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Introduction: The 17th and 18th centuries

The 17th and 18th centuries in European history are marked by a number of important events, ending with the French Revolution, the transformation whose continuing repercussions have done so much to shape the Europe of today. So we might expect that the decades before 1789 were very different, and indeed they were.

Mathematics at the time of Descartes was in an intriguingly mixed state. René Descartes himself had moved to the new, and Protestant, country of the Netherlands, where he felt less likely to be prosecuted by the Catholic Church that had recently and harshly disciplined Galileo for seeming to disagree with Church doctrine about the rotation of the Earth. There he found people able to read his work, some of whom, like him, were independent scholars only loosely attached to the university world. In France a few mathematicians had university positions, while others, including Marin Mersenne, Pierre de Fermat, and Blaise Pascal, did not. In England and Scotland, too, innovative work was also done by people in various walks of life, from surveyors and astronomers to the professionals attached to Gresham College in London.

One factor that changed things in England was the English Civil War of 1642–1651. By the time it was over and the monarchy had been restored in 1660 under a constitution that now limited its powers, an influential group of scholars based in Oxford and London had come together, and in 1662 they persuaded the new King, Charles II, to establish the world's first strictly scientific society, the Royal Society. Its significance lay in its attitude to knowledge, summed up in its motto 'Nullius in verba' ('On the word of no-one'). Claims of any kind were to be accepted only on the basis of evidence, not on rumour, nor even on the word of a gentleman. Robert Hooke, Christopher Wren, Edmond Halley, William Brouncker, and others applied themselves to observation and argument, and the King, who paid for their work, expected that the nation would in some way benefit from their labours — for example, by finding reliable ways to determine longitude at sea and by assisting in navigation, trade, and the expansion of British naval power.

To help bring this about, the Royal Society created a journal called the *Philosophical Transactions*, the first volume of which was published in 1665. This mirrored a trend being felt elsewhere in Europe: the first issue of the first academic journal to be

Figure 1.1. Frontispiece to Thomas Sprat's *History of the Royal Society* (1667)

founded there, the Parisian *Journal des Sçavans*, had appeared a few months earlier, on 5 January 1665.[1] Journals did not mark the end of personal correspondence — indeed, many of the articles they published took the form of open letters and responses to previous articles. But whereas in an earlier period such communications would have been private, or given a very limited circulation, now, if the author wished, they could be distributed among the subscribers to a journal, read more widely, and draw unknown others into the debate. Thus was created what historians now call the 'Republic of Letters', an informal international group of people who were interested in this or that

[1]This journal was later renamed the *Journal des Savants*.

aspect of science, or antiquity, or any form of scholarship. Relationships in that world might be close or hostile, amicable, or competitive, but the dynamic was conducive to the growth of enquiry, and often transcended national boundaries. Even during times of war, gentlemen scholars from opposing countries could travel freely and call upon the aid of other like-minded folk.

Although it may have been unclear at the time, and only slowly became clearer in England, we also see here the origins of another significant innovation in the life of mathematics and science: the learned academy, often with royal support. In the 18th century influential mathematicians increasingly had positions in the Academies of Sciences in Paris, Berlin, or St Petersburg. At the highest levels the place for gifted amateurs with their own money was shrinking.

Notably, few of these dominant mathematicians were attached to universities. Research was not then the primary task of any university, and one can wonder on occasion whether teaching was either. The Bernoulli brothers, Jakob and Johann, were professors in the Netherlands and Switzerland at the start of the 18th century, and Joseph-Louis Lagrange was briefly a professor in Turin at the start of his career before moving to the Academy in Berlin, but Leonhard Euler never held a university position and moved between the Academies in St Petersburg and Berlin.

So when Isaac Newton went up to the University of Cambridge in 1661, he was not going to a modern high-powered research institution but to a place where young men of varying degrees of wealth and influence were educated, as much or as little as they saw fit, so that they could pursue careers in the Church or at Court. In Isaac Barrow Cambridge possessed a mathematician of real ability, but he left in 1669 to seek a better life at Court, and Newton succeeded him as the Lucasian Professor at Cambridge. Newton then had to make his way with the assistance of members of the Royal Society.

The origins of the Lucasian Chair are revealing. It had been founded in 1663 with the bequest of Henry Lucas, who had been the Member of Parliament for Cambridge University and who left four thousand books and an endowment of one hundred pounds to pay for a professor who was not to be active in the Church; Charles II signed the Chair formally into existence in January 1664. Such bequests are a sure sign that reform was wanted, and the Chair became one for mathematical sciences, not least because of its first two occupants.

In due course, Newton left Cambridge for London, where he eventually became Master of the Royal Mint and President of the Royal Society. The first edition of his hugely influential *Philosophiæ Naturalis Principia Mathematica* (The Mathematical Principles of Natural Philosophy) had been published by the Royal Society in 1687, with the active assistance of Halley. The second and third editions were prepared with the assistance of other Cambridge mathematicians, such as Roger Cotes and Brook Taylor, but after that, British involvement in mathematics and many branches of science declined, for reasons that are not well understood, and by 1800 even the Royal Society had become something of a gentlemen's club where social status eclipsed scientific merit.

The only intellectual figure who can bear comparison to Newton during his lifetime was Gottfried Wilhelm Leibniz. Just as Newton's interests extended beyond mathematics to embrace (indeed, redefine) many areas of physics, and also theology, and even alchemy, Leibniz was also occupied with philosophy, linguistics, logic, law, and (to a lesser extent than Newton) physics. But he was unlucky with his employment, and wound up spending almost forty years as a rather minor Court Councillor in Hannover. This prevented him from applying his prodigious skills to the tasks that posterity would have preferred. Even so, not only did he create a version of the calculus that was as good as Newton's, he had the time and the opportunity to foster the next generation of mathematicians, and so his influence on mathematics was arguably the greater of the two. In some measure he had this opportunity because, unlike Newton, he was strongly in favour of publishing. The mathematical and scientific journal that he and the German philosopher Otto Mencke helped to create in 1682, the *Acta Eruditorum Lipsiensium* (Acts of the Scholars of Leipzig), was well received and it carried his two influential presentations of the calculus in 1684 and 1686 that we describe in Section 4.4. This was the first time that the new subject was put into print, and they established Leibniz as a major original mathematician.

Mathematics in France also had its vicissitudes. The country that had produced Descartes, Fermat, and Pascal at the start of the 17th century was unable to match them at the end. Instead, mathematics was briefly dominated by the feuding Bernoulli brothers from Switzerland and their children. The younger brother, Johann, began the 18th-century enterprise of mastering, extending, and applying the calculus, a development only hinted at by Newton in his *Principia* but which Newton's followers felt was almost forced upon them if they were to understand, let alone extend, his work.

The Bernoullis had learned the calculus by corresponding with Leibniz about it, and it fell to Johann to teach the man who was to dominate the middle decades of the 18th century, his fellow Swiss, Leonhard Euler. Euler transformed almost every branch of mathematics he touched, and created many others. He revived the theory of numbers that Fermat had begun a century earlier; he rewrote the calculus in the language of functions; he wrote on celestial mechanics and optics, fluid flow, and differential equations. Above all, he published, in the journals of the learned Academies, as well as his many books, and by his example — he was a lucid and an exceptionally prolific writer — he established that publication should henceforth be the rule. He also won prizes from the Academies for his researches into many specific topics and in turn organised prize competitions; these competitions were a characteristic feature of mathematical life in the 18th century and provided a way, not always successful, of promoting research in specific areas.

If Euler had a rival it was the French mathematician Jean le Rond D'Alembert, who was ten years younger than Euler and a major figure in the Parisian Académie des Sciences. In D'Alembert's view, the 18th century was the century of mechanics, and he was almost wholly focused on creating mathematics that would deepen our understanding of the physical world. He was, apparently, a gifted conversationalist and a popular figure in social circles and the salons of the time, but he was a poor writer, and much of his influence was to be exerted through the work of his protégé, Joseph-Louis Lagrange. That said, D'Alembert was the first to extend the calculus effectively to functions of several variables, and to describe the motion of a vibrating string correctly.

Lagrange was a more austere figure — an algebraist by temperament, who first came to people's notice when he took up and enriched two of Euler's subject areas, the calculus of variations (which we do not discuss) and the theory of numbers. He later wrote a very important paper on the algebraic solution of polynomial equations, as well as a thorough, if unsuccessful, attempt to base the calculus on solely algebraic considerations, and most extensively on celestial mechanics. Too shy ever to meet Euler, he nonetheless succeeded him in Berlin before moving to the Academy in Paris in 1787.

Celestial mechanics at that time may be summed up as the attempt to see whether Newton's law of gravity was capable of describing all the delicate and complicated behaviour of the motion of the planets, their satellites, and the Moon. As we shall discuss, the motion of even three bodies (such as the Sun, the Earth, and the Moon) under their mutual gravitational attraction is exceedingly difficult to understand, and throughout the 18th century numerous figures contributed to its analysis. Their work was consummated by Pierre-Simon Laplace, whose five-volume treatise on celestial mechanics showed that all the known complexities of the various orbits can indeed be explained satisfactorily on Newtonian grounds, although long-term predictions were to remain impossible.

In less than two hundred years, mathematics was rewritten around the calculus, which replaced geometry as the relevant core discipline in the subject: indeed, much of geometry was rewritten using the terms and methods of the calculus. But this was not the modern rigorous calculus that we know — that was to be a creation of the 19th century. This was a calculus with powerful new methods and uncertain foundations, and was largely cast in a formal, algebraic language. It discussed functions (generally expressed as infinite series), their derivatives, and their integrals, and above all, as in celestial mechanics, differential equations of many kinds. Its legacy to succeeding generations was both its astonishing efficacy and a growing need to re-establish mathematics on the rigorous foundations that had been associated, however generously, with Euclid's *Elements*.

8

18th-century Number Theory and Geometry

Introduction

It is possible to regard the 18th century as the century of algebra, not just because of the successes that mathematicians achieved in that field but also because it entered more and more into the foundations of the subject. Here we look at its uses in number theory, a subject that Euler and Lagrange did much to revive, and in geometry, where algebra grew from a useful method to become the dominant partner.

