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The Newton–Leibniz Calculus Controversy, 1708–1730

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Abstract and Keywords

This article examines the controversy between Isaac Newton and Gottfried Wilhelm Leibniz concerning the priority in the invention of the calculus. The dispute began in 1708, when John Keill accused Leibniz of having plagiarized Newton's method of fluxions. It will be shown that the mathematicians participating in the controversy in the period between 1708 and 1730—most notably Newton, Leibniz, Keill, and Johann Bernoulli—held different conceptions of mathematical method. The dispute began in a political climate agitated by the Hanoverian succession and was intertwined with tensions dividing the Royal Court. It developed into a discussion of technical issues concerning the relation between mathematics and natural philosophy and the methods of the integral calculus.

Keywords: method of fluxions, calculus, integral, Gottfried Wilhelm Leibniz, John Keill, Johann Bernoulli, Hanoverian succession

Introduction: On Priority Controversies

Among all the controversies that have punctuated the development of early-modern science, few are so infamous as the calculus priority dispute that in the first decades of the eighteenth century opposed the president of the Royal Society, Isaac Newton, and the counsellor of the Dukes of Hanover, Gottfried Wilhelm Leibniz. The debate was received by the Republic of Letters as one concerning priority of invention and, as such, was studied by historians until very recently.[1] At first sight, such a point of view is straightforward and approachable on the basis of factual evidence. The ability to access manuscript sources with unprecedented completeness and reliability allows twenty-first century historians to reach a consensus. Newton was the first to discover the method of series and fluxions, in the mid-1660s. During his stay in Paris in 1672–1676, Leibniz independently discovered the differential and integral calculus, which he first printed in a journal of his own founding, the *Acta Eruditorum*, in the 1680s. The heart of the matter consists in these apparently simple facts: Newton was the first discoverer, Leibniz the first to publish the discovery.

Yet this simple factual scenario immediately becomes far murkier when we ask ourselves questions concerning the equivalence of the two mathematical tools, and indeed the range of means of publication available to historical actors. It is far from evident whether the two calculi are one and the same thing. Furthermore, Newton, as was customary in his age, had circulated information about series and fluxions within his circle of correspondents in the 1670s. The discovery and publication of the calculus are highly complex historical events: this complexity becomes opaque when the question of priority of invention is asked in an overly simplistic, forensic manner.

Questions of credit and merit polarize the attention of scientists, as Robert Merton observed long ago, because they are related to norms of intellectual property that are accepted as moral obligations within the institution of science (Merton 1957). The priority dispute has often been studied by historians who shared these very same moral norms. Such an historiographical standpoint is pervasive. We thus have extensive debates—as sterile as they are misguided—about who discovered the law of inertia first, who should be credited as the first to have introduced the second law of motion, or who first proved the fundamental theorem of the calculus.

A historiography polarized on questions of credit attribution constitutes a delusion on two grounds. First, it fails to appreciate the semantic complexity of the laws and theorems it claims to study, since it reduces them to well-defined items that can be discovered by a single individual. And second, it fails to grasp the eminently social dimension of scientific discovery. Nowadays, most historians think that there is no single mathematician who can be credited with the discovery of the calculus. They would agree that neither Newton nor Leibniz “discovered” the calculus. Rather, the accepted view is that both Newton and Leibniz contributed, each in his own way, to a process that was begun by earlier generations of mathematicians and was concluded by posterity: the result of this long, nonlinear process is the calculus as we know it today. Moreover, Newton’s method of series and fluxions and Leibniz’s differential and integral calculus are not described in current historical narratives as one and the same thing, but rather as two formalisms that differ in the definitions of their basic concepts as well as in their algorithmic peculiarities.

Notwithstanding present-day historians’ awareness of such complexities, the “Newton-Leibniz calculus dispute” is often still studied as a matter of “thrust and parry” between two great combatants: one, the first to discover “the” calculus; the other, the first to publish it. Thus, the narrative of the calculus priority dispute is often dominated by two towering leading figures. In this chapter, we will steer clear of this perspective and devote attention not only to Newton and Leibniz, but also to lesser actors, all too often described simply as acolytes fighting for one of the two great masters. We will study how mathematicians aware of the confrontation between Newton and Leibniz positioned themselves with respect to this infamous quarrel, how they used it for their own purposes, and how they viewed the object at stake in the controversy. The calculus controversy indeed mobilized the European Republic of Letters at large and was discussed—sometimes with passion, sometimes with disenchantment—in salons and academies, coffee shops and princely courts. This chapter concentrates on those mathematically trained actors who could follow the technicalities of the debate. As we shall see, their reactions to the calculus controversy hint at their agendas, and at their views concerning mathematical method.

John Keill Attacks Leibniz

The spark that set fire to the priority dispute between Newton and Leibniz was a statement published in the September-October 1708 issue of the *Philosophical Transactions* of the Royal Society. The offending passage appeared in a paper by John Keill, a Scottish mathematician based in Oxford. Keill, in his letter addressed to Edmond Halley, after extensively using the characteristically Newtonian fluxional terminology and dot notation, wrote:

> All these things follow from the nowadays highly celebrated arithmetic of fluxions, which Mr Newton beyond any shadow of doubt first discovered, as any one reading his letters published by Wallis will readily ascertain, and yet the same arithmetic was afterwards published by Mr Leibniz in the *Acta Eruditorum* having changed the name and the symbolism.
>
> (Keill 1708b, p. 185)[2]

As we shall see, Leibniz, as a fellow of the Royal Society, felt fully entitled to request a formal apology for this "most impertinent accusation" published in the society's official journal (Newton 1959–1977, V, p. 97).

There are two things that are often forgotten about Keill's infamous letter to Halley. First, its main purpose was to provide a fluxional treatment of the inverse problem of central forces, a fundamental problem for Newton's mathematical natural philosophy that was at the top of the agenda for the most advanced practitioners of calculus.[3] The question of how to deal with central force motion mathematically was to be bitterly debated between Keill and Johann Bernoulli, among others, in the ensuing years (see the section "The Inverse Problem of Central Forces"). Second, the paper on central forces was not the only one authored by Keill to appear in the 1708 volume of the *Philosophical Transactions.* Keill had already engineered another bombshell that was to arouse criticism, starting with a rejoinder from Christian Wolff, the eminent Leibnizian philosopher based in Halle. Keill's first paper (included in the May-June issue) was an attempt to deal with matter theory in terms of interparticulate forces (Keill 1708a, 1710; Wolff 1710).

John Keill was born in Edinburgh in 1671. Through his mother, Sarah Cockburn, he was related to a family of Scottish Episcopalians who actively opposed the Presbyterian policy enforced in Scotland after the Glorious Revolution. Most probably because of the unfavorable political situation in Scotland, in 1694, Keill moved to the University of

Oxford, where Tories could find a more hospitable environment. Once in Oxford, he rejoined David Gregory (a nephew of the great mid-seventeenth century mathematician James Gregory), whose teaching in Newtonian philosophy he had already followed at the University of Edinburgh. Gregory, who also had left Scotland because of his ties to opponents of William and Mary's accession to the throne, was one of the first to adopt the natural philosophy of Newton's *Principia*. Early in 1687, he began writing a detailed commentary on the magnum opus, just after being appointed Professor of Mathematics at the University of Edinburgh.[4] It is thanks to Newton's recommendation that, in 1691, he was elected Savilian Professor of Astronomy in Oxford. Gregory and Keill belonged to a group—also including John Keill's younger brother James, John Freind, William Cockburn, George Hepburn, John Craig and Colin Campbell—that Anita Guerrini in a classic paper dubbed the "Tory Newtonians" (Guerrini 1986). This group displayed a certain political and religious homogeneity: in broad brushstrokes, one might say that they shared political sympathies for the Tories. Some of them were also united by a certain degree of family kinship, and by their common fate of having been compelled to leave Presbyterian Scotland. Preeminent witin this group was Archibald Pitcairne, a leading Edinburgh physician with strong ties to Leiden, where he had taught medicine for a short time. Indeed, a major interest of all the members of Gregory and Pitcairne's circle was a theory of matter based on interparticulate forces aimed at explaining physiology. They were keen to identify this theory (known, as "iatro-mathematics") as Newtonian.

