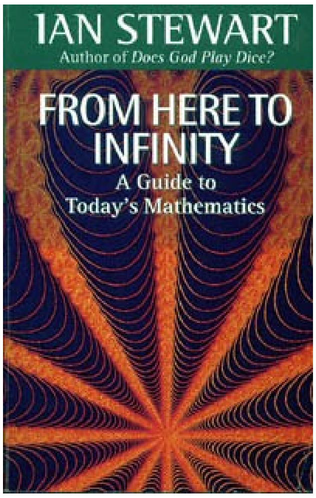

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From Here to Infinity

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From Here to Infinity

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Foreword

Mathematics is not a book confined within a cover and bound between brazen clasps, whose contents it needs only patience to ransack; it is not a mine, whose treasures may take long to reduce into possession, but which fill only a limited number of veins and lodes; it is not a soil, whose fertility can be exhausted by the yield of successive harvests; it is not a continent or an ocean, whose area can be mapped out and its contour defined: it is as limitless as that space which it finds too narrow for its aspirations; its possibilities are as infinite as the worlds which are forever crowding in and multiplying upon the astronomer's gaze; it is as incapable of being restricted within assigned boundaries or being reduced to definitions of permanent validity, as the consciousness of life, which seems to slumber in each monad, in every atom of matter, in each leaf and bud cell, and is forever ready to burst forth into new forms of vegetable and animal existence.

JAMES JOSEPH SYLVESTER

Preface

What is mathematics? What is it for? What are mathematicians doing nowadays? Wasn't it all finished long ago? How many new numbers can you invent anyway? Is today's mathematics just a matter of huge calculations, with the mathematician as a kind of zookeeper, making sure the precious computers are fed and watered? If it's not, what is it other than the incomprehensible outpourings of super-powered brainboxes with their heads in the clouds and their feet dangling from the lofty balconies of their ivory towers?

Mathematics is all of these, and none. Mostly, it's just different. It's not what you expect it to be. Even when it looks as if it *is* what you expect it to be, you turn your back for a moment and it's changed. It's certainly not just a fixed body of knowledge, its growth is not confined to inventing new numbers, and its hidden tendrils pervade every aspect of modern life.

Mathematics is changing very rapidly indeed. You can tell that just by following the career of this book, which first saw daylight in 1987 under the title *The Problems of Mathematics*. By 1992 several fundamental new discoveries had made a second edition imperative. The title remained the same, but the cover became more jazzy, with a fancy computer graphic in place of a design made from coloured pins. And now it has become necessary to produce a third edition, with a user-friendly title and a distinct shift of emphasis. The new title, by the way, is supposed to indicate that mathematics combines relevance to everyday life ('here') with sweeping intellectual invention ('infinity'). The new emphasis is more on the overall ebb and flow of mathematics, and less on particular problems.

A lot happened during those eight years. The most dramatic was Andrew Wiles's proof of Fermat's Last Theorem, announced in 1993 but not completed until 1995, and then only after some heart-stopping glitches. Chaos and fractals outgrew their origins and smeared themselves across the face of science like bread and jam in

the hands of a toddler. The intellectual equivalent of egg smeared itself over the faces of a few diehard scientists who had alleged that chaos was just computer error and didn't really exist. Oh, Chaos Theory was hyped, no doubt about itbut it was hyped because it was *important*. The classical understanding of knots took a totally unexpected step sideways, matured overnight like a field of mushrooms, and transferred itself into the biochemistry of DNA and Richard Feynman's diagrammatic representations of the collisions of quantum particles. These areas in turn fed new ideas back into knot theory, and the dance still continues, swirling and whirling across the mathematical landscape. The circle was squared, literally, by chopping it into 10^{50} pieces and reassembling them into a square with not a single point out of place. Schrödinger's cat proved it was alive after all by letting itself out of the bag and revolutionizing cryptography. One mathematician found a way to construct billions upon billions of 'Carmichael numbers', and three others promptly trumped him by using the same method to prove the existence of infinitely many. You will find all these stories, and many others like them, within these pages.

Progress does not always move in the forward direction; mathematics must occasionally mount a 'strategic withdrawal'. A proof of the Poincaré conjecture, the holy grail of topology, was announced in the first edition of this book and deleted from the second edition when one key step fell to bits. It may yet resurface in the fourth edition, you can never tell in this subject. The second edition contained an entire new chapter on the solution to the Kepler Problem, which is to prove that the most efficient way to stack spheres is the one known to every greengrocer. You won't find it in this edition, because the proposed proof has become distinctly controversial. It has not so much collapsed as disappeared behind an impenetrable veil of fog. Even though mathematical truth is much more clear-cut than truth in any other area of science, controversy can still muddy its waters. One of the current long-running debates focuses around so-called 'experimental mathematics', which emphasizes the role of computers in suggesting new truths. This would not be terribly controversial, except that it has been interpreted by some people as a

downgrading of the traditional concept of proof. (Look, it *isn't*, OK?) So now we have a meta-controversy about whether the original controversy was or was not aimed at a straw man. Heady stuff, and it certainly makes you re-examine your prejudices. Some mathematicians don't like controversies, but they do serve to remind us that mathematics is created by human beings who *care* about it, and I think that's an important message.

One characteristic of science at the end of the twentieth century is that traditional subject boundaries are dissolving. The same is true in mathematics. No longer is it sensible to carve the subject up into algebra, calculus, geometry, and so on. Every area impinges on every other. Many areas of mathematical research are now enriched by direct and active contact with applied science. Often the most interesting areas are not those in which mathematics has traditionally been used, and the most interesting applications involve mathematics that has not normally been considered useful.

I've found this in small ways in my own research. First, I genuinely find it hard to answer the question 'what field do you do work in?' I work in several fields, or none, so I either mumble something vague about 'nonlinear dynamics' or give the enquirer a twenty-minute potted history. Second, my own creations keep turning round and biting me. A few years ago a physiologist friend and I did some very 'blue skies' work on how to use group theory to classify patterns of movement in animal locomotion. A few weeks ago I learned that engineers were using our ideas to build and control a walking robot. Four years ago an industrial engineer telephoned me with a query about chaos; today we have a joint patent application for a machine that performs quality control analysis in the springmaking industry.

This is my own personal microcosm of the ever-broadening sweep of science, and it has affected how I view mathematics and its role. Partly for that reason, you will find an idiosyncratic selection of topics here. This is not in any sense a comprehensive study of the whole of today's mathematical sciences; it's just my personal picture of some of the bits that have captured my interest. But I do believe that I can justify my choices in terms of their wider significance. We

are living in an increasingly mathematical world. Mathematics is sometimes considered an art, but I think that mathematical thought patterns lie much closer to science. Mathematics is one of the keystones of the scientific endeavour, and science and its technological legacy affect every one of us in more ways than we know, or can know. So it pays to understand the kinds of things that mathematicians do; and my selection, idiosyncratic though it may be, is certainly a valid sample.

My aim in *From Here to Infinity* is to give you as much insight as I can into the questions raised at the start of this prefacewhat mathematics is, what it's for, what mathematicians do when they create new mathematics, and why that task wasn't finished long ago. At the very least, I guarantee that you won't emerge from the book thinking that mathematical research consists of inventing new numbers.

Though *sometimes*, of course

I.N.S.
COVENTRY 1995

Contents

1
The Nature of Mathematics

Go down I times three, one third of me, one fifth of me is added to me; return I, filled am I. What is the quantity saying it?
*A'h-mosé The Scribe*Rhind Papyrus

One of the biggest problems of mathematics is to explain to everyone else what it is all about. The technical trappings of the subject, its symbolism and formality, its baffling terminology, its apparent delight in lengthy calculations: these tend to obscure its real nature. A musician would be horrified if his art were to be summed up as 'a lot of tadpoles drawn on a row of lines'; but that's all that the untrained eye can see in a page of sheet music. The grandeur, the agony, the flights of lyricism and the discords of despair: to discern them among the tadpoles is no mean task. They are present, but only in coded form. In the same way, the symbolism of mathematics is merely its coded form, not its substance. It too has its grandeur, agony, and flights of lyricism. However, there is a difference. Even a casual listener can enjoy a piece of music. It is only the performers who are required to understand the antics of the tadpoles. Music has an immediate appeal to almost everybody. But the nearest thing I can think of to a mathematical performance is the Renaissance tournament, where leading mathematicians did public battle on each other's problems. The idea might profitably be revived; but its appeal is more that of wrestling than of music.

Music can be appreciated from several points of view: the listener, the performer, the composer. In mathematics there is nothing analogous to the listener; and even if there were, it would be the

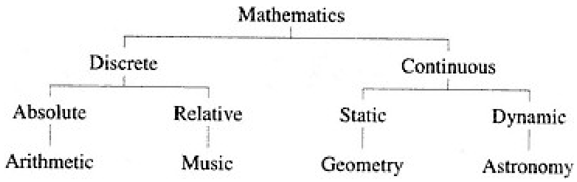
composer, rather than the performer, that would interest him. It is the creation of new mathematics, rather than its mundane practice, that is interesting. Mathematics is not about symbols and calculations. These are just tools of the tradequavers and crotchets and five-finger exercises. Mathematics is about *ideas*. In particular it is about the way that different ideas relate to each other. If certain information is known, what else must necessarily follow? The aim of mathematics is to understand such questions by stripping away the inessentials and penetrating to the core of the problem. It is not just a question of getting the right answer; more a matter of understanding why an answer is possible at all, and why it takes the form that it does. Good mathematics has an air of economy and an element of surprise. But, above all, it has *significance*.

