter by which he had received this Series from Mr. *Oldenburgh* he concealed from his Friends, and pretended to Mr. *Oldenburgh* that he had this Series a Year or two before the Receipt of that Letter. And the next Year, upon receiving two of Mr. *Newton*'s Series again by one *George Mohr*, he wrote to Mr. *Oldenburgh* in such a manner as if he had never seen them before, and upon Pretence of their Novelty, desired Mr. *Oldenburgh* to procure from Mr. *Collins* Mr. *Newton*'s Method of finding them. If Mr. *Leibnitz* thinks fit to obviate this Suspicion, he is in the first Place to prove that he had Mr. *Gregory*'s Series before he received it from Mr. *Oldenburgh*.

It lies upon him also to tell the World what was the Method by which the several Series of Regression for the Circle and Hyperbola, sent to him by Mr. *Newton June* 13. 1676, and claimed as his own by his Letter of *August* 27. following, were found by him before he received them from Mr. *Newton*.

And whereas Mr. *Newton* sent him, at his own Request, a Method of Regression, which upon the first reading, he did not know to be his own, nor understood it; but so soon as he understood it he claimed as his own, by pretending that he had found it long before, and had forgot it, as he perceived by his old Papers: it lies upon him, in point of Candor and Justice, either to prove that he was the first Inventor of this Method, or to renounce his Claim to it for preventing future Disputes.

Mr. *Leibnitz* in his Letter to Mr. *Oldenburgh* dated *Feb.* 3. $167\frac{2}{3}$ claimed a Right to a certain Property of a Series of Numbers Natural, Triagular, Pyramidal, Triangulo-Triangular, *&c.* and to make it his own, represented that he wondred that Monsieur *Pascal*, in his Book entituled *Triangulum Arithmeticum*, should omit it. That Book was published in the year 1665, and contains this Property of the Series; and Mr. *Leibnitz* has not yet done him the Justice to acknowledge that he did not omit it. It lies upon him therefore in Candor and Justice, to renounce his Claim to this Property, and acknowledge Mr. *Paschal* the first Inventor.

He is also to renounce all Right to the Differential Method of *Mouton* as second Inventor: for second Inventors have no Right. The sole Right is in the first Inventor until another finds out the same thing apart. In which case to take away the Right of the first Inventor, and divide it between him and that other, would be an Act of Injustice.

In his Letter to Dr. *Sloane* dated *Decem.* 29. 1711. he has told us that his Friends know how he came by the Differential Method. It lies upon him, in point of Candor, openly and plainly, and without further Hesitation, to satisfy the World how he came by it.

In the same Letter he has told us that he had this Method above Nine Years before he published it; and it follows from thence that he had it in the Year 1675 or before. And yet it is certain that he had it not when he wrote his Letter to Mr. *Oldenburgh* dated *Aug.* 27. 1676, wherein he affirmed that Problems of the Inverse Method of Tangents and many others, could not be reduced to infinite Series, nor to Equations or Quadratures. It lies upon him therefore, in point of Candor, to tell us what he means by pretending to have found the Method before he had found it.

We have shewed that Mr. *Leibnitz* in the End of the Year 1676, in returning home from *France* through *England* and *Holland*, was mediating how to improve the Method of *Slusius* for Tangents, and extend it to all sorts of Problems, and for this end proposed the making of a general Table of Tangents; and therefore had not yet found out the true Improvement. But about half a Year after, when he was newly fallen upon the true Improvement, he wrote back; *Clariss.* Slusii *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 before those Days. It lies on him, in point of Candor, to make us understand that he pretended to this Antiquity of his Invention with some other Design than to rival and supplant Mr. *Newton*, and to make us believe that he had the Differential Method before Mr. *Newton* explained it to him in his Letters of *June* 13. and *Octob.* 24. 1676, and before Mr. *Oldenburgh* sent him a Copy of Mr. *Newton*'s Letter of *Decem.* 10. 1672 concerning it.