The members of the group who most actively contributed to mathematics were Craig, Gregory, and John Keill. Their mathematical production is often a hybrid of Newtonian and Leibnzian influences, or more broadly of English and Continental ones.[5] The period between the 1690s and early 1700s was one in which Newtonian mathematics was an extremely volatile concept. Newton had circulated his ideas via correspondence, and some of his work on fluxions had appeared in print in Wallis's *Opera* (1693–1699), but it was difficult to grasp in what his contribution consisted, whereas Leibniz's calculus was accessible in print in the *Acta Eruditorum* and the *Mémoires* of the Paris academy, to which the brothers Bernoulli had already contributed several papers. Consequently, many mathematicians in Britain, and especially in Scotland, relied upon the Leibnzian calculus as published in the *Acta*. Most notably, in 1685, John Craig had published a short treatise, written in Leibniz's notation, on the quadrature of curvilinear figures in which Newton's contributions were mentioned only in passing (Craig 1685). Craig continued to use the differential and integral notation in papers published in the *Philosophical Transactions* until 1708. Meanwhile, David Gregory was claiming for himself a theorem on quadratures that Newton had privately communicated to Leibniz in the epistola posterior, dated October 24, 1676, and to Craig, who had visited Newton in his rooms at Trinity in 1685. It seems likely that it was through Craig that the theorem had passed into Gregory's hands. In 1688, Archibald Pitcairne had published the theorem attributing it to Gregory (Pitcairne 1688).[6]

This notwithstanding, Gregory was able to maintain more than cordial relations with Newton: after a momentous visit to Cambridge in May 1694, he became one of Newton's most faithful acolytes. After May 1694, for reasons that are still unclear, Gregory was allowed unlimited access to Newton's most secret manuscripts, including those pertaining to the wisdom of the ancients. As a mathematician and astronomer, Gregory showed particular interest—as his *Memoranda* reveal—in two topics of Newtonian research: quadratures and the project for a second edition of the *Principia*. These are two related fields of enquiry, as Newton was thinking of adding an appendix on quadratures to the *Principia* where the reader was to be given details on how the squaring and rectification of curves could be used in the mathematization of natural philosophy.[7] For a while, Gregory entertained hopes to be the editor of the second edition.

His Newtonian inclinations notwithstanding, Gregory based much of his mathematical research on the papers by the Bernoullis published in the *Acta*. Indeed, the manuscript treatise on fluxions that Gregory circulated in the mid-1690s reveals, even from the title, the hybrid character of his mathematical background.[8] Because of his first-hand knowledge of Newton's still largely unpublished mathematical work, his *Notæ* to the *Principia*, his mid-1690s treatise on fluxions and his treatise on astronomy (Gregory 1702), Gregory acquired a high mathematical reputation. Yet he suffered a setback when, in a paper published in the *Philosophical Transaction* for 1697 (Gregory 1697), he attempted an alternative solution of the *catenaria* problem posed by Jacob Bernoulli in 1690.[9] This is the topic Johann Bernoulli chose in 1691 when, on a visit to Paris, he positively impressed the French Oratorians gathered around Nicolas Malebranche by determining the shape of a free-hanging chain (the catenaria) via the integration of a differential equation. The solution of the catenaria problem was a typical instantiation of the power of the Leibnizian algorithm: the problem had been attempted unsuccessfully since Galileo's times. The Pisan's suggestion that the solution might be approximated by a parabola was deemed unsatisfactory. Bernoulli showed that by introducing a representation of the forces acting on an element of the curve in terms of infinitesimals and by employing Leibniz's notation, one could get a differential equation. He obtained the solution in terms of transcendental curves (see Figure 1). The above characteristics of Bernoulli's solution should be underlined: Leibniz's *nova methodus* showed all its strength in the integration of differential equations in terms of what nowadays we would call transcendental functions. The fact that Gregory's solution was faulty (he achieved the correct differential equation but via the wrong reasoning) was stigmatized, albeit in a polite form, by Leibniz in an anonymous review in the *Acta*.[10] Leibniz opined that Gregory's failure was due to the inferiority of Newton's method compared to his own, and Gregory attempted a rebuttal (Leibniz 1699, Gregory 1699).

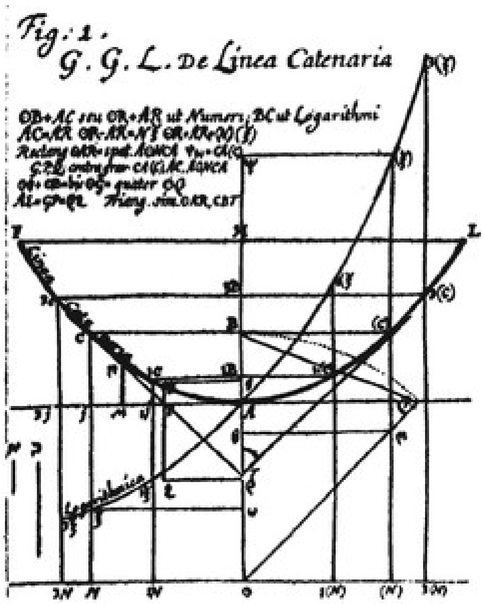


Figure 1 Tav. VII, Figure 1 in (Leibniz 1691, 278). A modernized solution of the catenaria problem inspired by Johann Bernoulli's lengthy calculation is provided in (Hairer and Wanner 2008, 136–137). One seeks the shape of a flexible, non-extensible, homogeneous chain subject to constant gravitation. The chain will be in equilibrium when $cdy = sdx$, where x and y are the abscissa and the ordinate of the curve, and s is the arc-length calculated from the lowest point. Introducing the variable $p = dy/dx$, after differentiation one obtains cdp=ds=1+p2dx, a differential equation whose variables p and x are separable. The integration is now easy: $p = \sinh((x - x_0)/c)$ and $y = K + c\cosh((x - x_0)/c)$. The figure here reproduced is taken from Leibniz's paper in *Acta Eruditorum* (1691). One can see the shape of the catenaria and the exponential curve.

Thus, behind Keill's attack one may discern a complex web of motivations, all pointing against Leibniz. Keill, as a faithful student of Gregory, might have had the catenaria setback in mind when he decided to launch an anti-Leibnzian attack with his paper on central forces. The paper on matter theory was instead meant to provide mathematical support for Pitcairne's and his brother James Keill's iatro-mathematical theories, which were at odds with Leibniz's and Wolff's philosophy and physiology. Lastly, one should not forget that the group Keill belonged to had good reasons to frown upon the political events preceding the Hanoverian succession. Leibniz, as we shall see in the section "Newton engineers the *Commercium*", was close to the German Elector destined after the Act of Settlement (1701) to ascend to the throne of Great Britain and Ireland (1714).

Newton Engineers the *Commercium*

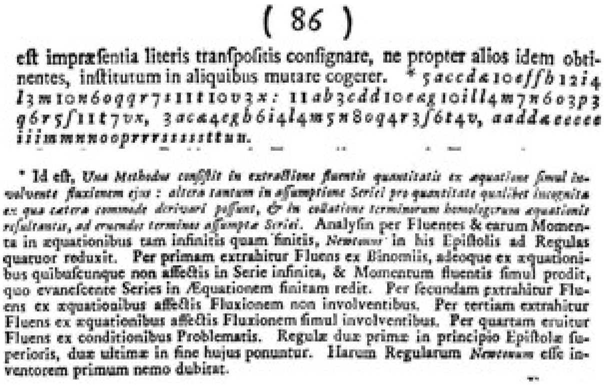
On December 18/29, 1711, Leibniz demanded that the Royal Society protect him from the "empty and unjust braying" of such an "upstart" as Keill (Newton 1959–1977, V, p. 207). Consequently, a committee of the Royal Society, appointed on March 6, 1712, and secretly guided by its president, Isaac Newton, produced a detailed report based on letters in the custody of Newton, of the Royal Society and of the manuscript collector and mathematics tutor William Jones. The *Commercium Epistolicum* ([Newton] 1712) was sketched just 50 days after the committee's nomination and distributed free of cost only in January–February 1713 (N.S.). The committee concluded the *Commercium* with a "sententia" declaring that Newton was the "first inventor" and that "[Leibniz's] Differential Method is one and the same with the Method of Fluxions, excepting the Name and Mode of

Notation." It was also strongly suggested that Leibniz, after his visits to London in 1673 and 1676, and after receiving letters and other material from Newton's friends, and in 1676 from Newton himself, had gained sufficient information about the method of fluxions to allow him to publish the calculus as his own discovery, after changing the symbols.[11]

It is documented by compelling manuscript evidence that Newton was the principal author of the *Commercium*: he wrote the opening "Ad lectorem"; he selected the excerpts from the available correspondence; and he wrote the highly tendentious accompanying footnotes. What did Newton claim in the *Commercium*? What's the thesis he strived to substantiate in this infamous collection of letters? These are questions historians are just beginning to ask themselves. Since it is far from obvious what one should understand by "discovering the calculus", these questions open some interesting historiographical issues. By reading the *Commercium* and its accompanying anonymous review, the "Account" ([Newton] 1715), one learns a great deal about Newton's point of view in the dispute with Leibniz: most notably, one learns what Newton thought about his own contribution to mathematics.