8.1 Number theory

It was Euler, and Lagrange after him, who brought number theory into the mainstream of mathematics, where it has remained ever since. Euler's involvement drew on ancient sources, as well as the writings of Fermat who had often written about problems involving numbers but had largely failed to interest his contemporaries.[1]

One claim by Fermat, inspired by noticing that the first five numbers in the sequence

$$2^1+1=3,\ 2^2+1=5,\ 2^4+1=17,\ 2^8+1=257,\ 2^{16}+1=65,537,\ \ldots,$$

are prime numbers, was that all such numbers, where the exponents are themselves powers of 2, are prime. In December 1729, Euler received a letter from Christian Goldbach, who challenged him to find out whether Fermat's claim that all such numbers of the form $2^{2^n}+1$ are prime. Euler replied in January to say that he had nothing to add to Fermat's discovery, but some time later he found an ingenious argument to prove that the next Fermat number

$$2^{32}+1=4{,}294{,}967{,}297$$

[1]Fermat's work in number theory is discussed in *Volume* 1, Chapter 11.

is not prime, but is divisible by 641. Since then, no other ‘Fermat number’ has been shown to be prime, so Fermat’s conjecture was particularly unfortunate. Euler presented his discovery to the St Petersburg Academy in 1732, without indicating how he found it. This paper (E26) was published only in 1738, and Euler published a proof only in 1747/48 (E134), which shows how slowly publications were being handled at the time.

Fermat had also claimed that if a prime number p does not divide an integer a, then p must divide $a^{p-1} - 1$; for example, the prime number $p = 5$ does not divide the integer $a = 6$, but 5 does divide $6^{5-1} - 1 = 6^4 - 1 = 1295$. But he never published a proof of the result, which is often called Fermat’s ‘little theorem’. Euler listed it in E26 in a series of results that he could not prove, and then gave a proof in 1736 that relies on little more than the binomial expansion of $(1 + a)^p$ and induction on a, which he later replaced with a simpler proof.[2]

Perfect numbers. Certain numbers, such as 6 and 28, have the property that the sum of their factors (excluding the number itself), equals the original number. For example, the factors of 6 are 1, 2, and 3, and $1 + 2 + 3 = 6$, and the factors of 28 are 1, 2, 4, 7 and 14, and $1 + 2 + 4 + 7 + 14 = 28$. Numbers with this property were held to have special religious or mystic significance, and were considered ‘perfect’: thus, a number is *perfect* if it is the sum of its factors (other than the number itself). After 6 and 28, the next perfect numbers are 496 and 8128, and then there are no more until 33,550,336.

The study of perfect numbers can be traced back to Greek times, and in Book IX, Proposition 36, of the *Elements*, Euclid had proved that whenever $2^n - 1$ is a prime number, the number $2^{n-1}(2^n - 1)$ must be perfect;[3] the five perfect numbers above correspond to $n = 2, 3, 5, 7, 13$. If $2^n - 1$ is not prime, then $2^{n-1}(2^n - 1)$ is not perfect; for example, $2^4 - 1 = 15$ is not prime, and $2^3(2^4 - 1) = 8.15 = 120$ is not perfect.[4]

The search for perfect numbers led Mersenne to investigate when numbers of the form $2^n - 1$ are prime.[5] These numbers, when prime, have become known as *Mersenne primes*.[6]

When studying perfect numbers in the 1740s, Euler observed that all known perfect numbers are even, and he wondered whether this was the case for all perfect numbers. He was unable to prove this, observing in a paper written in 1747, but published only posthumously in 1849, that ‘whether …there are any odd perfect numbers is a most difficult question’, but he was at least able to prove that all *even* perfect numbers must have the form $2^{n-1}(2^n - 1)$, where $2^n - 1$ is prime.[7]

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[2] The earlier paper, E54, was published only in 1741. In 1758 Euler (E271) gave his simpler proof and generalised the result by showing that, for any integers n and a, the number $a^{\varphi(n)} - 1$ is always divisible by n, where $\varphi(n)$ is the number of integers less than n that have no factors in common with n.

[3] See *Volume* 1, Chapter 5.

[4] We note that if n is composite then $2^n - 1$ is composite, but if n is prime it does not follow that $2^n - 1$ is prime. For example, $2^{11} - 1 = 2047 = 23 \times 89$.

[5] See *Volume* 1, Chapter 11.

[6] Currently, the largest currently known (October 2019) is $2^{82,589,933} - 1$; it is not known whether there are infinitely many Mersenne primes. For an extract from Mersenne on these primes, see (Stedall 2008, 158–159).

[7] See (Euler 1849, 88), E798. We still do not know whether there exist any odd perfect numbers, but it is known that if they do they must be huge, indeed greater than 10^{1500}, which is a number greater than the number of atoms in the visible universe.

Fermat's last theorem. Fermat had managed to prove, using his 'method of infinite descent', that there are no positive integers a, b, c that satisfy $a^4 + b^4 = c^4$, and rashly wrote in the margin of his copy of Bachet's edition of Diophantus's *Arithmetica* that he had a marvellous proof that for no integer $n > 2$ are there positive integers a, b, c that satisfy $a^n + b^n = c^n$, but the margin was too small to contain it. It is unlikely that he had a proof, but the claim eventually grew into Fermat's famous 'last theorem'.[8]

A charming indication of how Euler viewed his work, his aims, and his partial achievements, can be gleaned from a letter describing his success with $x^3 + y^3 = z^3$ that he wrote to Goldbach on 4 August 1753.[9]

Euler on a case of Fermat's last theorem.

> There's another very lovely theorem in Fermat whose proof he says he has found. Namely, on being prompted by the problem in Diophantus, find two squares whose sum is a square, he says that it is impossible to find two cubes whose sum is a cube, and two fourth powers whose sum is a fourth power, and more generally that this formula $a^n+b^n = c^n$ is impossible when $n > 2$. Now I have found valid proofs that $a^3 + b^3 \neq c^3$ and $a^4 + b^4 \neq c^4$, where $\neq$ denotes cannot equal. But the proofs in the two cases are so different from one another that I do not see any possibility of deriving a general proof from them that $a^n + b^n \neq c^n$ if $n > 2$. Yet one sees quite clearly as if through a veil that the larger n is, the more impossible the formula must be. Meanwhile I haven't yet been able to prove that the sum of two fifth powers cannot be a fifth power. To all appearances the proof just depends on a brainwave, and until one has it all one's thinking might as well be in vain. But since the equation $aa+bb = cc$ is possible, and so also is this possible, $a^3 + b^3 + c^3 = d^3$, it seems to follow that this, $a^4 + b^4 + c^4 + d^4 = e^4$, is possible, but up till now I have been able to find no case of it. But there can be five specified fourth powers whose sum is a fourth power.

We give two examples to illustrate Euler's observations:[10]

$$3^3 + 4^3 + 5^3 = 6^3 \text{ and } 30^4 + 120^4 + 272^4 + 315^4 = 353^4.$$

It is clear from Euler's comments that he was dissatisfied. Although he had found a proof, indeed one by infinite descent, it seemed to him that he would not be able to solve the general case ($x^n + y^n = z^n$, for any $n > 2$) in this way, so we see that he had both a specific and a general aim in mind.

Euler's claim, that he had proved Fermat's Last Theorem in the case $n = 3$, rested on an argument that he did not publish until his *Algebra* appeared in 1770.[11] A brief look at this ultimately inconclusive argument is sketched in Box 17 and brings out some important features of Euler's number theory.

Perhaps the most interesting thing about Euler's argument — which is more important than the 'proof' of Fermat's Last Theorem for $n = 3$ — is that Euler extended

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[8] See *Volume* 1, Chapter 2, Section 2. The theorem was eventually proved, see (Wiles 1995) and (Wiles and Taylor 1995).

[9] This is letter 169 in the Euler–Goldbach correspondence; see (Euler 2015) for a different translation.

[10] The sum of the fourth powers was found only in 1911.

[11] See Euler, *Algebra*, Part II, Chapter XV, §243.

Box 17. Euler on $x^3 + y^3 = z^3$.
Euler first observed that if $x^3 + y^3 = z^3$, then the integers x, y, and z cannot all be even, for then they would have a common factor, and we can assume without loss of generality that they do not. Nor can they all be odd, because the sum of two odd numbers is even. So exactly one of x, y, z is even, and Euler assumed that z is even. There is no loss of generality here, provided that x, y, and z are allowed to be negative.

So Euler assumed that $x + y = 2m$, say, and $x - y = 2r$, say. It follows that $x^3 + y^3 = 2m(m^2 + 3r^2)$ is a cube. Therefore, said Euler, either $2m$ and $m^2 + 3r^2$ are both cubes, or they are not, and in the latter case m must be a multiple of 3, because this is the only way that $2m$ and $m^2 + 3r^2$ are not relatively prime.

Euler was able to show that this last possibility cannot happen, and he next considered how a number of the form $m^2 + 3r^2$ can be a cube. Inspired by the identity

$$(a^2 + 3b^2)^3 = (a^3 - 9ab^2)^2 + 3(3a^2b - 3b^3)^2,$$

which says that a cube of a number of the form $m^2 + 3r^2$ is itself of that form, Euler claimed that if a number of the form m^2+3r^2 is a cube, then it is a cube of a number of that form — that is, there are integers a and b such that $(a^2+3b^2)^3 = m^2 + 3r^2$.

Euler now appealed to Fermat's method of infinite descent. The numbers $u = a - 3b$, $v = a + 3b$, and $w = 2a$ are cubes with the appropriate sum, $u + v = w$. They can be shown to be relatively prime (for if they were not, then m and n would not be) and $w = 2a < z^3$. So the descent can begin, and the contradiction is immediate.

Euler believed that this showed that Fermat's last theorem follows in the case $n = 3$, and indeed it would do, if one could be certain that there are integers a and b for which $(a^2 + 3b^2)^3 = m^2 + 3r^2$. Euler argued that one can factorise $m^2 + 3r^2$ as

$$m^2 + 3r^2 = (m + r\sqrt{-3})(m - r\sqrt{-3}),$$

and claimed that if $m \pm r\sqrt{-3}$ is a cube then it is cube of a number of the same form $a \pm b\sqrt{-3}$. This is true, as it turns out, but not for the reasons that Euler gave. In his *Algebra* (1770, Part II, §182) he claimed that $x^2 + cy^2$ could always be regarded as a square, and this is false when $c = 5$ and in many other cases. So there is a difference between $x^2 + 3y^2$ and $x^2 + 5y^2$, which Euler was unable to explain.

his reasoning about integer numbers to new numbers of the form $x + y\sqrt{-c}$. Euler boldly proposed to discuss prime, relatively prime, square, and cube numbers of this kind, treating then as if they were integers, and presuming that concepts such as 'prime' would similarly apply. He did so when imaginary quantities were still a source of controversy in mathematics.

