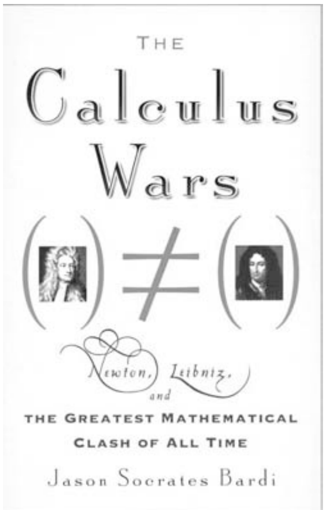
Book Review



The Calculus Wars

Reviewed by Brian E. Blank

The Calculus Wars: Newton, Leibniz, and the Greatest Mathematical Clash of All Time
Jason Socrates Bardi
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According to a consensus that has not been seriously challenged in nearly a century, Gottfried Wilhelm Leibniz and Isaac Newton independently coinvented calculus. Neither would have countenanced history's verdict. Maintaining that he alone invented calculus, Leibniz argued that his priority should be recognized for the good of mathematics. As he reasoned [12, p. 22], "It is most useful that the true origins of memorable inventions be known, especially of those that were conceived not by accident but by an effort of meditation. The use of this is not merely that history may give everyone his due and others be spurred by the expectation of similar praise, but also that the art of discovery may be promoted and its method become known through brilliant examples." Newton believed that Leibniz, for all his fustian rhetoric, was a plagiarist. More importantly to Newton, Leibniz was a second inventor. As Newton framed the issue [15: VI, p. 455], "Second inventors have no right. Whether Mr Leibniz found the Method by himself or not is not the Question… We take the proper question to be,… who was the first inventor of the method." Probity and principle, he argued, demanded a correct answer: "To take away the Right of the first inventor, and divide it between him and that other [the second inventor], would be an Act of Injustice."

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There is no doubt that Newton's discoveries preceded those of Leibniz by nearly a decade. Stimulated by the Lucasian Lectures Isaac Barrow delivered in the fall of 1664, Newton developed his calculus between the winter of 1664 and October 1666. Two preliminary manuscripts were followed by the so-called *October 1666 tract*, a private summation that was not printed until 1962. Because of Newton's dilatory path to publication, word of his calculus did not spread beyond Cambridge until 1669. In that year, Newton, reacting to the rediscovery of his infinite series for $\log(1+x)$, composed a short synopsis of his findings, the *De analysi per aequationes numero terminorum infinitas*. The *De analysi* was written near the end of an era in which scientific discoveries were often first disseminated by networking rather than by publication. Henry Oldenburg, Secretary of the Royal Society, and John Collins, government clerk and de facto mathematical advisor to Oldenburg, served as the principal hubs of correspondence in England. Dispatched by Barrow on behalf of a "friend of mine here, that hath a very excellent genius," the *De analysi* reached Collins in the summer of 1669. "Mr. Collins was very free in communicating to able Mathematicians what he had receiv'd," Newton later remarked.

The second thread of the calculus controversy can be traced to 1673, the year that Leibniz took up infinitesimal analysis. During a two-month visit to London early in that year, Leibniz made contact with several English mathematicians and purchased Barrow's *Lectiones opticæ* and *Lectiones geometricæ*. However, Leibniz neither met Collins nor gained access to Newton's *De analysi* before returning to Paris. The first intelligence of Newton that Leibniz is certain to have received was contained in a report prepared by Collins that Oldenburg transmitted in April 1673. In this summary of English mathematics, Collins referred to

Newton's work on series and asserted that Newton had a general method for calculating a variety of geometric objects such as planar area, arc length, volume, surface area, and center of gravity. Undeterred, and without the benefit of any details of Newton's methods, Leibniz proceeded with his own investigations. Influenced by Pascal's calculation of a moment of a circular arc, Leibniz quickly discovered the "transmutation" formula $\int_0^x y\,dx = xy - \int_0^x (x \cdot dy/dx)\ dx$. By the fall of 1673 he had used this identity to obtain his celebrated series, $\pi/4 = 1 - 1/3 + 1/5 - 1/7 + 1/9 - \cdots$. Although his progress continued somewhat fitfully, Leibniz was in possession of the basic skeleton of calculus by the end of November 1675 [10, pp. 175, 187–200].

It is when we backtrack half a year that the tale becomes tangled. In April 1675 Oldenburg sent Leibniz a report from Collins that contained Newton's series for $\sin(x)$ and $\arcsin(x)$ as well as James Gregory's series for $\tan(x)$ and $\arctan(x)$. Leibniz's reply to Oldenburg was not candid: he professed to have found no time to compare these expansions with formulas he claimed to have obtained several years earlier. He neither divulged his avowed results nor followed through with his promise of a further response. After Collins and Oldenburg pressed the matter by conveying the same series a second time, Leibniz offered to share his infinite sum for $\pi/4$ in exchange for derivations of Newton's formulas. Yielding to the entreaties of Oldenburg and Collins, Newton, who likely had not previously heard of Leibniz, consented to participate in the correspondence using Oldenburg as an intermediary. His letter of 13 June 1676, now known as the *Epistola prior*, was amicable and informative.

