Dismissis Incruciationibus

Montpellier Workshop summary by Paul Garcia

The original purpose of this workshop was to introduce to secondary school teachers the idea of using history in English A level mathematics syllabuses, and in particular in the new Nuffield Advanced Mathematics course.

There were three themes:

1. The intellectual struggle of mathematics; that is, mathematics is a creative process and ideas emerge after long and difficult periods of gestation.
2. The personalities of mathematicians; mathematicians, even geniuses, are still human and subject to the same kinds of personality problems as ordinary mortals.
3. The continuing development of mathematics; even today, the utility of ideas causes disputes between mathematicians.

Section 1

The first theme was to show how those concepts which we regard as being elementary may not always have been so obvious, even to the great geniuses of mathematical history. The struggle to develop ideas is rarely, if ever, made clear to students. They assume from standard text-book presentations of mathematical topics that the development of the idea was straightforward and obvious. This often causes the student to believe that he or she is stupid for not being able to immediately comprehend the subject. Teachers are also guilty of not taking into account that many concepts are difficult when encountered for the first time. To illustrate my point, I outlined the history of the development of complex numbers, beginning with Cardan's method for solving $x(x-10)=40$ in chapter 37 of the Ars Magna, printed in 1545, from which the title of the workshop, Dismissis Incruciationibus, is taken.
To begin, I put up a slide with a number of quotes from mathematicians on the subject of complex numbers:

"....... as subtle as it is useless" **Cardan**, 1545

"...useless and sophistic" **Bombelli**, 1572

"......imaginary" **Descartes**, 1637 (La Geometrie)

".....a sort of amphibian, halfway between existence and non-existence" **Leibniz**

"....absurd, void of meaning, self-contradictory...but of great utility"
De Morgan, 1831

The booklet accompanying the workshop contains some writing by Albert Girard (1595 - 1623) from his 1629 work 'L'invention nouvelle dans l'algebre'. Participants were asked to read this and try to interpret into modern language and notation precisely what Girard was describing, and how it leads to complex numbers.

I then outlined how Newton (1642-1727) did not regard complex numbers as significant because of their lack of physical reality, and how D'Alembert (1717-1783) tried to prove Girard's fundamental theorem, and was only able to prove the form of a root, but not its existence, but managed to write a dissertation on complex numbers almost as a by-product. Euler later provided a better algebraic proof of Girard's theorem, and showed that if a root x+iy exists, then x-iy is also a root (and started to use the symbol i, later made popular by Gauss).
A third piece of writing, from 1806 by Argand, taken from the IREM papers, is used to show the emergence of the geometric representation for complex numbers, first written about by Wessel (1745-1818) in 1797

The fact that as late as 1831 De Morgan made the remark quoted above shows that complex numbers were not an obvious or even acceptable extension of the number system for nearly 300 years.

Section 2

The second theme I illustrated by means of the famous dispute between Leibniz and Newton over the invention of the calculus. I used three extracts, lasting a total of about 30 minutes, from a BBC Radio production called 'The Virgin Fathers of the Calculus', a dramatised history of the dispute. Accompanying the extracts is a complete transcript of the whole (50 minute) broadcast, and a separate leaflet containing short biographies of the protagonists and a chronology of events during dispute (prepared as part of the Nuffield materials and intended to be a supplement to the main course and the History of Mathematics option).
Between the first and second extracts I asked participants to look at copies of a letter from Newton (found in the workshop handbook pages 11 and 12) describing his method of series and a copy of his unsolvable anagram, both referred to in the broadcast. I also showed a slide showing the problem Newton described in the letter in modern notation.
At the end of the extracts I used two slides to show Leibniz's Transmutation Rule, and asked participants to try to translate it into modern notation.
There are other materials in the handbook to which I did not refer, but were provided for teachers interested in pursuing some of the ideas with students.

Section 3

The third theme was illustrated by the recent development of chaos theory, and in particular the invention of fractals by Benoit Mandelbrot. I outlined the history of the subject from the discovery of chaotic effects by Edward Lorenz in 1960 to the discovery of the Mandelbrot set, and asked participants to look at some articles and correspondence (pages 25 - 26 in the handbook) published in the New Scientist magazine in 1990 in which the utility of fractals is attacked by a mathematician, Stephen Krantz, and defended by Mandelbrot himself. The handbook also contains some further material to which I did not refer.

Paul Garcia
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The Virgin Fathers of the Calculus

broadcast transcript

(start of first extract)

When two scientists laid claim to the same discovery that contest for paternity of the brain child is often both bitter and deceitful. The history of science is liberally peppered with such priority disputes borne from the very nature of scientific enterprise. Of all priority disputes through the ages none compares in ferocity or duration to the famous 17th century conflict between Isaac Newton and Godfried Wilhelm Leibnitz as to who should take credit for the calculus.

Professor Lewis Wolpert of University College London presents The Virgin Fathers Of The Calculus, a documentary drama with David Kelsey as Isaac Newton and Albert Welling as Godfried Wilhelm Leibnitz.

Isaac Newton was a genius, undoubtedly the greatest British scientist there has ever been. Born in Lincolnshire prematurely one hour after midnight on Christmas Day 1642, the 17th century metaphysicals would have remarked on the fullness of the moon. His mother Hannah didn't expect her child to live and her feeling that his survival was a miracle was communicated to him at an early age. Newton's father, a Yeoman who couldn't sign his name, had died three months before he was born and when the child was 3 his mother remarried.

At the age of 30 Hannah left Newton in the care of his maternal grandmother and moved a mile and a half away to live with the Reverend Barnabus Smith. It was a marriage of financial convenience but child and mother remained largely separated until, when Newton was 11, Barnabus Smith died and Hannah returned. Newton's mother, biographers agree, was the central figure of his life and his separation from her, albeit temporary, proved a traumatic event from which he never recovered. His fixation upon her was absolute and her original departure generated a mixture of anguish, aggression and fear that never left him.

At Trinity College Cambridge, the 20 year old Newton confessed his guilt as sin number 13 in the Fitzwilliam notebook; "Threatening my step-father and mother Smith to burn them and the house over them".