Two things should be emphasized. First, Newton did not depict himself as the discoverer of a new notation or of the rules of the "direct method of fluxions" (the differential calculus). The message that Newton apparently wished to deliver was that the notation and the rules of the direct method of fluxions were in essence already the possession of mathematicians to which both Newton and Leibniz were indebted: the names of James Gregory, Pierre de Fermat, René- François de Sluse, and Isaac Barrow are cited in this context (Guicciardini 2009, pp. 377–379).

Second, Newton claimed that his mathematical prowess and superiority over Leibniz consisted in having found new rules for the quadrature of figures via infinite series and in having applied these rules to the solution of the "higher problems" (see Figure 2). Briefly: in his confrontation with Leibniz, Newton played down the importance of notation and of the differential calculus; rather, he stressed the importance of "quadratures" (integration) and its applications to natural philosophy.[12]



(86)

est impræsentia literis transpositis consignare, ne propter alios idem obtinentes, institutum in aliquibus mutare cogerer. * 5accdæ10effh12i4l3m10n6oqqr7s11t10v3x: 11ab3cdd10eæg10ill4m7n6o3p3q6r5s11t7vx, 3acæ4egh6i4l4m5n8oq4r3s6t4v, aaddææeeeeeiiimmnnooprrrsssssttuu.

* 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 Seriei.* Analysin per Fluentes & earum Momenta in æquationibus tam infinitis quam finitis, *Newtonus* in his Epistolis ad Regulas quatuor reduxit. Per primam extrahitur Fluens ex Binomiis, adeoque ex æquationibus quibuscunque non affectis in Serie infinita, & Momentum fluentis simul prodit, quo evanescente Series in Æquationem finitam redit. Per secundam extrahitur Fluens ex æquationibus affectis Fluxionem non involventibus. Per tertiam extrahitur Fluens ex æquationibus affectis Fluxionem simul involventibus. Per quartam eruitur Fluens ex conditionibus Problematis. Regulæ duæ primæ in principio Epistolæ superioris, duæ ultimæ in fine hujus ponuntur. Harum Regularum *Newtonum* esse inventorem primum nemo dubitat.

Figure 2 In this footnote to the *Commercium* (Newton 1712, p. 86), in commenting on his rules for the squaring of curves via infinite series, Newton states: “no one doubts that Newton is the first discoverer of these rules.” This is the strongest claim of priority in the *Commercium* and might be taken as indicative of what was at stake for Newton in his confrontation with Leibniz. He communicated one of these rules to Leibniz in the *epistiola posterior* of October 24, 1676 (Newton 1959–1977, II, p. 134). This method of quadrature was proposed to Leibniz as the first of a series of theorems devised in order to simplify the “speculations concerning the squaring of curves”; it is thus known in the literature as the prime theorem (the “theorema primum”) on quadratures. It was communicated privately to John Craig in 1685 and thus reached David Gregory, who published it as his own discovery in Pitcairne (1688). Fatio communicated it to Huygens in the early 1690s. The manuscript annotation in Newton’s hand for this footnote is in Add. 3968.41: 33r (Cambridge University Library).

Newton’s Acolytes Defend Their Master

The position Newton upheld in the *Commercium* was a shared one within the circle of his closest acolytes. For example, Fatio de Duillier, in his correspondence with Huygens, had aroused the interest of the great Dutch polymath by communicating Newton’s new results on quadratures (see Figure 2), and his emendations and projects for a second edition of the *Principia.* The inverse method of fluxions applied to problems in natural philosophy was also the topic Fatio chose to address in the *Lineæ Brevissimi Descensus Investigatio Geometrica Duplex* (1699), which dealt—in terms of Newton’s fluxional techniques—with the brachistochrone problem and, in an appendix, with the solid of least resistance (a topic Newton broached in Book 2 of the *Principia*).[13] Fatio’s *Investigatio* also contained a rude accusation of plagiarism directed at Leibniz. This episode was dealt with diplomatically, and the case was soon silenced. Via Wallis, Leibniz received reassurance from the president of the Royal Society, Hans Sloane, that Fatio had obtained the imprimatur of the Royal Society by means of trickery (Hall 1980, p. 121).

The case of Abraham De Moivre can also be cited in this context. The Huguenot refugee entertained a rather tense correspondence with Johann Bernoulli concerning a formula for central force motion, a Newtonian contribution that appeared particularly promising for dealing with central forces in fluxional terms via the representation of the radius of curvature of the orbit (see Figure 3).

During the 1690s and the early 1700s, Newton and his acolytes were circulating an image of their mathematical contribution that gave pride of place to quadratures and applications of the fluxional method to natural philosophy.

Leibniz vs. Newton

Leibniz Reads the *Commercium*

When Leibniz managed to get his hands on the *Commercium*, he was simply mystified. In his opinion, the authors of the collection of letters had provided no evidence whatsoever of Newton's knowledge of calculus. By this he meant an efficient notation and an algorithm for the differential calculus. Leibniz held a completely different viewpoint compared to Newton's concerning the essence of his own contribution to mathematics. In an attempt to provide an answer to the *Commercium*, he wrote:

> They have changed the whole point of the controversy, for in their publication … one finds hardly anything about the differential calculus; instead every other page is made up of what they call infinite series…. This is certainly a useful discovery, for by it arithmetical approximations are extended to the analytical calculus; but it has nothing at all to do with the *differential calculus….* Since therefore his opponents, neither from the *Commercium* that they have published, nor from any other source brought forward the slightest bit of evidence whereby it might be established that his rival used this calculus before it was published by our friend; therefore all the things that they have reported may be rejected as extraneous to the matter. They have made recourse to the skill of pettifoggers with the purpose of diverting attention of judges from the matter on trial to other things, namely to infinite series.
>
> (Leibniz (1849–1863), V, pp. 393, 410, my emphasis)

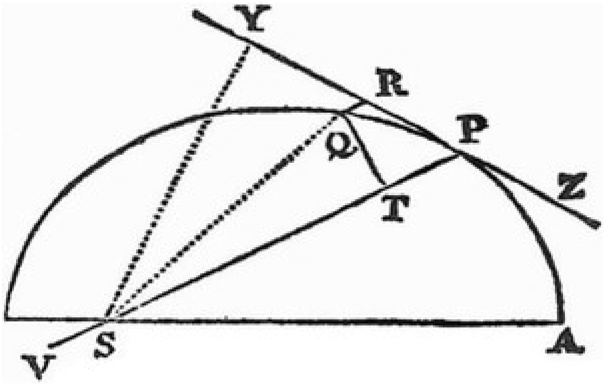


Figure 3 Diagram for Proposition 6, Book 1, of the *Principia*, 2d ed. (1713). At the turn of the century, the Newtonians had achieved a complete understanding of how central forces could be expressed in terms of fluxions (Guicciardini 1999, pp. 226–228). They employed the following representation for a central force F acting on a body located at P: $F \propto r/(p^3 \cdot \rho)$, where S is the centre of force, $r = SP$ is the radius vector, $p = SY$ is normal to the tangent PY at P, and ρ is the radius of curvature at P. This formula was systematically employed by Newton in the second edition of the *Principia*, but also appears sporadically in the first edition. The radius of curvature ρ, expressed in the coordinates r and p, is: $\rho = (r \cdot \dot{r})/\dot{p}$. De Moivre's formula—as it was sometimes called—allows an easy application of fluxions to central force motion, since $F \propto \dot{p}/(p^3 \cdot \dot{r})$. When De Moivre showed the formula to Newton, Newton replied that he had already obtained a similar formula, as De Moivre wrote to Johann Bernoulli (Newton 1967-1981, VI, p. 548) in 1705. In 1706, in a reply to De Moivre, Johann Bernoulli presented his own demonstration of the formula by publishing it, without acknowledging De Moivre's priority, in (Bernoulli 1710). Meanwhile, De Moivre had communicated his results to the Oxford group composed of Edmond Halley, David Gregory, and John Keill. David Gregory's own proof of De Moivre's result is in "Codex E" in Christ Church, Oxford MS 346 (manuscript dated 1707). Varignon had already published an equivalent result in (1701). In his (1708b) paper on central forces John Keill made use of this fluxional representation of central force.

