3

Newton versus Leibniz

Credit for the Calculus

At the turn of the 18th century, Isaac Newton, an Englishman, and Wilhelm Gottfried Leibniz, a German, engaged in a ferocious battle. Having never met personally, they obviously used neither fists nor knives, yet the science historian Daniel Boorstin has christened their dispute "The spectacle of the century."[1] Ernst Cassirer, writing in the prestigious *Philosophical Review*, called it "one of the most important phenomena in the history of modern thought."[2]

It's usually described as a priority battle over the invention of the calculus. With neither money nor a woman at its heart, this hardly sounds like the stuff of a knock-down-drag-out fight, yet it went on for years, becoming more bitter as it proceeded.

What made it such a big deal? First, it engaged two of the greatest geniuses who ever lived. The more familiar genius is Isaac

Newton, who hardly needs any introduction here. Less familiar is Wilhelm Gottfried Leibniz, a German philosopher/mathematician who did important early work on symbolic logic and on the calculus, and in a variety of other fields as well, including especially cosmology and geology.

Second, though few of their contemporaries could understand or follow the workings of the calculus at first, it shortly became clear that this was a new, useful, and general method for dealing with a wide variety of scientific and mathematical problems that had been unsolvable until then.

The feud also had several curious results. For example, it played an important part in the development of the modern scientific paper—specifically, one that is refereed and that includes explicit, clear references to what has been accomplished previously.

Furthermore, it raged on for centuries, fed mainly by the jingoistic behavior of the two men's followers. It even played a part in an ongoing tug-of-war between British and Hanoverian leaders for the throne of England. Among the Hanoverian claims to distinction was, said Leibniz's followers, his invention of the calculus. Newton's supporters derided such claims. One Briton, John Keill, considered these assertions to be attempts at stealing the fruits of Newton's genius.

It was a battle in which Newton used some heavy-duty fighting tactics—some people would use stronger language—and emerged the clear winner. The result was a gray cloud over Leibniz's later years, but, had he lived long enough, he would have seen another, totally unexpected, outcome. Although Leibniz lost the battle, it's probably fair to say that he actually won the war—though you'd hardly know it from the reputations of the two men as they are known today.

Newton

Nothing as complex and far-reaching as the calculus emerges full-blown from the minds of even men like Newton and Leibniz. Fermat's method of finding maxima and minima was already a direct step along the road to differentiation, one of the important routines in the calculus. For a mind like Newton's, however, no one can say for sure how he built the foundation for his work on this area of mathematics. We do know that he read widely in the mathematical literature of the time, and that one of the books he read, digested, and worked through carefully was Descartes' *Geometry*. He also studied Euclid, whose geometry he is said to have found trifling.

Among other authors there was James Gregory, an accomplished Scottish mathematician; Galileo; and Newton's immediate master at school, Isaac Barrow. We have a specific clue: Newton did tell us that he was led to his first discoveries in the field by his reading of *Arithmetica Infinitorum* (1655), a text by the distinguished British mathematician, cryptographer, and cleric John Wallis that dealt with the quadrature of curves (finding the area beneath curves).

Newton began his mathematical work while a student at Trinity College, Cambridge, and he earned a bachelor's degree in June 1665. Then an attack of plague shut down the university for 18 months. He simply continued his studies, on his own, at his family home in Woolsthorpe, a small town some 30 miles southeast of Nottingham in central England. He may, however, have made a brief visit to the university at some point during this time, perhaps to do some reading and/or experiments.

His enforced home study time was apparently the best thing that could have happened, at least for Newton. During this period, spanning the years 1665 to 1666, he built the foundations for his work in optics, celestial mechanics, and mathematics, including the foundations of the calculus. As part of this work, he extended Wallis's work on the use of infinite series. Newton realized, and capitalized on, the fact that many mathematical functions can be expressed as infinite series. Using them, he was able to generate general expressions for the lengths and the tangents of curves, along with a method for handling quadrature problems (calculating the areas bounded by curved lines). Practitioners of the calculus will recognize here the beginnings of their craft.[3]

At this point, Newton, an unknown youth in his mid-20s, had already sailed past his own teacher at Cambridge, and even past Wallis, one of the top mathematicians of the day. Mathematicians until then had thought of the path of a moving body as a series of points. Newton was arguing that it should be seen as a graph made by a continuously moving point. Since the velocity of a point moving in the direction x is the distance moved divided by the time, t, that is, x/t, interesting things can happen, he suggested, if we shrink both x and t. Thus a continuous and finite motion is equal to the quotient of an infinitesimal distance and an infinitesimal time. He gave the name *fluent* to the moving point, and he used the term *fluxion* for its velocity, that is, its derivative or rate of change with respect to time.