Throughout his life, whenever Newton's work was challenged, he reacted with the same inner fury generated by this first searing deprivation. He saw all his later inventions as part of his spiritual self and the mere threat of them being questioned would overwhelm and discolour his every move and response.

Godfried Wilhelm Leibnitz, just three years younger than Newton, was also a genius. An astonishing polymath, he made major contributions to many branches of science, mathematics and philosophy. It is said that Leibnitz was capable of thinking for several days at a stretch remaining in one chair. An indefatigable worker and a statesman of considerable influence, he travelled Europe winter and summer and corresponded with over 600 fellow scholars. In 17th century science he was a giant amongst giants.

Whilst Newton's formative and creative years were financially secured by Hannah's second marriage, Leibnitz always needed to earn his living in the service of wealthy patrons. He was trained in law and spent much of his life researching the family history of his rich and famous employers. At the age of 27 while working in Paris for the Archbishop of Mainz his patron died. In a sense between jobs, Leibnitz now pursued his hobby, an interest in science. He invented a calculating machine and came to London to present it at Europe's newest and most important centre for the announcement of scientific advances and the free exchange of ideas.

The Royal Society had been born in the middle of the 17th century and Charles II granted his first royal charter on July 15th 1662. The first Secretary of the Society was Henry Oldenburg who, like Leibnitz, was of North German origin. But whilst his calculator was not the success he had hoped for, Leibnitz returned home satisfied that he had made important contact with Oldenburg and Fellows of the Society.

> "March 8th, 1673. As soon as I arrived safely at Paris it was among my first tasks to write you a grateful letter and take the first step towards a correspondence. You may certainly and safely promise on my behalf that I will strive to prevent such great and illustrious men of the Royal Society from regretting their kind reception of so insignificant a person, though one inspired by the best intentions."

Shortly afterwards, the ambitious and diplomatic Leibnitz was delighted when he learned that he had been elected to the Royal Society. His correspondence with Oldenburg now became more mathematical. He was essentially an amateur mathematician and was keen to know just what had been discovered by the British, particularly as it affected his own work. Leibnitz in the early 1670's was working on problems that would lead to his original discovery of the calculus. However, and this is the core of the dispute, Isaac Newton had already made the discovery for himself. In the plague years of 1664, 1665 the 24 year old Newton, whilst thinking of gravity and optics, had developed what he called his Theory of Fluxions, essentially the calculus. Before the 1670's, Newton had written his book on analysis which contains the idea of fluxions. His friend, the mathematician John Collins, had urged him to publish but Newton was reluctant. He kept fluxions to himself, didn't publish, and Leibnitz was therefore pursuing something that was to all intents essentially unknown.

The year is 1676 and Isaac Newton is by now the 34 year old Lucasian professor of mathematics in Cambridge. He is himself a member of the Royal Society with a rapidly advancing reputation. When Leibnitz writes to Oldenburg describing some of his own achievements and asking some mathematical questions, Oldenburg passes the questions on to Newton. With Newton's reply a correspondence begins between himself and Leibnitz using Oldenburg as an intermediary.

> "June 13th, 1676. Most Worthy Sir, Though the modesty of Mr Leibnitz pays great tribute to our countrymen for a certain theory of infinite series, I have no doubt that he has discovered a similar, if not even better, method than our own. But since, however, he very much wants to know what has been discovered in this subject by the English and since I myself fell upon this theory some years ago, I have sent you some of those things which occur to me in order to satisfy his wishes. At any rate, in part."

> "Godfried Wilhelm Leibnitz to the illustrious Henry Oldenburg. Your letter contains more numerous and more remarkable ideas about analysis than many thick volumes published on these matters. For this reason, I thank you as well as the very distinguished men, Newton and colleagues, for wanting me to partake of so many excellent speculations. Newton's discoveries are worthy of his genius. His method of obtaining the roots of equations and the areas of figures by means of infinite series is quite different from mine so that one may wonder at the diversity of paths by which one can reach the same conclusion."

To Newton this was covering old ground. He had moved on from optics and the calculus and his great mind was now working on different problems. In 1672 he had demonstrated his theory of colours to the Royal Society and made his first famous enemy, Robert Hook. When Newton was elected to the Society, Hook was already established as its most outstanding inventor and experimenter but when he challenged Newton's work on colour the response was both fierce and long-lasting.

Newton threatened to resign from the Society and was to challenge Hook's work for ever more. This gives us a clue as to why Newton was reluctant to publish some of his discoveries. Perhaps he saw himself as constantly under the surveillance of rivals whose only intention was to find fault in his work. More pertinent, it is well-known that Newton was deeply religious and very involved in biblical interpretation. As an alchemist and hypochondriac his search to discover the elixir of life was based upon his own understanding of the scriptures. There is evidence that he saw himself as God's prophet, potentially immortal and personally responsible for the re-discovery of the laws of nature which were embedded in the Bible. If that was so, there was not only little incentive for him to publish his work, it would also follow that those who challenged him could be construed of being possessed by evil.

When Newton received Leibnitz's second letter requesting yet more information on matters relating to the calculus, his response was already less generous.

> "Cambridge, 24th October 1676. Dear Oldenburg, To Mr Leibnitz's ingenious letter I have returned an answer which I doubt is too tedious. I could wish I had left out some things since to avoid greater tediousness I left out something else on which they have some dependence but I had rather you should have it anyway than write it over again. Sir, I am in great haste, Yours, Isaac Newton. PS. I hope this will so far satisfy Mr Leibnitz that it will not be necessary for me to write any more on this subject. Having other things in my head it proves an unwelcome interruption."

Included with this letter to Oldenburg, Newton enclosed a separate letter intended for despatch to Leibnitz.

> "Most Worthy Sir, I can hardly tell with what pleasure I have read the letter of the very distinguished Leibnitz. His method is certainly very elegant and it would have sufficiently revealed the genius of its author even if he had written nothing else and what he has scattered elsewhere is most worthy of his reputation. It leads us also to hope for very great things from him."