The Editors of the *Acta Eruditorum* in *June* 1696, in giving an Account of the two first Volumes of the Mathematical Works of Dr. *Wallis*, wrote thus, in the Style of Mr. *Leibnitz: Cæterum ipse* Newtonus*, non minus Candore quam præclaris in rem Mathematicam meritis insignis, publice & privatim agnovit* Leibnitium*, tum cum* (*interveniente celeberrimo Viro* Henrico Oldenburgo *Bremensi, Societatis Regiæ* Anglicanæ *tunc Secretario*) *inter ipsos* (*ejusdem jam tum Societatis Socios*) *Commercium intercederet, id est jam fere ante annos viginti & amplius, Calculum suum differentialem, Seriesque infitas, & pro iis quoque Methodos generales habuisse; quod* Wallisius *in Præfatione Operum, factæ inter eos communicationis mentionem faciens, præteriit, quoniam e eo fortasse non satis ipsi constabat. Cæterum Differentiarum consideratio* Leibnitiana*, cujus mentionem facit* Wallisius (*ne quis scilicet, ut ipse ait, causeretur de Calculo Differentiali nihil ab ipso dictum fuisse*) *meditationes apernit, quæ aliunde non æque nascebantur, &c.* By the Words here cited out of the Preface to the two first Volumes of Dr. *Wallis*'s Works, it appears that Mr. *Leibnitz* had seen that Part of the Preface, where Mr. *Newton* is said to have explained to him (in the Year 1676) the Method of Fluxions found by him Ten Years before or above. Mr. *Newton* never allowed that Mr. *Leibnitz* had the Differential Method before the Year 1677. And Mr. *Leibnitz* himself in the *Acta Eruditorum* for *April* 1691. pag. 178. acknowledged that he found it after he returned home from *Paris* to enter upon Business, that is, after the Year 1676. And as for his pretended general Method of infinite Series, it is so far from being general, that it is of little or no use. I do not know that any other Use hath been made of it, than to colour over the Pretence of Mr. *Leibnitz* to the Series of Mr. *Gregory* for squaring the Circle.

Mr. *Leibnitz*, in his Answer to Mr. *Fatio* printed in the *Acta Eruditorum* for the year 1700. pag. 203. wrote thus. *Ipse* [Newtonus] *scit unus omnium optime, satisque indicavit publice* *cum sua* Mathematica Naturæ Principia *publicaret, Anno* 1687*, nova quædam inventa Geometrica, quæ ipsi communica mecum fuere, NEUTRUM LUCI AB ALTERO ACCEPTÆ, sed meditationibus quemque suis debere, & a me decennio ante* [i. e. anno 1677] *exposita fuisse*. In the Book of *Principles* here referred unto, Mr. *Newton* did not acknowledge that Mr. *Leibnitz* found this Method without receiving Light into it from Mr. *Newton*'s Letters above-mentioned; and Dr. *Wallis* had lately told him the contrary without being then confuted or contradicted. And if Mr. *Leibnitz* had found the Method without the Assistance of Mr. *Newton*, yet second Inventors have no Right.

Mr. *Leibnitz* in his aforesaid Answer to Mr. *Fatio*, wrote further: *Certe cum elementa Calculi mea edidi anno* 1684*, ne constabat quidem mihi aliud de inventis ejus* [sc. 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, et si cæterorum ejus Calculi adhuc expers. Sed majora multo consecutum* Newtonum*, viso demum libro* Principiorum *ejus satis intellexi*. Here he again acknowledged that the Book of *Principles* gave him great Light into Mr. *Newton*'s Method: and yet he now denies that this Book contains any thing of that Method in it. Here he pretended that before that Book came abroad he knew nothing more of Mr. *Newton*'s Inventions of this kind, than that he had a certain Method of Tangents, and that by that Book he received the first Light into Mr. *Newton*'s Method of Fluxions: but in his Letter of *June* 21. 1677 he acknowledged that Mr. *Newton*'s Method extended also to Quadratures of curvilinear Figures, and was like his own. His Words are; *Arbitror quæ celare voluit* Newtonus *de Tangentibus ducendis ab his non abludere. Quod addit, ex hoc eodem fundamento Quadraturas quoque reddi faciliores me in sententia hac confirmat; nimirum semper figuræ illæ sunt quadrabiles quæ sunt ad æquationem differentialem.*

Mr. *Newton* had in his three Letters above-mentioned (copies of which Mr. *Leibnitz* had received from Mr. *Oldenbergh*) represented his Method so general, as by the Help of Equations, finite and infinite, to determin *Maxima* and *Minima*, Tangents, Areas, solid Contents, Centers of Gravity, Lengths and Curvities of curve Lines and curvilinear Figures, and this without taking away Radicals, and to extend to the like Problems in Curves usually called Mechanical, and to inverse Problems of Tangents and others more difficult, and to almost all Problems, except perhaps some Numeral ones like those of *Diophantus*. And Mr. *Leibnitz* in his Letter of *Aug.* 27. 1676, represented that he could not believe that Mr. *Newton*'s Method was so general. Mr. *Newton* in the First of his three Letters set down his Method of Tangents deduced from this general Method, and illustrated it with an Example, and said that this Method of Tangents was but a Branch or Corollary of his General Method, and that he took the Method of Tangents of *Slusius* to be of the same kind: and thereupon Mr. *Leibnitz*, in his Return from *Paris* through *England* and *Holland* into *Germany*, was considering how to improve the Method of Tangents of *Slusius*, and to extend it to all sorts of Problems, as we shewed above out of his Letters. And in his third Letter Mr. *Newton* illustrated his Method with Theorems for Quadratures and Examples thereof. And when he had made so large an Explanation of his Method, that Mr. *Leibnitz* had got Light of it, and had in his Letter of *June* 21. 1677 explained how the Method which he was fallen into answered to the Description which Mr. *Newton* had given of his Method, in drawing of Tangents giving the Method of *Slusius*, proceeding without taking away Fractions and Surds, and facilitating Quadratures; for him to tell the *Germans* that in the Year 1684, when he first published his Differential Method, he knew nothing more of Mr. *Newton*'s invention, than that he had a certain Method of Tangents, is very extraordinary and wants an Explanation.