Sums of two squares. Yet another subject that Fermat had broached and that Euler took up to lasting effect is called the 'theory of quadratic forms'. It began with the observation, known, it is said, to Plato, that all the integer solutions of $x^2 + y^2 = z^2$ are of the form $x = p^2 - q^2, y = 2pq, z = p^2 + q^2$ (or multiples of these). This had led mathematicians to ask: Which non-zero numbers are the sum of two squares? The smallest answers are $1, 2, 4, 5, 8, 9, 10, 13, \dots$ — note that we allow one of x or y to be zero. It was soon seen that these numbers are either 1, 2, squares, primes of the form $4n + 1$, or products of numbers of these kinds.

It is also clear that no number of the form $4n + 3$ can be the sum of two squares. For, each of the two squares is the square of either an even number or an odd number. An even number has, by definition, the form $2k$ so its square is of the form $4k^2$, and an odd number has, by definition, the form $2k+1$ so its square is of the form $4k^2+4k+1$. Sums of two such numbers are therefore of the form $4n$, $4n + 1$, or $4n + 2$, but never $4n + 3$.

Fermat was the first to claim that every prime number of the form $4n+1$ is uniquely a sum of two squares; for example, $41 = 4^2+5^2$. He mentioned it in a letter to Mersenne in 1640, where he said that his proof used his method of infinite descent; but no proof of his survives.

Euler took up this problem a century later, in 1747, when he wrote to Goldbach about it.[12] The identity

$$(x^2 + y^2)(u^2 + v^2) = (xu + yv)^2 + (xv - yu)^2$$

shows that a product of two numbers, each of which is a sum of two squares, is itself a sum of two squares. So Euler looked at the *divisors* of numbers that are a sum of two squares, and attempted to show that these divisors are also of the same form. In his letter, he presented a proof that every prime divisor of a sum of two squares is itself a sum of two squares. His argument relied on the method of infinite descent.

It remained to show that if a prime p is of the form $4n + 1$ then there are integers x and y such that $x^2 + y^2 = p$, but it was only in 1749 that Euler could conclude the argument, writing to Goldbach on 12 April 'Now I have finally found a valid proof'.[13]

We can see this using modern notation.[14] By Fermat's little theorem, any two integers x and y satisfy

$$x^{p-1} \equiv y^{p-1} \pmod{p},$$

and so, in the present case,

$$x^{4n} \equiv y^{4n} \pmod{p}.$$

But

$$x^{4n} - y^{4n} \equiv (x^{2n} + y^{2n})(x^{2n} - y^{2n}) \equiv 0 \pmod{p},$$

so if there are values of x and y for which

$$x^{2n} - y^{2n} \not\equiv 0 \pmod{p}$$

then they must satisfy

$$x^{2n} + y^{2n} \equiv 0 \pmod{p},$$

and this shows that p divides a sum of two squares, since $x^{2n} + y^{2n} = (x^n)^2 + (y^n)^2$. This shows that p is a sum of two squares.

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[12]See (Euler 2015, nr. 115); the letter was written on 6 May 1747.

[13]See (Euler 2015, nr. 138); the letter was also published in (Fuss 1843, 493).

[14]See Box 55 in Chapter 18.

Today the existence of the above x and y is immediate, because mathematicians after Euler developed a theory of polynomial equations modulo a prime number. But for Euler it required a novel argument involving finite differences, which we omit.

Fermat had also claimed — once again, without any proofs surviving — that he could tackle new problems that were to enlarge the field of enquiry considerably. He had claimed, for example, that prime divisors of numbers of the form $x^2 + 2y^2$ are also of the form $x^2 + 2y^2$, and that prime divisors of numbers of the form $x^2 + 3y^2$ are also of the form $x^2 + 3y^2$. It took Euler about 30 years to establish these claims, and to characterise the primes of these forms. He found that an odd prime is of the form $x^2 + 2y^2$ if and only if it is of the form $8n + 1$ or $8n + 3$; and a prime is of the form $x^2 + 3y^2$ if and only if it is of the form $6n + 1$.

But Fermat had also come up against a problem that he could not solve, and this was to prove much more interesting. He saw that with x^2+5y^2, something unexpected happens. The identity

$$(x^2 + 5y^2)(u^2 + 5v^2) = (xu + 5yv)^2 + 5(xv - yu)^2$$

shows that the product of two numbers of the form $x^2 + 5y^2$ is again of this form. But it is not true that every divisor of a number of this form must also be of this form. For example,

$$21 = 1^2 + 5.2^2 = 3.7, \quad \text{and} \quad 161 = 6^2 + 5.5^2 = 7.23,$$

but 3, 7, and 23 are not of the required form. Fermat conjectured, but admitted that he could not prove, that primes that are congruent to 3 or 7 (modulo 20) do have products that are of the form $x^2 + 5y^2$. Primes of the form $x^2 + 5y^2$ other than 5 are all of the form $20n + 1$ or $20n + 9$, but Fermat seems not to have noticed this.[15]

This puzzle was to be elucidated by Lagrange, as we now discuss.

Lagrange's work on quadratic forms. If Euler's work often had the character of a deep but informal exploration and opening-up of a subject, then Lagrange's represents the next stage: the systematic study and rigorous development of the main ideas.[16] Such was to be the case with his study of quadratic forms. In Lagrange's theory, a quadratic form is an expression of the form $ax^2 + 2bxy + cy^2$, where a, b, c are integers and the *discriminant* $\Delta = b^2 - ac$ is not a square (because otherwise the form would factorise as the product of two linear terms).

Lagrange addressed Fermat's problem with numbers of the form $x^2 + 5y^2$, which has discriminant $\Delta = 0^2 - 1.5 = -5$, by introducing a second quadratic form $2x^2 + 2xy + 3y^2$ which has the same discriminant, $\Delta = 1^2 - 2.3 = -5$. He then showed that these are essentially the only two quadratic forms with this discriminant, using a definition of when two forms represent the same collection of numbers, that we explain in Box 18.

More precisely, Lagrange showed that any quadratic form with discriminant $\Delta = -5$ is equivalent to one of the two inequivalent forms $x^2 + 5y^2$ and $2x^2 + 2xy + 3y^2$. Each of these forms represents integers that the other one does not. For example, $2x^2 + 2xy + 3y^2$ represents 3 when $x = 0, y = 1$, and 7 when $x = 1, y = 1$. So the anomalous behaviour of primes of the form $x^2 + 5y^2$ is explained by observing that it is not the

[15]See Fermat, *Oeuvres*, Vol. 2, p. 432.

[16]See (Lagrange 1773/1775).

Box 18. Equivalence of forms
Lagrange took a quadratic form $ax^2 + 2bxy + cy^2$ and considered changes of variable, such as

$$x' = \alpha x + \beta y, \quad y' = \gamma x + \delta y,$$

where $\alpha, \beta, \gamma, \delta$ are integers, and $\alpha\delta - \beta\gamma = 1$.

Suppose this transforms the quadratic form into a new quadratic form $a'x'^2 + 2b'x'y' + c'y'^2$. The transformation is invertible, because

$$x = \delta x' - \beta y', \quad y = -\gamma x' + \alpha y'.$$

Therefore any integer represented by the form $ax^2 + 2bxy + cy^2$ is also represented by the corresponding form in x' and y', and vice versa. For, if x_0 and y_0 are such that

$$ax_0^2 + 2bx_0y_0 + cy_0^2 = m,$$

then $x' = \alpha x_0 + \beta y_0$ and $y' = \gamma x_0 + \delta y_0$ satisfy

$$a'x'^2 + 2b'x'y' + c'y'^2 = m.$$

Lagrange decided that these two forms should be regarded as equivalent.

only quadratic form of its kind. In fact, as you can check, setting $b^2 - ac = \Delta$,

$$(ax^2 + 2bxy + cy^2)(au^2 + 2buv + cv^2) = (axu + xbv + byu + cyv)^2 - \Delta(xv - yu)^2,$$

which goes some way to explaining Fermat's observations when $\Delta = -5$.

Lagrange's observation opened the way to the study of all quadratic forms of the general form $ax^2 + 2bxy + cy^2$. In this broader setting he showed that, for an odd number m to be represented by a form $ax^2+2bxy+cy^2$ with a given value of $\Delta = b^2-ac$, it is necessary that Δ is a square (modulo m).

This result shows that progress with the problem of deciding which numbers m are represented by a quadratic form $ax^2+2bxy+cy^2$ with a given discriminant $\Delta = b^2-ac$ depends on being able to decide which numbers are squares (modulo m). This was a difficult question that neither Lagrange nor Euler was able to solve, although Euler had written to Goldbach as early as 1742 with an impressive array of evidence that pointed to the correct theorem.[17] This and other open questions raised by the work of Euler and Lagrange were to be a powerful stimulus to the work of Gauss in the next generation.

8.2 Infinite series

Euler's work in number theory led him into related areas that Fermat had not looked at, such as the summation of infinite series.

[17] See (Fuss 1843), Vol. 1, pp. 144–153, and (Edwards 1983).

Box 19. The harmonic series has no finite sum.

$$1 + \tfrac{1}{2} + (\tfrac{1}{3} + \tfrac{1}{4}) + (\tfrac{1}{5} + \tfrac{1}{6} + \tfrac{1}{7} + \tfrac{1}{8}) + (\tfrac{1}{9} + \tfrac{1}{10} + \cdots + \tfrac{1}{16}) + \cdots$$
$$> 1 + \tfrac{1}{2} + \tfrac{1}{2} + \tfrac{1}{2} + \tfrac{1}{2} + \cdots$$

because each of the bracketed terms is greater than ½. So the successive partial sums eventually exceed any finite amount, and therefore the harmonic series does not converge.

By Euler's time it was well known that some infinite series have finite sums. For example, the infinite series

$$1 + \tfrac{1}{2} + \tfrac{1}{4} + \tfrac{1}{8} + \tfrac{1}{16} + \dots$$

converges to 2, in the sense that we can get as close to 2 as we wish by adding together enough terms of the above series. In the same way, the sum of

$$1 + \tfrac{1}{3} + \tfrac{1}{9} + \tfrac{1}{27} + \dots,$$

where each term is one-third of the previous one, is 1½. In fact, for any number $n > 1$, the sum of

$$1 + 1/n + 1/n^2 + 1/n^3 + \dots$$

is $n/(n-1)$; we shall need this result later.