Newton then gave a long and detailed response to Leibnitz's questions but when it came to his fluxions he held back, concealing his discovery in a tantalising and unsolvable anagram.

> "Nobody if he possessed my basis could draw tangents any other way unless he were deliberately wandering from the straight path and the same is true in questions of maxima and minima and in some others too. The foundation of these operations is evident enough but because I can not proceed with explanation of it now, I have preferred to conceal it thus; 5 a's, 2 c's, 2 d's, 6 e's, 3 f's, g 9 i's, 3 l's, 11 m's, 5 o's, 2 q's, r, 3 s's, 5 t's, 3 u's, 5 v's, w, x, y."

Newton's anagram concealed the sentence; '*Given any equation involving flowing quantities to find the fluxions and vice versa*'. In fact, this was a hidden claim for priority for his discovery of fluxions to be revealed and decoded at a later date when and if necessary. This was common practice in the 17th century when printers were reluctant to accept books on mathematics because of difficulties with type-setting and a limited potential readership. So, when a mathematician discovered something he would often register his claim for priority in a letter pending possible publication at a later date.

When mathematicians wrote to one another swapping ideas or communicating results, they seldom gave away valuable information without receiving something in return. Their most prized possessions, that is any original discovery, would be referred to but disguised beyond value or clue to the recipient. In this way, Leibnitz would have been neither surprised nor offended by Newton's use of cryptography.
(end of first extract)

But there is another more telling reason for this lack of surprise. Before Leibnitz even received Newton's letter, he had made a brief visit to London en route to Hanover. Surprisingly, he didn't visit Newton. However, in October 1676, he visited John Collins who showed him Newton's unpublished book on analysis containing the theory of fluxions. This wouldn't have helped Leibnitz in any way since he had already made the decisive steps towards his own invention of the calculus the previous year.

> "June 21st, 1677. Leibnitz presents compliments to Henry Oldenburg. Most Honourable Sir, I have received your long-awaited letter together with its enclosure of Newton's truly excellent letter. I shall read it more than once with the care and attention which it certainly deserves. I am enormously pleased that he has described by what path he happened on some of his really very elegant theorems."

Leibnitz went on to describe some of the details of his differential calculus in terms which Newton must have clearly understood. But just then, Henry Oldenburg, the fulcrum for the entire correspondence, died, bringing the period of exchanging letters via the secretary of the Royal Society to an end. However, with reference to the upcoming dispute it is crucial to recognise that from this moment in 1677 both Newton and Leibnitz knew that the other had discovered a method of calculus. From now on their relative behaviour should be measured in the light of this knowledge.

Leibnitz continued his mathematical work and 8 years later in 1684 published a short paper describing the calculus in a new scientific journal based in Leipzig. This was the very first public appearance of the calculus. In spite of the brevity of the article, the power of this new mathematical technique was rapidly recognised particularly on the Continent. The importance of the discovery can not be over-emphasised. It provides the mathematical technique for describing change. If, for example, one has a curve which describes the movement of an object then the calculus enables one to calculate the speed and acceleration of that object at any instant. It also allows one to find the maximum and minimum speeds and accelerations. It's power to deal with change, whether it be a moving object, a shifting population, the flow of water, or a chemical reaction, is enormous. Its discovery brought about a revolution in mathematics and it is still fundamental to modern mathematics and engineering.

Newton responded to the Leibnitz paper three years later when, in 1687, he inserted a small paragraph about his theory of fluxions in his great Principia. He was now publicly staking his claim for priority.

> "Ten years ago in letters exchanged between myself and that most skilled geometer G. W. Leibnitz, I indicated that I possessed a method of determining maxima, of drawing tangents and performing similar operations, which served for irrational terms just as well as for rational ones. I concealed my method in transposed letters which, when correctly arranged, expressed this sentence: 'Given any equation involving flowing quantities to find the fluxions and vice versa.' That famous person replied that he too had come across a method of this kind and imparted a method to me which hardly differed from mine except in words and notation."

How Newton must now have regretted not publishing his theory of fluxions 22 years earlier. However, if he was not prepared to take a more active role in staking his claim for priority, others were. Fatio de Duiller was a volatile young Swiss and a brilliant mathematician in his own right. He became an ardent admirer of Newton who was to treat him with a rare kindness and affection. Fatio resented the idea that Leibnitz should be thought of as having invented the calculus. In 1691 he wrote to the great Dutch scientist, Huygens:

> "18th December, 1691. Marvellous Sir, It seems to me according to all that I have seen so far that Mr Newton is undoubtedly the first author of the differential calculus and that he knew it as well or better than Mr Leibnitz knows it now. It appears that the latter gained an inkling of the subject only when Mr Newton wrote to him so I can not be sufficiently surprised that Mr Leibnitz does not mention anything about it."

Leibnitz, who was now historian to the House of Hanover, knew nothing of this and wrote to Newton in order to renew contact.

> "To the Celebrated Isaac Newton, March 7th, 1693. Godfried Wilhelm Leibnitz, sends cordial greetings. How great the debt owed to you by our knowledge of

mathematics and of all nature. I have acknowledged this in public whenever occasion offered. You have given an astonishing development to the geometry by your series but when you published your work, The Principia, you showed that even what is still not understood by others is an open book to you. I write this rather that you should not understand my devotion to you, a devotion that has lost nothing by the silence of so many years. Heaven forbid that with empty and worse than empty letters I should interrupt the devoted studies by which you increase the patrimony of mankind. Farewell."

"October 16th, 1693. Dear Leibnitz, I did not reply at once on receipt of your letter because it slipped my hands and was long mislaid among my papers. This vexed me since I value your friendship very highly and have for many years considered you one of the leading geometers of this century. Although I do my best to avoid philosophical and mathematical correspondences, I was however afraid that our friendship might be diminished by silence and this at the very moment with the letters I once wrote you in which I concealed my method of fluxions in the form of an anagram are about to be published. I hope indeed that I have written nothing to displease you and if there is anything that you think deserves censure please let me know of it by letter since I value friends more highly than mathematical discoveries."