Raw Materials

I suppose the stereotype of the mathematician is an earnest, bespectacled fellow poring over an endless arithmetical screed. A kind of super-accountant. It is surely this image that inspired a distinguished computer scientist to remark in a major lecture that 'what can be done in mathematics by pencil and paper alone has been done by now'. He was dead wrong, and so is the image. Among my colleagues I count a hang-gliding enthusiast, a top-rank mountaineer, a smallholder who can take a tractor to bits before breakfast, a poet, and a writer of detective stories. And none of them is especially good at arithmetic.

As I said, mathematics is not about calculations but about ideas. Someone once stated a theorem about prime numbers, claiming that it could never be proved because there was no good notation for primes. Carl Friedrich Gauss proved it from a standing start in five minutes, saying (somewhat sourly) 'what he needs is *notions*, not *notations*'. Calculations are merely a means to an end. If a theorem is proved by an enormous calculation, that result is not properly understood until the reasons why the calculation works can be isolated and seen to appear as natural and inevitable. Not all ideas are mathematics; but all good mathematics must contain an idea.

Pythagoras and his school classified mathematics into four branches, like this:



Three of these branches remain major sources of mathematical inspiration. The fourth, music, is no longer given the same kind of prominence, but it can be re-interpreted as the algebraic or combinatorial approach. (There is still a pronounced tendency for mathematicians to be musically talented.) To these four, modern mathematics has added a fifth: Lady Luck. Thus there are now at least five distinct sources of mathematical ideas. They are number, shape, arrangement, movement, and chance.

The most basic and well known is number. Originally the number concept must have arisen through counting: possessions, days, enemies. Measurement of lengths and weights led to fractions and the 'real' numbers. A major act of mathematical imagination created 'imaginary' numbers such as $\sqrt{-1}$. From that point on mathematics was never quite the same. Shape, or form, leads to geometry: not just the stereotyped and pedantic style of geometry that used to be blamed on Euclid, but its modern offspring such as topology, singularity theory, Lie groups, and gauge field theories. Novel geometric formsfractals, catastrophes, fibre bundles, strange attractorsstill inspire new developments in mathematics. Problems about ways to arrange objects according to various rules lead to combinatorics, parts of modern algebra and number theory, and what is becoming known as 'finite mathematics', the basis of much computer science. Movementof cannonballs, planets, or wavesinspired the calculus, the theories of ordinary and partial differential equations, the

calculus of variations, and topological dynamics. Many of the biggest areas of mathematical research concern the way systems evolve in time. A more recent ingredient is chance, or randomness. Only for a couple of centuries has it been realized that chance has its own type of pattern and regularity; only in the last fifty years has it been possible to make this statement precise. Probability and statistics are obvious results; less well known but equally important is the theory of stochastic differential equationsdynamics plus random interference.

The Driving Force

The driving force of mathematics is *problems*. A good problem is one whose solution, rather than merely tidying up a dead end, opens up entirely new vistas. Most good problems are hard: in mathematics, as in all walks of life, one seldom gets something for nothing. But not all hard problems are good: intellectual weight-lifting may build mental muscles, but who wants a muscle-bound brain? Another important source of mathematical inspiration is *examples*. A really nice self-contained piece of mathematics, concentrating on one judiciously chosen example, often contains within it the germ of a general theory, in which the example becomes a mere detail, to be embellished at will. I'll combine the two, by giving some examples of mathematical problems. They are drawn from all periods, to emphasize the continuity of mathematical thought. Any technical terms will be explained later.

(1) Is there a fraction whose square is exactly equal to 2?

(2) Can the general equation of the fifth degree be solved using radicals?

(3) What shape is the curve of quickest descent?

(4) Is the standard smooth structure on 4-dimensional space the only one possible?

(5) Is there an efficient computer program to find the prime factors of a given number?

Each problem requires some comment and preliminary explanation, and I'll consider them in turn.

The first was answerednegativelyby the ancient Greeks. The discovery is generally credited to the Pythagorean school in about 550 BC. It was an important question because the Greek geometers knew that the diagonal of the unit square has a length whose square is 2. Thus there exist natural geometric quantities that cannot be expressed as fractions (ratios of whole numbers). There is a legend that a hundred oxen were sacrificed to celebrate the discovery. It may well be false (no documents from the Pythagoreans survive), but the discovery certainly warranted such a celebration. It had a fundamental effect on the next 600 years of Greek mathematics, tilting the balance away from arithmetic and algebra towards geometry. Despite the marvellous discoveries of Greek geometry, this lack of balance seriously distorted the development of mathematics. Its repercussions were still being felt 2,000 years later, when Isaac Newton and Gottfried Leibniz were inventing the calculus.

The second problem came to prominence during the Renaissance. A *radical* is a formula involving only the usual operations of arithmetic (addition, subtraction, multiplication, and division), together with the extraction of roots. The *degree* of an equation is the highest power of the unknown occurring in it. Equations of degrees 1 and 2 (linear and quadratic) were effectively solved by the ancient Babylonians around 2000 BC. Degrees 3 and higher presented an unsolved problem of great notoriety, which withstood all attacks until the sixteenth century, when the cubic (degree 3) equation was solved by Scipione del Ferro of Bologna and Niccolo Fontanta (nick-named Tartaglia, 'the stammerer') of Brescia. Soon after, Lodovico Ferrari solved the quartic (degree 4). In all cases the solution was by radicals. But the quintic equation (degree 5) resisted all efforts. In the nineteenth century two young mathematicians, Niels Henrik Abel and Évariste Galois, independently proved the impossibility of such a solution. The work of Galois led to much of modern algebra and is still a fertile field of research.

The third problem concerns (an idealized mathematical model of) the motion of a particle under gravity down a frictionless wire, from one point to a lower one (displaced some distance sideways). Depending on the shape of this wire, the particle will take differing

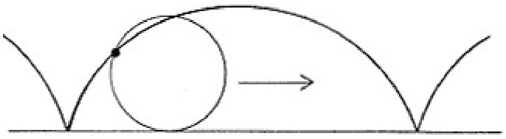


Fig. 1.
A point on the rim of a moving wheel traces out a cycloid.

times to make the journey. What shape of wire leads to the shortest trip? Galileo Galilei got the wrong answer in 1630 (he thought it was an arc of a circle). John Bernoulli posed the problem again, under the name 'brachistochrone', in 1696: solutions were found by Newton, Leibniz, Guillaume L'Hôpital, Bernoulli himself, and his brother James. The solution is an inverted cycloid (the path traced by a point on the rim of a moving wheel). This elegant problem, with its tidy and satisfyingly classical answer, led to the Calculus of Variations, which in short order revolutionized first mechanics, then optics. Today it remains a cornerstone of mathematical physics, with its influence being felt in Quantum Mechanics and General Relativity, as well as pure mathematics.

The fourth problem is of modern vintage, and belongs to an area known as Differential Topology. In this subject, one studies multidimensional analogues of surfaces, or *manifolds*, and these are equipped with a concept of 'smoothness'so that, for example, you can decide whether a curve drawn on a manifold has a sharp corner or not. A major question concerns the *uniqueness* of the smooth structure on a given manifold. In 1963 John Milnor startled the mathematical world by finding more than one smooth structure on the 7-dimensional sphere. Generalizations of his *exotic spheres* have since been found in other dimensions. However, it was still expected that more homely manifolds, especially ordinary *n*-dimensional space, ought to possess only *one* smooth structurethe one everybody uses to do calculus. And this could be proved (not easily) for all dimensions except four. But in 1983 Simon Donaldson showed that there is an *exotic* smooth structure on 4-dimensional space, in addition to the standard one. In fact Donaldson solved a rather different problem,

using some very new ideas born of an interaction between topology, algebra, and mathematical physics. When Donaldson's results are combined with the earlier work of Michael Freedman, Karen Uhlenbeck, and Clifford Taubes, the exotic 4-space emerges.

The fifth problem comes from a novel branch of mathematics known as Complexity Theory. The advent of the electronic computer has focused attention not just on ways to solve mathematical problems, but on the *efficiency* of their solutions. Donald Knuth has remarked that a major difference in style between mathematicians and computer scientists is that the former do not, on the whole, worry about the *cost* (in effort, time, or money) of a solution. In Complexity Theory, it is this cost (measured in terms of the total number of arithmetical operations) that is paramount. A basic distinction is the way the cost rises as the size of the input data grows. Typically it either grows slowly (slower than some fixed power of the input size) or very rapidly ('exponential growth', where the cost is multiplied by some definite amount for each single increase in input size). Recall that a *prime* number is one having no divisors, such as 11 or 29. Every number is a product of primes, but it is far from easy to find these factors. Trying all possible factors in turn is a procedure whose cost grows exponentially as the size of the number increases. The question is: can a more efficient method be foundone whose cost grows like a fixed power? Nobody knows the answer. The solution is important not only in Complexity Theory, but for cryptography: a number of 'theoretically unbreakable' military and diplomatic codes would cease to be unbreakable if the answer were 'yes'.