The argument adopted in the *Commercium* struck Leibniz as simply a clever way to shift the attention to another topic—infinite series and quadratures—in order to avoid a fair confrontation. For Leibniz, whose greatest ambition was the construction of a *characteristica universalis* (a general algorithm capable of subjecting any reasoning to algebraic manipulations), it was obvious that the issues at stake in the "discovery of the calculus" were not the rules for squaring curves via infinite series Newton boasted about, but rather the notation and the rules for the differential calculus he had published first in 1684. One may surmise that what must have positively struck Leibniz about the differential calculus is that a whole class of problems concerning tangents, curvatures, and so on, had been reduced to an algorithm, to a symbolic method that could be applied blindly by manipulating symbols. The integral calculus was, of course, considered by him an important aspect of the new analytic method, but Leibniz was conscious that it had not been developed into a general method. There was no general algorithm for the integral calculus (and the situation has not changed since). In order to square or rectify curves, or in order to integrate differential equations, one had—and still has—to make recourse to a plurality of partial strategies in which the inventiveness of the algebraists plays an important role. The integral calculus was a craft, not an algorithm. As we shall see in the section " Johann Bernoulli Challenges the 'English Analysts'", this characteristic of the integral calculus that may have worried Leibniz was exactly what

Bernoulli needed in order to aggrandize himself, as he excelled in Europe precisely because of his skill in applying this craft. It was a craft that could be learned by corresponding with him, or reverently visiting him (as many did, including the Italian Giuseppe Verzaglia, the Genevan Gabriel Cramer, the English John Arnold, and the French Pierre de Maupertuis) in order to be schooled by exercise and imitation in techniques such as separation of variables or integrating factors.

Political Enmities

Leibniz was writing the above lines in Havover, in 1714 on his way back from Vienna, where he had spent two years in the service of the Emperor. The philosophical and political dimension of the debate between Leibniz and Newton should not escape our attention. They disagreed over issues concerning the foundations of the calculus: Newton was convinced of the superiority of his method of limits over Leibniz's mathematical practice where infinitesimals occurred.[14] They were divided over physical questions such as the nature of gravity, time and space, atoms and the void, as well as the issue of which quantities are properly conserved by the laws of nature. In short, they were not just mathematical opponents: they were philosophical enemies as well. Even in the political arena, in which both took on leading roles, their activities only served to fuel their intellectual disputes. Little wonder, then, that their purely mathematical differences could ignite so much fury (Bertoloni Meli 1993).

Newton had been elected to the Convention Parliament (1689). He was very close to the entourage of Charles Montagu (the 1st Earl of Halifax), to the Whigs who had promoted the Glorious Revolution. In 1696, he left Cambridge for London, where he became rich and powerful as Warden (1696) and Master (1700) of the Mint. In the meantime, Leibniz had served as a counsellor to the Emperor and to the Czar, and had long been in the service of the Dukes of Hanover. It was also thanks to Leibniz's diplomatic efforts that Duke Ernst August had gained the status of Elector. When his son acceded to the throne of Great Britain and Ireland in 1714 as George I, Leibniz planned to cross the English Channel. The prospect of having Leibniz—an experienced diplomat and towering metaphysician who actively pursued an ecumenical policy of reconciliation between the Christian Churches—in London in the capacity of "Historiographer of Great Britain" must have been a daunting one for Newton's party, which rather favored anti-Catholicism and an interpretation of the Anglican faith difficult to reconcile with Lutheranism. After Sophia's death in June 1714, Leibniz had the Princess of Wales initially on his side in London. But the Newtonian entourage (most notably, Samuel Clarke) could divert Caroline's sympathies. Leibniz was not allowed by the King—who, unlike his mother, Sophia, was never on good terms with his polymath courtier—to join the English court and died in bitter isolation in 1716. The Leibniz-Clarke correspondence has its origin in this context, and the calculus controversy too is intertwined with these manoeuvres at court (Bertoloni Meli 2002). Leibniz was certainly aware of the political dimension of his polemic with Newton. The public exchange of letters between Leibniz and Clarke indeed began with Leibniz expressing his doubts to Caroline about the religious orthodoxy of the Newtonians, which he vaguely, at some point, identified as Socinians (Bertoloni Meli

1999, 486). In his correspondence with Thomas Burnet, Leibniz shared the conjecture that the members of the Royal Society committee had treated him badly because of their Tory sympathies (Hall 1980, p. 166). He was, of course, wrong as far as Clarke and Newton were concerned, yet the group Keill belonged to had indeed Tory leanings: Leibniz's political radar was not entirely malfunctioning.

Johann Bernoulli Suggests a Strategy to Leibniz

The Basel mathematician Johann Bernoulli was soon to play a major role in the confrontation between Newton and Leibniz. He was one of the few mathematicians in Europe who could measure up to the two main combatants. As we shall see, he provided Leibniz with mathematical expertise that proved crucial in contesting Newton's claims in the *Commercium*. However, it would be reductive to depict him as Leibniz's watchdog. His ambitions were much higher than that. As we shall see in the section "Bernoulli Claims the Integral Calculus", Bernoulli's claims concerning the originality and importance of his mathematical contributions were immense and expressed in a way that even challenged Leibniz's supremacy in the field of integration.

Born into a family faithful to the principles of the Reformed Church, Bernoulli got embroiled in theological issues when he was teaching mathematics in Groningen (1695–1705). However, in his subsequent dealings with other members of the Republic of Letters, he mostly presented himself as a professional mathematician who could promote his expertise in the new calculus in different political and religious milieux. Even though his correspondence is sometimes colored with Protestant rhetoric, he was consistent in bracketing philosophical and theological issues out of the professional interests of the mathematician. This does not mean that natural philosophy was extraneous to Bernoulli's intellectual biography. On the contrary, his interests spanned from medicine to cosmology, including fields like mechanics, hydrodynamics, ship manoeuvring, barometric light, and optics. Nevertheless, Bernoulli was inclined to bracket these interests out when he practiced mathematics, and by doing so he was, in effect, defining a new professional role for the mathematician.[15]

Copies of the *Commericum Epistolicum* were distributed free of cost in the early months of 1713. Bernoulli's nephew, Nicolaus I, carried a copy to Basel in the spring, and Johann, after consulting it, wrote a momentous letter to Leibniz, dated June 7, 1713 (Newton 1959–1977, VI, pp. 1–3). In this letter, Bernoulli maintained that in the *Commercium Epistolicum* there was evidence of Newton's research on series, but no evidence of his advancements in the calculus. Further, Bernoulli continued, there was no trace of dotted notation in the letters edited in the *Commercium Epistolicum* and indeed no trace of this notation in the *Principia*, "where he [Newton] must have had so many occasions for using his calculus of fluxions." This would be a clear proof that Newton had discovered the calculus after the publication of the Leibnizian calculus in 1684.

Bernoulli continued his letter by pointing out that Newton did not know "the true way of differentiating differentials." In short: Newton, in his opinion, did not know higher-order differentials. The evidence, in his opinion came from a mistake Newton had made in Proposition 10, Book 2, of the *Principia*. Bernoulli claimed that the mistake he had spotted was caused by the following: when Newton applied the binomial theorem obtaining

(x+o)n=x+n1xn−1o+n(n−1)1•2xn−2o2+n(n−1)(n−2)1•2•3xn−3o3+…,

he erroneously thought that the second differential of x^n was equal to n(n−1)1•2xn−2, that the third differential was equal to n(n−1)(n−2)1•2•3xn−3, and so on. This interpretation, which appears an absurdity to anyone knowledgeable about Newton's mathematics, was sincerely believed to be true by Johann and Nicolaus I Bernoulli. They found confirmation in the Scholium ending the *Tractatus de quadratura curvarum* (which was appended both to the English (1704) and to the first Latin translation (1706) of the *Opticks*), where one could read the statement that the third term of the above series "will be its [of x^n] second increment or second difference", the fourth "its third increment or third difference" (Newton 1704, p. 207).

After receiving Bernoulli's incendiary letter, Leibniz circulated a flysheet, the notorious *Charta volans*, dated July 29, 1713 (Newton 1959–1977, VI, pp. 15–17). This was his anonymous reply to the *Commercium epistolicum*. It included the judgement of a "leading mathematician most skilled in these matters and free from bias." The judgement, of course, was a slightly edited version of Bernoulli's technical critique of Newton's supposed misunderstandings of higher-order infinitesimals evinced by Proposition 10, Book 2, of the *Principia*, and from the Scholium to *De quadratura*.[16]

Johann Bernoulli Challenges the "English Analysts"

Keill's and Taylor's Challenges

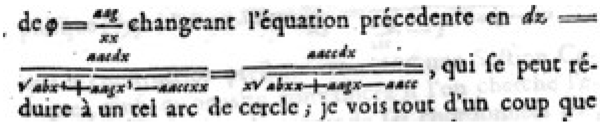
After the publication of the *Commercium* and Leibniz's reply to it, the *Charta volans*, the polemic continued with an intense exchange of papers mostly published in the *Journal Littéraire* and the *Acta Eruditorum.* As already noted, Wolff criticized James and John Keill's theory of interparticulate forces, a theory that extended Newton's controversial conception of action at a distance to the level of microscopic phenomena.[17] In the years 1713–1720, John Keill conducted a violent campaign to defend Newton against Johann Bernoulli's systematic criticism of the mathematical methods employed in the *Principia.* He was backed by Brook Taylor, Abraham De Moivre, and Newton himself (most notably, Keill 1714, 1716, 1719; Taylor 1719).