At that time he explained nothing more concerning his own Method, than how to draw Tangents and determin *Maxima* and *Minima* without taking away Fractions or Surds. He certainly knew that Mr. *Newton*'s Method would do all this, and therefore ought in Candor to have acknowledged it. After he had thus far explained his own Method, he added that what he had there laid down were the Principles of a much sublimer Geometry, reaching to the most difficult and valuable Problems, which were scarce to be resolved without the Differential Calculus, *AUT SIMILI*, or another like it. What he meant by the words *AUT SIMILI* was impossible for the *Germans* to understand without an Interpreter. He ought to have done Mr. *Newton* justice in plain intelligible Language, and told the *Germans* whose was the *Methodus SIMILIS*, and of what Extent and Antiquity it was, according to the Notices he had received from *England*; and to have acknowledged that his own Method was not so ancient. This would have prevented Disputes, and nothing less than this could fully deserve the Name of Candor and Justice. But afterwards, in his Answer to Mr. *Fatio*, to tell the *Germans* that in the Year 1684, when he first published the Elements of his Calculus, he knew nothing of a *Methodus SIMILIS*, nothing of any other method than for drawing Tangents, was very strange and wants an Explanation.

It lies upon him also to satisfy the World why, in his Answer to Dr. *Wallis* and Mr. *Fatio*, who had published that Mr. *Newton* was the oldest Inventor of that Method by many Years, he did not put in his Claim of being the oldest Inventor thereof, but staid till the old Mathematicians were dead, and then complained of the new Mathematicians as Novices; attacked Mr. *Newton* himself, and declined to contend with any Body else, notwithstanding that Mr. *Newton* in his Letter of *Octob.* 24. 1676 had told him, that for the sake of Quiet, he had Five Years before that time laid aside his Design of publishing what he had then written on this Subject, and has ever since industriously avoided all Disputes about Philosophical and Mathematical Subjects, and all Correspondence by Letters about those Matters, as tending to Disputes; and for the same Reason has forborn to complain of Mr. *Leibnitz*, untill it was shewed him that he stood accused of Plagiary in the *Acta Lipsiæ*, and that what Mr. *Keill* had published was only in his Defence from the Guilt of that Crime.