Leibniz reciprocated with a few of his own discoveries, as he had promised. He also asked for a further explanation of the methods Newton employed in the calculation of series. This request occasioned Newton's second letter for Leibniz, the *Epistola posterior* of 24 October 1676. Historians who read between the lines of this nineteen page manuscript are divided in their assessments of Newton's state of mind. Derek Whiteside speaks of Newton's "friendly helpfulness". A. Rupert Hall finds no word that would "upset the most tender recipient." However, Richard Westfall states, "An unpleasant paranoia pervaded the *Epistola posterior*." Certainly there is evidence that Newton's guard was up. One sentence in the letter sent to Oldenburg, for example, is heavily crossed out. The less carefully obliterated passage in the copy Newton retained reveals his admission that he had not previously known of Leibniz's series for $\pi/4$. Additionally, by rendering two critical passages as insoluble anagrams, Newton concealed the scope of his fluxional calculus. In the cover letter for Oldenburg that accompanied the *Epistola posterior*, Newton declared his intention to terminate the correspondence. Two days later, still brooding on the matter, Newton directed Oldenburg, "Pray let none of my mathematical papers be printed w$^{\text{th}}$out my special licence."

What Newton learned decades later was that while he was crafting the *Epistola posterior*, methodically deciding what to disclose and what to secrete in code, Leibniz was back in London rummaging through the hoard of documents that Collins maintained. Leibniz emerged from the archive with thirteen pages of notes concerning the series expansions he found in Newton's *De analysi*, but he took away nothing pertaining to Newton's fluxional calculus, of which he had no need, having already found an equivalent. A few weeks later Newton, in one of the last letters he would ever send to Collins, declared his intention to keep his mathematical discoveries private, letting them "ly by till I am out of y$^{\text{e}}$ [the] way." Prudently steering clear of Newton's ire, Collins did not mention the access he had already granted Leibniz. For his part, Leibniz saw no need to breathe a word of it. A long, quiet period was broken in October 1684 when Leibniz staked his claim to calculus by publishing his *Nova methodus pro maximis et minimis* [18, pp. 121–131]. With this paper, which did not allude to Newton, the seeds of a poisonous priority dispute were sown. In the words of Moritz Cantor, it "redounded to the discredit of all concerned."

Historians and sociologists of science have long been fascinated with *multiple discoveries*—clusters of similar or identical scientific advances that occur in close proximity if not simultaneously. Such discoveries are even more noteworthy when they exemplify the phenomenon of *convergence*—the intersection of research trajectories that have different initial directions. Throw in a priority dispute, charges of plagiarism, and two men of genius, one vain, boastful, and unyielding, the other prickly, neurotic, and unyielding, one a master of intrigue, the other a human pit bull, each clamoring for bragging rights to so vital an advance as calculus, and the result is a perfect storm. The entire affair—*the* most notorious scientific dispute in history—has been exhaustively scrutinized by scholars. Three of the most prominent, Hall, Westfall, and Joseph Hofmann, have given us thorough analyses ([8], [20], [10], respectively). Now there is a new account, *The Calculus Wars*, which, according to its author, Jason Socrates Bardi, "is the first book to tell the story of the calculus wars in a popular form." Passages such as "He [Leibniz] began to read more Latin than a busload of pre-law students at a debate camp" and "Newton became a sort of Greta Garbo of the science world" attest to the popular form of Bardi's narrative. There is also truth to Bardi's priority claim: Westfall's account is embedded in a thick biography, Hofmann's requires a mastery of calculus, and Hall's is too comprehensive to be considered popular. Each of these three earlier books exhibits the considerable skills of its author, and yet each becomes mired in the tiresome, repetitive nature of the feud. Thus, in his review of Hofmann's volume, André Weil regretted that its readers had not been "spared a great deal of dull material" [19]. And is there a reader of Hall's *Philosophers At War* who does not applaud when the author, near the end of his story, disregards a petty accusation against Newton and exclaims, "Who can care?"

It is possible, then, that a skimpier treatment of the quarrel might form the center of an attractive, useful book. We would expect such a book, despite its abridgment, to cover the essential elements of the dispute. We would expect it to be informed by the historical research undertaken in the quarter century since the publication of the previous accounts. We would expect a diminution of detail, not of accuracy. And we would expect the squabble, given its barren nature, to serve primarily as a vehicle for illuminating either the mathematics that sparked the war or the remarkable men who prosecuted it. As we will see, *The Calculus Wars* does not meet any of these expectations. Moreover, with its frequent misspellings, its many sentences that would not pass muster in a high school writing class, and its abundance of typographical errors, *The Calculus Wars* falls short of a reader's most basic requirements.