Publish or . . .

Had Newton been working today, he might have published something quickly in, say, the *Bulletin of the London Mathematical Society* and then perhaps more fully in a journal like Princeton's *Annals of Mathematics*. There he would likely start out by giving credit to the mathematicians on whose work he had built. Then he would explain his new work clearly, pointing out where and how he had moved ahead. In this way, he would clearly establish his priority, for the earlier publications would have been in peer-reviewed journals.

Unfortunately, there were at the time no such journals. This type of journal developed slowly and did not come into being until around the mid-1800s. Its objective appears to have been less to share new discoveries with the scientific community–there were already such journals in existence–than to provide a solid route to establishing one's priority in his or her discovery.

What did happen is that in 1669, Newton wrote up his early work as a tract, which he called *Analysis with Infinite Series* (often shortened to *De Analysi*), but it circulated only in manuscript form among a few colleagues, including Isaac Barrow, his teacher at Cambridge. It could, of course, have been published early on in book form–still a common form of establishing priority in Newton's day–but was not, for several reasons.

First, there was a severe recession in the book trade following the Great Fire of London in 1666, and technical works were at a particular disadvantage. Barrow, ironically, was in some measure to blame, for the publisher of his work had gone bankrupt, and so book publishers were particularly leery of publishing mathematical works.

Even so, things might have turned out differently but for yet another turn of fate's wheel. Newton was basically a loner. We have already seen that by age 23, and while still a student, he had already gone past the leading mathematicians of his day, and none but a few of his correspondents knew it. In 1669, thanks in part to his unpublished manuscripts, he was made Lucasian Professor of Mathematics at Cambridge University, which gave him the time and the freedom to continue his work.

He turned to his other interests, which included his first great discoveries in light and color, which had also been made in those remarkable years of the mid-1660s. Always hesitant to open his work to outside criticism, he nevertheless decided to try it, and he did so with a paper on this work in the *Philosophical Transactions of the Royal Society of London* in 1672. Although the paper received a generally good reception, Newton also found himself devoting precious time to answering sometimes inane challenges to his claims, always a danger when new ideas are presented. Among those objecting, unfortunately, were some eminent scientists, including the Dutch physicist Christiaan Huygens and the British scientist Robert Hooke. He found Hooke's criticisms especially troubling and distasteful.

As a result, although Newton continued to work on optics, he published no more papers on it and held off on publishing his major work on it, his *Opticks*, until after Hooke died–more than 30 years later! It may well be that this experience was a factor in his decision not to open his mathematics to the world. He seemed to believe that his discoveries belonged to him and not to the world, to science, or even to posterity. He may also have chosen to keep his discoveries close his chest to give him further time to refine them.

Whatever the reason or reasons, it was a decision that would cause him major problems, and mathematical historians considerable uncertainty, in the years to come.

By the 1680s, however, Newton had developed his work on mechanics, gravitation, and the movement of bodies to the point where he decided, after strong urging by his friend and colleague Edmund Halley, that he would put it into print. He began serious work on what would become his most famous work, the *Mathematical Principles of Natural Philosophy*, commonly known as the *Principia*, in

1684–1685. Published in 1687, it would go on to become possibly the most important and best-known publication in scientific history.

In it, he gave just a hint of the new calculus. He may have used the method to solve some of the problems he tackled in the book, then recast them and presented them in classic geometric fashion—perhaps to keep his calculus methods a secret for a while longer, but also because those were the standard methods of demonstration and proof.

Among these solutions was a conclusive demonstration that Cartesian vortices could not account for the planetary motions. Nevertheless, it took many decades before the authority of Descartes gave way to Newton's gravitational view of the universe.

Up to his *Principia* period, Newton's few contacts with Leibniz had been, on the whole, quite respectful and even friendly, but now he saw something in print that, if it didn't cause an immediate break, was surely a factor in what was to come. Before we get to it, however, we need to know something about its author.

Leibniz

Leibniz, born in 1646, was four years younger than Newton. Like Newton, he read and was influenced by Descartes' *Geometry*, as well as by other mathematical works. Yet even more, his interest in mathematics was stimulated by his earlier readings in philosophy. By the age of 6, he was already reading widely in the library of his father, a professor of moral philosophy at the University of Leipzig. By the age of 14, he was well-read in all areas of the classics.