It was also known that some series cannot be summed. It had been realised in the 14th century that the *harmonic* series

$$1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \tfrac{1}{5} + \cdots$$

has no finite sum, because an arbitrarily large number can be exceeded (such as one million) by adding together enough terms of the series (see Box 19).

Euler was particularly fascinated by the harmonic series and series similar to it, such as the sum of the reciprocals of the squares. The problem of finding the sum of the reciprocals of the perfect squares,

$$1 + (\tfrac{1}{2})^2 + (\tfrac{1}{3})^2 + (\tfrac{1}{4})^2 + (\tfrac{1}{5})^2 + \cdots = 1 + \tfrac{1}{4} + \tfrac{1}{9} + \tfrac{1}{16} + \tfrac{1}{25} + \cdots,$$

exercised many minds around the mid-1730s. It was already an old problem; it seems to have been posed first by the Italian mathematician and clergyman Pietro Mengoli in 1644. When it defeated Jakob Bernoulli in Basel in 1689 he communicated it to the mathematical community as an important challenge, after which the problem became known as the *Basel problem*.

Simple calculations like the one used to handle the harmonic series show that the sum of the reciprocals of the squares lies between 1 and 2, and by summing the first few terms, we can begin to suspect that this series converges to a number that is a little over 1.64. Indeed, in one of his earliest papers, Euler showed that the sum is approximately 1.644934.[18]

[18]See (Euler 1731/1738, 33, E20). Later Euler calculated this sum to 20 decimal places.

Box 20. Solving the Basel problem.
Euler used the usual power series expression for $\sin x$, noting that the coefficient of x^3 is $-1/6$:

$$\sin x = x - x^3/3! + x^5/5! - x^7/7! + \cdots.$$

Now a polynomial equation such as $1 - x^2/4 = 0$, with solutions $x = 2$ and $x = -2$, can be factorised as

$$1 - x^2/4 = (1 - x/2)(1 + x/2).$$

Similarly (Euler claimed), since $\sin x = 0$ when

$$x = 0,\ \pi \text{ or } -\pi,\ 2\pi \text{ or } -2\pi,\ 3\pi \text{ or } -3\pi,\ \ldots,$$

that is, when

$$x \text{ or } (1 - x/\pi) \text{ or } (1 + x/\pi) \text{ or } (1 - x/2\pi) \text{ or } (1 + x/2\pi) \text{ or } \ldots = 0,$$

we can then 'factorise' $\sin x$ as follows:

$$\sin x = x(1 - x/\pi)(1 + x/\pi)(1 - x/2\pi)(1 + x/2\pi)(1 - x/3\pi)(1 + x/3\pi)\cdots.$$

Combining terms in pairs gives

$$\sin x = x(1 - x^2/\pi^2)(1 - x^2/4\pi^2)(1 - x^2/9\pi^2)\cdots.$$

We now find the coefficient of x^3 in this expression. This is formed from the x term, together with each of the single terms $-x^2/k^2\pi^2$ in turn when $k = 1, 2, 3, \ldots$, the remaining terms (from the other brackets) all being 1. This gives

$$-1/\pi^2 - 1/(4\pi^2) - 1/(9\pi^2) - 1/(16\pi^2) - \cdots.$$

But the coefficient also equals $-1/6$ (from the series for $\sin x$ given earlier).

Equating these gives

$$-1/6 = -1/\pi^2(1 + 1/4 + 1/9 + 1/16 + 1/25 + \cdots),$$

from which it follows that

$$1 + 1/4 + 1/9 + 1/16 + 1/25 + \cdots = \frac{\pi^2}{6},$$

as required.

Can the exact sum of this series be found? One of Euler's earliest achievements, in 1734, was to show (initially by somewhat dubious means) that this sum is $\pi^2/6$, and this brought him international fame; his original method is outlined in Box 20.[19] The method consists of 'factorising' $\sin x$ and looking at the coefficient of x^3.

By similarly considering the coefficients of x^5 and x^7, Euler extended his calculations to find the sum of the reciprocals of the 4th powers and the 6th powers, obtaining the results

$$1 + (1/2)^4 + (1/3)^4 + (1/4)^4 + \cdots = 1 + 1/16 + 1/81 + 1/256 + \cdots = \frac{\pi^4}{90},$$

[19] See (Euler 1735/1740b, E41) for the original; there is an English translation in the Euler Archive. For instructive and more detailed accounts than we can provide here, see (Dunham 1999) and (Sandifer 2007, 157–165).

and

$$1 + (1/2)^6 + (1/3)^6 + (1/4)^6 + \cdots = 1 + 1/64 + 1/729 + 1/4096 + \cdots = \frac{\pi^6}{945}.$$

He then continued in this way, concluding with the sum of the reciprocals of the 12th powers, and later went on to the 26th powers, obtaining the answer:

$$1 + (1/2)^{26} + (1/3)^{26} + (1/4)^{26} + \cdots = \frac{691}{6825 \times 93555}\pi^{26},$$

which is indeed correct!

Euler's way of treating the infinite series for sine as though it were a finite polynomial expression requires more justification than he gave it. It seems to have worried him too, for, as the historian Ed Sandifer has pointed out, Euler later gave a much more rigorous account (E63) that was published in 1743 in an obscure French journal. His method involves little more than a clever use of the formula for the arc length of the circle and term-by-term integration of an infinite series.[20]

Another important result of Euler's, published in his paper (1734/1740a, E43), is that the sum of the first n terms of the harmonic series

$$1 + 1/2 + 1/3 + 1/4 + 1/5 + \cdots + 1/n$$

is exceedingly close to the natural logarithm of n. In fact, as n becomes large, their difference

$$(1 + 1/2 + 1/3 + 1/4 + 1/5 + \cdots + 1/n) - \log n$$

approaches a fixed number, which is about 0.5772; this number is now known as 'Euler's constant', or the 'Euler–Mascheroni constant'. Little is known about it — we do not even know for certain whether it is an irrational number, although this seems likely.

Euler continued his investigations, and in (1737/1744, E72) and again in his *Introductio in Analysin Infinitorum* (1748) he observed that there was what has become known as a product formula. The later account is clearer. Euler began by observing that if one expands an infinite product of the form

$$(1 + \alpha x)(1 + \beta x)(1 + \gamma x) \cdots$$

as the infinite series

$$1 + Ax + Bx^2 + Cx^3 + \cdots$$

then $A = \alpha + \beta + \gamma + \cdots$ is the sum of the individual numbers in the product, B is the sum of pairs $\alpha\beta + \alpha\gamma + \beta\gamma + \cdots$, C is the sum of triples, and so on.

The quotient

$$\frac{1}{(1 - \alpha x)(1 - \beta x)(1 - \gamma x) \cdots} = 1 + Ax + Bx^2 + Cx^3 + \cdots$$

is particularly interesting. When each term $1/(1 - kx)$ is expanded by the binomial theorem and the results are multiplied together, then once again A is the sum of the numbers $\alpha, \beta, \gamma, \ldots$ taken singly, B is now the sum of the products taken two at a time (squares included), C is the sum of the products taken three at a time (repeats included), and so on.

[20] We shall not describe this proof of Euler's here — see (Sandifer 2007, 209–211) — because it does not seem to have had much impact in its day.

Euler now took $x = 1$ and $\alpha = 1/2, \beta = 1/3, \gamma = 1/5, \ldots$ — the reciprocals of the primes — and found that

$$\frac{1}{(1 - 1/2)(1 - 1/3)(1 - 1/5)\cdots} = 1 + 1/2 + 1/3 + 1/4 + \cdots,$$

which diverges. He next remarked that the same expansion works when any power of a prime is used, and wrote

$$\frac{1}{(1 - (1/2)^n)(1 - (1/3)^n)(1 - (1/5)^n)\cdots} = 1 + 1/2^n + 1/3^n + 1/4^n + \cdots,$$

'where all natural numbers occur without exception'.[21]

In his 1737 paper he proved the amazing result that, even if we throw away most of the terms of the harmonic series and keep only those that correspond to prime numbers,

$$1 + 1/2 + 1/3 + 1/5 + 1/7 + 1/11 + \ldots,$$

then there is still no finite sum: adding the reciprocals of the primes gives a divergent series. Although Euler did not say so explicitly, this result implies that there are infinitely many primes, and this theorem is sometimes referred to as Euler's proof of the infinitude of primes.

In these remarkable papers Euler brought together for the first time two seemingly unrelated topics: infinite series and prime numbers. This is the basis for the subject now known as analytic number theory.

8.3 Euler and geometry

Among the geometrical topics that Euler investigated were polyhedra. In a letter to Christian Goldbach, written partly in German and partly in Latin and dated 14 November 1750 (see Figure 8.1), he observed that the numbers of vertices S (angulae solidae, or solid angles, in Latin), H faces (hedrae), and A edges (acies) seem to be related by the simple formula:[22]

$$H + S = A + 2.$$

For example,

- a cube has 6 faces, 8 vertices, and 12 edges, and $6 + 8 = 12 + 2$
- a triangular prism has 5 faces, 6 vertices, and 9 edges, and $5 + 6 = 9 + 2$.

At first, Euler was unable to obtain a proof of his formula, but a year later he produced a dissection argument that involved slicing out tetrahedral pieces. Unfortunately, his proof was deficient, and a complete proof was not given until more than forty years later, in 1794, by the analyst and number-theorist Adrien-Marie Legendre.

[21]This result is known today as Euler's product formula. It was generalised by Riemann to the case where the positive integer n is replaced by a complex number s. The resulting function of s is called Riemann's zeta function, and it is the gateway to deep properties of prime numbers, as we briefly discuss in Section 18.2. As Sandifer remarked (2007, 256), Euler, unlike Riemann, never thought of the expressions as functions of the exponent.

[22]For a reprint of this letter, see the Euler–Goldbach correspondence (Euler 2015, nr. 149), and for a translation and commentary, (Biggs, Lloyd, and Wilson 1998, 74–89). This formula is sometimes incorrectly credited to Descartes, who did not have the terminology or motivation to derive it: indeed, it was Euler who first introduced the concept of an edge.