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In this review we present a more or less chronological outline of the developing tensions between Leibniz and Newton, noting along the way several of Bardi's more egregious missteps. We then turn our attention to Bardi's treatment of the mathematics in this story, a treatment that is as unsatisfactory for his intended readers as it is for readers of the *Notices*. Our last major discussion concerns the second front in the war between Leibniz and Newton, their confrontation over physics and metaphysics. It is this battleground that has been the subject of the most recent historical study. That research reveals that even in the ancient, academic wrangle between Leibniz and Newton, truth was the first casualty of war.

Trouble in *The Calculus Wars* begins immediately: the first sentence of the preface gives Newton's year of death as 1726. Bardi changes this to 1727 on page 237, but only a few lines later he states that Newton was interred on 28 March 1726, a date he later repeats. The actual chronology is this: in the English calendar of Newton's era, a calendar that marked 25 March as the legal first day of the new year, Newton died on 20 March 1726 and was buried eight days later on 28 March 1727. To avoid confusing readers with such a timeline, historians often state dates as if 1 January initiated the new year, a practice that this review follows. Bardi seems to have been confounded by the differing conventions of his sources. The year he twice gives for Newton's funeral is wrong by any standard.

In the second paragraph of the preface, Bardi tells us that Leibniz and Newton fought a brutal public battle "to the ends of their lives." In the case of Newton this statement is not true. Historians differ on when the calculus wars began, but they are unanimous about when the squabbling ended. After Leibniz died, his supporters continued to spar with Newton and his allies. However, in February 1723 when an old and infirm Newton, weary at last of the incessant bickering, chose not to respond to a mendacious letter of Johann Bernoulli, the priority dispute finally came to an end [5, p. 557], [7, pp. 66, 597], [8, p. 241], [20, p. 792].

In the fourth paragraph of the preface Bardi tells us that "He [Newton] preferred to circulate private copies of his projects among his friends, and did not publish any of his calculus work until decades after its inception." The first clause of this assertion is false. The second clause is true, but Bardi contradicts it when he states that "Barrow helped Newton publish." Only a few lines later Bardi reverses course again when he writes, "The problem was, he [Newton] didn't publish." In fact, Newton did *not* circulate his mathematical work. He lived for sixty years after writing the *October 1666 tract* and during that time he permitted fewer than ten mathematicians to *view* his manuscripts [16: I, pp. xvii, 11]. Barrow *encouraged* Newton to publish but did not succeed in overcoming Newton's disinclination. When the sixty-two year old Newton published his first mathematical work, Barrow had been dead for twenty-seven years.

If we put this inconsistency behind us and advance one page, we find Bardi derailing anew when he declares that the Great Fire, which ravaged London in 1666, was a "seminal event in the calculus wars." The idea here, advanced by Hofmann [10, p. 43] and Whiteside [16: I, p. xv; II, p. 168; III pp. 5–10], is that publishers, devastated by their losses of stock, could not afford to issue slow-selling mathematical titles. It is true that the conflagration brought the publishing industry close to ruin. As Collins wrote to Newton in 1672, "Latin Booksellers here are averse to y^{e} Printing of Mathematicall Bookes … and so when such a

Coppy is offered, in stead of rewarding the Author they rather expect a Dowry with y^{e} Treatise." Nevertheless, there are compelling reasons for rejecting the conclusion that Newton's publishing prospects were seriously impacted [6, pp. 71–73], [20, p. 232]. For one thing, the indefatigable Collins was not daunted. Thinking that Newton might be encountering resistance to publication in Cambridge, he offered his services in London: "I shall most willingly affoard my endeavour to have it well done here." Deploying a procrastinator's armamentarium of excuses to avoid publication—a need for revision, a wish to further develop the material, a shortage of time due to the demands of other activities—Newton never gave Collins the chance.

Even if we were to grant the impossibility of bringing a book-length mathematical work to press, we would still dismiss Bardi's argument that, "If he [Newton] were writing a popular pamphlet or clever little handbill, it could have been a different story." The implication that Newton had to write a weighty tome to secure his priority is untenable: when Leibniz advanced his priority claim, a six page article did the job. Newton could have taken a similarly decisive step. Moreover, his Lucasian salary was generous and he did not even depend on it; the cost of self-publication would have been "trifling" to Newton, as Hall has noted [8, p. 22]. Additionally, Newton had a *certain* opportunity for publication and refused it. Both Barrow and Collins urged him to append his *De analysi* to Barrow's *Lectiones opticæ*, which *was* going to press, the Great Fire notwithstanding. On 12 February 1670 Collins wrote optimistically to James Gregory, "I believe Mr. Newton… will give way to have it printed with Mr. Barrows Lectures." Alas, the young Newton was as obstinate as the old Newton, who boasted, "They could not get me to yield." The absence of Newton's calculus from the Barrovian lectures Leibniz purchased during his first visit to London must be attributed to a Newtonian quirk of character, not an incendiary twist of fate.