The controversy between Bernoulli and the British was fought mostly behind the scenes, via correspondence and private conversations. When it emerged in print, it was often in a disguised form: through the circulation of fly-sheets as well as anonymous papers. These were often signed by pupils but drafted by their masters, and appeared in several journals, whose editors were far from unbiased. The *Acta Eruditorum* was, of course, in the hands of the Leibnizian party, and Bernoulli corresponded with Johann Burkhard Mencke extensively about his remonstrations against Taylor and Keill, whereas Willem ‘s Gravesande most probably was responsible for the philo-Newtonian role played by the *Journal Littéraire.*

The debate came close to its end in 1717–1718, when Taylor and Keill, via the intermediation of Pierre Remond de Montmort, confronted Bernoulli with two challenges. The first concerned an integral of an irrational function that was found in Cotes’s Nachlass.[18] The second challenge (the ballistic problem) required the determination of the trajectory traversed by a projectile fired close to the earth’s surface and assuming a resistance proportional to the square of the speed. Bernoulli not only answered both challenges, but his (and Nicolaus I’s) solutions to the ballistic problem were clearly superior to those (in fact, nonexistent) of the two British mathematicians (J. Bernoulli 1719, 1721; N. Bernoulli 1719).

The Inverse Problem of Central Forces

Since 1710, Bernoulli had noted (in Bernoulli 1710) that in the *Principia* a proof that conic sections are necessary orbits for a body accelerated by an inverse-square central force is lacking. Of course, the so-called inverse problem of central forces is a fundamental one for Newton’s gravitation theory. What Newton proved in Section 3, Book 1, is that if the orbit is a conic section and the force is directed toward one focus, then the force is inverse-square. A proof of the converse is lacking in the first edition of the magnum opus. Bernoulli and Jacob Hermann (a pupil of Jacob Bernoulli’s then employed by the University of Padua) achieved a proof by integrating the pertinent differential equation (see Figure 4) (Bernoulli 1710, Hermann 1710). Bernoulli’s equation was derived from the study of central-force motion provided by Newton in Proposition 41, Book 1, as Keill did not fail to remark. It is interesting to remark that in the Corollary 3 to this proposition Newton provided a geometrical construction of the orbits traversed by a body accelerated by an inverse-cube force. As he explained to David Gregory, he had obtained this construction by a calculus procedure that closely resembles that deployed by Bernoulli for the inverse-square case. The application of quadratures (integration) to central force motion was not that foreign to Newton (Guicciardini 2016).



$d\varphi = \frac{aag}{xx}$ changeant l'équation précedente en $dz = \frac{aacdx}{\sqrt{abx^4 + aagx^3 - aaccxx}} = \frac{aacdx}{x\sqrt{abxx + aagx - aacc}}$, qui se peut réduire à un tel arc de cercle ; je vois tout d'un coup que

Figure 4 Detail from Bernoulli (1710), p. 539. Translating into algebraic symbols Proposition 41, Book 1, of the *Principia*, Johann Bernoulli obtained the differential equation in polar coordinates for central-force motion. Today, we are accustomed to writing polar coordinates with the symbols r and θ, whereas Bernoulli's coordinates are $x = r$ and $z/a = \theta$ (a constant). If we introduce this more familiar notation, taking l to be the angular momentum and E the energy, then we can rewrite Bernoulli's equation as follows: dθ=ldr/2Er4+2r4∫r∞Fdr−l2r2. For an inverse square force $\phi = a^2g/x^2$, (where g is a constant) Bernoulli obtained the following differential equation: dz=(a2cdx)/(xabx2+a2gx−a2c2), which he could integrate by substituting variables x=a2/y, and y=a2g/2c2−w. The integral, of course, is the equation for conic sections in polar coordinates. It should be noted that in the eighteenth century no notation for vectors was known. Mathematicians (such as Johann Bernoulli, Leonhard Euler, or Joseph-Louis Lagrange) who applied calculus to analytic mechanics, by suitably choosing the components of motion, obtained scalar differential equations.

Bernoulli's statement that a solution of the inverse problem was lacking in the *Principia* was a devastating one. Keill's replies—engineered with Newton's help—are highly interesting, since they reveal something about the values and aims adopted by Newton and his acolytes and how these values contrast with those pursued by Bernoulli. Keill stated that Bernoulli's integration was long and inelegant compared to Newton's geometric approach expounded in Corollary 1 to Proposition 13 (expanded in the second edition, 1713) and Proposition 17, Book 1 (see Figure 5) (Keill 1714, 1716, 1719). Newton had assumed the solution as given (i.e., that the orbit was a conic) and then had shown how the parameters of the conic orbit could be geometrically determined as a function of initial conditions (position and velocity). While Keill stressed that the aim Newton had set himself was the construction of an orbit whose nature was assumed as given, Bernoulli replied that only the integration of a differential equation provides a method that does not assume the solution sought for as given and that can be generalized to other force laws ([Bernoulli] 1716) (see Guicciardini, 1995).

The Ballistic Problem

Proposition 10, Book 2, was another crux of the *Principia* that was bitterly debated between Keill, Taylor and Bernoulli. In this proposition, Newton dealt with the motion of a projectile acted on by constant gravity and moving in a rare fluid exerting a resistance proportional to the square of the speed. Johann and Nicolaus I Bernoulli were working hard in order to find mistakes in the *Principia*. A joint paper was soon submitted to the Paris Academy: in the main paper by Johann, a mistake in Newton's proposition was noted and an alternative calculus solution was given; in the appendix by Nicolaus I, it was stated that this mistake (as we have seen in the section "Johann Bernoulli Suggests a Strategy to Leibniz") was due to Newton's misunderstanding of the meaning of the higher-order infinitesimals occurring in the coefficients of a Taylor series (Bernoulli 1711). Johann's integration of the differential equation for motion in resisting media made full use of the techniques in the integral calculus developed some ten years before by his brother Jacob and himself (see Figure 6).

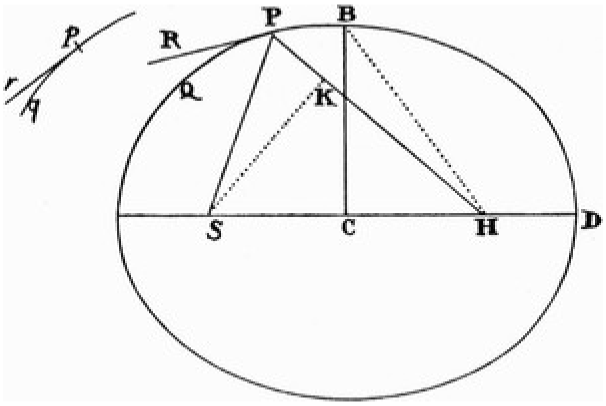


Figure 5 Diagram for Proposition 17, Book 1, of the *Principia* (1687). Newton considers a body of a given mass accelerated by a centripetal inverse square force directed toward S. The body is fired at *P*, in the direction *PR* with a given initial speed. Newton assumes that the orbit is a conic section and that one of the foci is located at the force center *S*. He determines the unique conic that satisfies the given initial conditions (position and velocity). This is a geometric constructive procedure that allows Newton to determine for any initial conditions the conic that satisfies the equations of motion. Since for any initial conditions such a conic can be constructed, and since for any initial conditions the orbit is unique, Newton concluded that conics are necessary orbits for an inverse-square force. The condition of uniqueness was assumed as intuitively true: that is, it was taken for granted that if a body is fired from a given place with a given velocity, only one orbit is possible. Newton and his contemporaries were used to tackling geometrical problems through the method of analysis, in which one assumes the sought-for results as given. Bernoulli accepted the cogency of Newton's reasoning, but favored what he called an "a priori" approach in terms of differential equations: in this case—Bernoulli contended—the solution is found without assuming that the orbits are conics. Further, Bernoulli added, the approach in terms of differential equations can be generalized to other force laws ([Bernoulli] (1716)).