Among the leaders in the enthusiastic further development of the calculus were the two Swiss brother mathematicians, Jacob and Johann Bernoulli, both colleagues of Leibnitz. The brothers initially worked together and Johann reported that on reading Leibnitz's paper it was only a matter of a few days to unravel all its secrets. In 1696 Johann Bernoulli, in order to show the power of the calculus, put forward a mathematical challenge to the ablest mathematicians in the world. The problem was to determine the line of the quickest descent of a body falling to some point not directly below it. Newton with his theory of fluxions solved it in an evening. Only four other mathematicians solved the problem, Leibnitz was one of them:-

"It is surely worthy of remark that t hey only solve the problem whom I guessed would be capable of solving it as being those alone who had penetrated sufficiently deeply into the mystery of our calculus."

"Our Calculus". Fatio was shocked and insulted and so probably was Newton with whom he shared an intimate relationship. When Fatio published a book in which his position with respect to Newton and Leibnitz was made very explicit, it is hard to believe that Newton did less than influence what he wrote.

"I recognise that Newton was the first and by many years the most senior inventor of the calculus. As to whether Leibnitz its second inventor borrowed anything from him I prefer to let those judge enough also see Newton's letters and other manuscript papers, not myself. Neither the silence of the more modest Newton nor the eager zeal of Leibnitz in ubiquitously attributing the invention of this calculus to himself will impose on any who have perused those documents."

The dispute over who had invented the calculus now entered a new phase, concealed deception giving way to open conflict. Leibnitz was stunned by Fatio's comments and published an article.

"One can readily conceal under a desire for justice sentiments which when plainly acknowledged would disgust us. Fatio fabricates suspicions that another person has won fame not by the straight road but by devious practices. In truth, the more I understand the defects of the mind the less I grow angry at any aspect of human behaviour. When I published the elements of my calculus in 1684 there was assuredly nothing known to me of Newton's discoveries in this area beyond what he had formally signified to me by letter. In fact, I did not know until recently that he practised a calculus so similar to mine."

Now Newton's supporters gathered force. The dominance of Continental mathematicians in the use and development of the calculus was obvious and Newton's contributions were being ignored. John Kiell, an ardent Newtonian working at Oxford University, now took up the cudgel and launched his attack upon Leibnitz. Perhaps he wished to impress Newton.

In a paper in the Philosophical Transactions of the Royal Society which appeared in 1710, he gratuitously inserted a statement that:

> "All these things evolve from the nowadays highly celebrated arithmetic of fluxions which Mr Newton beyond any shadow of a doubt first discovered, as anyone reading his letters will readily ascertain. And yet, the same arithmetic was afterwards published by Mr Leibnitz having changed the name and the symbolism."

Leibnitz now found himself under attack in a journal of the Royal Society of which both he and Kiell were members. This was an insult that could not be ignored. He wrote at once to the Secretary of the Society, Hans Sloan.

> "Berlin March 4th, 1711. Dear Secretary Sloan, I wish that an examination of the Philosophical Transactions did not compel me to make a complaint against your countrymen for the second time. Some while ago, Fatio de Duiller attacked me in a published paper for having attributed to myself another's discovery. I taught his to know better. As I understand things, Newton a truly excellent person disapproved of this misplaced zeal on behalf of your nation and himself. And yet, Mr Kiell in this very volume has seen fit to renew this most impertinent accusation when he writes that I have published the arithmetic of fluxions invented by Newton after altering the name and style of the notation. Whoever has read and believed this could not but suspect that I have given out another's discovery disguised by substitute names and symbolism. But no-one knows better than Newton himself how false this is. However, though I do not take Mr Kiell to be a slanderer I am driven to seek a remedy from your distinguished Royal Society for I think you yourself will judge it equitable that Mr Kiell should testify publicly that he did not mean to charge me with that which his words seem to imply. For the rest, farewell and thrive."

Kiell did not hesitate to reply and his letter was read out at a meeting of the Royal Society on 24th May, 1711. The Society decided to transmit it to Leibnitz without further apology.

> May 1711 a letter from John Keel, MA, FRS of Christchurch Oxford to the very famous and learned Hans Sloan MD, Secretary of the Royal Society.
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> "Famous Sir, Be so good as to receive what I have thought fit to reply. I perceive that this remarkable person has complained bitterly about me as though I had done him an injury transferring to another the glory of things discovered by himself. Though I confess that this complaint troubles me exceedingly, for I would not wish men to have formed the opinion that out of a desire to broadcast slanders, I would disparage anyone who is skilled in mathematics not to say a person who is most distinguished in that way. Nothing certainly is further from my nature than to deprecate anything in another man's work. Surely the merits of Leibnitz in the world of learning are very great. This I freely acknowledge, nor can anyone who has read his contributions deny that he is most learned in the more obscure parts of mathematics. But since he possesses so many unchallengeable riches of his own, I fail to see why he wishes to load himself with spoils stolen from others. Accordingly, when I perceive that his associates were so partial towards him that they heaped undeserved praise upon him, I supposed it no misplaced zeal on behalf of our nation to endeavour to make safe and preserve for Newton what is really his own.

Hear, hear! Hear, hear!

"For as it was proper of those of Leipzig to pin on Leibnitz others' garlands, so it is proper for Britain's to restore to Newton what was snatched from him without accusations of slander."

"Hamburg, December 29th, 1711. Godfried Wilhelm Leibnitz presents a grand salute to the very celebrated Mr Hans Sloan, Secretary of the Royal Society. What Mr John Kiell wrote to you recently attacks my sincerity more openly than he did before. No fair minded or sensible person will think it right that I at my age and with such a full testimony of my life should state an apologetic case for it, appearing like a suitor before a Court of Law, against a man who is learned indeed but an upstart with little deep knowledge of what has gone before and without any authority from the person chiefly concerned. Thus I throw myself upon your sense of justice. Farewell."

By now, Newton was President of the Royal Society, Master of the Royal Mint, had been knighted and was famous throughout Europe. He addressed the Royal Society at its meeting of the 14th February, 1712.