The next phase of the prehistory of the calculus dispute was the 1676 correspondence between the two future adversaries. Referring to the *Epistola prior*, Bardi writes, "There was nothing in the letter that was not already known to Leibniz in some form or another. Nothing." One page later Bardi flatly contradicts this unequivocal, emphatic declaration when he admits, "Leibniz was blown away by the *Epistola prior.*" Indeed, as we have observed, there was a great deal Leibniz could have learned about infinite series from Newton in 1676. The gap between them was made even more apparent by the *Epistola posterior.* In an astonishing display of one-upmanship, Newton pointed out that, for suitable choices of its parameters, the rational function $1/(e+fz+gz^2)$ provides not only the series communicated by Leibniz, but also "this series

$$1+\frac{1}{3}-\frac{1}{5}-\frac{1}{7}+\frac{1}{9}+\frac{1}{11}-\frac{1}{13}-\frac{1}{15}\qquad \text{etc.}$$

for the length $[\pi/(2\sqrt{2})]$ of the quadrantal arc of which the chord is unity." Bardi reports that Newton was "superlative with his praise" of Leibniz in the *Epistola posterior.* Although the quotations Bardi provides do contain superlatives, they merely demonstrate that when the normally plainspoken Newton was on his best behavior, he was capable of embroidering his formal correspondence with the customary encomiums of the era. By not delving deeper, Bardi leaves his readers with the wrong impression. Immediately following the quoted superlatives, Newton continued, "[Leibniz's] letter… leads us also to hope for very great things from him." Having pinged our faint praise radar, Newton follows through with an unsurpassable masterpiece of the art of damning: "Because three methods of arriving at series of that kind had already become known to me, I could scarcely expect a new one to be communicated." To ensure that Collins was not misled by the correspondence, Newton confided privately, "As for y^{e} apprehension y^{t} [that] M. Leibniz's method may be more general or more easy then [sic] mine, you will not find any such thing…. As for y^{e} method of Transmutations in general, I presume he has made further improvements then [sic] others have done, but I dare say all that can be done by it may be done better without it." To Oldenburg Newton confessed a fear that he had been "too severe in taking notice of some oversights in M. Leibniz letter." Nevertheless, Newton could not refrain from adding, "But yet they being I think real oversights I suppose he cannot be offended at it."

Six serene years followed this exchange. Employment brought Leibniz to provincial Germany where he was occupied with the mundane duties of his position, Librarian to the Duke of Hanover. In Newton's case, near isolation resulted from the deaths of Barrow and Oldenburg in quick succession in 1677. Preferring total isolation, Newton lost no time severing his correspondence with Collins. As he later explained, "I began for the sake of a quiet life to decline correspondencies by Letters about Mathematical & Philosophical matters finding them [sic] tend to disputes and controversies." His tranquility received a jolt in mid-June 1684 when he received a presentation copy of *Exercitatio Geometrica*, a fifty page tract authored by David Gregory, nephew of the deceased James Gregory (whose unpublished papers were the source of much of the material). The *Exercitatio* contained several of Newton's results as well as an announcement that more would follow.

Newton reacted with alacrity to this new threat to his priority. To secure his right of first discovery, he began a manuscript, the *Matheseos Universalis Specimina*, intended to explicate both his method of fluxions and its history. In the opening lines of the *Specimina*, Newton relegated the Gregorys to the status of second inventors: "A certain method of resolving problems by convergent series devised by me about eighteen years ago had, by my very honest friend Mr. John Collins, around that time been announced to Mr. James Gregory … as being in my possession.… From his papers… David Gregory also learnt this method of calculation and developed it in a neat and stimulating tract: in this he revealed… what he himself had taken from his predecessor and what his predecessor had received from Collins." Having parried one challenge, Newton attempted to forestall an expected second challenge by making public his correspondence with Leibniz. He explained that those letters would serve readers better than a fresh treatment "since in them is contained Leibniz' extremely elegant method, far different from mine, of attaining the same series—one about which it would be dishonest to remain silent while publishing my own."

Newton started work on the *Specimina* in late June 1684 but his impulse to publish was soon quelled. In July he put down his pen in mid-equation to embark on a new exposition of infinite series. In August he abruptly abandoned that manuscript too. According to tradition, it was at precisely that time that Edmond Halley visited Cambridge to pose the question that diverted Newton and precipitated the *Principia*. Here, perhaps, we may perceive a mischievous intervention of fate. The second thread of the calculus dispute, after its own extended hiatus, was becoming intricately interlaced with the first. With a window of less than two months for establishing an unencumbered claim to calculus, Newton became preoccupied with the planets. Leibniz, whose complacency had also been jolted by a publication from an unexpected source, had just submitted the discoveries that he had withheld for nearly nine years.