Strangely, although he came from a middle-class family, among all the mathematicians/scientists/philosophers of his time he was the only one who had to scrabble for a living. That and his wide-ranging mind led him into a surprising variety of fields. By the age of 26, he had already designed a calculating machine that could add, subtract, multiply, divide, and even take roots; he had devised a program of legal reform for the Holy Roman Empire; and he had presented a plan to Louis XIV that involved a French attack on Egypt as a way of weakening the Ottoman Empire and deflecting French aggression away from Germany. Nothing came of it. At various times he was also interested in, and made contributions to, religion, philosophy, philology, logic, economics, and, of course, science and mathematics.

Herein lay a major difference between him and Newton. Newton was mainly interested in using his mathematics to solve scientific problems. Leibniz, like Descartes, hoped to make a major contribution to philosophy and thought that mathematics would pave the way. He wanted to create a kind of alphabet of human thought, in which symbols could be used to represent fundamental concepts, which could then be combined into more complex thoughts–a kind of calculus of reasoning.

Yet whatever Leibniz did in the field of mathematics, it was as a sideline to his multihued career, which makes his accomplishments all the more amazing. In 1673, he visited London on a diplomatic mission as part of his position as adviser to the archbishop of Mainz. There he met Henry Oldenberg, the secretary of the Royal Society, and made enough of an impression that he was elected to the Society. In other travels he had been in contact with the likes of men such as Huygens, Spinoza, Malpighi, and Vincenzo Viviani, a prominent pupil of Galileo's.

During his 1673 visit with Oldenberg, he may, says one historian of mathematics,[4] have seen a copy of Newton's *De Analysi*, though that seems unlikely. Even if he did, he might not have understood it. In 1676, he traveled to London, again as part of his diplomatic duties, and this time he visited with another colleague of Newton's, John Collins, who we know for sure showed him some of Newton's papers.

It was at this point that direct relations began between the two men. Leibniz was probably just beginning to think about the calculus, and the general feeling is that he was not only well behind Newton, but apparently did not even know of Newton's work in the area. And so, when Leibniz wrote to Newton, which he did twice in 1676, it was to ask questions about infinite series and their use in quadrature. Newton responded with two very respectful letters, which would play a strong role in the dispute that developed in later years.

While Newton's answers did skirt some issues of his calculus, he was careful to hide them in a carefully constructed anagram, or he simply alluded to such a method but never spelled it out. It was this disparity between their two stages that was to lead Newton to trouble, for when Leibniz did publish, some eight years later, Newton could not believe that Leibniz could have progressed so far so fast on his own.

Notation

Although Leibniz was deeply impressed with his own idea about a calculus of reasoning, it found little resonance among his contemporaries. More important by far, however, was that the mathematics that emerged from this work was to become the key to a far wider—and more directly useful—world of application. As with Newton's calculus, it became easier to deal with complex curves, areas, and volumes. Furthermore, it could deal handily with change—with velocities and accelerations, with rates of growth and decay—in ways that were just not possible before.

Finally, what both Newton and Leibniz had come up with was a method that did not merely provide solutions to a few specific problems, as earlier methods had, but an algorithm that had wide and spectacular generality. It could be applied to functions that were algebraic or transcendental (Leibniz's coinage), rational or irrational.

Over the development years, Newton used a variety of symbols, which caused some confusion later on. Early on, he tended to use the "little zero" to denote an arbitrary increment of time, and, say, op to denote the increment of a variable p. Later, he moved to the somewhat more familiar dot notation—for example, $\dot{x}$ for the first derivative of x (such as velocity) and $\ddot{x}$ for the second derivative (acceleration).

Leibniz was more careful and more thoughtful about his symbolism, and this would stand him in good stead when the scales of justice were balanced later on. For his differential calculus, he came up—after some trial and error—with the much more useful symbols dx and dy for the differentials (smallest possible differences) in x and y; and with the sign $\int$ for the integral function. For both men, finding tangents called for the use of the differential function, and calculating quadratures (areas bounded by curves) required the use of their integral calculus.

By the 1680s, then, Leibniz had established himself as an up-and-coming mathematician, and in 1684 his first description of his differential calculus was published in the journal *Acta Eruditorum*, under the title "A New Method for Maxima and Minima as Well as Tangents, Which Is Impeded Neither by Fractional nor by Irrational Quantities, and a Remarkable Type of Calculus for This." Here we see, for the first time, a clear statement of the basic formula for differentiation:

$$dx^n = nx^{n-1}.$$

In the same way that Newton did, he thought of integration not only as a summation of areas under a curve, but as the inverse of differentiation, and two years later he published his early work on the integral calculus.

It was with Leibniz's first publication, in 1684, that we see the first stirrings of trouble. Newton was not well known to the public but was well known and respected among his peers. He was beginning serious work on his *Principia*, and suddenly he was confronted with the first publication of the calculus. But it was by Leibniz—and there was no mention of Newton!