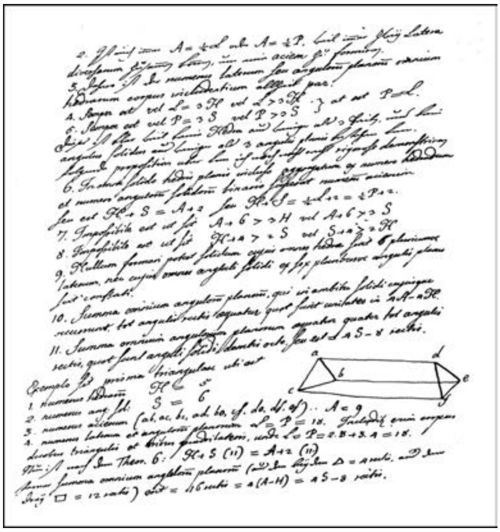

Figure 8.1. Euler's letter to Goldbach, 14 November 1750

The geometry of position. In September 1679, Leibniz had written to Christiaan Huygens, remarking:[23]

> I am still not satisfied with algebra, because it does not give the shortest proofs or the most beautiful constructions in geometry. That is why I believe that, so far as geometry is concerned, we need still another analysis which is distinctly geometrical or linear and which will express *situation* [situs] directly, as algebra expresses *magnitude* directly.

It is not clear what Leibniz had in mind, but it was interpreted by some, including Euler, as referring to a type of geometry in which metrical ideas, such as distance, length, and angle, do not arise. This subject is now known as topology.

On 26 August 1735 Euler presented to his colleagues at the Academy of Sciences at St Petersburg his solution of 'a problem relating to the geometry of position'. It concerned the medieval Prussian city of Königsberg which was divided into four areas by the river Pregel. Figure 8.2, taken from a 17th-century map of the city, shows the four areas and the seven bridges connecting them; Euler's own diagram (see Figure 8.3) is clearer because it ignores the strictly geometrical features of length and angle.[24] The problem was: To go for a connected walk around the city, crossing each of the seven bridges only once. Euler proved that the task is impossible, and showed how his arguments can be extended to any problem of a similar type.

Euler seems to have been intrigued by the fact that the Königsberg problem apparently belonged to the geometry of position (as he interpreted it), and on 13 March 1736 he wrote to Giovanni Marinoni, Court Astronomer in the court of Kaiser Leopold I in Vienna, in the following words:[25]

> This question is so banal, but seemed to me worthy of attention in that geometry, nor algebra, nor even the art of counting was sufficient to solve it. In view of this, it occurred to me to wonder whether it belonged to the geometry of position, which Leibniz had

[23]See (Leibniz 1969, 248–249).
[24]See (Euler 1736/41, 128, E53).
[25]Euler, *Opera Omnia* (4), Vol. 1, nr. 1468.

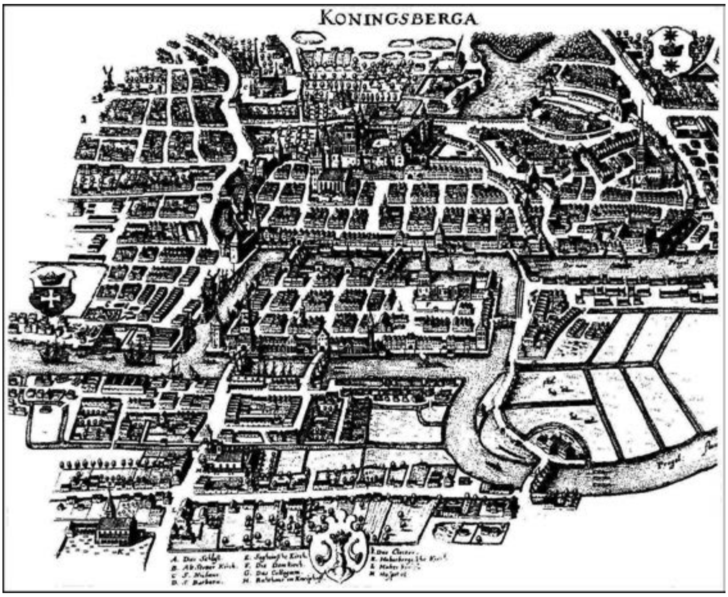

Figure 8.2. A map of the city of Königsberg.

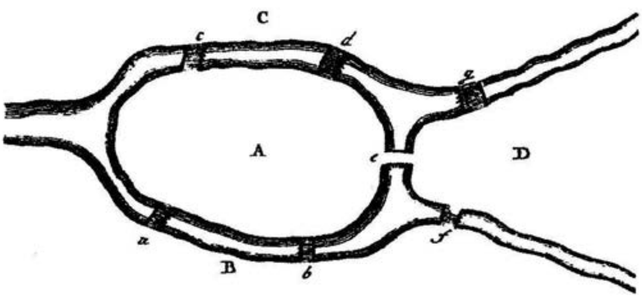

Figure 8.3. Euler's map of the seven bridges.

> once so much longed for. And so, after some deliberation, I obtained a simple, yet completely established, rule with whose help one can immediately decide for all examples of this kind, with any number of bridges in any arrangement, whether such a round trip is possible, or not.

In another letter, sent to Karl Ehler, an amateur mathematician and the Mayor of Danzig,[26] dated 3 April 1736, Euler observed:[27]

> Thus you see, most noble Sir, how this type of solution bears little relationship to mathematics, and I do not understand why you expect a mathematician to produce it, rather than anyone else, for the solution is based on reason alone, and its discovery does not depend on any mathematical principle … In the meantime, most noble Sir, you have assigned this question to the geometry of position, but I am ignorant as to what this new

[26] Danzig, now Gdansk in Poland, is some 80 miles west of Königsberg.

[27] Euler, *Pis'ma k ucenym*, Izd. Academii Nauk SSSR, Moscow–Leningrad (1963). See (Hopkins and Wilson 2007).

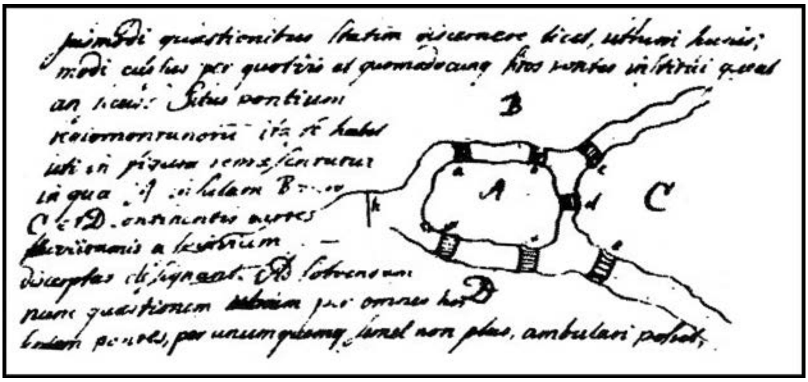

Figure 8.4. Euler's letter to Marinoni

> discipline involves, and as to which types of problem Leibniz and Wolff expected to see expressed in this way.

Euler wrote up his solution in a paper that has become celebrated.[28] It is in this paper that Euler first referred to the geometry of position (geometria situs) as the geometrical analysis mentioned by Leibniz.

Although several writers have claimed that Euler solved the problem by drawing a network or graph with four vertices (representing the four areas of the city) and seven edges (representing the bridges), he did not do so. His approach to the problem was to count the number of bridges emerging from each area and to note how many of these numbers are odd. His conclusions were as follows:

- If there are more than two areas to which an odd number of bridges lead, then such a journey is impossible.
- If, however, the number of bridges is odd for exactly two areas, then the journey is possible if it starts in either of these two areas.
- If, finally, there are no areas to which an odd number of bridges lead, then the required journey can be accomplished starting from any area.

For the Königsberg bridges problem, the numbers of bridges are 3, 3, 3, and 5; since this list contains more than two odd numbers the problem has no solution. However, for the system of bridges in Figure 8.5, Euler observed that the numbers of bridges that emerge from the six areas A–F are 8, 4, 4, 3, 5, 6, respectively. Since just two of these numbers (corresponding to areas D and E) are odd, a walk is possible as long as it begins in one of these areas and ends in the other.

8.4 The study of curves

We shall pursue two themes: first, the study of where two curves intersect, which has its origins in the 17th–century study of the solution of equations; second, the study of curves in their own right. We shall see that what ensued took the form of almost reversing the roles of algebra and geometry, until algebraic reasoning replaced geometrical reasoning as the source of validity in mathematics.

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[28]See (Euler 1736/1741, E53), and (Biggs, Lloyd, and Wilson 1998) for a translation.

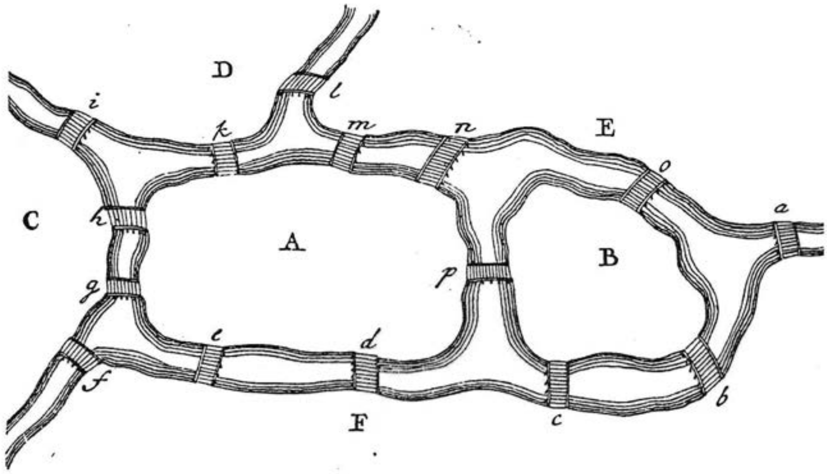

Figure 8.5. A new system of bridges

In the 17th century mathematicians generally solved equations in the manner of Descartes, namely, via a construction that exhibited their roots as the coordinates of points of intersection of curves.[29] By the middle of the 18th century this approach had begun to seem more trouble than it was worth. However, there was still interest in the question of saying something about the points in which two curves meet one another, because this question has both a geometrical and a more practical significance. To see why, we start as they did by asking this question: Given two curves, how many meeting points are there?

A case of this that we have already met is when one curve is given by a polynomial equation

$$y = a_n x^n + a_{n-1} x^{n-1} + \cdots + a_0$$

and the other curve is the straight line $y = 0$. Eliminating y between these equations gives

$$a_n x^n + a_{n-1} x^{n-1} + \cdots + a_0 = 0$$

as an equation specifying the x-coordinates of the points of intersection, and we have seen that the answer was widely believed to be that there are n solutions to this equation and so n points of intersection of the two curves. This case is already not easy to analyse and other cases are much harder, so the central topic in geometry became that of finding where one curve meets another (see Figure 8.6). More precisely, it became the algebraic version of this: Find the points that simultaneously satisfy two equations in x and y.