At the height of the calculus quarrel, in a rationalization that contained only part of the truth, Leibniz explained that his hand had been forced by a sequence of papers pertaining to tangents, extrema, and quadratures that appeared in the *Acta Eruditorum* beginning in December 1682 [1, p. 117]. The author of the articles Leibniz cited was Ehrenfried Walther von Tschirnhaus, a mathematician whose travels had taken him to London in May 1675 and then to Paris in August 1675. While in England, Tschirnhaus purchased Barrow's lectures and met Collins, who acquainted him with Newton's work. On arriving in Paris, Tschirnhaus entered into a close, working relationship with Leibniz. It was a collaboration that would have some significance in the priority dispute, for Newton deduced—mistakenly, it should be added—that Tschirnhaus passed on to Leibniz what he had obtained from Collins. To the contrary, Tschirnhaus, acknowledging an indebtedness only to Barrow, appropriated and published techniques of calculus that he had learned from Leibniz [11]. In reply to the protestations of Leibniz, Otto Mencke, the founding editor of the *Acta Eruditorum*, urged Leibniz to submit his own exposition [15: II, p. 397]. Referring to the tract of David Gregory, Mencke added, "I now have a reliable report that someone in England has undertaken to attribute publicly to Professor Newton of Cambridge a quadrature of the circle." Leibniz did not dally. Mencke's letter, which was dated 6 July 1684, was answered before the month was over. Leibniz reassured Mencke, "As far as Mr. Newton is concerned… I have succeeded by another way… One man makes one contribution and another man another." Along with his reply he enclosed his contribution, the hastily composed *Nova methodus*. The irony is worth noting: Leibniz's completion of the *Nova methodus* and Newton's abandonment of the *Specimina* were simultaneous.

In the autumn of 1691 David Gregory would play another crucial role in the calculus dispute when he sought Newton's approval of a paper on integration. That second jolt from David Gregory prompted Newton to draft his *De quadratura curvarum*, the revised version of which would become Newton's first mathematical publication a dozen years later. It is clear that both Tschirnhaus and David Gregory influenced the development of the calculus dispute in important ways. Bardi confines his notice of Tschirnhaus to one paragraph, stating that "Newton knew vaguely of Leibniz before their exchange, since he was familiar with one of Leibniz's fellow Germans, Ehrenfried Walther von Tschirnhaus." Where does the idea that Newton learned of Leibniz from Tschirnhaus come from? As for David Gregory, Bardi mentions him only twice: once to quote his hearty praise for Newton's *Principia* and once to say that he was the teacher of John Keill, a later participant in the dispute. Bardi *attempts* to mention David's part in Newton's first publication of fluxions, but botches it by stating that the *De quadratura* came about "only after the Scottish mathematician James Gregory had sent Newton his own method." After the correction of *James* to *David*, Bardi's Escher-like sentence becomes true if "his own" is understood to refer to Newton!

In March 1693 Leibniz initiated a direct exchange of letters with Newton in which he raised the subject of colors. Newton was then between drafts of the *De quadratura* and planning his *Opticks*, a work he did not intend for immediate publication, as he told Leibniz, "for fear that disputes and controversies may be raised against me by ignoramuses." (Robert Hooke was the particular ignoramus Newton had in mind.) Given the remark in the *Specimina* that Newton made concerning acknowledgment, he must have already judged Leibniz to be dishonest. Nevertheless, his letter to Leibniz was cordial. This surface amity was not seriously disturbed by a reckless insinuation of Nicolas Fatio de Duillier, who in 1699 declared that "Newton was the first and by many years the most senior inventor of the calculus… As to whether Leibniz, its second inventor, borrowed anything from him, I prefer to let those judge who have seen Newton's letters." By being the first to publicly suggest plagiarism, Fatio is historically noteworthy, but he was not "a key player in the calculus wars," as asserted by Bardi, who nonetheless consistently misspells *Duillier*.

The death of Hooke in 1703 cleared Newton's path to publication: his *Opticks* went to press in 1704 with the *De quadratura* appended. In the January 1705 issue of the *Acta Eruditorum*, an anonymous review authored by Leibniz proclaimed, "Instead of the Leibnizian differences, Mr. Newton employs, and has always employed fluxions, which are almost the same… He has made elegant use of these… just as Honoré Fabri in his *Synopsis geometrica* substituted the progress of motions for the method of Cavalieri." Here, for the first time, one of the disputants had publicly disparaged the other. Although Leibniz would deny both his authorship of the review and any imputation of plagiarism, Newton recognized both the style and the insult. Writing anonymously many years later, Newton complained, "The sense of the words is that… Leibniz was the first author of this method and Newton had it from Leibniz, substituting fluxions for differences." To Newton, "This Accusation gave a Beginning to this present Controversy."