Was this unreasonable? Newton's reputation in mathematics was growing among his peers in England, but he still had absolutely nothing in print, and his name would have meant little to most Continental mathematicians.

In any case, he seemed utterly unmoved. In fact, he even acknowledged in the *Principia* that Leibniz had "fallen on a method of the same kind, and communicated to me his method, which scarcely differed from mine, except in notation and the idea of the generation of quantities."[5]

Other Players

Some of Newton's followers were less sanguine about this development. John Wallis, for example, felt that Newton's notions about fluxions were passing on the Continent by the name of Leibniz's differential calculus. By 1692, Wallis was putting together a collection of his work, and he strongly urged Newton to permit him to include something about Newton's calculus. The result was a mention of it in the preface to volume 1 of Wallis's *Works* (1695) and some excerpts in volume 2 (1693). (There is some uncertainty about the dating.)

Left alone, Newton and Leibniz might even have been able to remain on good terms. In March of 1693–nine years after Leibniz's first publication–for example, he wrote to Newton, trying to renew their correspondence, and though it took a while, Newton answered in October. His manner was still friendly. Certainly, there were no hints of anger or charges of plagiary in either man's letters.

Unfortunately, there were other players in the wings, even aside from Wallis, who would influence both men's behavior.

Neither Newton nor Leibniz had students to whom they passed on their work. After Leibniz had published his 1684 paper, however, the Swiss Bernoulli brothers, Johann and Jakob, had not only figured out the method but had already put it to use and passed it on to others. They also contacted Leibniz and began to act as his champions. Johann was especially active in this area, both directly and inadvertently. In the latter case, he set in motion a series of events that may well have been the precipitating cause of the heartbreaking feud that was to erupt.

In June of 1696, he issued a mathematical challenge to the "shrewdest mathematicians in the world": determine the curve linking any two points, not in the same vertical line, along which a body would most quickly descend from a higher to a lower point under its own gravity. He gave a private copy to Leibniz and also sent copies to Wallis and Newton. This was a clear challenge to Newton's method, and Newton did indeed solve it in a day. Newton sent his answer anonymously to the Royal Society. When Bernoulli finally saw it, however, he guessed at once that the author was Newton. He recognized, he said, the "the lion from his claw."

The answer is that it is a brachistochrone, which others had been able to figure out. The curve, however, is also in the form of a cycloid,* which could be understood only through use of the calculus. Leibniz

*The path taken by a point on the edge of a rolling disc. There is more on the brachistochrone problem in chapter 4.

then did a foolish thing. In 1699, he chose to write up a review of the solutions given earlier (May 1697) in the *Acta Eruditorum*, and he put forth the solutions as a successful demonstration of his own calculus. He also noted that there were a few others who had solved it, including Newton, but the implication was that all the others had used Leibniz's calculus. Thus Newton came out looking like a copier, or as a sort of pupil, of Leibniz.

Bernoulli, too, suggested that Newton, along with the others, was in some way indebted to the Leibniz/Bernoulli group. Here are the real beginnings of trouble. Aside from Newton's dot notation, said Bernoulli, there is little difference between the two calculus methods, and since Leibniz published first . . .

Neither Newton nor his English followers could have been happy about this, but there was one follower who was particularly annoyed. Nicolas Fatio Duillier was a Swiss mathematician who had moved to England and had become friendly with Newton. He had earlier worked with Huygens and had been a member of the Royal Society since 1687. Variously described as eminent mathematician, adventurer, prophet, mystic, and rogue, he took the omission of his name from the list of "eminent mathematicians" as a personal insult.

What should be done? The general reasoning of the Newton followers might have gone something like: at this late date, having lost out to Leibniz as far as first publication is concerned, it might be best to show that Leibniz's fame on the Continent is undeserved, that his formulation is inferior to Newton's and perhaps was even copied from him.

Duillier issued a lengthy analysis of the brachistochrone problem in a paper sent to the Royal Society; but, apparently quite annoyed with Johann Bernoulli and, by association, with Leibniz, he included therein some highly incendiary words about the origins of the calculus: "I am now fully convinced by the evidence itself on the subject that Newton is the first inventor of this calculus, and the earliest by many years; whether Leibniz, its second inventor, may have borrowed anything from him, I should rather leave to the judgment of those who had seen the letters of Newton, and his original manuscripts. Neither the more modest silence of Newton, nor the unremitting vanity of Leibniz to claim on every occasion the invention of the calculus for himself, will deceive anyone who will investigate, as I have investigated, those records."[6]

There it is. Without actually charging Leibniz with plagiary, Duillier has set down in print at least the possibility, if not the implication, of "borrowing" by Leibniz.