The Historical Thread

One feature illustrated by my selection of problems is the unusually long lifetime of mathematical ideas. The Babylonian solution of quadratic equations is as fresh and as useful now as it was 4,000 years ago. The calculus of variations first bore fruit in classical mechanics, yet survived the quantum revolution unscathed. The *way* it was used changed, but the mathematical basis did not. Galois's ideas remain at the forefront of mathematical research. Who knows where Donaldson's may lead? Mathematicians are generally more aware of the historical origins of their ideas than many other scientists are. This is not because nothing important has happened in mathematics recentlyquite the reverse is true. It is because mathematical ideas have a permanence that physical theories lack.

Really good mathematical ideas are hard to come by. They result from the combined work of many people over long periods of time. Their discovery involves wrong turnings and intellectual dead ends. They cannot be produced at will: truly novel mathematics is not amenable to an industrial 'Research and Development' approach. But they pay for all that effort by their durability and versatility. Ptolemy's theory of the solar system is of historical interest to a modern cosmologist, but he doesn't *use* it in serious research. In contrast, mathematical ideas thousands of years old are used every day in the most modern mathematics, indeed in all branches of science. The cycloid was a fascinating curiosity to the Greeks, but they couldn't *do* anything with it. As the brachistochrone, it sparked off the calculus of variations. Christiaan Huygens used it to design an accurate clock. Engineers today use it to design gears. It shows up in celestial mechanics and particle accelerators. That's quite a career for one of such a humble upbringing.

Yes, But Look At It This Way

As well as problems and examples, we mustn't ignore a third source of mathematical inspiration: the search for the 'right context' for a theorem or an idea. Mathematicians don't just want to get 'the answer' to a problem. They want a method that makes that answer seem inevitable, something that tells them what's *really* going on. This is something of an infuriating habit, and can easily be misunderstood; but it has proved its worth time and time again.

For example, Descartes showed that by introducing coordinates into the plane, every problem in geometry can be reformulated as an equivalent problem in algebra. Instead of finding where curves meet, you solve simultaneous equations.

It's a magnificent idea, one of the biggest mathematical advances ever made. But let me 'prove' to you that it's totally worthless. The reason is simple: there is a routine procedure whereby *any* algebraic calculation can be transformed into an equivalent geometric theorem, so anything you can do by algebra, you could have done by good old geometry. Descartes's reformulation of geometry adds precisely nothing. See? Like I said, it's useless.

In point of fact, however, the reformulation adds a great dealnamely, a new viewpoint, one in which certain ideas are much more natural than they would have been in the original context. Manipulations that make excellent sense to an algebraist can appear very odd to a geometer when recast as a series of geometric constructions; and the same goes for geometrical ideas reformulated as algebra. It's not just the *existence* of a proof that is important to mathematicians: it's the flow of ideas that lets you think the proof up in the first place. A new viewpoint can have a profound psychological effect, opening up entirely new lines of attack. Yes, *after* the event the new ideas can be reconstructed in terms of the old ones; but if we'd stuck to the old approach, we'd never have thought of them at all, so there'd be nothing to reconstruct *from*.

This point is often misunderstood, even in today's enlightened atmosphere. Critics of 'non-standard analysis'which involves genuine infinitesimals (see Chapter 6) typically object that every theorem that can be proved from the non-standard point of view can also be proved using traditional 'standard' analysis. They're right; and they've missed the point. The point, again, is not the existence of a proof within a given framework: it is whether that proof has a natural flow of ideas, whether it might actually *occur* to somebody.

By exploiting Descartes's connection between geometry and algebra, it is relatively straightforward to prove that angles cannot be trisected using ruler and compasses. This problem remained unsolved from at least the time of the later Greek commentators, say around AD 600, until 1837, when Pierre Wantzel gave an algebraic proof of the impossibility of trisection. In *principle* you could rewrite each step in Wantzel's proof as a geometric theoremso why didn't the Greeks think of it? Or anybody else for two thousand years and

more? Because in *practice*, the only way anyone would ever find the geometric line of proof would be to start from Wantzel's algebra.

As I said, people often misunderstand this restless search for new formulations, new viewpoints, new ways of seeing old results and ideas. Mathematicans are often accused of wanting to reformulate problems from different points of view in order to put off the evil hour when they actually have to try to *solve* them. It's true that there's no point in reformulating a problem if, at the end of the day, you learn nothing new about it. But it's important not to expect major new results straight away. I call this kind of thinking the 'Rolls-Royce Problem' or the 'Dog-in-the-Manger Effect'. Suppose tomorrow you come up with a really good idea for improving the design of carssay, using alcohol as a fuel, or installing an electric motor. Opponents of your approach may then demand that you prove its superiority by manufacturingimmediatelya better car than a Rolls-Royce. Of course you can'teven if your idea really is better. Half a century or more of engineering has gone into the design of a Rolls-Royce; they're expecting you to do better, overnight, with no funding. The world is full of obsolete products for this very reasonthe state of the art in Mexican taxis is a Volkswagen Beetle; manufacturing industry is still hooked on the computer language FORTRAN. But propose a novel reformulation of somebody else's research areaespecially if it uses ideas that they've not been trained inand ten to one they'll challenge you to justify it by building a mathematical Rolls-Royce. We'll see several case histories of this as we go: I'll leave you the pleasure of spotting them.

Despite all this, fashions do change, formulations do get revampedand Rolls-Royces do get built overnight, sometimes. After all, this is mathematics, not manufacturing, and the only limitations are those of human imagination.

The Mainstream

The Mississippi river is 2,348 miles long, and drains an area of 1.24 million square miles. In many places it seems just a maze of twists and turns and dead-end backwaters, yet it manages to flow virtually the entire length of the continent of North America, from Lake Itasca in Minnesota to New Orleans in Louisiana. There are tributaries and meanders and ox-bow lakes cut off entirely from the main river; but the main river is there, and it can be picked out by anyone able to gauge the strength of the current. At its mouth, the Mississippi enters the Gulf of Mexico in an enormous muddy delta, with its own side-shoots and main channels, whose shape and interconnections are constantly changing.

Mathematics is much like the Mississippi. There are side-shoots and dead ends and minor tributaries; but the mainstream is there, and you can find it where the currentthe mathematical poweris strongest. Its delta is research mathematics: it is growing, it is going somewhere (but it may not always be apparent where), and what today looks like a major channel may tomorrow clog up with silt and be abandoned. Meanwhile a minor trickle may suddenly open out into a roaring torrent. The best mathematics always enriches the mainstream, sometimes by diverting it in an entirely new direction.

The Golden Age

Until quite recently, most mathematics taught in schools belonged to the 'classical' period, the most recent part being the calculus (dating from about 1700). Even with the advent of so-called 'modern mathematics', most material taught is at least a century old! Partly in consequence of this, mathematics is thought by many to be a dead subject, and it often comes as a revelation that anything new remains to be discovered.

Can we place in history the Golden Age of Mathematics? Was it the time of Euclid, when the logical foundations of geometrical reasoning were laid? The development of the Hindu-Arabic number system? The Renaissance flowering of algebra? Descartes's coordinate geometry? The calculus of Newton and Leibniz? The vast edifice of eighteenth-century natural philosophy, bending mathematics to the needs of astronomy, hydrodynamics, mechanics, elasticity, heat, light, and sound? If there has indeed been such a thing as a golden age of mathematics, it is none of these. It is the present. During the

past fifty years, more mathematics has been created than in all previous ages put together. There are more than 2,000 mathematical research journals, publishing some 30,000 articles every year (in over a hundred languages). In 1868 The *Jahrbuch über die Fortschritte der Mathematik (Yearbook of Mathematical Research)* listed just twelve categories of mathematical activity; in *Mathematical Reviews* for 1995 there are more than sixty. Of course it is quality, not quantity, that we should use as our yardstick; but by this standard too, the golden age is the modern one.

Some observers have professed to detect, in the variety and freedom of today's mathematics, symptoms of decadence and decline. They tell us that mathematics has fragmented into unrelated specialities, has lost its sense of unity, and has no idea where it is going. They speak of a 'crisis' in mathematics, as if the whole subject has collectively taken a wrong turning. There is no crisis. Today's mathematics is healthy, vigorous, unified, and as relevant to the rest of human culture as it ever was. Mathematicians know very well where they think they and their subject are going. If there appears to be a crisis, it is because the subject has become too large for any single person to grasp. This makes it hard to get an effective overview of what is being done, and what it is good for. To an outside observer, the mathematician's love of abstract theory and hair-splitting logic can appear introverted and incestuous. His apparent lack of attention to the real world suggests the self-centred complacency of the ivory tower. But today's mathematics is not some outlandish aberration: it is a natural continuation of the mathematical mainstream. It is abstract and general, and rigorously logical, not out of perversity, but because this appears to be the only way to get the job done properly. It contains numerous specialities, like most sciences nowadays, because it has flourished and grown. Today's mathematics has succeeded in solving problems that baffled the greatest minds of past centuries. Its most abstract theories are currently finding new applications to fundamental questions in physics, chemistry, biology, computing, and engineering. Is this decadence and decline? I doubt it.