Even after Leibniz's indiscretion, open warfare was not inevitable. Had it not been for John Keill, the future Savilian Professor of Astronomy at Oxford, Newton may never have seen the offending review. In a paper that appeared in 1710, Keill asserted that "beyond any shadow of doubt" Newton first discovered the "celebrated arithmetic of fluxions." Keill then charged that "the same arithmetic… was afterwards published by Mr Leibniz in the *Acta Eruditorum* having changed the name and the symbolism." Because Keill and Leibniz were both fellows of the Royal Society, the body that published Keill's paper, it was to the Society that Leibniz turned for redress. By demanding a retraction [15: V, p.96], Leibniz crossed the point of no return, for Newton was the Society's president. Keill defended himself by directing Newton to the insinuations Leibniz had inserted into his 1705 review of *Opticks*. Newton advised the secretary of the Royal Society, "I had not seen those passages before but upon reading them I found that I have more reason to complain… than Mr Leibniz has to complain of Mr Keill."

And complain Newton did. Having convened a committee of the Society in March 1712 to investigate the priority issue, he furnished the committee with those documents necessary for establishing his priority. He also drafted the report of the committee and had the Society print the report in the form of a book, the *Commercium Epistolicum*, for international circulation. His conclusion was that "Mr Newton was the first inventor [of the calculus] and… Mr Keill in asserting the same has been noways injurious to Mr Leibniz." A few years later, Newton composed and published the lengthy *Account of the Book entituled Commercium Epistolicum*, which he also translated into Latin for the benefit of continental mathematicians. Unstinting in his efforts, Newton had De Moivre prepare a French version, saw to its publication, and arranged for its positive review. Not yet assuaged some six years after the death of Leibniz, Newton modified the *Commercium* so that his priority was even more evident in its second edition. "Finally, Newton was the prolific author his contemporaries had wanted him to be for so many years," Bardi quips in his best line.

There is no need to describe the remaining battles, but one further error in *The Calculus Wars* should be corrected. In 1713 a condemnation of Newton known as the *Charta Volans* circulated widely. To buttress his charge of plagiarism, the anonymous author of the *Charta Volans* quoted from a letter written by an unnamed "leading mathematician" (*primarii Mathematici*), who referred to an article written by a "certain eminent mathematician" (*eminente quodam Mathematico*). It is a measure of the tedium of the priority dispute that we must distinguish between two translated adjectives that are so similar. Despite all the subterfuge, the authorship of the *Charta Volans* was never in doubt: everybody knew that it had been penned by Leibniz. Because the referenced article was written by Johann Bernoulli, the identity of the "eminent" mathematician was not a mystery either. That left only the "leading" mathematician in question and before long there was a prime suspect. To quote Hall [8, p. 200], "When [Johann] Bernoulli's identity as the 'leading mathematician'… began to be guessed… the joke went around that he had praised himself as the 'very eminent mathematician.' " Bardi loses his way in this comedy of concealed identities, stating, "Leibniz would later be mocked for calling himself an eminent mathematician."

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There is little mention of mathematics in *The Calculus Wars,* but when such discussions are unavoidable, the results are invariably frustrating. Leibniz is said to have "solved with ease a problem Descartes was unable to solve in his lifetime." Might Bardi's reader not want to know what that problem was? A mere sixteen pages after mentioning this 1684 application of calculus, Bardi tells us that an article Jakob Bernoulli published in 1690 "was an important document because… it was the *first* in a long series that applied calculus to solving problems in mathematics." The reviewer's italics point to an inconsistency, but the main grievance is that Bardi has tantalized us once again. It surely would not have transcended the bounds of a popular work to cite and even briefly discuss the isochrone problem [18, pp. 260–264]. Against this background, complaints about inaccurate sentences such as "Calculus makes solving quadrature problems trivial" and meaningless phrases such as "to draw a line perpendicular to any point on the curve" will seem futile.

Bardi's characterizations of the mathematics of Newton and Leibniz are particularly misleading. "Newton's big breakthrough," Bardi informs us, "was to view geometry in motion. He saw quantities as flowing and generated by motion." This point of view was not a breakthrough at all: the kinematic construction of curves was a standard method of analysis well before Newton [2, pp. 75–81, 174–177]. Barrow, who mentioned its prior use by Marin Mersenne and Evangelista Torricelli, recognized its value in his geometric lectures because it allowed him to analyze conics without resorting to cases. During the calculus controversy, Newton recollected, "Its probable that D^{r} Barrows Lectures might put me upon considering the generation of figures by motion" [16: I, pp. 11 (n. 26), 150, 344]. He also encountered the method in the appendices of Frans van Schooten's second Latin edition of the *Géométrie* of Descartes, which he started to study in the summer of 1664 [16: I, pp. 146, 371 (n. 11)]. According to another of Bardi's claims, Leibniz "developed calculus more than had Newton." Bardi should have offered some evidence for this assertion since it is contrary to the opinion of the leading expert on Newton's mathematics [16: VII, p. 20], an opinion that is endorsed by other prominent scholars [8, p. 136], [20, p. 515]. To quote Hall, "Well before 1690… [Newton] had reached roughly the point in the development of the calculus that Leibniz, the two Bernoullis, L'Hospital, Hermann and others had by joint efforts reached in print by the early 1700s."