Things are heating up.

Was Duillier's attack made with Newton's connivance? There is no sure evidence either way. Was Newton sufficiently angry at this point that he would condone such an attack? Some say no, that he had not yet reached a boiling point. Others maintain that it is not likely that Duillier would have published such an attack without Newton's consent. We must leave it there.

More interesting is what came next. Leibniz was, of course, furious, yet he still felt that Newton might be innocent since Newton had given Leibniz credit for his work on the calculus in the first edition of his *Principia.* (Alert readers will wonder why I mention "first edition." Stay tuned.)

Leibniz had published a defense of his own activities and behavior in the *Acta.* To this, Duillier attempted to publish an answer, but the editors refused to accept it, on the grounds that the journal was no place for personal disputes. The dimensions of the feud are becoming clearer. Leibniz, it is worth noting, had helped to establish the journal and exerted some influence over its editorial activities, which might explain why his defense was published while Duillier's answer was not.

And there the feud lay for a few years.

Flashpoint

By the mid-1690s, Newton's interests were turning from science and mathematics (not to mention philosophy, religion, alchemy, and mysticism) to the political/administrative arena. In 1695, he contributed to discussions regarding reform of the country's currency, and a year later he was appointed warden of the Mint. This would require a move to London and a major change in his life in many ways. He continued his work on scientific subjects, but on a much-reduced scale, for he took his duties at the Mint very seriously. He was promoted to master of the Mint in 1699.

By now, he was hobnobbing with the wealthy and the powerful, aided by his attractive and vivacious niece Catherine Barton, who (most probably) lived with him in comfortable quarters in London.

He became more interested in the Royal Society and began to attend meetings regularly. He showed an improved design for a sextant, an instrument used for determining longitude at sea, but had another run-in with Hooke. In 1701, he read a paper on chemistry at one of the meetings, which was subsequently published. At the end of the same year he resigned his professorship at Cambridge, and, in recognition of his honored position, the university elected him one of its representatives in Parliament. He didn't do much there but continued his activities at the Royal Society.

In 1703, Hooke, whom Newton had tried so assiduously to sidestep, and who provided the reason why he had not published his work on optics for three decades, died. In the same year, Newton was elected president of the Society and was reelected each year until his death. Both events were to have profound consequences for both him and Leibniz.

True to his promise, Newton finally permitted his work on optics to be published. It was his second major publication, but this time, he apparently began to think of Leibniz as competition, because now he included in the book two papers on his calculus. One was "On Quadrature," which he had begun in 1691 and never finished. It finally appeared in 1704, but only as a supplement to his great book *Opticks.*

The "Quadrature" is interesting historically, in that it is to some extent a restatement and an expansion of the second 1676 letter to Leibniz and includes a translation of the fluxional anagram he had sent to Leibniz.

Further Conflict

To this point, we have looked at the situation mostly from Newton's point of view, but Leibniz, too, was enjoying a fast-growing reputation. In 1699, for example, the French Academy of Sciences created a list of eight Foreign Associates. Newton was seventh on the list; Leibniz was first. Newton's reputation, in other words, was still growing but far more slowly on the Continent than in Great Britain. Leibniz, on the other hand, may have been disturbed by the challenge posed by Newton; perhaps this lay at the heart of his next step.

A year later, Leibniz reviewed Newton's *Opticks*, anonymously, in the *Acta.* He called the book profound but found fault with the two mathematical supplements. Bad enough. Worse was a literary comparison that reminds us of the letters of Fermat in the previous chapter of this book. Leibniz stated that Newton had used his fluxions elegantly in his *Principia* and in other publications. No problem there. But then he added, just as Honoré Fabri in his *Synopsis of Geometry* had substituted progressive motions for the method of Cavalieri.

Cavalieri, a disciple of Galileo, was an excellent mathematician. Leibniz had in fact learned from Cavalieri's writings a technique for determining the area under a curve. Fabri, on the other hand, was a lesser light who had copied the thoughts of Cavalieri. The implication seems to be that Newton substituted his fluxions for the differences of Leibniz. Certainly, this is how Newton interpreted the comparison. He also immediately suspected Leibniz as being the author of these remarks and took this as an indirect but highly suggestive implication that Leibniz was the original inventor and that he, Newton, had somehow built on Leibniz's work or copied from it.

Could Leibniz really have meant to make such a direct attack? A. Rupert Hall, who has studied the matter carefully, argues, "I do not really think that Leibniz was expressing in this most sly and secretive fashion a hot and nourished resentment against Newton, as the latter came to suppose. Leibniz worked in great haste . . . [I]n this review he did not mean to bare his inward doubts and grievances; they slipped out, with fatal results." Hall concludes, "As wit wounds when laughter is intended, so did Leibniz's too-clever historical analogy."[7] Leibniz himself would later maintain that he had not meant to imply plagiarism.