It has been said that the Royal-Society gave judgment against Mr. *Leibnitz* without hearing both Parties. But this is a Mistake. They have not yet given judgment in the Matter. Mr. *Leibnitz* indeed desired the Royal-Society to condemn Mr. *Keill* without hearing both Parties; and by the same sort of Justice they might have condemned Mr. *Leibnitz* without hearing both Parties; for they have an equal Authority over them both. And when Mr. *Leibnitz* declined to make good his Charge against Mr. *Keill*, the Royal-Society might in justice have censured him for not making it good. But they only appointed a Committee to search out and examin such old Letters and Papers as were still extant about these Matters, and report their Opinion how the Matter stood according to those Letters and Papers. They were not appointed to examin Mr. *Leibnitz* or Mr. *Keill*, but only to report what they found in the ancient Letters and Papers: and he that compares their Report therewith will find it just. The Committee was numerous and skilful and composed of Gentlemen of several Nations, and the Society are satisfied in their Fidelity in examining the Hands and other Circumstances, and in printing what they found in the ancient Letters and Papers so examined, without adding, omitting or altering any thing in favour of either Party. And the Letters and Papers are by order of the Royal-Society preserved, that they may be consulted and compared with the *Commercium Epistolicum*, whenever it shall be desired by Persons of Note. 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 Mr. *Newton* in his *Principles* and *Optiques* has pursued is Experimental; and it is not the Business of Experimental Philosophy to teach the Causes of things any further than they can be proved by Experiments. We are not to fill this Philosophy with Opinions which cannot be proved by Phænomena. In this Philosophy Hypotheses have no place, unless as Conjectures or Questions proposed to be examined by Experiments. For this Reason, Mr. *Newton* in his Optiques distinguished those things which were made certain by Experiments from those things which remained uncertain, and which he therefore proposed in the End of his Optiques in the Form of Queries. For this Reason, in the Preface to his *Principles*, when he had mention'd the Motions of the Planets, Comets, Moon and Sea as deduced in this Book from Gravity, he added: *Utinam cætera Naturæ Phænomena ex Principiis Mechanicis eodem argumentandi genere derivare liceret. Nam multa me movent ut nonnihil suspicer ea omnia ex viribus quibusdam pendere posse, quibus corporum particulæ per causas nondum cognitas vel in se mutuo impelluntur & secundum figuras regulares cohærent, vel ab invicem fugantur & recedunt: quibus viribus ignotis Philosophi hactenus Naturam frustra tentarunt.* And in the End of this Book in the second Edition, he said that for want of a sufficient Number of Experiments, he forbore to describe the Laws of the Actions of the Spirit or Agent by which this Attraction is performed. 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*, near the Beginning thereof, in these Words: *Virium causas & sedes Physicas jam non expendo.* And a little after: *Voces Attractionis, Impulsus, vel Propensionis cujuscunque in centrum indifferenter & pro se mutuo promiscue usurpo, has Vires non Physice sed Mathematice tantum considerando. Unde 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 verè & physicè tribuere, si forte aut Centra trahere aut vires Centrorum esse dixero.* And in the End of his Opticks: *Qua causa efficiente hæ attractiones* [sc. gravitas, visque magnetica & electrica] *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 edoctos oportet quænam corpora se invicem attrahant, & quænam sint leges & proprietates istius attractionis, quam in id inquirere par sit quanam efficiente causa peragatur attractio.* 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; and distinguishes them from the occult Qualities which are supposed to flow from the specifick 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 harum Gravitatis proprietatum ex Phænomenis nondum potui deducere, & Hypotheses non fingo. Quicquid enim ex Phænomenis non deducitur Hypothesis vocanda est; & Hypotheses seu Metaphysicæ seu Physicæ, seu Qualitatum occultarum, seu Mechanicæ, in Philosophia experimentali locum non habent. —— satis est quod Gravitas revera existet & agat secundum leges à nobis ex-*

positas, & ad Corporum cœlestium & Maris nostri motus omnes sufficiat. And after all this, one would wonder that Mr. *Newton* should be reflected upon for not explaining the Causes of Gravity and other Attractions by Hypotheses; as if it were a Crime to content himself with Certainties and let Uncertainties alone. And yet the Editors of the *Acta Eruditorum*, (*a*) have told the World that Mr. *Newton* denies that the cause of Gravity is Mechanical, and that if the Spirit or Agent by which Electrical Attraction is performed, be not the *Ether* or *subtile Matter* of *Cartes*, it is less valuable than an Hypothesis, and perhaps may be the Hylarchic Principle of Dr. *Henry Moor*: and Mr. *Leibnitz* (*b*) hath accused him of making Gravity a natural or essential Property of Bodies, and an occult Quality and Miracle. And by this sort of Railery they are perswading the *Germans* that Mr. *Newton* wants Judgment, and was not able to invent the Infinitesimal Method.

It must be allowed that these two Gentlemen differ very much in Philosophy. The one proceeds upon the Evidence arising from Experiments and Phænomena, and stops where such Evidence is wanting; the other is taken up with Hypotheses, and propounds them, not to be examined by Experiments, but to be believed 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 Enquiry) attributes it to the Power of the Creator that the least Particles of Matter are hard: the other attributes the Hardness of Matter to conspiring Motions, and 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 and move and have our Being) is Omnipresent; but not as a Soul of the World: the other that he is not the Soul of the World, but *INTELLIGENTIA SUPRAMUNDANA*, an Intelligence above the Bounds of the World; whence it seems to follow that he cannot do any thing within the Bounds of the World, unless by an incredible Miracle. The one teaches that Philosophers are to argue from Phænomena and *Experiments* to the Causes thereof, and thence to the Causes of those Causes, and so on till we come to the first Cause: the other that all the Actions of the first Cause are Miracles, and all the Laws imprest on Nature by the Will of God are perpetual Miracles and occult Qualities, and therefore not to be considered in Philosophy. But must the constant and universal Laws of Nature, if derived from the Power of God or the Action of a Cause not yet known to us, be called Miracles and occult Qualities, that is to say, *Wonders* and *Absurdities*? Must all the Arguments for a God taken from the Phænomena of Nature be exploded by *new hard Names*? And must Experimental Philosophy be exploded as *miraculous* and *absurd*, because it asserts nothing more than can be proved by Experiments, and we cannot yet prove by Experiments that all the Phænomena in Nature can be solved by meer Mechanical Causes? Certainly these things deserve to be better considered.

(*a*) *Anno* 1714; *mense Martio*, p. 141, 142.

(*b*) *In tractatu de Bonitate Dei & in Epistolis ad D.* Hartsoeker *& alibi.*