The dramatic succession of events related to Johann Bernoulli's discovery of a mistake in Proposition 10 has been described many times (Newton 1967–1981, VIII, p. 48ff). In September 1712, Nicolaus I Bernoulli, Johann's nephew, arrived in London. He met Newton and informed him that Johann had detected a mistake in Proposition 10. Newton recognized immediately that Nicolaus I was right. He was busy preparing the second edition of the *Principia.* Since Roger Cotes had not noticed any error in Proposition 10, the pages with the unaltered 1687 version were already printed. Newton worked strenuously in order to reach an understanding of his mistake and produce a correct demonstration. Contrary to what the Bernoullis thought, his mistake was not related to a lack of understanding of the Taylor coefficients. Newton's mistake consisted in equating two infinitesimal lengths which differ by a third-order differential: it was a mistake with the geometry of higher-order infinitesimals, not with the algebra of the Taylor coefficients.

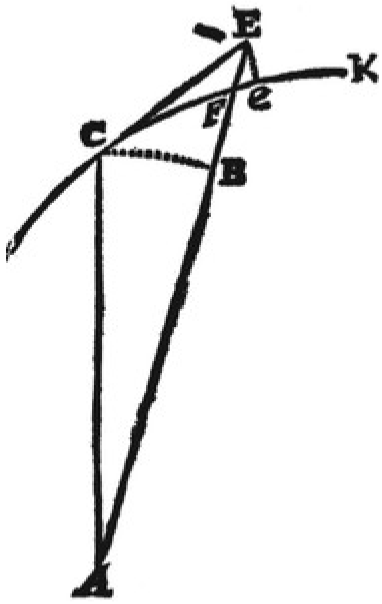


Figure 6 Diagram from Bernoulli (1711), 48. In 1711, Johann Bernoulli studied the motion of a body (the mass of which is assumed unitary, $m = 1$) acted upon by a central force F directed toward A and moving in a medium exerting a resistance R proportional to some power of the speed v. In order to write the differential equation of motion, Bernoulli decomposed the central force into a tangential F_T and a normal F_N component (the former directed along the tangent CE, the latter along the orthogonal—not so well rendered by the engraver—direction Ee). As noted in Figure 4, such decompositions allowed eighteenth-century mathematicians to write scalar equations of motion (with no need for vector magnitudes). For the normal component the equation is $FN=\nu 2/\rho$, since no resistance acts normally to the direction of motion. For the tangential component Bernoulli wrote $-d\nu=FTdt\pm Rdt$. For a resistance proportional to a power of the speed, $R=\varsigma\nu n$, the resulting nonlinear equation (still called Bernoulli equation) could be integrated by seeking a solution in the form of the product of two unknown functions $v = m(r)n(r)$. This powerful integration technique gives a measure of the excellency of the Basel school in the theory of differential equations. For details see Guicciardini (2013), pp. 257–259.

Keill replied to the Bernoullis' attack concerning Proposition 10 by attempting to prove that it was Leibniz who had made mistakes with higher-order infinitesimals in the *Tentamen de motuum coelestium causis* (1689). Further, Keill inflamed Newton's anger by insinuating in his correspondence with the President of the Royal Society that Leibniz's mathematical treatment of planetary motions in the *Tentamen* was just the result of the skillful German philosopher's reverse engineering of the *Principia*. Leibniz always denied that he had read the *Principia* before writing his essay in which he tried to offer a mathematical treatment of the planetary system based not on distant-action gravitation, but on the contact-action of a planetary vortex. Nico Bertoloni Meli has proven that Keill and Newton were indeed right in being suspicious (Bertoloni Meli 1993). It was important for them to prove that Leibniz—whom they considered just a plagiarist—had got the mathematics wrong.[19] Leaving the technical details of this dispute aside, it is certainly noteworthy that so much interest was shown in the techniques for handling higher-order infinitesimals. The ability to deal with second- and third-order infinitesimals was perceived by both parties as a way to identify those who were in possession of the "new" method. The moral of this debate is that geometrical intuition can be a source of errors in dealing with higher (especially higher than second-order) infinitesimals (see Figure 7). The control of the level of approximation guaranteed by algebraic techniques in this regard suggests that geometry was not after all as powerful as the Newtonians often liked to claim.

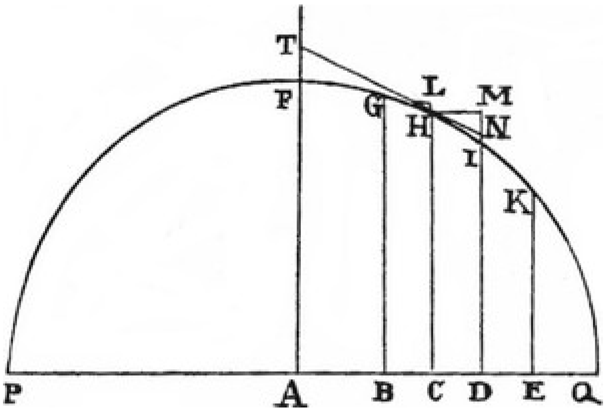


Figure 7 Diagram for Proposition 10, Book 2, of the *Principia*, 2d ed. (1713). In the study of projectile motion in resisting media, Newton needed to take into consideration third-order infinitesimal elements of the trajectory. In the corrected version of this proposition (1713), he subdivided the abscissa into equal intervals $BC = CD = DE = o$. The infinitesimal arcs GH and HI, are first-order infinitesimals. The deviation from inertial motion NI is a higher-order infinitesimal. In his study of central motion in a void (Book 1), a second-order approximation of NI was sufficient, thus any curve that makes second-order contact with the trajectory (for example, the osculating circle) can be substituted for the actual trajectory. Such an approximation can be achieved geometrically (e.g., by constructing the osculating circle). In the case of motion in a resisting medium, the approximation must be pushed to third-order, and it is unclear how to proceed by geometrical means in this case. Indeed, Newton's geometrical intuition (in the 1687 edition) was—in this particular instance—faulty. In the second edition (1713), Newton carefully re-expressed all the infinitesimal elements of the trajectory in terms of Taylor series truncated to third order in o^3.

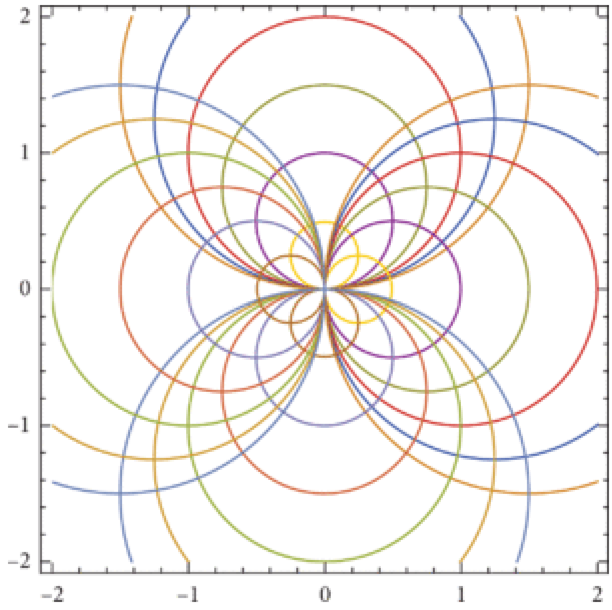


Figure 8 Two families of orthogonal trajectories, corresponding to equations (i) $y^2 + x^2 = my$, and (ii) $x^2 + y^2 = cx$. In general, a one-parameter family of plane curves will be expressed by $f(x, y, a) = 0$. A general method for finding orthogonal trajectories was sought in the first decades of the eighteenth century. Leibniz, Jacob and Johann Bernoulli, but especially Nicolaus I Bernoulli and Alexis Fontaine des Bertins, contributed important results, in which the concept of multivariate function and partial derivative were developed. See Engelsman (1984) and Greenberg (1995).