In any case, there was an uneasy truce for a few years. Then, once again, a follower of one of the two men set the blaze roaring. This was John Keill, a Scottish student of James Gregory, who became Newton's next major champion. In what might otherwise have gone down as an unimportant paper in the *Philosophical Transactions* (October 1708), Keill included a statement about "fluxions which without any doubt Dr. Newton invented first, as can readily be proved by anyone who reads the letters about it published by Wallis; yet the same arithmetic afterwards, under a changed name and method of notation, was published by Dr. Leibniz in the *Acta Eruditorum*."[8]

Newton, apparently still not looking for direct confrontation, was initially annoyed by this publication. By some shrewd manipulation, however, in which Keill presented Leibniz's review along with his own paper to the Royal Society, Newton's anger was transferred from Keill back to Leibniz, and Keill's position as Newton's champion solidified.

For a variety of reasons, Leibniz didn't see Keill's article until 1710, and there was also some more back-and-forth activity. Perhaps with some prodding from Bernoulli, Leibniz then made a major tactical mistake. He appealed directly to the Royal Society for some sort of public exoneration. In a letter to the Royal Society (February 21, 1711), he protested that he had never heard the word *fluxions* before it appeared in Wallis's *Works*, nor was it to be found in Newton's letters to him of 1676; that in general the accusations were both "absurd and contemptible."

There are several reasons why this was a tactical mistake. To begin with, the feelings between the two men had been deteriorating for years. Now there was real anger on both sides. The problem for Leibniz was that he was walking directly into the lion's den. He was appealing to the very society of which Newton was the president. True, he had addressed his letter to Hans Sloane, the secretary of the Royal Society. What was the likelihood that Newton would stay out of the process? Furthermore, the British government was bitterly hostile to the threatened Hanoverian succession, with which Leibniz was connected. The very name Royal Society should have given Leibniz pause. Anything connected with the Hanoverians had a bitter taste for the Society's members, who tended to be Newton's supporters.

All of this was surely instrumental in Newton's decision to give Keill his full support in building a case against Leibniz. Excuse me. I meant to say, in setting up an official and unbiased commission of the Society, with the objective of investigating and answering the charges brought by Leibniz. It became, however, the occasion for Newton and his colleagues to amass a huge quantity of evidence, which was summarized and published as a report in February 1713, under the title *Commercium Epistolicum*. It was published anonymously, but those in the know could see Newton's hand in the process.

It hardly seems necessary to say that the report came down on Newton's side.

Later that year, Leibniz responded with the *Charta Volans*,[9] another supposedly anonymous publication. In it, he belittled the *Commercium* and directly accused Newton and his followers of stealing the differential calculus of Leibniz and, furthermore, of committing serious errors in their use of it.

Things, in other words, were getting worse instead of better. Now Keill published an article in the May–June 1713 issue of the *Journal Literaire de la Haye*, a French literary magazine, so the battle was leaking out onto public ground. In the same year, Johann Bernoulli published some mathematical criticisms of Newton's *Principia*. Bernoulli also referred to some of Newton's comments as "twice cooked cabbage."

Not to be outdone, Leibniz published his own *History and Origin of the Differential Calculus* (1714), spelling out his own answers to claims by the British mathematicians that he had taken his methods from Newton.

Then an "Account" of the *Commercium* was published, again anonymously, and again by Newton, in the Society's *Transactions*, in 1714.[10]

Among other claims, we find Newton's attempt to show that his calculus is superior to Leibniz's: "It has been represented that the use of the letter *o* is vulgar, and destroys the advantages of the differential method; on the contrary, the method of fluxions, as used by Mr. Newton, has all the advantages of the differential and some others. It is more elegant, because in his calculus there is but one infinitely small quantity represented by a symbol, the symbol *o*. . . . It [his calculus] is more natural and geometrical. . . . Mr. Newton's method is also of greater use and certainty. . . . When the work succeeds not in finite equations, Mr. Newton has recourse to converging series, and thereby his method becomes incomparably more universal than that of Mr. Leibniz, which is confined to finite equations."[11] And so on.

The feud was spreading in another way. Leibniz had, earlier (1710), criticized Newton's theory of gravitational attraction and its associated concept of action-at-a-distance, complaining that they smacked of the occult. Now (1716), lashing out again, he began to attack and even ridicule Newton's philosophical ideas. Newton felt, for example, that the universe could be thought of as a clock, one that God had wound up at the beginning of creation. Leibniz argued that if the clock ran on forever without God's help, what need is there for God?