"Gentlemen. The letter of Mr Leibnitz which was read before you when I was last here, relating to myself and Mr Kiell, I have considered and can acquaint you that I did not see the relevant papers 'til last summer and therefore had no hand in beginning this controversy. The controversy is between the author of those papers and Mr Kiell and I have as much reason to desire that Mr Leibnitz would set the matter right without engaging me in a dispute as Mr Leibnitz has to complain of Mr Kiell for questioning his candour and to desire that I would set the matter right without engaging him in a controversy with Mr Kiell. And if that author in giving an account of my book of quadratures gave every man his own as Mr Leibnitz affirms, he has taxed me with borrowing from other men and thereby has opposed my candour as much as Mr Kiell has opposed the candour of Mr Leibnitz. Mr Leibnitz and his friends allow that I was the inventor of the method of fluxions..

Hear, hear! Hear, hear!

"and claim that he was the inventor of the differential method.

Rubbish!

"Both may be true because the same thing is often invented by several men."

(start of second extract)
The Royal Society decided to appoint a Committee to look into the dispute and in a very short time, just fifty days, a report was ready. Most astonishing for its audacity was the choice of its writer.

"Gentlemen. We have consulted the letters and the letter books in the custody of the Royal Society and those found among the papers of Mr John Collins dated between the years 1669 and 1677 inclusive and have extracted from them what relates to the matter referred to us. All which extracts herewith delivered to you we believe to be genuine and authentic and by these letters and papers we find:

"First, that Mr Leibnitz was in London in the beginning of the year 1673 and went thence to Paris where he kept a correspondence with Mr Collins by means of Mr Oldenburg 'til about September 1676 and then returned by London and Amsterdam to Hanover, and that Mr Collins was very free in communicating to him what he had received from Mr Newton.

"Secondly, that by Mr Newton's letter of the 13th June, 1676, it appears that he had the method of fluxions about five years before the writing of that letter.

"Thirdly, that the differential method is one and the same with the method of fluxions excepting the name and the mode of notations.

"Therefore, we take the proper question to be not who invented this or that method, but who was the first inventor of the method, and we believe that those who have reputed Mr Leibnitz the first inventor knew little or nothing of his correspondence with Mr Collins and Mr Oldenburg long before nor of Mr Newton's having the method above fifty years before Mr Leibnitz published. For which reasons, we reckon Mr Newton the first inventor and are of the opinion that Mr Kiell in asserting the same has been no ways injurious to Mr Leibnitz."

The report's vigour in damning Leibnitz was hardly surprising since Newton wrote it. The Committee was entirely under his influence. Described officially as an impartial Committee of gentlemen from several nations, it was packed full of Newton's supporters, all their names being kept secret until many years later.

Meanwhile, Leibnitz although officially still working on his history of the House of Hanover knew nothing about it. He was now temporarily living in Vienna where his influence with, amongst others, the Tsar Peter the Great had led the emperor to appointing him an Imperial Privy Councillor. The Elector of Hanover wanted him back but Leibnitz was enjoying his work and was trying to establish in Vienna a new Society of Sciences to rival the Royal Society.

That Society now published its so-called impartial report on the dispute as an official document. Johann Bernoulli was very sympathetic and wrote to Leibnitz:

"Basle, May 27th, 1713. My Dear Leibnitz, this hardly civilised way of doing things displeases me particularly. You are at once accused before a tribunal consisting as it seems of the participants and witnesses themselves. Then documents against you are produced, sentence is passed, you lose the case and you are condemned. But I am driven to break off for the present. I do indeed beg you to use what I now write properly and not to involve me with Newton and his people for I am reluctant to be involved in these disputes or to appear ungrateful to Newton who has heaped many testimonies of his goodwill upon me. Farewell."

"Vienna, June 17th, 1713. My Dear Bernoulli, I have not yet seen the little English book directed against me and those idiotic arguments which they have brought forward deserve to be lashed by satirical wit. They would maintain Newton in possession of his own invented calculus. He knew fluxions but not the calculus of fluxions which as you rightly judge he put together at a later stage after my own was already published. Thus, I have myself done him more than justice and this is the price I pay for my kindness."

Leibnitz now had to defend himself. His response was to publish an anonymous pamphlet known today as the Charter Volans or 'flying sheet'. Taking the offensive, he suggested that Newton's fluxions had only been developed after he had seen Leibnitz's calculus. Now, Newton was accused of being the second inventor and the plagiarist.

"29th July, 1713. As Leibnitz is now living in Vienna he has not yet, because of the distance between the places, seen the little volume lately published in England in which certain people endeavour to claim the first discovery of the differential calculus for Newton. But when he learnt later that his own simplicity had been turned against him and that certain persons in England with an unnatural xenophobia had gone so far that they meant not merely to impress Newton among the discoverers but to exclude himself from their number, he decided to consider the question more carefully.

"Because his business prevented him from looking sufficiently deeply into the matter at the time, he consulted a leading mathematician most skilled in matters and free from bias. After considering everything, this leading mathematician declared himself as follows: "*It is clear that the way of doing the calculus was not known to Newton until long after it was familiar to others.*"

The leading mathematician Leibnitz anonymously referred to was of course his colleague Johann Bernoulli, and shortly afterwards, when writing to Bernoulli he expressed what he really felt.

"Vienna, August 8th, 1713. Paying no attention to Kiell and others of Newton's toadies, I am compelled to blame Newton himself for insincerity and acts contrary to the dictates of conscience, for he himself cannot but know that infinitesimal analysis could not have been furnished by him to me. And yet he caresses and indulges the ignorant triflers who assert such things. I understand that in England there are amongst the learned those who do not approve this procedure, which is an affront to grave and good men and your guess just about hits the nail on the head, that is, that those who have little love for the House of Hanover have also meant to wound me. An English friend has even written to me that it seems that certain persons have acted not as mathematicians and Fellows of the Royal Society against a Fellow but as Tories against a Whig. But I shall, I believe, get out such a little paper as will make them smart for their nonsense. For the rest, farewell and flourish.