Orthogonal Trajectories

Equally instructive for the historian of mathematics is the debate on orthogonal trajectories, since it throws into relief a difference between the British and the Continental algorithmic practices that is seldom appreciated. This is the topic Leibniz chose to propose as a challenge in a letter to Antonio Schinella Conti dated November 25, 1715, after having received some suggestions from Johann Bernoulli, in order to "feel the pulse of our English analysts" (Newton 1959–1977, VI, p. 253). The challenge Leibniz posed to the English mathematician was to find the plane curves that intersect at right angles all of the members of a given one-parameter family of curves that lie in the same plane (see Figure 8). The problem was understood as a way to compare the Newtonian and the Leibnizian versions of the calculus in relation to a particularly difficult problem. The interesting feature of this problem is that a family of curves is defined by a functional relationship in which three variables occur: the abscissa x, the ordinate y, and one parameter a. Basically, in order to deal with the orthogonal trajectories problem, one needs to differentiate both as a function of a coordinate, x, and as a function of the parameter a (this was called "differentiating from curve to curve"). Some prototype notions of a function in more variables and of partial derivative are necessary: these notions can be discerned in the works on orthogonal trajectories by Leibniz, Jacob and

Johann Bernoulli, and Jacob Hermann. The attempts to solve this problem are a chapter of the discovery of partial differential calculus, a technique that remained largely untouched by British mathematicians until the work of Edward Waring in the second half of the eighteenth century. Leibniz and Bernoulli had indeed chosen a problem that was quite hard for Newton and his followers.

Bernoulli Claims the Integral Calculus

In 1716, Johann Bernoulli carried out a systematic attack against Keill and Taylor in a long anonymous paper entitled "Epistola pro Eminente Mathematico" ([Bernoulli] (1716). The fact that at some point the phrase "meam formulam" was used to refer to a formula occurring in one of Bernoulli's papers, thus revealing that the "eminent mathematician" was Bernoulli himself, was to arouse as much hilarity in the Republic of Letters as it did embarrassment in Basel.

In the opening lines of the "Epistola", Bernoulli claimed the discovery of the integral calculus for himself. A better example cannot be found to show that Bernoulli used the polemic between Newton and Leibniz in order to aggrandize (not without reason) himself, rather than simply defend Leibniz. He wrote:

> He who assures you, famous Sir, that Mr John Bernoulli gained the invention of the integral calculus by his own efforts, speaks nothing but the truth; especially if we mean to distinguish this calculus from the differential calculus which even according to Bernoulli himself is, beyond all controversy, owed entirely to the great Leibniz.
>
> (Newton 1959–1977, VI, pp. 302–303)

Bernoulli's opinions concerning the issue at stake in the calculus controversy constitute a third position within the debate, which can be identified neither with Leibniz's nor with Newton's. While Leibniz insisted on the algorithm and rules of the differential calculus, and Newton on quadratures via infinite series, Bernoulli claimed the discovery of the integral calculus. The sequel of the "Epistola" allows one to appreciate Bernoulli's claims in finer detail. In reviewing his polemic with Keill and Taylor, Bernoulli insisted on the generality of his integration methods, on the use of transcendental curves to come up with solutions, on the techniques aimed at finding integrals in closed form rather than as infinite series approximations, on his ability to deal with higher-order infinitesimals and to differentiate from curve to curve. These features of calculus, which Bernoulli eulogized in the "Epistola", were to dominate the scene of European mathematics in the eighteenth century.

The European Reception

The position upheld by Johann Bernoulli in his polemic with Keill and Taylor was echoed by many eighteenth-century European mathematicians. The evaluations of two French mathematicians, Pierre Rémond de Montmort (Feigenbaum 1992) and Pierre-Louis Moreau de Maupertuis (Terrall 2002), can be cited, in conclusion, as examples of the successful reception on the Continent of the techniques elaborated by Leibniz and by the Bernoullis and their acolytes (most notably Hermann and Euler) in Basel.

In 1718, Montmort, a correspondent of Nicolaus I Bernoulli's and an eminent figure on account of his contributions to the calculus of probability, in writing to his friend Brook Taylor, summarized the results of Leibniz and the brothers Johann and Jacob Bernoulli as follows:

> [I]t is untenable to say that Leibniz and the brothers Bernoulli are not the true and almost unique promoters of these calculi [the differential and the integral calculus]…. It was they who first expressed mechanical curves by means of equations, who taught us to separate the variables in differential equations, to reduce their dimensions, and to construct them by means of logarithms, or by means of rectification of curves when that is possible; and who by pretty and numerous applications of these calculi to the most difficult problems of mechanics, such as those of the catenaria, the sail, the elastic, the curve of quickest descent, the paracentric, have put us and our successors on the path to the most profound discoveries. Those are facts not to be contradicted. To convince oneself of them it suffices to open the journals of Leipzig [i.e., the *Acta Eruditorum*].
>
> (Newton 1959–1977, VII, pp. 21–22)

These long lists of results attributed to the Basel school and Leibniz are often to be found in Johann Bernoulli's correspondence.[20] Yet, such perorations—convincing as they may sound to us with the benefit of hindsight—did not impress the mathematicians belonging to the core group of Newton's acolytes. For example, John Keill, while comparing his researches on central forces and matter theory to Bernoulli's mathematical results, stated: "what I have demonstrated are more useful matters for the understanding of [natural] philosophy compared to all the great discoveries that you [Bernoulli] have made" (Keill (1719), p. 285). During his challenge to Bernoulli via Montmort), Keill restated the same concept: he lampooned Bernoulli for having "a particular genius adapted for trifles, the only part of Newton's philosophy which is of no moment and which signifies nothing to explain the phenomena of nature, he [Bernoulli] has most diligently studied and examined."[21] Yet, for the Leibnizians these problems—notwithstanding their weak significance for Newtonian natural philosophy—were important for a reason that escaped the Newtonians: they were exercises that proved tremendously useful for the development of integration, in particular the theory of differential equations.

It is also worth noting one significant difference between the study in integration pursued by Jacob and Johann Bernoulli and their pupils, Hermann and Euler, and that pursued by Newton, Cotes, and Taylor: the Newtonians preferred integration via infinite series to integration in closed form.[22] Most of Newton's methods of quadrature (with the notable exception of the tables included in *De quadratura* (1704)) were in terms of series expansions. His methods of quadrature celebrated by Fatio and Gregory and in the

Commercium Epistolicum (see Figure 2), provide only a local solution (they can be calculated in the interval of convergence of the series solution).

An elementary example of a Newtonian solution of a fluxional (i.e., differential) equation can help explain this feature (Giusti 2007, pp. 45–46). Let us consider the fluxional equation $\dot{y}=1-3x+y+x2+xy$, with initial condition $y(0) = 0$. An approximation in the vicinity of the origin is $\dot{y} = 1$ (we retain only terms of zero-degree on the right-hand side). Squaring (i.e., integrating), we obtain $y = x$. Substituting this value in the original equation and retaining only terms of first-degree on the right-hand side, we get an approximation in the vicinity of the origin: $\dot{y} = 1 - 2x$. Squaring, we obtain $y = x - x^2$. Substituting this value in the original equation and retaining only terms of second-degree on the right-hand side, we get an approximation in the vicinity of the origin: $\dot{y} = 1 - 2x + x^2$. Squaring, we obtain $y = x - x^2 + x^3/3$. This process can be reiterated so that one stepwise obtains the higher-order terms of the series. Such a method allows a numerical approximation of the solution in the interval of convergence of the series, in this case in the vicinity of the origin, but does not deliver any information on the global features of the solution (in modern terms, we can approximate the graph in the vicinity of the origin).

Leibniz and the Basel mathematicians instead searched for integrations of ordinary differential equations in closed form, in terms of elementary or transcendental curves. Their program proved to be fruitful. The modernized example of the solution of the catenaria problem can illustrate the advantage of closed solutions over series expansions (see Figure 1). The solution obtained, $y(x)=K+c\cosh((x-x0)/c)$, provides information on the global character of the solution (in modern terms: we can plot a graph over the *R*-axis). Another example might be the solution of the differential equation for damped oscillations. Getting a solution in the form $y(t)=A\exp{-\zeta\omega0t}\sin(1-\zeta2\ \omega0t+\phi)$ provides information on the global characteristics of the solution (its initial amplitude A and phase ϕ, the undamped angular frequency ω_0, the damping ratio ζ, the decay time $\tau = 1/(\zeta\omega_0)$). These solutions, of course, emerged much later, roughly at the end of the eighteenth century, when mathematicians learnt to express solutions of differential equations in terms of functions (rather than constructions, as was still the case with Leibniz and Bernoulli), and when they began using dimensional constants endowed with a physical meaning.