Newton feared that some unexplained irregularities in the planets' motions might add up and finally throw the whole solar system out of kilter. God, he felt, would step in and set things right. Leibniz ridiculed Newton's idea of God as some sort of astronomical maintenance man. Leibniz argued that God would create the best possible world, for that is the nature of God. In addition, Leibniz and others had pointed to what they called the anti-Christian influence of Newton's *Principia*, which was worrisome.

The two men also had very different ideas on their concepts of space and time. Curiously, Leibniz's ideas were in some ways more modern than Newton's. It was essentially a clash between Newton's absolute concept of space and Leibniz's relational one, and we know who eventually lost out on that one. As for the solar system, Pierre Simon Laplace later proved that the solar system is stable. At the time, of course, Newton could only feel annoyed.

Again there were some back-and-forth exchanges, to little effect.

On November 14, 1716, Leibniz died. Was that the end of the dispute? No. Newton still felt the need to keep it going, as did a few of Leibniz's followers. In 1722, Newton arranged for a second edition of the *Commercium*. It was supposedly an exact reprint, but in Latin and with a few additions at the front. It was also, supposedly, edited by Keill, but Newton was really behind it. Because the original edition was hard to come by, this edition has become the basic reference for later scholars. It is a carefully reasoned document and presents Newton's case clearly and precisely—to the clear detriment of Leibniz.

There's just one small problem. A century later, when a scholar, Augustus De Morgan, compared the two editions, he saw clearly that Newton had changed, added, and omitted passages in the text—to his advantage, of course. The depth of Newton's anger begins to come clear. And then, some 12 years after Leibniz's death, when the third edition of the *Principia* was published, Newton removed all mention of Leibniz! As he argued, second inventors have no right. Thanks to his position at the Royal Society and his growing reputation as one of the foremost mathematical scientists of all time, he almost made that a true statement.

A Question Mark

Recall the modus of Duillier and the other followers of Newton. The best, and perhaps the only, way to counter Leibniz's growing reputation as the real inventor of the calculus was to demonstrate conclusively that Newton was first, that his calculus was superior, and to suggest that Leibniz may even have copied from him. We've seen the claim to the superiority of Newton's calculus in the *Commercium.* That was Newton's opinion, and he's entitled to his opinion–but there is more.

Note the date of Leibniz's first publication: 1684. Like Newton, he seemed in no rush to publish. Though he didn't wait almost 40 years, as Newton did, he did hold back for 9 years. Today's feverish rush to get into print seems not to have held sway at the time. Each man was, perhaps, hoping to further perfect the method before going into print.

Summarizing, then, Newton was surely first in its development: 1665–1666; Leibniz: 1673–1676. Leibniz, however, clearly published first: 1684–1686; Newton: 1704–1736.

Does this help us decide anything in terms of the priority dispute? Newton and his followers, of course, believed that he, being unquestionably the first to come up with the method, deserved all the credit, but the question is not really this simple.

First, Leibniz did publish first, and, as a result, his work was taken up and began to be applied before Newton's. Leibniz's notation was also superior and is the form we tend to use today. So posterity disagreed with Newton's claims of superiority.

As I noted earlier, Newton's first major publication in mathematics appears as a supplement to the *Opticks* book. Its overall title is

"Two Treatises of the Species and Magnitude of Curvilinear Figures"–work that he *may* have developed as early as the mid-1660s, but that did not see the light of day until 1704.

Similarly, Newton's next writeup of his technique, titled *Method of Fluxions and Infinite Series*, written in 1671, was not published until 1736 in English, and in Latin even later.

In other words, when Newton's publications began to appear, Leibniz had already become a threat. Newton and his followers could make a variety of claims, which may or may not have been true. They could say that Newton already had his calculus in hand by the middle and late 1660s; that he was already using the dot notation early on, whereas it may be that he actually did not begin using it until after he had seen some of Leibniz's work. Several historians maintain that Newton actually did not *begin* using his dot notation until the early 1690s.[12]

In the *Commercium*, Newton claims that he had written his Quadrature treatise as early as 1676, which it was later found was not so. He had actually written it in 1691.

Another claim in the *Commercium* is that Leibniz had seen a letter of Newton's dated December 10, 1672, concerning a problem on the tangent in which the method of fluxions was sufficiently described that "any intelligent person" would be able to come up with it. Later scholars agree that, first, any intelligent person would not be able to build a calculus on such flimsy hints; second, that Leibniz never did see the letter;[13] and third, that Newton knew, or should have known, this.