2 The Price of Primality

Suppose that the cleaning lady gives p and q by mistake to the garbage collector, but that the product pq is saved. How to recover p and q? It must be felt as a defeat for mathematics that the most promising approaches are searching the garbage dump and applying memo-hypnotic techniques.
Hendrik W. Lenstra Jr.

Some of the best problems in mathematics are also the simplest. It is difficult to think of anything simpler and more natural than arithmetic. The additive structure of the whole numbers 1, 2, 3, is a bit too simple to hold any great mysteries, but the multiplicative structure poses problems that still inspire creative work after thousands of years. For instance, take the very simple and natural idea of a prime numberone that cannot be obtained by multiplying two smaller numbers. It was known from ancient times, as an empirical fact, that all numbers are products of primes in a unique way; and the Greeks managed to prove it. The problem immediately arises of *finding* these primes for any given number. It isn't hard to think of 'theoretical' methods. For example, the Greeks developed one called the sieve of Eratosthenes, which boils down to working out all possible products or primes and checking whether the number you want occurs anywhere. It's a bit like building a house by systematically trying all possible ways to arrange ten thousand bricks and then choosing the one that seems most habitable, and for practical purposes it's hopelessly inefficient. Even today, there remains a great deal of room for improvement, and basic questions are still unanswered.

The source of the difficulty lies in the schizophrenic character of the primes. Don Zagier, an expert number theorist, describes it like this:

> There are two facts about the prime numbers of which I hope to convince you so overwhelmingly that they will be permanently engraved in your hearts. The first is that they are the most arbitrary objects studied by mathematicians: they grow like weeds among the natural numbers, seeming to obey no other law than that of chance, and nobody can predict where the next one will sprout. The second fact states just the opposite: that the prime numbers exhibit stunning regularity, and there are laws governing their behaviour, and that they obey these laws with almost military precision.

Zagier is referring in particular to the extensive theories developed during the past century on the distribution of prime numbers. For example there is the Prime Number Theorem, guessed on numerical evidence by Carl Freidrich Gauss in 1792 and proved independently by Jacques Hadamard and Charles-Jean de la Vallée Poussin in 1896. This states that for large x the number of primes less than x is approximated ever more closely by $x/\log x$. But in this chapter we confine attention to less lofty matters, concentrating on the computational aspects of the primes. Even with these simple raw materials, mathematics has tailored a stunning garment. Almost as a side issue, we observe that number theory, long considered the most abstract and impractical branch of mathematics, has acquired such a significance for cryptography that there have been serious attempts to have some of its results classified as military secrets.

Divide and Conquer

The sequence of primes begins 2, 3, 5, 7, 11, 13, 17, 19, 23, 29, and extends, in rather irregular fashion, beyond reasonable limits of computation. The largest *known* prime is $2^{1257787}$ 1, found by David Slowinski and Paul Gage in 1994. But we can be certain that even larger primes exist, even if we don't know what they are, because Euclid proved that there are infinitely many primes. The gist of the proof is to assume that there are only finitely many primes, multiply the lot together, and add 1. The result must have a prime

factor, but this can't be any of the original list, since these all leave remainder 1. This is absurd, therefore no finite list existed to begin with. So if we form the enormous number $2 \cdot 3 \cdot 4 \cdot 5$ (2^{858433} 1) + 1 we know that *all* of its prime factors must be larger than Slowinski and Gage's gigantic prime. However, we have no practical way to find any of these factors, and thereby hangs a tale.

Primality Testing

The two central practical problems in prime number theory are to:

(1) Find an efficient way to decide whether a given number is prime.

(2) Find an efficient way to resolve a given number into prime factors.

Clearly (2) is at least as hard as (1), since any solution to (2) will yield (1) as a special case. The converse is not obvious (and probably not true): we shall see below that it is possible to prove that a number is not prime, without actually exhibiting any factor explicitly. 'Efficient' here implies that the length of the calculation should not grow too rapidly with the size of the number. I'll have a lot more to say about *that* idea in Chapter 17.

The most direct way to find a factor of a number, or prove it prime if no factor exists, is *trial division*: divide by each number in turn (up to its square root) and see if anything goes exactly. This is *not* efficient, however. Imagine a computer capable of performing a million trials per second. It will take about a day to deal with a 30-digit number; a million years for a 40-digit number; and ten quadrillion years for a 50-digit numberlonger than the current predictions for the age of the universe! Obviously there may be short cuts; but the fastest method known takes about a month for a 120-digit number, and a century or more for a 130-digit number. Until very recently, both problems were in a very sorry state. But in the last five years, enormous progress has been made on (1), primality testing. A 100-digit number can be proved prime (or not) in about 15 seconds on

a fast computer; a 200-digit number takes 2 minutes. A 1000-digit number might take a day or two. In contrast, to solve (2) for a 200-digit number still looks utterly impracticable.

Finite Arithmetic

To understand how this progress has come about, we must start with an idea first codified by Gauss in his *Disquisitiones Arithmeticae*, a milestone in number theory. Consider a clock, numbered (in unorthodox fashion) with the hours 0, 1, 2,, 11. Such a clock has its own peculiar arithmetic. For example, since three hours after 5 o'clock is 8 o'clock, we could say that 3 + 5 = 8, as usual. But 3 hours after 10 o'clock is 1 o'clock, and 3 hours after 11 o'clock is 2 o'clock; so by the same token, 3 + 10 = 1 and 3 + 11 = 2. Not so standard! Nevertheless, this 'clock arithmetic' has a great deal going for it, including almost all of the usual laws of algebra. Following Gauss, we describe it as arithmetic to the *modulus* 12, and replace '=' by the symbol '≡' as a reminder that some monkey-business is going on. The relation '≡'is called *congruence*. In arithmetic *modulo* (that is, to the modulus) 12, all multiples of 12 are ignored. So 10 + 3 = 13 ≡ 1 since 13 = 12 + 1 and we may ignore the 12.

Any other number *n* may be used as the modulus: now multiples of *n* are neglected. The resulting arithmetical system consists only of the numbers 0, 1, 2, ,*n* 1, and has its own multiplication and subtraction, as well as addition. Division is less pleasant; but if *n* is a *prime* then it is possible to divide by any non-zero number. For example, modulo 7 we find that 3/5 ≡ 2, since 2.5 = 10 ≡ 3. This bizarre arithmetic was introduced by Gauss because it is ideal for handling questions of *divisibility*. Number theory has used it for this purpose ever since.

Fermat's Little Theorem

Two important theorems in mathematics bear the name of Pierre de Fermat, a French lawyer and amateur number theorist *par excellence*. Until very recently one of them, Fermat's Last Theorem, wasn't

a real theorem at all, but a major open problem (see chapter 3). The other, often called the 'little theorem', is so simple that a proof is only a few lines long; yet it is a masterpiece of ingenuity and a result with astonishing implications.

It is this. Suppose *p* is a prime and *a* is any number not divisible by *p*. Then $a^{p1} \equiv 1$ (mod *p*). The 'mod *p*' just reminds us that *p* is the modulus. In other words, a^{p1} leaves remainder 1 on division by *p*. For example, we expect a remainder 1 on dividing $2^{10} = 1024$ by 11, since 11 is prime. Now $1024 = 93 \cdot 11 + 1$, so this is correct. On the other hand, $2^{11} = 2048 = 170 \cdot 12 + 8$, so leaves remainder 8 on division by 12. We have thus succeeded in proving that 12 is not prime, *without* exhibiting any specific factors. This fact is hardly novel, but similar calculations can be made for very large values of the number *p*, where the outcome is less obvious. Calculating a^{p1} *(mod p*) can be done efficiently, in a time that grows like the *cube* of the number of digits in *p*. So here we have an efficient test for non-primalityprovided we know which value of *a* to choose!

Trapdoor Codes

Why should anyone be interested in large primes, and factorization? Aside from the sheer joy of it all, there is a practical reason, which I hinted at earlier: applications to cryptography. A great deal of military effort has gone into the search for an 'unbreakable' but effective code. In the late 1970s Ralph Merkle, Whitfield Diffie, and Martin Hellman proposed a new kind of code, called a public-key cryptosystem. In using any code, there are two basic steps: *encoding* a message, and *decoding* it again. For most codes, anyone who knows how to perform the first step also knows how to perform the second, and it would be unthinkable to release to the enemy (or the public, which many governments seem to view in the same light) the method whereby a message can be turned into code. Merely by 'undoing' the encoding procedures, the enemy would be able to break all subsequent coded messages.