""November the 22nd, 1713. Like you, I think that Mr Newton will sometimes smart for so easily lending his ear to flatterers. Meanwhile, it will be wise for you to concentrate on your reply. Finish it in good time and lay it before the public lest they should have reason to rejoice in the delay. Especially it seems that Kiell deserves to be publicly satirised because he has been the most eager of Newton's toadies and the most unjust to foreigners, but I fear that you will have no leisure for this affair before you see your own fireside."

(end of second extract)

On August 1st, 1714, Queen Anne died and that month Leibnitz's employer, the Elector of Hanover, became George I of England. Leibnitz wanted to get closer to the dispute, raced back from Vienna to petition for the job as histiographer of England, his reward for forty years faithful service to the Hanoverian Court. George, however, was still displeased with him for his two year sojourn in Vienna and ordered Leibnitz to remain in Hanover and not to undertake any further long journeys until the books of his family's history were complete. Indeed, there is evidence that the new King had a somewhat brutish streak in his nature and sought amusement at the prospect of two heroes from either side of his realm in open dispute.

The situation looked more hopeful for Leibnitz to win towards the end of 1715. He learned he was about to gain a much needed advocate. The Abbe Conte, a Venetian aristocrat and a previous colleague, wrote him a letter telling him he was travelling to London and promising to support his cause. On his arrival, Conte, somewhat playfully but with the support of the King, adopted the role of an intermediary. For the first time Newton was drawn into the open and his direct letter to Leibnitz via Conte was duly despatched.

"London, 26th February, 1716. Sir, You know that the report of the Royal Society contains the ancient letters and papers preserved in the archives and letter books relating to the dispute between Mr Leibnitz and Dr Kiell and that they were collected and published by a Committee of gentlemen from several nations appointed by the Royal Society for that purpose. Mr Leibnitz has hitherto avoided returning an answer to the same which is because the book is matter of fact and incapable of any such answer. To avoid answering it he pretended the first year that he had not seen this book nor had leisure to examine it. He then desired an eminent mathematician or pretended mathematician to insert a defamatory letter and published this in Germany without the name of the author, or printer, or city where it was printed. The whole of this has since been translated into French and inserted into another abusive letter, I suspect by the same author.

"Hitherto, Mr Leibnitz avoided returning an answer to the Royal Society report by pretending that he had not seen it. And now he avoids it by telling you that the English shall not have the pleasure to see him return an answer to their slender reasoning as he calls it and by endeavouring to engage me in dispute about philosophy and about solving of problems both of which are nothing to the question. I have left off mathematics twenty years ago and look upon solving of problems as a very unfit argument to decide who was the best mathematician or invented anything above fifty years ago. He complains of the Committee of the Royal Society as if they had acted impartially in omitting what he made against me. But he fails in proving the accusation."

By this time Conte, the impartial intermediary, was behaving more like a double agent seeking pleasure from the dispute and enjoying the consequent gossip over supper with the King and his Court. A few years later Newton was to find out the role played by this petty Venetian iago and take his customary revenge. Leibnitz meanwhile still saw him as an intermediary who was on his side.

(start of third extract)

Sir, it is probably out of love of truth that you have taken upon yourself a kind of challenge on the part of Mr Newton. I have never wanted to enter the fray with the lost children which has been unleashed against me. But since he wants now to be involved himself, I am very pleased to give him satisfaction. I was surprised at the beginning of the dispute to learn that I was accused of being the aggressor for I have never talked to Mr Newton except in a very civil manner. I never had any knowledge of the numerous Committee of gentlemen of several nations relating to the dispute for I was not informed of it in any way and I still do not know the names of all its members, particularly not those from the British Isles.

"With reference to their report I notice that all the remarks favoured Mr Newton and that there was an attempt to disparage me by unfounded suspicions which were sometimes ridiculous. To respond to the accusations against me point by point would require another report at least as large as the original. It would be necessary to enter into great detail concerning a number of minutiae that occurred thirty or forty years ago and which I scarcely remember. I would have to look through all my old letters of which several have been lost.

"So, all in all.and taking into account so many signs of ill-will, I feel it unworthy to enter into discussion with people who treat me so badly. I have no desire to make a public spectacle and I wish to use my time so precious to me in a more profitable way. Mr Newton accuses me of being a plagiarist, but where have I done so?."

Newton continued to wait for Leibnitz's detailed response to the accusations of the Royal Society report but Leibnitz never gave Newton an answer. On November 29th, 1716, Newton received a letter from Conte who was visiting Germany with George I.

"Mr Newton, Sorry that I have not been able to write to you until now. I have been ill since I arrived here and am still not well. I have seen neither the King nor the Court and I have been obliged to lie in my rooms for the past twenty days.

(In French) - "Mr Leibnitz is dead and the dispute is finished."

Leibnitz died alone. On the day of his funeral the King went hunting nearby, leaving a servant as the sole mourner. He was buried in an unmarked pauper's grave and for several years Newton ungallantly pursued him beyond it.. His great wrath, pouring forth in some five hundred folios, each devoted to self-vindicating re-draughts of the Royal Society report and wider attacks on his arch rival.

And so, two of the greatest geniuses of the European world not only of their own time but of its whole long history, had spent thousands of hours deceiving and belabouring one another. The calculus was indeed a great prize, seemingly too great to be shared by two such characters. They never met but they had some things in common. Neither married, they would both die childless and it is widely suspected that they also shared a lifetime of absolute celibacy.

In the age of reason, Newton and Leibnitz behaved like two endangered fathers, each jealously fighting, not for the custody of a human being, but for the exclusive possession of his brain child. In that respect, Newton had the victory. And today he still holds the credit for the discovery.

And yet, just as fame can be fickle, so this story has a final ironic twist in its tail. In 20th century mathematics it is the Leibnitz notation and symbolism which forms the basis of the calculus. Newton's fluxions are now no more than a brilliant historical relic.

(end of third extract)

Isaac Newton was played by David Kelsey and Godfried Wilhelm Leibnitz by Albert Welling.