Near the end of his epilogue, Bardi suggests the possibility that neither Newton nor Leibniz deserves all the credit he was seeking. "In some ways, the development of calculus owes just as much all those who came before [sic]." This is a pertinent consideration and it is too bad Bardi drops the issue as soon as he has raised it. Both Lagrange and Laplace, for example, deemed Pierre de Fermat to be the first inventor of the differential calculus [4]. John Mark Child, who edited and interpreted the *Lectiones Geometricæ* of Isaac Barrow, proposed the following set of minimal requirements for a first inventor: "A complete set of standard forms for both the differential and integral sections of the subject, together with rules for their combination, such as for a product, a quotient, or a power of a function, and also a recognition and demonstration of the fact that differentiation and integration are inverse operations." Using these criteria, Child proclaimed that Isaac Barrow was the first inventor of calculus [3]. Historians nowadays augment Child's list—and thereby exclude Barrow—by requiring an awareness that the diverse problems that made up the "research front" of seventeenth century infinitesimal analysis could all be tackled in a comprehensive, algorithmic manner using symbolic language.

It is unfortunate that we know so little of Barrow's influence on either Newton or Leibniz. In a letter of 1716 [15: VI, p. 310], Leibniz argued, "It is possible that Mr. Barrow knew more than he ever published and gave insights to Mr. Newton which we do not know of. And if I were as bold as some, I could assert on the basis of these suspicions, without further evidence, that Newton's method of fluxions, whatever it may be, was taught to him by Mr. Barrow." Leibniz was artful enough to talk only of what Barrow *might* have known, for by then it had often been alleged that the Leibnizian calculus was merely a symbolic recasting of Barrow's published work. For example, Collins, who knew exactly what had passed from Newton to Leibniz, never suggested that Leibniz plagiarized Newton, but he did wonder whether results of Leibniz were "learnt or… derived from Dr Barrows Geometrick Lectures" [15: II, p.241]. Tschirnhaus *did* believe that Leibniz took from Barrow [10, pp. 76, 173]. So did Jakob Bernoulli, who in 1691 asserted, "To speak frankly, whoever has understood Barrow's calculus… will hardly fail to know the other discoveries of Mr. Leibniz, considering that they were based on that earlier discovery, and do not differ from them, except perhaps in the notation of the differentials and in some abridgment of the operation of it."

In more modern times, Margaret Baron has remarked that Leibniz's notes "suggest that he had been dipping into Barrow's *Lectiones,*" whereas Jacqueline Stedall has stated that Leibniz studied Barrow "intensively" [2, p. 288], [18, p. 119]. Since the topic of this review is priority, the key question for us is, *When?* Hofmann [10, pp. 76-78] and Dietrich Mahnke [13] have argued that Leibniz did not read Barrow until the winter of 1675, which is to say, after he had completed his outline of calculus. (Interested readers must judge for themselves how persuasive those arguments are—the reviewer is not convinced by them, but his position is not that of an expert and he offers it only so that he does not appear as weaselly as Fatio.) Leibniz himself strongly and repeatedly denied any debt to Barrow. To Jakob Bernoulli he wrote that he had filled some hundreds of sheets with calculations based on characteristic triangles before the publication of Barrow's *Lectiones* [8, p. 76]. That chronology is impossible. In a letter to another correspondent [15: VI, p. 310], Leibniz wrote, "As far as I can recall, I did not see the books of M. Barrow until my second voyage to England." That recollection is wrong.

Hofmann explained the frequent infelicities of Leibniz's accounts by saying "He must have meant… " or "He wrote in haste… ." Child either cited "memory lapses" or proposed other scenarios to avoid the "brutal" conclusion that Leibniz lied. For André Weil, however, Leibniz's say-so obviated the need for any explanation: "In the early stages he [Leibniz] could have learned a good deal from Barrow's *Lectiones geometricæ*; but, by the time he read them, he found little there that he could not do better. At any rate he says so… In the absence of any serious evidence to the contrary, who but the surliest of British die-hards would choose to disbelieve him?" In the same review [19], Weil repeats, "As he [Leibniz] says, he could well have derived some of his inspiration from Barrow, had he read him at the right moment; there is no point in disputing this fact. He could have; but he says he did not; so he did not, and that is all."