Merkle, Diffie, and Hellman realized that this argument is not totally watertight. The weasel word is 'merely'. Suppose that the

encoding procedure is very hard to undo (like boiling an egg). Then it does no harm to release its details. This led them to the idea of a *trapdoor function*. Any code takes a message M and produces a coded form (M). Decoding the message is tantamount to finding an inverse function $^{-1}$, such that $^{-1}$ ((M)) = M. We call a trapdoor function if is very easy to compute, but $^{-1}$ is very hard, indeed impossible for practical purposes. A trapdoor function in this sense isn't a very practical code, because the legitimate user finds it just as hard to decode the message as the enemy does. The final twist is to define in such a way that a single extra piece of (secret) information makes the computation of $^{-1}$ easy. This is the only bit of information you can't tell the enemy.

In the RSA system, designed by Ted Rivest, Adi Shamir, and Leonard Adleman, this is achieved with the help of Fermat's little theorem. Begin with two large primes p and q, which are kept secret; but release to the public their product $n = pq$ and a further number E, the *encoding key*. It is easy to represent any message by a sequence of digits; break this into blocks which, thought of as numbers, are less than n. Then it is enough to encode block by block. To do this, change each block B into the number $C \equiv B^E$ (mod n). Then C is the coded message. To decode, you need to know a *decoding key* D, chosen so that DE is congruent to 1 modulo (p 1) (q 1). Then a slight generalization of Fermat's little theorem, due to Leonhard Euler, shows that $C^D \equiv B$ (mod n), so you can recover B from C by a similar process.

The point is that while Joe Public knows n and E, he doesn't know p and q, so he can't work out (p 1)(q 1) and thereby find D. The designer of the code, on the other hand, knows p and q because those are what he started from. So does any legitimate receiver: the designer will have told him. The security of this system depends on exactly one thing: the notorious difficulty of factorizing a given number n into primes, alluded to in Lenstra's remark about the cleaning-lady at the head of this chapter. You don't believe him? OK, here's what the cleaning-lady left: $n = 2^{67}$ 1. If you can work out p and q *by hand* in less than three years of Sundays, you'll beat F. N. Cole, who first did it in 1903. (See the end of this chapter.)

Pseudoprimes

With this application in mind we return to the two theoretical problems: primality testing and factorization. The Fermat test for non-primality, mentioned earlier, is not foolproof. For each value of a there exist infinitely many non-primes p for which $a^{p1} \equiv 1$ (mod p). These p are called *pseudoprimes* to base a. Worse, there are non-primes p that are pseudoprimes to *all* bases a (not having a factor in common with p). Such p are called *Carmichael numbers*: the smallest is $561 = 3 \cdot 11 \cdot 17$. It works because 3 1, 11 1, and 17 1 all divide 561 1, a pattern typical of Carmichael numbers. Fermat's Little Theorem can be used to explain this. In 1992 'Red' Alford, Andrew Granville, and Carl Pomerance proved a longstanding conjecture: there exist infinitely many Carmichael numbers. More precisely, for large enough numbers x, there are at least $x^{2/7}$ Carmichael numbers less than x. So, for example, there are at least 100 Carmichael numbers less than ten million, and at least a million Carmichael numbers less than one sextillion (that is, 10^{21}). This implies that the problems created by Carmichael numbers cannot be avoided just by listing a finite number of exceptional cases.

There is a related, but more complicated idea, due to G. L. Miller, which I won't describe in detail. It has the same feature that any number that fails the Miller test is known not to be prime, but passing the test is not of itself a guarantee of primality. A number that passes the Miller test is called a *strong pseudoprime* to base a. If not, then a is called a *witness* for (the non-primality of) p. It turns out that there is no analogue of Carmichael numbers for strong pseudoprimality. In fact most non-primes have a very small witness. For example, John Selfridge and S. S. Wagstaff found by direct computation that the only non-prime number less than 25 billion that does not have one of 2, 3, 5, and 7 as witness is 3215031751. But then 11 *is* a witness. By exploiting this we can obtain an efficient primality test for 9-digit numbers which will run on a small programmable calculator within two minutes.

Miller took this idea further. If we can show that every non-prime number has a sufficiently small witness, then we have an efficient

test for primality which works for *all* numbers. He was able to establish that any odd non-prime n has a witness less than $70(\log n)^2$; but only by assuming one of the most notorious unproved conjectures in the whole of mathematics, the Riemann Hypothesis (see chapter 11). This puts us in a very curious position. We have a primality test, which we can be pretty certain is efficient; but we can't prove that it is. If in fact the Riemann Hypothesis is *true*as most experts think likelythen the test really *is* efficient. As a practical matter, it presumably works efficiently on almost all numbers. An engineer, for instance, might well consider the problem to be solved. (I mean no offence herein engineering the criterion 'it works' is arguably the only sensible test anyway.) Mathematicians find the situation totally unsatisfactoryI suppose because, although they think they have 'the answer', there is a gaping hole in their understanding of it. This isn't a point a mathematician is likely to slide over, either, because experience has shown that whenever a gap isn't understood, all sorts of beautiful and important ideas are lurking within it.

The AdlemanRumely Test

In 1980 Adleman and Robert Rumely *found* some of those lurking ideas, allowing them to modify the Miller test so that it would give a *guarantee* of primality or non-primality, with no probabilistic or conjectural fudging. The running-time has to be a little longer, but the method (especially as improved by Hendrik Lenstra soon afterwards) is entirely practicable for, say, 200-digit primes. Their idea is to extract extra information from a strong pseudoprime test, other than just 'pass' or 'fail', by using some ideas from algebraic number theory related to the so-called 'higher reciprocity laws'. Some very sophisticated mathematics, both classical and modern, is getting into the act, despite the relatively mundane problem that it is being applied to. For a k-digit number, their method has running time approximately $k^{\log \log k}$. This is 'nearly polynomial time' and makes the method a practical one. The exact bound on the running-time was conjectured by Adleman and Rumely, and proved soon after by Andrew Odlyzko and Pomerance.

Schrödinger's Cat

A very recent development in cryptography has nothing whatever to do with primes, but it's so nice that I can't bring myself to miss it out. Public key cryptosystems permit the methods for encrypting messages to be made public, with decryption being virtually impossible in the absence of a secret key. An emerging branch of the subjectquantum cryptographygoes much further. Charles C. Bennett, François Bessette, Gilles Brassard, Louis Salvail, John Smolin, and Artur Ekertthe first four in joint work, the last somewhat independentlyhave recently explained how ideas based upon the EinsteinPodolskyRosen 'paradox' in quantum theory can be used to guarantee secure distribution of a cryptographic key.

In fact, quantum cryptography began with unpublished work of Stephen Wiesner in the late sixties, who explained how quantum effects can in principle be used to manufacture banknotes immune to counterfeiting, and to transmit information securely. His work was eventually published in 1983 at Bennett's instigation. The unfakeable banknote requires the ability to store a single polarized photon for several days without change of state. As Bennett and his co-authors say: 'Unfortunately the impact of the CRYPTO 82 conference had left most people under the impression that everything having to do with quantum cryptography was doomed from the start to being unrealistic.' However, in 1982 Bennett and Brassard realized that photons are not for *storing* information, but for *transmitting* it. Quantum versions of many mainstays of cryptography then appeared, including the self-winding reusable one-time pad, quantum coin-tossing, quantum oblivious transfer, zero-knowledge protocols, and secure two-party computations. Here I'll consider only the quantum key distribution channel, also originally invented by Bennett and Brassard.

Security of a cryptotext used to depend upon keeping the entire process of encryption and decryption secret. As we have seen, the encryption and decryption procedures can be public knowledge, provided only that a specific piece of information, the *key*, is kept secret from illegitimate users or eavesdroppers. The key can be any random string of digits. The danger with this approach is that all

eggs are invested in one basket, the key. Before secure communication can begin, the key must be distributed *securely* to users. During World War II one rather amateur group of spies distributed new code keys by radio to possibly compromised agents, using the old (equally compromised) code. This is hardly an acceptable solution to the problem; but more subtle distribution methods might also be open to eavesdropping.

Consider a specific example, an eavesdropper trying to monitor a communication channel, say by tapping a telephone line. Observations of any system disturb it and so leave traces. Legitimate users can try to guard against this by making their own measurements on the line, to detect the effects of tapping. However, the tappers will be safe provided they can make measurements that produce smaller disturbances than the legitimate users can detect. In classical (that is, non-quantum) mechanics, this kind of 'arms race' of increasingly sensitive measurements can go on indefinitely. ('Big bugs have little bugs', et cetera.) However, in quantum mechanics it cannot, because of the Heisenberg uncertainty principle. Measurements of some quantities, however delicately conducted, may change other related quantities by significant and irreducible amounts. In the conventional interpretation, a quantum system exists in a superposition of possible states, *until* a measurement is made, 'forcing' it into a pure state. This view is dramatized by the unenviable fate of Schrödinger's cat, which lives inside an impenetrable box, which is equipped with a device that ensures that the cat is alive if a particular particle has positive spin, dead if it has negative spin. Until the box is opened, the unfortunate beast is neither alive nor dead, butadherents of the 'Copenhagen interpretation' of quantum mechanics maintainin a quantum superposition of the two states.