The method of attack was always the same. There are two equations, one for each curve, and we first eliminate y, obtaining an equation in x alone (known as the 'resultant' of the original equation). Then we solve this resultant equation in x. The branch of algebra involved in the first stage was called elimination theory, and it entailed dealing with various other problems, such as: Can we eliminate y? If we could rewrite any equation in x and y so that y occurs only once (and the equation has been written in the form $y = f(x)$) then the problem would be easy to solve — but this cannot always

[29]See *Volume* 1, Section 11.2.

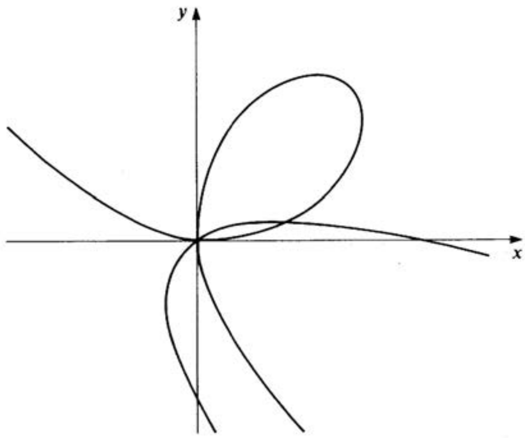

Figure 8.6. Two curves crossing — but in how many points?

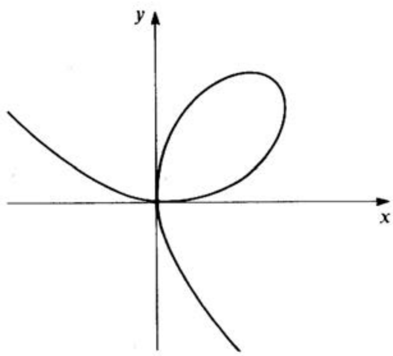

Figure 8.7. The folium of Descartes

be done, as is shown by the folium of Descartes (see Figure 8.7), which is given by the equation

$$x^3 + y^3 = 3xy.$$

We can get some idea of the importance of elimination theory from this call for a good attack on the problem in 1770 by the Académie des Marines, the scientific school attached to the French Navy:[30]

> The elimination of unknowns is one of the most important parts [of mathematics] to perfect, both because the extreme length of ordinary methods makes it so repugnant and because the general resolution of equations depends on it.

The first thing that mathematicians sought to establish was how many common points two curves should have. This obviously depends on the curves, but there was a widespread 17th-century belief that if one curve is of degree m and the other is of degree

[30]Quoted in (Rider 1981, 168).

n then they have mn points of intersection. In the 18th century this 'truth' gradually changed its status from a useful heuristic principle to something requiring a proof.

But before this principle could be proved true, there were some difficulties to be overcome. For a start, it was evidently false in general: curves of degree m and n do not always appear to intersect in precisely mn points. Two circles (curves of degree 2) may meet in 0 or 2 points (and perhaps in 1 point on occasion, depending on how you count points of tangency), but not visibly in the $2 \times 2 = 4$ points that the formula implies. So the principle seems to have collapsed even before it gets going. On the other hand, it is certainly true in many cases. Two conics meet in at most four points, and that they do sometimes meet in precisely four is evident from Figure 8.8.

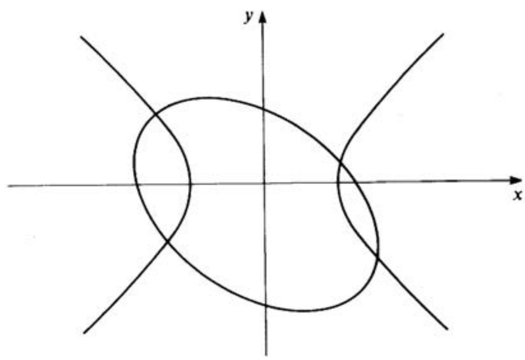

Figure 8.8. Two conics that meet in four points

In fact, the principle that curves of degrees m and n intersect in precisely mn points is a rare and interesting example of something more common in science than in mathematics: a true result that seems superficially to be false. On such occasions it often transpires that one is not looking at the situation in quite the right way: with a slight redefinition of terms, the apparent exceptions can be seen to have been correct all along. Here, the key issue is: How do you count a point of intersection? If one is sufficiently convinced that the principle must be true, then it may not be beyond human wit to count intersections in such a way as to ensure this.

Consider each of the figures in Figure 8.9, showing various intersections of a circle (degree 2) and a line (degree 1). By the principle there should be $2 \times 1 = 2$ points of intersection.

In figure (a) there are clearly two points of intersection. In figure (b) we can persuade ourselves that the point of tangency is a 'double point' and count it as two points — wobble the line a bit as in figure (c) if you are still in doubt.[31] But what of figure (d)? How can the most vivid imagination conjure two points of intersection out of curves that patently do not intersect at all?

However, 18th-century mathematicians were gradually becoming accustomed to using complex numbers. Here the answer seemed perfectly straightforward: the fourth figure is a picture of a line meeting a circle in two complex points. This may be hard to see if you have a purely geometrical perception, but to mathematicians of a firmly

[31] Descartes and Fermat's ideas about tangents were discussed in *Volume* 1, Section 9.1.

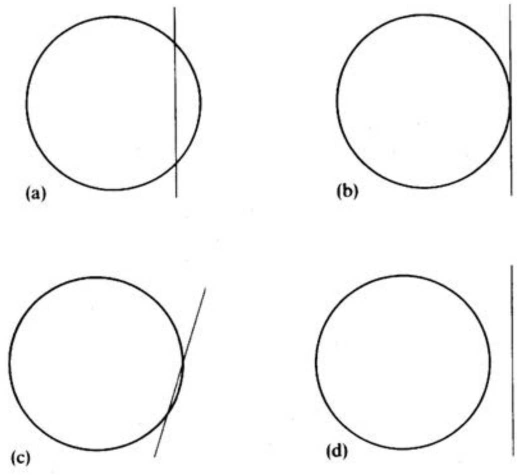

Figure 8.9. Four possibilities for a circle meeting a line

algebraic turn of mind it made excellent sense. For, in just the same way as a quadratic equation with no real roots (such as $x^2+2x+5=0$) turns out to have two complex roots — thus preserving the principle that every polynomial equation has as many roots as its degree — so here complex points can be imagined to preserve the *mn* principle. We see vividly here the motivation for people to shift geometrical questions into an algebraic form, and how algebra and complex numbers came to be preferred to geometry and only real intersection points.

This is illustrated very elegantly for us by a short paper (E148) that Euler published in 1748, where he pointed out that for the theorem to be true one must be able to say a number of rather implausible things. The problems that Euler saw were that:

- even parallel lines meet in a point — this requires the invention of points at infinity
- the parabola in Figure 8.10 meets the line ℓ_1 in two points, both imaginary, and meets the line ℓ_2 in two points, one of which is at infinity.

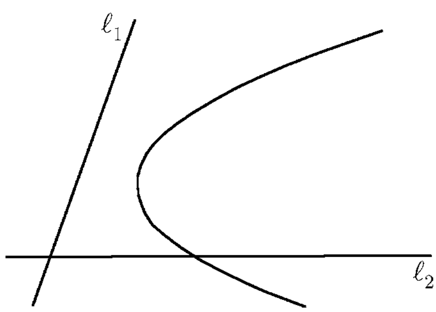

Figure 8.10. A parabola and two lines

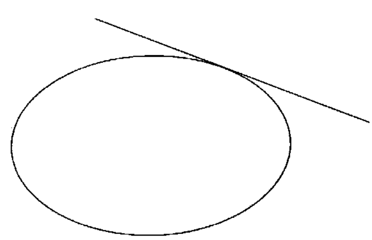

Figure 8.11. An ellipse and a tangent

A case that Euler did not mention, that of tangency, is exemplified in Figure 8.11. For the ellipse to meet the line in two points, common points to the two curves must be counted the 'correct' number of times — in this case, two, not one. (The term for this is 'multiplicity'.) In other words, we must alter the way we count, in order to make the theorem correct.

Combinations of these difficulties are also possible. As we have already remarked, two circles should meet in $2 \times 2 = 4$ points, but it is clear they meet in at most 2. Where are the others? In this case the resolution is that the other points are both complex and at infinity! A simple algebraic conjecture is turning into something rather difficult. The hardest thing to define was multiplicity, and Euler was never to find a suitable definition of it: the problem was too complicated. In November 1751 Euler wrote to the mathematician Gabriel Cramer (who had also considered the problem) that it led to 'such tangled formulas that one completely loses patience in pursuing the calculations'.[32]

The Académie des Marines disparaged the attempts by Euler and Étienne Bézout, another person who had tackled the problem, because they had not shown that the resultant had the right degree (*mn*). But in 1779 Bézout announced a solution that satisfied his contemporaries, and the principle has been called Bézout's theorem ever since. A remarkable feature is that it does not determine the solutions of the resultant equation and so locate the common points. It is an example of a non-constructive existence proof: it tells you that certain points necessarily exist, but it does not tell you where they are or how to find them. All that it does is to establish that the resultant has degree *mn*, and so the two curves meet in *mn* points.

Bézout's proof of his theorem was ingenious but artificial, and it was soon replaced by one due to a leading French mathematician of the early 19th century, Siméon-Denis Poisson. But his proof also fails to clinch the result by modern standards, for the same reason that attempts on the Fundamental Theorem of Algebra were flawed: a rigorous theory of complex numbers is required in order to deal with these existence questions.

Note how algebraic the original problem about curves has become. A complicated algebraic exercise was required to show in how many points two curves meet, by exhibiting a polynomial of the right degree. The answer to a geometrical question is now the degree of a polynomial! A hundred years earlier, the answer to a question about a polynomial was sought in the behaviour of suitably chosen curves. So over the course of a century the roles of geometry and algebra became completely reversed.