Steve Hodson played Fatio de Duiller, Fraser Carr - George Chaney, Michael Deacon - John Kiell, Steven Thorne - Johann Bernoulli and Olivier Pierre, the Abbe Conte.

The Virgin Fathers of the Calculus was written and created by Stuart Kerr and Professor Lewis Wolpert. It was presented by Lewis Wolpert and the producer was Stuart Kerr.

Biographical Information

Newton was born on Christmas Day in 1642. His family were farmers; they lived in the village of Woolsthorpe, just south of Grantham, in Lincolnshire. He was born prematurely, and was a very weak and sickly child. His mother did not expect him to survive (his father, another Isaac, had already died before he was born). When Newton was three years old, his mother, Hannah Ayscough, married the Reverend Barnabas Smith from the neighbouring parish of North Witham and moved to live with her new husband. She left the infant Isaac in the care of his grandmother.

Although Newton was very bitter about being abandoned by his mother in this fashion, it was from the proceeds of his mother's second marriage (the good Reverend died when Newton was eleven years old) that he acquired an income of about £80 a year (worth a lot more in the late seventeenth century - you could try to work out how much that might be now). It was this income that meant he never had to worry about earning a living, and so was able to devote himself to his studies.

He attended Grantham Grammar School and then went on to Trinity College, Cambridge.

His teacher at Cambridge was Isaac Barrow, the first Lucasian Professor of Mathematics. He resigned in 1669 so that Newton could take over the post!

It was Barrow's lectures on geometry and in particular on methods he had devised for finding areas and tangents that undoubtedly provided the foundation for Newton's inventions of integral and differential calculus. His first paper on fluxions was written in 1666 and he extended the work in a paper called *De methodis fluxionum* written in 1670 - 71. However, he was a secretive kind of person, and kept his discoveries to himself. By not publishing his work, it left the way open for others to dispute whether or not he had priority over the invention of the calculus.

(More on this later).

Newton did not devote his whole life to mathematics. He was also an avid theologian, and spent lots of time trying to prove that the prophecies of Daniel and the poetry of the Apocalypse made sense. He reagard this work as far more serious than his mathematical work, although he was very jealous of his mathematical discoveries.

The peak of Newton's mathematical career was the publication in 1687 (at the expense of Edmund Halley) of a book called *Philosophiae Naturalis Principia Mathematica*, detailing all of his important mathematical and scientific discoveries. After that, things went downhill. In 1693, recovering from a serious illness, he discovered that his calculus was being used on the Continent, and that its invention was being attributed to Leibniz. In1699 he was made Master of the Royal Mint and subsequently spent a lot of time trying to prove that Leibniz was a plagiarist. He continued the attack even after Leibniz had died!

Biographical Information

Leibniz was born on 21st June 1646 in Leipzig. His father was a Professor of Moral Philosophy at the University, so his early life was spent in a very intellectual atmosphere. Indeed, he entered the University of Leipzig in 1661 (how old was he?) to study Law. He spent a term at Jena, where he came under the influence of Edward Weigel, the mathematics professor, and so became interested in mathematics. In November 1666 he was awarded his doctorate in Law.

Although his family were well-to-do, Leibniz had no guaranteed income and had to work all his life. Between 1672 and 1675 he worked in Paris as a diplomatic attaché for the Elector of Palatine. During this period, he visited London (in Spring 1673) where he met some English mathematicians and was told of Mercator's quadrature of the hyperbola - something which Newton had used in developing the calculus. This introduced Leibniz to the method of infinite series, a technique he improved. He also presented a calculating machine he had invented to the Royal Society and was elected a foreign member of the Society as a result. He also met Christian Huygens in Paris, who encouraged him in his mathematical work.

He was remarkable thinker, who could reputedly sit and think for days on end without moving. He had a grand plan, sadly never realised through pressure of more menial work (namely writing up the history of the House of Hanover), to develop an *universal calculus* which could be used to solve any kind of problem and reveal all possible truths.

He was also a prodigious writer, corresponding with over 600 people on a variety of topics.

In 1676 he left Paris for a new job with the Duke of Brunswick. In 1677 he published his own version of the calculus. Although Leibniz tried not to become embroiled in the dispute which then began over who had first invented calculus, he was inexorably sucked into it.

Between 1677 and 1704 the calculus Leibniz had published was developed by many Continental mathematicians (principally the Bernoulli brothers) into a really powerful and useful mathematical tool. In England, by contrast, Newton's secretiveness meant that calculus was largely unknown.

In 1714 Leibniz' employer, the Elector George Louis left Hanover to become the first German King of England. He refused to allow Leibniz to join him in London and insisted that he remain in Hanover to finsh the family history.

Leibniz died two years later in 1716, and was buried in a pauper's grave.

The twist in the tale, is that although Newton spent vast amounts of time and energy claiming precedence for the invention of the calculus, it is Leibniz' notation which we use today.

The Dispute - a chronology

1664	Newton discovers binomial theorem for any rational index
1664/5	Newton, sheltering in Woolsthorpe from the Bubonic Plague, invents the method of fluxions
1665	A paper dated 20th May shows Newton had developed enough calculus to be able to find tangent and curvature at any point of any continuous curve
1666	Leibniz sets out his master plan to create a general method by which all truths may be found by calculation
1669	Newton writes *De analysi per aequationes numero terminorum infinitas*, describing infinite series calculated using the Binomial Theorem (but not published until 1711) which circulates among friends
1671	Newton introduces use of 'pricked letters' to represent fluxions ($\dot{x}$ & $\dot{y}$) in *Method of Fluxions* (Written in latin)
1672	Leibniz goes to Paris and meets Huygens
1673	Leibniz visits London and learns of method of infinite series
1673	Leibniz reads a letter by Amos Dettonville about sines in quarter circle and reports a great light burst upon him: he realises that the determination of the tangent to a curve depends on the ratio of the differences in the ordinates and abscissae.
1675	Leibniz discovers fundamental theorem of calculus
1675	Between 25th October & 11th November Leibniz develops the main ideas of his calculus
1675	Leibniz introduces the integral sign, $\int$.(29th October)
1676	Newton describes Binomial Theorem in letters to Henry Oldenburg. The first, dated 13th June, is intended for Leibniz. The second, dated 24th October, describes the processes which led him to the discovery.
1676	Newton writes a third account of his calculus called *De quadratura curvarum*. Again not published.
1676	Leibniz realises he has possession of a method of great importance because of its generality
1676	Newton uses an unsolvable anagram to make a prior claim on the calculus in a letter to Leibniz via Henry Oldenburg
1676	In October Leibniz makes a flying visit to John Collins in London and is shown a copy of Newton's paper outlining the method of fluxions
1677	11th July: Leibniz writes up his calculus with most of the problems ironed out
1677	Newton & Leibniz are by now both aware of each other's work, largely as a result of correspondence through Henry Oldenburg
1678	Leibniz publishes an explanation of the integral calculus in the *Acta Eruditorum*.
1684	Leibniz publishes his differential calculus in the *Acta Eruditorum* as method for finding tangents, maxima and minima
1686	Leibniz publishes an article describing his integral calculus
1687	*Principia* published, including a description of the method of fluxions, and referring to earlier correspondence with Leibniz
1690	The Bernoulli brothers start spreading Leibniz' calculus among mathematicians on the Continent