If I may digress for a moment: this interpretation may well be wrong. Recent experiments suggest that the cat will be in a good old classical state, either alive or deadbut you won't know which until you open the box. This is *not* the same as a quantum superposition, since, as T. S. Eliot put it, 'THE CAT HIMSELF KNOWS, and will never confess'. The device that converts particle spin into condition-of-cat itself makes a measurement within the meaning of the act. It is the

amplification of quantum information into a large-scale effect that constitutes 'measurement', not necessarily the action of a human mind. How this really happens remains mysterious. This possibility notwithstanding, I'll stick to the Copenhagen interpretation, because it lets me use the next section heading:

Schrödinger's Cat-flap

At this point the EPR paradox, proposed in 1935 by Albert Einstein, Boris Podolsky, and Nathan Rosen, enters the story. Their ideas were simplified by David Bohm, and it is his version that I'm going to describe. The proton is a particle with spin ½. Its (spin) angular momentum, measured in any direction, is therefore ± ½ħ, where ħ = *h*/2π and *h* is Planck's constant. Begin with a system of two protons, very close together, of total angular momentum zero, a situation that can be realized in practice. If the angular momentum of one proton in a given direction is ½ħ, then that of the other must be ½ħ; if that of the first is ½ħ then that of the second must be ½ħ. It is not necessary to measure these quantities for this relation to holdindeed it is important not to, since measurements in quantum mechanics disturb the system being measured. Assume that the protons move apart until the distance between them is large: the relations between the angular momenta will remain valid. If we now measure a component of angular momentum for one proton, then the result forces a definite state of angular momentum in that direction for its distant companion. This kind of 'action at a distance' seemed to Einstein, Podolsky, and Rosen to be paradoxical, and they concluded that quantum mechanics must be an incomplete description of reality. However, John Bell subsequently derived a theoretical relationship, Bell's inequality, that permits experiments to test the EPR paradox. The results confirmed the predictions of quantum mechanics.

The quantum key distribution channel repeats the EPR scenario once per bit (binary digit's worth) of transmitted message: it is a source that emits pairs of spin ½ particles in opposite directions. That is, a box with a cat-flap at each end, which emits pairs of perfectly anticorrelated Schrödinger's cats in a constant stream. If

one cat in a given pair is alive, then the other is dead, but until their state is measured, we do not know which is which. At opposite ends of this axis are two time-honoured characters in the lore of cryptography who represent the legitimate users of the channel, known as Alice and Bob. Their task is to extract from the sequence of EPR-linked particles a common key, and also to monitor the channel against eavesdropping.

To this end, they each measure components of angular momentum, in orientations that they select randomly and independently for each incoming particle, thereby determining the conditions of their respective cats. Alice has the choice of measuring at angles of 0°, 45°, or 90° in a plane perpendicular to the communication axis; Bob at angles of 45°, 90°, or 135°. Each such measurement yields a result of +1 or 1 (in units of $\frac{1}{2}\hbar$), and potentially reveals one bit of information. If Alice and Bob measure angular momentum in identical orientations, then their results are totally anticorrelated: the state of Bob's cat is always the exact opposite to that of Alice's. However, if they use different orientations, it can be shown that a particular combination S of correlations must be $-2\sqrt{2}$.

After transmission, Alice and Bob publicly announce the sequence of orientations they have chosen, and thus can privately divide their measurements into two groups: those for which they used the same orientation, and those for which they used different ones. They also announce the actual measurements they obtained in the second group, so that they both can compute the quantity S. If the channel has not been disturbed by an eavesdropper, the value should be $S = -2\sqrt{2}$. If that is the case, then the first group of perfectly anticorrelated measurements (whose values are known only to Alice and Bob) can be used to establish a common key.

Experimental methods designed to test Bell's inequalities could be used to implement this scheme (over short distances) in a relatively practical manner. For quantum cryptography, Schrödinger's cat is out of the bag.

Cole's Answer

2^{67} 1 = 193707721 × 761838257287.

3
Marginal Interest

To resolve a cube into the sum of two cubes, a fourth power into two fourth powers, or in general any power higher than the second into two of the same kind, is impossible; of which fact I have found a remarkable proof. The margin is too small to contain it.
Pierre de Fermat

The Isaac Newton Institute in Cambridge is a newly founded international research centre for mathematics. In June 1993 it was running a conference on number theory. The organizers could have had no conception of how their small meeting was going to redraw the map of mathematics, but soon after the meeting started, rumours began to circulate. Andrew Wiles, a quiet, rather diffident Englishman working at Princeton University, had announced a series of three lectures on the topic 'Modular forms, elliptic curves, and Galois representations'. Only an insider could read between the lines. Wiles seldom gave lectures; three in a row was unprecedented. And the target of his rather technical researches was something far less esoteric and far more dramatic: nothing less than Fermat's Last Theorem.

Wiles's series of talks was scheduled for Monday 21Wednesday 23 June, one lecture per day. Early on he explained that he had solved a very special case of the so-called Taniyama conjecture, a highbrow but very important assertion about 'elliptic curves'. That grabbed the audience's attention, all right, because they all knew that one of the consequences of a more general case of the Taniyama conjecturefor so-called semistable elliptic curveswould be Fermat's Last Theorem. How far had Wiles got? Will he or won't

he? They knew that if he was in any way unsure of his results, he would wait until he *was* sure. Maybe next year?

The tension mounted as Wiles approached the third and final lecture, having revealed absolutely nothing about just how far his work had progressed. According to Karl Rubin of Ohio State University 'The excitement was increasing each day.' Then, on the Wednesday, Wiles revealed what was up his sleeve. He had found a clever way to take his very special case of the Taniyama conjecture and extend it to a proof of the semistable case. Although the audience was well aware of the implication, Wiles quietly spelled it out. There was a sudden silence. Then the entire room burst into spontaneous applause.

Alexandrian Algebra

What is Fermat's Last Theorem, and why is it such a great prize? For that matter, who was Fermat? The story of Fermat's Last Theorem begins in 332 BC, when Alexander the Great founded a city in Egypt and modestly named it Alexandria. He died in 323 BC, before his city could be completed, and in the ensuing political instability Alexander's empire split into three, one part being Egypt under the Ptolemaic dynasty. The main mathematical activity after the classical Greek period was within the Ptolemaic empire, especially at Alexandria. Science and mathematics flourished there until the destruction of the Alexandrian library, begun by the Romans under Theodosius in AD 392 and completed by the Moslems in 640'if it's in the Koran, it's superfluous; if not, then it's heresy'. It is estimated that under Theodosius over 300,000 manuscripts were destroyed.

A century or so before its final decline, Alexandria produced the high point of Greek algebra, the work of Diophantus. Very little is known about Diophantusfor example, it is only an assumption that he was Greek. He wrote a number of books, of which the *Arithmetica* was the most important. One might describe him as the Euclid of algebra. He introduced a symbolic notation with different symbols for the square, cube, and so forth of the unknown. And he wrote about the solution of equations.

Whether an equation has solutions depends on two things. One is obvious: the equation itself. The other, less obvious but usually clear from context, is what counts as a solution. Over the years mathematicians have distinguished many different contexts for the solution of equationsmany different 'number systems'. The simplest are the *natural numbers* 0, 1, 2, 3, and the *integers* 3, 2, 1, 0, 1, 2, 3, . Next in order of sophistication are the *rational* numbers, which comprise all fractions p/q where p and q are integers and q is nonzero. The numbers 2/3 and 22/7 are rational. On the other hand numbers like $\sqrt{2}$ and π are *irrational*they cannot be represented exactly as fractions. They can, however, be represented as *real* numbershaving decimal expansions such as 3.14159 that in principle go on forever. Beyond the real numbers lie other, more exotic number systems; in particular the *complex* numbers (see Chapter 11) in which 1 has a square root.

Whether an equation has solutions, and how many there are, depend on which number system the solution is required to belong to. The equation $3x = 2$ has no solutions in integers, but it has the solution $x = 2/3$ in rationals. The equation $x^2 = 4$ has only the solution $x = 2$ in natural numbers, but it has another one, $x = 2$, in integers. The equation $x^2 = 2$ has no rational solutions but two real solutions $\pm\sqrt{2}$. The equation $x^2 = 1$ has no real solutions but two complex ones. When Diophantus sought a solution to an equation, what he had in mind was a positive rational number, and usually a natural number. This was not so much an explicit requirement as a tacit assumption: those were the only kinds of number that could be handled by *arithmetic*. Today we have fewer hang-ups about negative numbers, and we commemorate Diophantus' discoveries by using the term *Diophantine equation* for one whose solutions are required to be integers. (Equations whose solutions are required to be rational come under the same heading, because they can be turned into equations whose solutions are required to be integers by a simple algebraic trick: replace every rational unknown x by y/z where y and z are integer unknowns.)