Wallis also contributed, perhaps inadvertently, to the barrage being launched against Leibniz. When he included material on Newton's calculus in his *Works* of the 1690s, he stated in his text that what he published was what Newton had sent to Leibniz in his letters, which was not the case at all. In fact, the collection he put together was not based on the original documents but on copies in which various passages were adapted to suit Newton's purposes.

In other words, much of what we "know" today about Newton's early work has come down to us via writings created *after* Leibniz had already become a threat. This doesn't automatically make them untrue, but they must be taken with a grain of salt.

Was There Plagiarism?

Rumors are given wings when there is both motive and plausibility. As the feud heated up, both sides were accusing the other side of plagiarism.

Certainly, there was motive on both sides. Both men felt that their reputations were on the line, and both had been prodded and poked by others until they were angry enough to do things that they might not have done—or permitted to be done—otherwise.

As for plausibility: Leibniz had seen some of Newton's papers. Furthermore, Newton had kept a few of his colleagues informed of his progress, some of whom were in contact with Leibniz. So Leibniz *could* have stolen from Newton. That in no way says he did.

The Newton side continued to complain about the fact that Collins showed Leibniz some of Newton's papers. Even here, though, it is not certain how much of Newton's calculus was contained in these papers. Current thinking is that there was very little, or at least that he used very little, and that what Leibniz saw and used had more to do with infinite series than with Newton's further work on the calculus itself.

Did Newton use the work of Leibniz? Here the plausibility scenario is even less certain. As we have seen, Newton and colleagues apparently had no compunctions about later changing facts to fit their own agenda. Yet after much thought and research by people who studied the matter in later years, the general consensus is that while both men can be accused of some impolite and even nasty conduct, neither was guilty of any form of plagiarism. That is, that each man came up with his calculus independently and without any direct input from the other.

Unexpected Outcome

It took a while for Newton's fame to spread, particularly on the Continent—but it did. At the same time, Leibniz's light seemed to go out, thanks to Newton's help, and toward the end of their respective lives, their situations were different indeed. Newton was idolized and had been knighted. When he died in 1727, he was given a state funeral, and he still lies buried in a prominent position in the nave of Westminster Abbey.

Leibniz's situation was very different. For him, nothing seemed to go right. In 1714, when the elector of Hannover, his employer, became George I of England, Leibniz even lost favor in his own court. This was almost surely because, amid all the diplomatic maneuvering, he was on the losing side of this important feud. In addition, he had tried to get the Roman Curia to release Galileo's *Dialogue* from the Index, also without success. He had hoped to help unify the Catholic and Protestant Churches, with obvious lack of success.

When Leibniz died in Hannover in 1716, unfulfilled in his many schemes, it seemed that he had hardly a friend in the court where he had labored for almost four decades. His funeral was attended by no one other than his former secretary. A friend noted in his memoirs that Leibniz "was buried more like a robber that what he really was, the ornament of his country."[14]

As a sort of final blow to the poor man's name, the French satirist Voltaire would do some serious lampooning of Leibniz with his *Candide* in 1759. It was to be Voltaire's most famous single work. Although a savage satire on 18th-century life and thought, it took particular aim at Leibniz. While the hero of the story is Candide, his mentor is Dr. Pangloss, a disciple of Leibniz. In spite of an extraordinary set of seriocomic misadventures, Pangloss maintains, as did Leibniz, that all is for the best in this best of all possible worlds. (Leibniz certainly spouted the best-possible-world part.) Voltaire was a devoted advocate of Newtonian ideas and did much to help spread them on the Continent. Recall, too, Leibniz's hope that his calculus might be able to unlock the secrets of human behavior. Voltaire ridiculed this idea as well.

Who Deserves the Credit?

Does Leibniz deserve any credit? Newton thought not, particularly in his later years.

There Newton was wrong, on at least two counts. First, he may not even have realized that the calculus he had come up with was a true advance. In other words, it wasn't until Leibniz and his followers showed the way that Newton understood that they had a general method that could be widely applied.

More important, however, is what happened in the years that followed the dispute. Although it was no longer going on between the two men, it had important repercussions. To put it simply, the British mathematicians stayed loyal to their man and would use only his calculus and his notation. On the Continent, however, from the very time of Leibniz's first publication, Leibniz's followers–and particularly the two elder Bernoulli brothers–took his new mathematics in hand and put it to work.

There were thus two important results of the feud. One was a breach between the two sets of mathematicians that lasted until the 19th century, which prevented the benefits that might have come from intercommunication between the two.

The second is even more significant: the rapid strides taken by mathematicians on the Continent–based largely on Leibniz's calculus–far outstripped those of the British during all of the 18th century! Here is where it may finally be said that Leibniz lost the battle but won the war.