1691	Nicolas Fatio de Duiller writes to Huygens claiming priority for Newton and accusing Leibniz of plagiarism. Leibniz knows nothing of this!
1693	Newton hears that the invention of calculus is being attributed to Leibniz
1696	The Bernoulli brothers set the brachistochrone problem for mathematicians in Europe to solve
1696	Newton solves brachistochrone problem after dinner on 29th January
1699	Nicolas Fatio de Duiller implies in paper to the Royal Society that Leibniz may have stolen his calculus from Newton.
1703	Newton elected President of the Royal Society
1704	Leibniz complains in the Acta Eruditorum about de Duiller's imputation of plagiarism but insists he has priority of publication
1705	Newton knighted by Queen Anne
1710	John Keill vigorously supports Newton's claims against those of Leibniz in Royal Society paper
1711	*De analysi* published
1711	Leibniz writes to the Royal Society and complains about Keill's remarks (his *second* complaint)
1711	John Keill responds on May 11th in a letter read out to the Royal Society, but fails to apologise. In fact, he makes further accusations about Leibniz' integrity
1711	December 12th: Leibniz writes again to the Royal Society to complain about John Keill's further remarks
1712	A Royal Society commission reports on the dispute and concludes that Newton had priority of invention(which Leibniz didn't argue about; he was only claiming prior publication and refuting charges of plagiarism). Newton actually wrote the report!
1713	Johann Bernoulli writes to Leibniz expressing sympathy but asks not be involved in the dispute
1713	Leibniz writes the *Charter Volans* anonymously, accusing Newton of not discovering the calculus until after he had seen Leibniz method.
1715	Abbé de Conte travels to London and tries to mediate in the dispute. Newton rejects the attempt.
1716	Leibniz sets problem of finding orthogonal trajectories of any one-parameter family of curves. Solved by Newton after work (before supper)
1716	Leibniz dies and is buried in an unmarked pauper's grave
1726	Newton deletes all references to Leibniz in the third edition of the *Principia.*
1727	Newton dies and is buried in Westminster Abbey

What they discovered

At the time Newton & Leibniz were working, mathematicians were concerned with methods of finding tangents to curves and areas enclosed by curves. Several geometrical methods had already been invented, but these lacked generality[1]. That is, each curve required a slightly different method. The power of calculus is that it is perfectly general and can be applied to *any* curve.

Newton's approach was a physical one; his variables changed with time. Flowing quantities were *fluents* and the rate at which they flowed was the *fluxion*. The first step he took was to extend the well-known Binomial Theorem used for expanding expressions of the form

$$(1 + x)^n$$

where n is an integer to the case where n is any rational number.
He was able to use this result to find the area under any curve of the form

$$y = (1 - x^2)^{m/n}$$

Find out how Newton did this

Other problems Newton investigated using his fluxions were the quantification of "y^e...crookednesse in lines", or *curvature* as we now call it, and the calculation of the length of curves, known as *rectification*.

Find out how Newton solved these problems.

Leibniz was concerned with the same problems, but his approach was less physical than Newton's. He did not think of quantities flowing, but began by considering differences. In 1672 Huygens asked him to calculate

$$\sum_{r=1}^{\infty} \frac{2}{r(r+1)}$$

Leibniz altered the form of the problem so that he could use a method involving difference sequences.

Find out how he did this.

He was led to the conclusion that summing sequences and finding their difference sequences were inverse operations, and subsequently realised that the determination of areas under curves and of tangents are inverse operations.
Leibnix used a rule he called the *transmutation rule* to find areas under curves.

Find out how Leibniz rule worked.

[1]e.g Van Heurat's method for parabolas and hyperbolas

Nuffield Advanced Mathematics

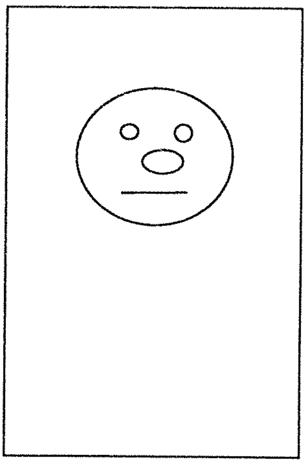
History of Mathematics

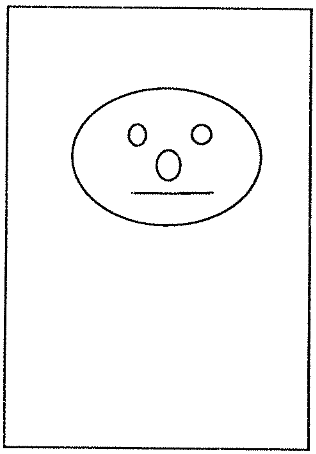
Background Information Sheet

Newton
(1642 - 1727)

&

Leibniz
(1646 - 1716)





prepared by Paul Garcia
Harlow College

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WORKSHOP HANDBOOK

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