But if questions about plane curves were no longer to be raised in order to solve equations, what about those questions about curves that arose naturally? The Académie des Marines was in no doubt about their importance, and the geometry of curves exerted its own fascination, independently of any applications. The two most influential textbook presentations were due to Euler and Cramer; Euler's was his *Introductio in Analysin Infinitorum*. Euler had originally asked Cramer to oversee the printing of the *Introductio*, but because of a conflict of interests that job eventually went to someone else, for Cramer was by then at work on his own *Introduction à l'Analyse des Lignes Courbes Algébriques* (Introduction to the Analysis of Algebraic Curved Lines), which

[32]The Euler–Cramer correspondence is published in Euler, *Opera Omnia* (IVA) Vol. 7, number OO474, quoted in (Rider 1981, 168).

appeared in 1750. Cramer's was, if anything, the more influential treatment of geometry, but we start with that of Euler.

The first significant difference between the 17th and 18th centuries in this respect is the much greater readiness of later mathematicians to define curves by means of equations, and not merely to introduce the algebra as a technical convenience. This is made quite clear in Euler's *Introductio*, Book II. Here he began by describing how any function y of a variable quantity x can be exhibited as a curve, by imagining x to vary along a straight line ℓ, and plotting the y-values corresponding to each x along a line at right angles to ℓ. Conversely, each curve defines a (possibly many-valued) function when we reverse the process. So it is Euler who standardised what are now inappropriately called Cartesian coordinates (though others before him had used right-angled coordinate systems when it suited them). In so doing, Euler put into a textbook a way of working that he had already found in dealing with mechanics, as we shall see in Section 10.2.

A good example of the benefits of his new approach can be seen in Euler's discussion of conic sections in the *Introductio*.[33] The balance of algebra and geometry in Euler's text is most interesting. Euler started from 'a general equation for lines of the second order' and reached the division of conics into the three basic kinds: hyperbola, ellipse, and parabola.

There is one bit of algebraic technique that Euler slipped through in a line, but is rather important so we dwell on it longer. At the beginning of the second paragraph, Euler spoke of moving from his starting point of the 'general equation for lines of the second order', by which he meant an equation of the form

$$axx + bxy + cyy + dx + ey + f = 0,$$

to the equation that he used for distinguishing the different conics, which was

$$yy = \alpha + \beta x + \gamma xx.$$

This latter equation was just as general for his purposes — 'all the lines of the second order may be contained in this equation' — and once he had reached it his results on conic sections fell out very rapidly, by considering the different values γ could have, as the next extract shows.

But first, let us see how he got from one equation to the other. This is where Euler's new algebraic perspective on coordinate geometry came into play: what he had done was to transform the coordinates. Euler recognised that it made life much simpler to set up coordinate axes beforehand and at right angles — this is the advance he made on Descartes — and also that a useful technique could be to transform the coordinate axes in mid-problem. This can be thought of geometrically, as shifting the figure about in relation to its background axes, or algebraically, as changing the labelling of points in relation to the axes. In either way one can reach a simpler equation that retains all the crucial properties.[34]

[33]See Vol. 2, Chapter 5.

[34]See Euler, *Introductio in Analysin Infinitorum*, II, and F&G 14.A4.

Euler on conic sections.

> However the greatest difference in the curved lines which are included in the equation $yy = \alpha+\beta x+\gamma xx$ is produced by the character of the coefficient γ, depending on whether it has a positive or a negative value. For if γ has a positive value, assume that the abscissa x is infinite, in which case the term γxx turns out to be infinitely greater than the remainder $\alpha+\beta x$ and for that reason the expression $\alpha+\beta x+\gamma xx$ acquires a positive value. The ordinate y will likewise acquire two infinitely large values, one positive, one negative, because the same thing happens, if $x = -\infty$. In this case, however, the expression $\alpha + \beta x + \gamma xx$ assumes an infinitely great positive value. On account of this, if y becomes a positive quantity, the curve will have four branches stretching out to infinity, two corresponding to the abscissa $x = +\infty$ and two corresponding to the abscissa $x = -\infty$. Therefore these curves which have four branches stretching out to infinity, are thought to constitute one type of lines of the second order, and are called 'hyperbolas'.
>
> If, however, the coefficient γ has a negative value, then if $x = +\infty$ or $x = -\infty$, the expression $\alpha + \beta x + \gamma xx$ will have a negative value and therefore the ordinate y becomes imaginary. Therefore nowhere in these curves will an abscissa or an ordinate be able to be infinite. For that reason no part of the curve will be able to extend to infinity, but the whole curve will be contained in a finite and limited space. So this type of lines of the second order acquires the name of 'ellipses', on account of the fact that their character is contained in this equation $yy = \alpha + \beta x + \gamma xx$, if γ is a negative quantity.
>
> Therefore if the value of γ produces such a different character of lines of the second order, depending on whether it is positive or negative, that on this account two different sorts are rightly created: if $\gamma = 0$, a value which is midway between affirmative and negative numbers, the curve resulting from this also constitutes a certain type midway between hyperbolas and ellipses, called the 'parabola', which therefore expresses its nature by the equation $yy = \alpha + \beta x$.

Notice the dramatic change in perception consolidated by Euler's work. Even those quintessentially geometrical objects, the conic sections, were barely geometrical any longer, but were defined by equations: in their definition, treatment, and the style of argument in deducing properties, the conic sections had become algebraic objects. There is still a faintly geometrical haze, it is true, in that the algebra refers to geometrical figures and their properties. Also, Euler's argument for distinguishing the conic sections according to the value of γ amounts to what we would think of as 'curve sketching' — that is, making general inferences about the form of a curve from the form of its equation. But we are now close to a situation where the geometry of conic sections could drop out of sight altogether, for all the difference that would make to the richness and power of the algebraic analysis. Indeed, Euler went on to derive almost all the elementary properties of conics algebraically — properties such as the existence and location of their centres, foci, axes, diameters, and asymptotes — and for better or worse succeeding generations could, if they chose, discard the geometrical methods in geometry altogether.

Newton would surely not have approved of the way that Euler began by deriving equations for the curves and then deducing their properties from these equations. He might, however, have approved of Chapter IX of the *Introductio*, in which Euler treated cubic curves in the same way, because even Newton had needed to rely on algebra then — plainly, Newton knew when not to take his own advice!

Newton had worked out a classification of cubic curves by type in 1667, but he published it only in 1704, as an Appendix to his *Opticks*. His account was daunting and sometimes obscure, but at one point he remarked that it followed from his classification that there are five basic cubic curves; any other cubic can be obtained as the shadow of one of these five under projection from a point source of light.[35]

Here is a fascinating blend of geometry and algebra. A cubic curve is a curve, and so a geometrical object, and the adjective 'cubic' refers to geometrical ideas — it meant, according to Newton, that a straight line meets the curve in 3 points. But the analysis of cubics starts from their equations and proceeds by symbol manipulation until each equation is reduced to its simplest form — it is algebra all the way. Their forms were then drawn and described geometrically, and above all the argument about shadows, being about shape, is geometrical. So, when we think of the way that algebra was to grow until geometry was swamped, it is interesting to see how Newton's successors took their stands on the question of geometry versus algebra. What do we find?

Euler's approach to the study of algebraic curves was geometrical in its aims, but algebraic in its methods. It was geometrical, in that he investigated what might happen in general to a curve as x and y become very large — or go to infinity, as he put it — and he looked in particular for curves of lower degree that closely approximate the curve near infinity. It was algebraic, in that his investigation was firmly rooted in coordinate methods.

When Euler applied these techniques to cubic curves he confirmed Newton's results, as did Cramer in his *Introduction*. He noted that the subdivision into species was rather arbitrary, and gave canonical equations for each of the five main families. He then turned to properties of cubic curves. Here, several results were already known, of which the most striking may be this (although neither Euler nor Cramer mentioned it):

> any line through two inflection points on a cubic curve meets the curve again in a third inflection point.

This result had been proved first by de Gua (1740), and again by the Scottish mathematician Colin MacLaurin in an Appendix to his *Treatise on Algebra* of 1748.[36] But Euler did not take up the idea of shadows, nor did Cramer, and nor did anybody else apply it to the theory of curves for over half a century afterwards. It is tempting to believe that its neglect by Euler and Cramer led to this idea disappearing for such a long time. Instead, and in line with his treatment of geometrical approach to curves in terms of their behaviour for large values of the x and y coordinates, Euler classified cubics in 16 species, and noted which of Newton's curves belong to which species.

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[35]See Whiteside, *MPIN* VII, 589, 635, and F&G 12.D2.

[36]We discuss MacLaurin's work in greater detail in Chapter 9.

Euler also attempted a classification of curves of degree 4, but found that there would be 146 different types, not all of which he could analyse, and accordingly he did not progress to curves of degree 5 or more.

Gabriel Cramer was Swiss, like Euler, and seems to have been on good terms with him. At the age of 20, with another scholar, Giovanni Calandrini, he applied for the professorship in philosophy at the Académie de Calvin in Geneva in 1724. The Académie was so impressed with both of them that a new Chair in mathematics was created, and both men were appointed and instructed to share the work. They were both privately wealthy, and they took it in turns to travel and study while the other stayed in Geneva and taught (and drew the salary). Eventually, in 1734, Calandrini was appointed Professor of Philosophy and Cramer became Professor of Mathematics, a post he retained until his unexpected death at the age of 48.

On his travels Cramer spent five months in Basel, where he got to know Johann Bernoulli, Daniel Bernoulli, and Euler, before going on to London and Paris. He was asked by Johann Bernoulli to be the sole editor of his works, which he published in four volumes in 1742, and also of his brother Jakob's works, of which he published two volumes in 1744.

Cramer's best work was his *Introduction à l'Analyse des Lignes Courbes Algébriques*, which he published in 1750. Although he had had a draft copy of Euler's *Introductio* for a month in 1744, scholars incline to the view that his own work was largely independent, and in any case, Cramer's became the better-known treatment of geometry. The French mathematician Michel Chasles, in his history of geometry of 1837, called it 'the most complete and even today the most highly regarded treatise of this vast and important branch of geometry".[37]

The success of Cramer's *Introduction* derived partly from being written in French (Euler wrote his in Latin), partly from being very thorough (it is twice the size of Euler's), and partly from being lavishly and beautifully illustrated (over 300 curves were drawn, see Figure 8.12). It necessarily overlaps considerably with Euler's treatise. Cramer defined curves algebraically and studied them algebraically, as had Euler — as Cramer put it, 'his object being almost the same as mine, it is not surprising if we are often together in our conclusions'. Nonetheless, Cramer emphasised different topics. He was more interested in points where the curve intersects itself, and in how it looks 'near infinity' (when x or y is very large). But he was not interested in the way in which those two topics interrelate, a question on which another mathematical writer, de Gua, was particularly insightful. Still, Cramer's questions seem to be a geometer's questions, because they are about shapes. The direction of his thought was: Given this equation, what does it mean and what does it describe?