In the time that has passed since Weil's pronouncement, serious evidence undermining Leibniz's good faith *has* been uncovered. In February 1689, two years after the *Principia* became available, Leibniz published a fifteen-page paper, the *Tentamen de motuum coelestium causis*, concerning the planetary orbits. Leibniz's background story for this article was that he had not yet seen the *Principia* because of his travels. He asserted in the *Tentamen* (and elsewhere) that his knowledge of the *Principia* was limited to an epitome that had appeared in the June 1688 *Acta*. Leibniz implied that his work was done some time before that review, but that he had suppressed it until such time as he would have the chance to test his ideas against the most recent astronomical observations. The publication of Newton's theories, he continued, stimulated him to allow his notes to appear "so that sparks of truth should be struck out by the clash and sifting of arguments." To his mentor, Christiaan Huygens, Leibniz drafted a letter in which he affirmed that he did not see the *Principia* before April 1689 (by which time the *Tentamen* had already appeared).

It is likely that Newton, who had had a copy of the *Principia* sent to Leibniz immediately after its publication, was unaware of the *Tentamen* until after 1710. Newton sensed plagiarism when he was apprised of it, but he charged Leibniz with bad manners instead: "Through the wide exchange of letters which he had with learned men, he could have learned the principal propositions contained in that book [the *Principia*] and indeed have obtained the book itself. But even if he had not seen the book itself [before writing the *Tentamen*], he ought nevertheless to have seen it before he published his own thoughts concerning these same matters, and this so that he might not err through… stealing unjustly from Newton what he had discovered, or by annoyingly repeating what Newton had already said before." It now appears that Newton's suspicions were justified. In the 1990s Domenico Bertoloni Meli discovered and presented compelling evidence to reject the cover story Leibniz prepared for the *Tentamen*. Meli concluded that "Leibniz formulated his theory in autumn 1688 [i.e., *after* the *Acta* review], and the *Tentamen* was based on direct knowledge of Newton's *Principia*, not only of Pfautz's review" [14, p. 9]. Given that Meli's book appears in the bibliography of *The Calculus Wars*, it is difficult to understand why Bardi repeats Leibniz's fabrications about the *Tentamen* as if they have never come into question.

There have been several recent reminders that priority disputes remain topical.

We may contrast Bardi's neglect of a serious matter with his excited denunciation of a trivial yet iconic anecdote: "The legend of Isaac Newton and the apple… is probably completely fabricated. Perhaps the only thing that is true about it is that Newton loved apples. The story is no more true than the one about the alligators in the sewers of New York." In fact, Isaac Newton himself told the apple anecdote to at least four close friends and relatives in 1726 and 1727 [7, p.29], [20, p.154]. If Bardi had any reason to think Newton fibbed or confabulated, then he should have shared it. Having repudiated one of the best-known stories of science, Bardi proceeds to spoil one of the best-known quotations of science by paraphrasing it as "Joseph-Louis Lagrange… called Newton the greatest and the luckiest of all mortals for what he accomplished." Bardi does not explain why Lagrange considered Newton so lucky. The missing explanation can be found in the actual quotation, "… et le plus heureux; on *ne* trouve *qu'une fois* un système du monde à établir." This insider's appreciation of priority (with Lagrange's implicit regret that he, unlike Newton, had the misfortune to follow a Newton) could have been a perfect insight for Bardi's readers, but instead it is just one more mystery.

There have been several recent reminders that priority disputes remain topical. Shortly before this review was suggested, the United States Senate tabled a patent reform act that would have aligned the U.S. with the rest of the world in recognizing claims based on first-to-file status rather than first-to-invent. During the writing of this review, Hollywood released *Flash of Genius*, a film that depicted the zeal with which an inventor will battle for recognition. At the same time, a Nobel Prize committee arrived at its own resolution of the most acrimonious scientific dispute of recent years, the fight over the discovery of the human immunodeficiency virus. The battle between Newton and Leibniz, one imagines, should continue to interest many readers. *The Calculus Wars*, however, is an appalling book that cannot be recommended to them. For those seeking a popular treatment of the priority dispute, either one of the reliable, comprehensive biographies of Newton [5], [20] would make a good choice. Those who require a more detailed treatment will continue to consult Hall [8]. If a concise outline is preferred, then Hall has written that too [9]. Newton's correspondence [15], which was edited with an eye to the controversy, is the ultimate resource for a thorough understanding of the affair.

In a bygone era a reviewer might have passed over *The Calculus Wars* in preference to subjecting it to an excoriating review. Incompetent books could safely be allowed to sink quietly into oblivion. Search engines and the Internet have changed that. Because Bardi has had a book published, he is now an expert on its subject as far as the World Wide Web is concerned. At the time of this writing, the Wikipedia page devoted to the priority dispute cites both *Philosophers at War* and *The Calculus Wars*. No indication is given there that one book is authoritative and the other is not. Google finds Bardi's unseemly assessment of the career of John Pell, an appraisal that was out-of-date before he started writing his book, just as easily as it finds the reasoned reappraisal of Pell given in [17]. At the time of this writing, WorldCat locates *The Calculus Wars* in more than twice as many libraries as Meli's book and nearly as many as Hall's. If readers of the future are not to be swamped with misinformation, then the searches that turn up specious books must also find them critically reviewed.

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