Among the problems treated by Diophantus is that of Pythagorean triples: whole numbers that form the sides of a right triangle. It was

known from very ancient times that a triangle whose sides are 3, 4, and 5 units will have a right angle. Using Pythagoras' Theorem, the general problem boils down to finding integers a, b, c such that $a^2 + b^2 = c^2$. The Old Babylonian tablet Plimpton 322, dating from about 19001600 BC, lists fifteen such triples. Diophantus tackles the general problemthough using special cases to exemplify the method. For example, Problem 8 in Book 1 asks for a division of 16 into two squares, and obtains the answer 256/25 and 144/25. In modern notation, the general solution is given by

$a = k(u^2\ v^2)$, $b = 2kuv$, $c = k(u^2 + v^2)$

for arbitrary integers k, u, v. Thus there are infinitely many solutions, and they may be given in terms of polynomial functions of three parameters. Diophantus studied many other kinds of equation, for example making a cubic expression equal to a square. His aim was to find *some* solution rather than all solutions. As Morris Kline says: 'He has no *general methods*. His variety of methods for the separate problems dazzles rather than delights. He was a shrewd and clever virtuoso but apparently not deep enough to see the essence of his methods and thereby attain generality.' To some extent the same problem has plagued the theory of Diophantine equations for most of its history; but in the last two decades there has been enormous progress, and a great deal of unity and order is at last emerging.

The Professional Amateur

Pierre de Fermat was born in 1601. His father Dominique Fermat sold leather, his mother Claire de Long was the daughter of a family of parliamentary lawyers. In 1631 he married his mother's cousin Louise de Long. In 1648 he became a King's Councillor in the local parliament of Toulouse, where he served for the rest of his life, dying in 1665, just two days after concluding a legal case. He never held an academic position of any kind, and certainly not in mathematics, but mathematics was his passion. Eric Temple Bell called Fermat 'the Prince of amateurs', but the mathematical historian Julian Coolidge refused to include Fermat in his book *The Mathematics of*

Great Amateurs on the grounds that Fermat was so good that he should be considered a professional. Indeed, most of today's professionals would be happy with half of Fermat's ability.

Fermat had a powerful mathematical brain, and he didn't confine it to number theory. He worked out many of the basic ideas of calculus, half a century before Isaac Newton and Gottfried Leibniz independently sorted out the whole subject. He formulated the Principle of Least Time, which says that a ray of light takes the shortest path available. This principle was a forerunner of the variational calculus, one of the most powerful tools of mathematical physics. But Fermat's most influential ideas were in number theory, the study of integersordinary whole numbers, either positive or negative. It is an area in which it is easy to observe patterns, and guess that they are generally valid, but amazingly difficult to pin them down with a proper proof. A typical theorem of Fermat's is that any prime number that is one greater than a multiple of four is always a sum of two integer squares. Anybody can check special casesfor example, $13 = 2^2 + 3^2$, $137 = 11^2 + 4^2$. But Fermat could not only dig such patterns out of the numerical mire: he could polish them up with a shiny proof.

Ironically, few of Fermat's proofs survive, but we know he didn't just guess his results, because his letters to other mathematicians occasionally include details of proofs. Fermat proved this particular result by a method of his own invention, called *infinite descent*. The basic idea is to show that if the theorem does not hold for some prime that is one greater than a multiple of four, then it also fails for some smaller prime of that form. Descending indefinitely, we see that it must fail for the smallest such prime, namely 5. But $5 = 2^2 + 1^2$, a contradiction.

Fermat's method of infinite descent has a slightly disturbing logic that makes some people feel uncomfortable. It is worth remarking that apart from differences in presentation it is a standard weapon in every mathematician's armoury. Nowadays we would rephrase the entire proof in terms of a 'minimal criminal', as follows. Suppose the theorem is false for *some* prime that is one greater than a multiple of four. Then we may choose the *smallest* such prime for which

the theorem fails. The central step in Fermat's method of descent is that there must be an even smaller prime for which the theorem failsbut this contradicts the choice of a *minimal* criminal. Digging a little deeper, the logical basis for the existence of a minimal criminal is the principle that every non-empty set of positive integers contains a smallest member. This in turn is just a variant of the principle of mathematical induction, which says the following: if some property of integers holds for any number n provided that it holds for the previous number $n\ 1$, and if it also holds for the number 0, then it holds for all positive integers. So it's all rather prosaic now; but in Fermat's time it was original.

Fermat owned a copy of Diophantus' *Arithmetica*, which inspired many of his investigations. He used to write down his conclusions in the margin, and set them as challenges to his mathematical friends and correspondents. Some time around 1637 he must have been thinking about the Pythagorean equation, and he asked himself what happens if instead of squares you try cubes. He presumably tried some numerical experiments. Is $1^3 + 2^3$ a cube? No, it equals 9a square, but not a cube. He was unable to find any solutions, except for 'trivial' ones like $0^3 + 1^3 = 1^3$. The same happened when he tried fourth powers, fifth powers, and so on. In the margin of his copy of the *Arithmetica* he made the most famous note in the history of mathematics, reproduced at the head of this chapter: 'To resolve a cube into the sum of two cubes, a fourth power into two fourth powers, or in general any power higher than the second into two of the same kind, is impossible; of which fact I have found a remarkable proof. The margin is too small to contain it.'

Fermat was stating that what is now called the *Fermat Equation*

$a^n + b^n = c^n$

has no non-trivial solutions in whole numbers a, b, c, whenever n is a positive integer greater than or equal to 3.

It is easy to see that it suffices to prove this statement for two cases: $n = 4$ and n any odd prime. The basic idea is that, for example, because a 35th power is also a 5th power, then any solution in 35th powers is also one in 5th powers. The key point in this argument

is that 35 is divisible by 5. Now, every number bigger than 2 is divisible either by an odd prime or by 4; so those are the basic cases to worry about.

Fermat devised a proof for the case $n = 4$, and the gist of his proof has survived in sufficient detail for us to know what he had in mind. That left the odd primes. Euler obtained a proof when $n = 3$. The case $n = 5$ was proved by Peter Lejeune Dirichlet in 1828 and Adrien-Marie Legendre in 1830. In 1832 Dirichlet proved it for $n = 14$: it is curious that this is easier to deal with than the correponding prime $n = 7$, and others tried to repair the omission. In 1839 Gabriel Lamé offered a proof for $n = 7$ but there were errors. Gauss had a go at this case, failed, and wrote to Heinrich Olbers that the problem 'has little interest for me, since a multitude of such propositions, which one can neither prove nor refute, can easily be formulated'. Even the greatest mathematician suffers from the occasional attack of sour grapes.

Algebraic Numbers

The key to further progress is the idea of an *algebraic number*one satisfying a polynomial equation with rational coefficients. A *polynomial* is an algebraic expression made up from various different powers of the unknown x by adding numerical multiples of them together, for example $5x^7$ $163x^4 + 73x$ 9. The numbers 5, 163, 73, 9 are the *coefficients*. To get a polynomial *equation* you equate such an expression to zero (here getting $5x^7$ $163x^4 + 73x$ $9 = 0$) and try to find a solution x. The number $\sqrt{2}$ is algebraic since it satisfies the equation $(\sqrt{2})^2 - 2 = 0.$)^2 2 = 0. Some numbers, such as π, are not algebraic: these are called *transcendental*. It turns out that algebraic numbers have their own kind of arithmetic, with satisfactory generalizations of integers, primes, and so on. For instance, in the system of *Gaussian integers* $a + b\sqrt{-1}$, where a and b are ordinary integers, every number is a unique product of primes. More generally, *algebraic integers* are numbers that satisfy a polynomial equation with integer coefficients whose first coefficient is 1. Sums and products of algebraic integers are again algebraic integers. The main

motivation for introducing algebraic numbers into number theory is that they provide information on Diophantine equations. For example, Fermat stated that the only integer solutions of the equation $y^2 + 2 = x^3$ are $y = \pm 5$, $x = 3$. Here's a sketch proof using algebraic integers of the form $a + b\sqrt{-2}$. We bring these into the problem because we can factorize the left-hand side, to get

$(y + \sqrt{-2})(y \sqrt{-2}) = x^3$.

By a bit of fiddling we find that we can assume the two numbers on the left have no common factor. Now, in ordinary integers, if the product of two numbers without common factors is a cube, then each separately is a cube. So, assuming this result is still true for algebraic integers, we have in particular that

$y + \sqrt{-2} = (a + b\sqrt{-2})^3$.

Expanding and comparing coefficients of $\sqrt{-2}$ we see that

$1 = b(3a^2\ 2b^2)$

whose only solutions are easily found by trial and error. We must have $b = \pm 1$, and $3a^2\ 2b^2 = \pm 1$. So $a = \pm 1$ and $b = 1$. Then $x = 3$ and $y = \pm 5$ as claimed. As it stands this isn't a rigorous proof, because we can't be sure that the result about cubes is still true for integers of the form $a + b\sqrt{-2}$. In fact this can be proved, as a consequence of unique prime factorization of such integers. The proof isn't totally obvious, but it's not especially deep.