We can gain a vivid sense of how geometry and algebra stood if we look at the prefaces to the books by de Gua and Cramer.

De Gua had more than his share of personal misfortune — his parents plunged from riches to bankruptcy when he was a child. At first he contemplated a religious life before becoming a mathematician and scientist. In 1742 he was appointed Professor of Greek and Latin Philosophy at the Collège Royal in Paris (later the Collège de France) — apparently on the strength of his mathematical work of 1740 — and lectured on mathematics and Newtonianism until he resigned in 1748. During this period he worked with D'Alembert and Diderot in setting up the *Encyclopédie*. Then he turned

[37] See (Chasles 1837, 152).

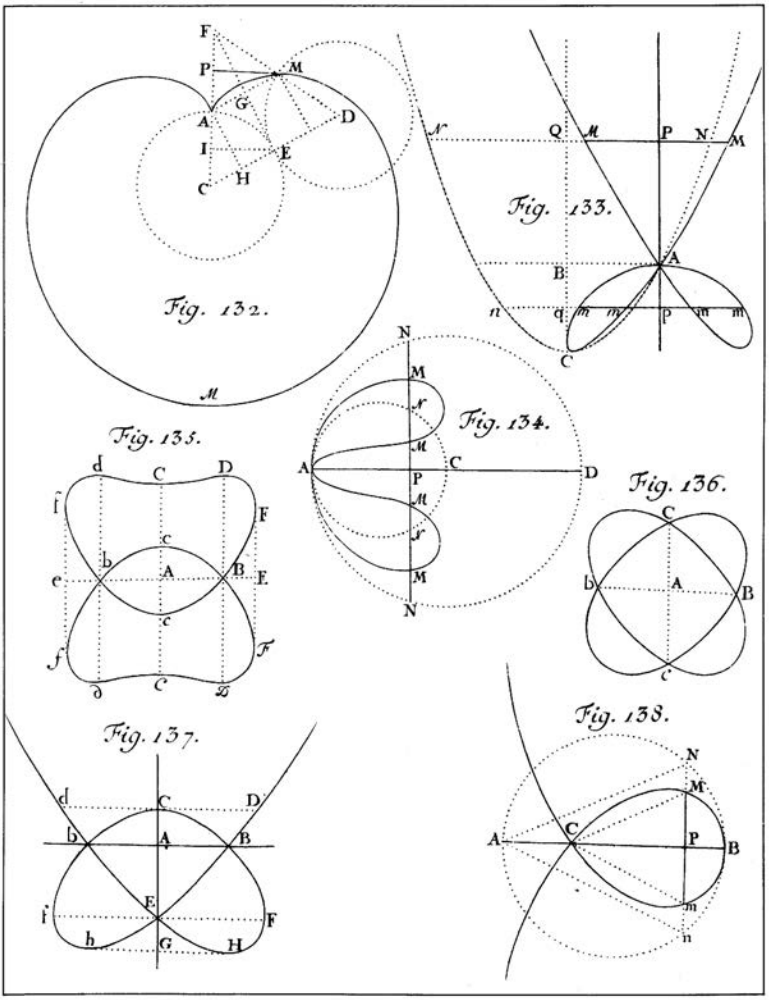

Figure 8.12. Some algebraic curves, from Cramer's *Introduction*

to economic theory, became a gold prospector in the Languedoc, which nearly ruined him, and as an old man wrote voluminously on mineralogy and conchology.

De Gua's study of curves has a title that is almost a story in itself: *Usages de l'Analyse de Descartes pour Découvrir, Sans le Secours du Calcul Differentiel, Les Propriétés, Ou Affections Principales des Lignes Géometriques de Tous les Ordres.* (Uses of Cartesian Analysis for Discovering, without the Help of Differential Calculus, the Properties of Geometrical Lines of all Orders) (1740). The avoidance of the calculus is indicative of a distinctive geometrical preference.

The book was influential. De Gua had absorbed not only the principles of Descartes's algebraic analysis, but also Newton's approach to cubics, the hints of projective shadow geometry. Putting these together he was able to reach some novel perceptions about aspects of curves and ways of approaching them. In particular, he saw the virtues of transforming coordinates so as to produce different equations relating to a curve, equations that would highlight some aspect of the curve's properties — and in making the coordinate transformations he was guided by the Newtonian vision of lights and shadows, so as to make what we might call 'projective transformations'; an example is given in Box 21.

It is clear from the preface of de Gua's book that although (in his opinion) the calculus is more general — it can deal with mechanical or transcendental curves as well as algebraic ones — algebraic analysis is much better when it comes to dealing with geometrical curves, and geometrical properties, such as singular points and infinite branches.[38] In this spirit, he set himself the task of understanding many of the things that Newton had said about cubic curves. His methods were algebraic: he used coordinate transformations to transform the equations until the properties of the curves became more apparent: this led him to recognise an unexpected use for the differential calculus. But when he came to consider his main result, the connection between singular points and infinite branches, he found that 'the source of the analogy [lay] in the theory of shadows'. Thus the techniques were algebraic, but he considered the underlying explanation to be geometrical.

By contrast, Cramer's preface balances the merits of algebra and geometry differently.[39] Whereas de Gua had listed the possible kinds of singular points and infinite branches of curves of the first five degrees, Cramer attempted to go further and sort them into types, but he stopped with quartics because they proved too complicated. In this sense, de Gua gave more detail about the 'pieces' a curve comes in, but Cramer did more to fit the pieces together to provide a global picture. Only de Gua connected singular points with infinite branches, and invoked the idea of projection, but Cramer studied many more curves. Cramer seems to have been nearer to Euler's position, namely that geometrical properties should be explained by algebraic arguments.

We note in passing how full and warm were the tributes that both de Gua and Cramer paid to their predecessors; Cramer, especially, comes over as a very courteous and friendly person, although even he let slip a barbed comment about Newton preferring 'the pleasure of being admired to that of instruction'.

We conclude, however, on a downbeat note. Under the influence of Euler and Cramer, the study of curves became more and more algebraic. Other authors did not disdain the subject either, although none wrote books with such authority as these two. But the study of geometry proceeded at a much slower pace than that of other subjects, so that it is legitimate to speak of a real relative decline. Geometry was no longer the source of rigour — algebra supplied that — nor of the most powerful techniques — the calculus supplied those. Geometry survived because of its own interest and charms, but in the course of the 18th century the new algebraic analysis became the most vital branch of mathematics.

Indeed, if there was anything that Euler did not do, or on which his influence was not wholly generous and beneficial it was the geometry of curves. His study of algebraic curves was considered very complicated by Julius Plücker,[40] the geometer who took up the subject most energetically in the early 19th century, and arguably it contributed to a feeling that algebraic geometry was just too hard beyond degree 4. In particular, it contributed to the growing neglect of projective methods in geometry, after

[38] See (de Gua 1740), and F&G 14.D1.

[39] See (Cramer 1750) Preface, and F&G 14.D2.

[40] For details of Plücker's work in geometry, see Section 15.2.

Box 21. De Gua's projective transformations.
We consider the curve defined by the equation $y^2 = x^3$, depicted below.

How can we begin to understand the properties of this curve? In particular, what happens as it goes off to infinity? De Gua found that a suitable projective transformation would give him the curve $y = x^3$ (also below) — and by this process the curve was transformed into a new curve with an inflectional tangent.

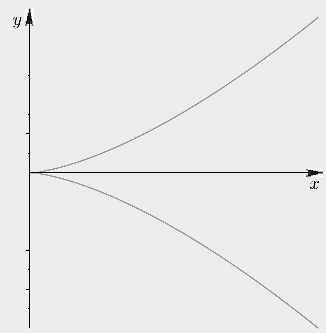

Figure 8.13. Graph of $y^2 = x^3$

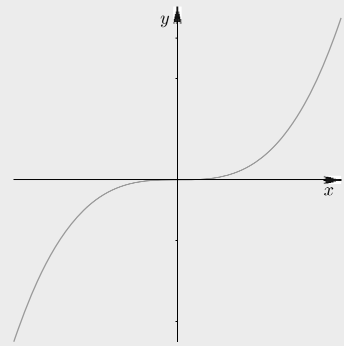

Figure 8.14. Graph of $y = x^3$

To picture this remarkable finding, think of a torch as casting the shadow of the x-axis to infinity. Then all of the curve has a shadow image, except for the cusp point itself (because it lies on the x-axis). To understand which points are transformed we argue as follows. On the original curve we can travel continuously along, albeit with a somewhat abrupt reversal, or turn, at the cusp point; in the transformed curve we travel steadily in an increasingly north-easterly direction when suddenly we find ourselves coming in from the south-west. This is also disconcerting in its way, but we have learned more about the original curve: it is a curve with an inflectional tangent in projective disguise.

Observations of this kind were the basis of de Gua's analogy, through projective transformation, between singular points and infinite branches of curves.

the start given to them by Desargues, Blaise Pascal, de la Hire, and Newton. The revival of projective methods, and the creation of projective geometry, were to be among the achievements of the early 19th century, as we shall see in Chapter 15.

8.5 Further reading

Bradley, R.E. and Sandifer, C.E. 2007. *Leonhard Euler: Life, Work, and Legacy*, Studies in the History and Philosophy of Mathematics, Vol. 5, Elsevier. This is a varied collection of useful articles, including Hopkins and Wilson's 'The Truth about Königsberg'.

Richeson, D.S. 2008. *Euler's Gem: The Polyhedron Formula and the Birth of Topology*, Princeton University Press. The author looks at the study of polyhedra from the ancient Greeks to the modern theory of surfaces.

Takase, M. 2007. Euler's theory of numbers, in *Euler Reconsidered*, R. Baker (ed.), Kendrick Press, 377–421. This is not an easy read for a beginner, but is one of a number of useful essays in a rich volume.

Watkins, J.J. 2013. *Number Theory: A Historical Approach*, Princeton University Press. This book combines an approach to number theory through its problems with an account of its history.

Weil, A. 1984. *Number Theory: An Approach through History from Hammurapi to Legendre*, Birkhäuser. André Weil was one of the leading mathematicians of the 20th century, and his surprisingly readable and stimulating account repays careful study.