The difference between the Newtonian and the Leibnizian programs in integration is correlated with the difference in the programs in mechanics they investigated. Newton was exploring the mathematization of mechanical problems of unthinkable difficulty for his age (from the study of planetary perturbations to that of tidal motions). The mathematical models he required were non-integrable in closed form: the best he could do was to devise numerical approximations, and his integration techniques via infinite series provided just that. One might note, for example, that the important problem of central force motion can be integrated in closed form in terms of elementary (circular and hyperbolic) functions for only a handful of force laws ($F = kr^n$, for n = 1, –2, –3), exactly those covered by Newton in the *Principia*.[23] The Newtonians were interested in integration via infinite series because in most cases this was the only viable choice, given the imperfect knowledge of integration techniques at the time. Leibniz and the Basel mathematicians, instead, devoted their attention to problems (such as the brachistochrone, the catenaria, and orthogonal trajectories) that—as we have just seen—were considered rather dull questions to ask in Newton's milieu; yet they led to interesting problems in integration in closed form. This difference between the Newtonian and the Leibnizian integration methods was emphasized by one of Johann Bernoulli's most gifted pupils, Maupertuis, who in 1731 wrote:

> It is true that this method of series that we owe to M. Newton is general, and the only absolutely general method that the integral calculus has; but it is also true that the solutions gotten by using it are very far from the elegance of the solutions found by integration or quadratures; one should only consider it as the last resort in the hopeless cases.[24]
>
> (Greenberg 1995, p. 253)

Conclusion

The dispute between Newton and Leibniz, which began in the murky philosophical and theological waters agitated by the Hanoverian succession, developed into a bitter confrontation between Keill, Taylor, and De Moivre on one side of the English Channel, and Johann Bernoulli, Nicolaus I Bernoulli, and Hermann on the other—to mention just the main actors. We know quite a great deal about the events surrounding the calculus priority controversy, the making of the *Commercium*, and how the controversy developed into a wide-ranging philosophical confrontation between Newton and Leibniz (Hall 1980; Bertoloni Meli 1993). The debate between the younger generation of mathematicians is known to historians of mathematics, but it is still an open field of research. In the concluding sections of this chapter, I have tried to render the mathematical content of the debate between Johann Bernoulli and some British mathematicians, such as Keill and Taylor, accessible to the general reader. It is an important but much-neglected debate in which we witness the birth of mathematical techniques and programs in the mathematization of mechanics that rendered obsolete the quadrature methods employed (often implicitly) by Newton in the *Principia* and expounded in *De quadratura.* The theory of ordinary and partial differential equations, an essential tool for the development of rational mechanics, became the specialty of Continental mathematicians. It is this progress in calculus and mechanics that allowed the mathematicians active in the mid-eighteenth century, such as Alexis-Claude Clairaut, Jean-Baptiste Le Rond D'Alembert, and Euler, to more forcefully tackle some of the problems, such as the determination of the moon's motion and the shape of the earth, which had been left open by Newton's magnum opus (Blay 1992; Truesdell 1960).

Author Note

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Notes:

([1]) Classic works on the subject include Hall (1980) and Derek Tom Whiteside, "Introduction" in Newton (1967–1981), VIII, pp. 469–538.

([2]) Transl. in Hall (1980), 145. Keill was referring to the letters and material provided by Newton that Wallis had published in volumes 2 and 3 of his *Opera*. Most notably, the full text of the two *epistolæ* that Newton had addressed to Oldenburg for Leibniz in 1676 was printed in the third volume (Wallis 1693–1699, III, pp. 622–629, 634–645).

([3]) Today this problem is usually called the "direct" problem of central forces: to determine the motion of a particle under the influence of a single central force.

([4]) "Notæ in Newtoni Principia Mathematica Philosophiæ Naturalis." Royal Society Library (London).

([5]) This openness toward Continental influences can be traced back to James Gregory, whose mother was an Anderson, a family that comprised several mathematicians—one of whom had been an assistant to François Viète. Further, James had studied in Padua under Stafano degli Angeli, a pupil of Bonaventura Cavalieri.

([6]) “Gregory’s” method of quadrature was also printed in Wallis 1693–1699, II, pp. 337–380. On this episode, see the commentary by Whiteside in Newton 1967–1981, VII, pp. 3–13.

([7]) This project fell through, even though a calculus treatment of two propositions of the *Principia* was published in an appendix to Motte’s English translation (1729). See Newton (1726/1972) for changes to the *Principia* through its three editions; Newton (1999) is now the standard English translation.

([8]) “Isaaci Newtoni Methodus Fluxionum Ubi Calculus Differentialis Leibnitij, et Methodus Tangentium Barowij Explicantur, et Exemplis Quamplurimis Omnis Generis Illustrantur” (Christ Church, Oxford).

([9]) Johann Bernoulli’s, Leibniz’s, and Huygens’s solutions appeared in *Acta Eruditorum* (1691), pp. 274–276, 277–281, and 281–282.

([10]) Gregory did not realize that it is the difference between tensions applied on each endpoint of an infinitesimal element of the chain that balances its gravity, thus guaranteeing equilibrium. This mistake notwithstanding, after writing the correct equation, Gregory dealt with its integration faultlessly. See Newton (1967–1981), V, p. 522.

([11]) As demonstrated in Hall (1980), this accusation was unjust.

([12]) In Newton’s *epistola posterior* the first theorem is expressed as follows: “For any curve let dzθ×(e+fzη)λ be the ordinate, standing normal at the end of *z* of the abscissa or the base, where the letters *d, e, f* denote any given quantities [N.B *d* is a constant!], and *θ, η, λ* are the indices [N. B. they can be rational numbers] of the powers of the quantities to which they are attached. Put (θ+1)/η=r, λ+r=s, (d/(ηf))×(e+fzn)λ+1=Q,rη−η=π then the area of the curve will be Q×{zπs−r−1s−1×eAfzη+r−2s−2×eBfzη+r−3s−3×eCfzη+r−4s−4×eDfzη, etc} the letters *A, B, C, D*, etc., denoting the terms immediately preceding; that is *A* the term z^{π} */s*, *B* the term –(r−1)/(s−1)×(eA)/(fzη),etc. This series, when *r* is a fraction or a negative number, is continued to infinity; but when *r* is positive and integral it is continued only to as many terms as there are units in *r* itself; and so it exhibits the geometrical squaring of the curve.” (Newton 1959–1977, II, p. 134).

([13]) The brachistochrone problem requires to find the shape of the curve down which a body sliding from rest and accelerated by gravity will slip (without friction) from one point to another in the least time. The solid of least resistance is the solid of revolution that experiences the least resistance in moving through a medium.

([14]) Newton developed a theory of limits that he first published as the “method of first and ultimate ratios” in Section 1, Book 1, of the *Principia*. Leibniz, notwithstanding his practice, in which differentials were freely deployed, had developed a rather deep understanding of limiting procedures. See Leibniz (1993).

([15]) “Barometric light” is emitted when a mercury-filled barometer tube is shaken. Bernoulli studied the phenomenon while in Groningen. Newton was much interested in this and promoted Francis Hauksbee’s experiments.

([16]) The *Charta volans* was printed and circulated by Wolff in the form of a fly-sheet. A French translation of it also appeared in print in *Journal literaire* (1713), pp. 448–53 and in *Nouvelles Literaires* (1715), pp. 413–414; a Latin one in *Acta Eruditorum 19* (1713), pp. 591–594.

([17]) It should be noted that the Oxford group Keill belonged to had been in possession of a copy of Newton’s “De natura acidorum” since 1692. The opposition between Wolff and the iatro-mathematicians inspired by Pitcairne was spelled out in his review of Freind (1710) (Rowlinson 2007); Clark 1999).

([18]) Cotes’s work on integration was actually superb, and the British were right in stressing its importance. The challenge concerned the integration of y=∫x(δλ)q−1/(e+fxq+gz2q)dx, where δ is a positive or negative integer, and λ a power of 2 (Gowing 1983, pp. 75–79). Replies came from Johann Bernoulli in (1719) and Jacob Hermann in (1719). Hermann’s paper also included a solution of the ballistic problem.

([19]) One should note that, even though the physics in the *Tentamen* is problematic, to say the least, Leibniz obtained a beautiful differential equation for radial acceleration.

([20]) See, for example, Johann Bernulli to Leibniz (July 29, 1713) in Leibniz (1849–1863), *III*(2), p. 916.

([21]) Universitätsbibliothek (Basel) L Ia 665, Nr.16*.

([22]) This should be read as a tendency, not as a rule. It is true that Cotes and Colin Maclaurin, for example, contributed to techniques of integration in closed form. Of course, series solutions were studied also in the Bernoullis’ school.

([23]) In propositions I.10, I.11–13, I.41 (Cor. 3). For a limited number of other exponents n, closed solutions in terms of elliptic functions (not available to Newton’s contemporaries) are possible.

([24]) Maupertuis’s statement remained unpublished (it can be found in the minutes of the Academy of Sciences), yet it must have reached Jean E. Montucla, who included it in Montucla (1799–1802, III, p. 165).

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