This is an excellent example of the way in which the introduction of a new gadgethere the numbers $a + b\sqrt{-2}$ lets us deduce results about more ordinary objects of mathematics, here the integers. The new gadget brings with it some extra structure that can be exploited, after which it is eliminated and we fight our way back to the original problem.

Cyclotomic Arithmetic

Several mathematicians noticed that it is possible to obtain a similar factorization of the expression $a^n + b^n$ by making use of the complex

nth root of unity, usually denoted ζ. As in the previous section, if $a^n + b^n$ is an exact nth power, then this should imply that each factor is also a perfect nth power. In 1874 Lamé announced a proof of Fermat's Last Theorem based on this argument. Joseph Liouville, who had clearly been thinking about the matter for some time, promptly wrote to Lamé to point out that the proof only works if you can show that unique prime factorization holds for *cyclotomic integers*: polynomials in ζ with integer coefficients. Liouville's fears were confirmed when Ernst Kummer wrote to him to say that unique prime factorization fails for cyclotomic integers when $n = 23$. It looked as if this particular approach to Fermat's Last Theorem had hit an insuperable obstacle.

The Ideal Solution

One of the cardinal rules of mathematical research is not to give up on a good idea just because it doesn't work. Kummer, a theologian turned mathematician and a student of Gauss and Dirichlet, found a way to restore uniqueness of prime factorization, not just to the cyclotomic integers, but to *any* system of algebraic numbers.

He got this idea when working on a completely different problem in number theory, known as the higher reciprocity laws. I mention only that these have to do with conditions under which the equation $a \equiv b^n \pmod{p}$ holds: the subject is extensive and important but would take us too far afield. He developed a theory of *ideal numbers*. In his original formulation, these are not numbers at all: they are the ghosts of non-existent numbers. An ideal number n is a relation between actual numbers that behaves like congruence modulo n, except that there is no n to be congruent modulo. Later it was found possible to exorcize the ghost in two different ways. An ideal number can be interpreted as a genuine number in a larger system of algebraic numbers, or as a *set* of numbers in the original system. The second interpretation found more favour, and led to a modern concept called an *ideal*. Kummer proved that although it is not always possible to factorize algebraic numbers into primes, it *is* possible to factorize them uniquely into prime ideal numbers. Armed with this new weapon

(which must be wielded with the skill of a master duellist, parrying from ordinary to ideal numbers or riposting back again when the right opening appears), Kummer proved Fermat's Last Theorem for exponents n that are 'regular' primes. There is a somewhat technical definition of what this means: suffice it to say that among primes less than 100 it covers all save 37, 59, and 67. Using additional arguments, Kummer and Dimitri Mirimanoff dealt with these cases too. It was conjectured that the result could be proved this way for infinitely many primes n: ironically, what is actually known is that the method *fails* for infinitely many primes! However, this is now something of a dead issue.

By extending these techniques, various mathematicians drove the frontiers further out; and by 1980 Wagstaff had shown that Fermat's Last Theorem is true for all n up to 125,000. It was clear that you could keep pushing the numbers up almost indefinitely. Unless Fermat's Last Theorem was *false*, of coursein which case no amount of work of this kind could rule out the possibility that next time you tried to push the number still higher, you would find a solution to the Fermat equation instead. A new method was needed, and the way to find it was to step back from the special question of Fermat's equation, and ask some basic questions about *all* Diophantine equations.

Euler's Conjecture

We'll start with equations that are close relatives of Fermat's. If it's not possible for two cubes to sum to a cube, might it be possible for *three*? It is; in fact $3^3 + 4^3 + 5^3 = 6^3$. Leonhard Euler conjectured that for all n it is possible for n nth powers to add to an nth power, but *impossible* for n 1 to do so. However, Euler's conjecture is false. In 1966 L. J. Lander and T. R. Parkin found four fifth powers whose sum is again a fifth power:

$$27^5 + 84^5 + 110^5 + 133^5 = 144^5.$$

In 1988 Noam Elkies of Harvard University found the first counter-example to Euler's conjecture for *fourth* powers. Instead of looking for

integer (whole number) solutions to Euler's equation $x^4 + y^4 + z^4 = w^4$ he divided out by w^4, which leads to a rational solution $r = x/w$, $s = y/w$, $z = t/w$ of the new equation $r^4 + s^4 + t^4 = 1$. Conversely, any rational solution of the new equation yields an integer solution of the original equation, by putting r, s, and t over a common denominator w. The new equation defines a surface in three-dimensional space, like a flattened ellipsoid. V. A. Demjanenko found a rather complicated condition for a rational point (r, s, t) to lie on the related surface $r^4 + s^4 + t^2 = 1$, which involves t^2 rather than t^4. A solution to this will yield a solution to Euler's equation, provided t can be made a square. A series of simplifications shows that this can be done provided the equation

$$Y^2 = 31790X^4 + 36941X^3\ 56158X^2 + 28849X + 22030$$

has a rational solution. This equation belongs to a general class known as *elliptic curves*, widely studied by number theorists because they have very beautiful properties. The general theory of elliptic curves includes powerful tests for non-existence of solutions. They fail here, which hintsbut does not of itself provethat a solution may in fact exist. Encouraged by this indirect evidence, Elkies tried a computer search, and found one. Working backwards, he was led to a counterexample to Euler's conjecture for fourth powers, namely

$$2682440^4 + 15365639^4 + 187960^4 = 20615673^4.$$

The theory of elliptic curves also provides a general procedure for constructing new rational solutions from old ones, using the geometry of the curve. Using this, Elkies proved that rational points are dense on the surface $r^4 + s^4 + t^4 = 1$that is, any patch of the surface, however tiny, must contain a rational point. There are, in short, *lots* of solutions. The numbers get very big, though. The second solution generated by this geometric construction is

$$x = 1439965710648954492268506771833175267850201426615300442218292336336633;$$
$$y = 4417264698994538496943597489754952845854672497179047898864124209346920;$$
$$z = 9033964577482532388059482429398457291004947925005743028147465732645880;$$
$$w = 9161781830035436847832452398267266038227002962257243662070370888722169.$$

After Elkies had discovered there *was* a solution, Roger Frye of the Thinking Machines Corporation did a computer search, and found the smallest possible one:

$$95800^4 + 217519^4 + 414560^4 = 422481^4.$$

New Solutions for Old

Let's take a closer look at the trick that Elkies used to find an infinite set of solutions, starting from just one. It's most easily explained for elliptic curves of a slightly simpler form than Demjanenko's, where y^2 equals a *cubic* polynomial in x:

$$y^2 = ax^3 + bx^2 + cx + d.$$

Any straight line cuts such a curve at three points. If the coordinates of two are known, it is possible to calculate the coordinates of the third. If the first two have rational coordinates, the third does too. You can think of the third point as a sort of 'product' of the other two. Astoundingly, this product obeys reasonable algebraic laws, so the set of solutions forms what an algebraist would call an Abelian group. L. J. Mordell proved that there exist a *finite* number of rational solutions to the equation for any elliptic curve, such that every other rational solution can be obtained from them by repeated products of this kind. The group of solutions is 'finitely generated'. In particular, the equation has infinitely many rational solutions. Other Diophantine equations are much more miserly with their solutions, having only finitely many, or even none at all! For example there is an important theorem of Axel Thue and Carl Siegel, to the effect that a general class of equations of degree 3 or higher has only a finite number of solutions. A more specific example is W. Ljunggren's result of 1942 that there are exactly two solutions to the equation $x^2 + 1 = 2y^4$, namely $(x, y) = (1, 1)$ and $(239, 13)$. So the question is, how can we distinguish Scrooge from Santa Claus?

Equations With Holes

The answer to this came from a very different direction: algebraic geometry. Algebraic geometers study (very generalized versions of)

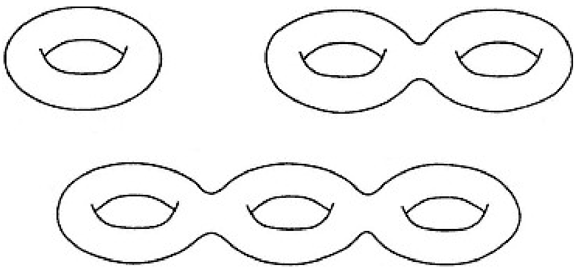


Fig. 2.
Surfaces of genus 1, 2, and 3.

curves and surfaces defined by polynomial equations, called *algebraic varieties*. The main impetus for the theory came from nineteenth-century work on polynomials over the complex numbers. An equation $f(x, y) = 0$ in two real variables defines a *curve* in the (x, y)-plane. A similar equation in two complex variables defines an analogous object, a complex curve. However, the complex plane is two-dimensional whereas the real line is one-dimensional, so complex objects have a habit of doubling their dimensions in comparison with the corresponding real objects. It follows that a complex curve is two-dimensional over the reals, that is, it is really a *surface*. According to topologists, a general surface looks like a sphere with lots of handles glued to it, or equivalently a torus (doughnut) with lots of holes. The number of holes is called the *genus* of the surface. So every complex equation has a number, its genus, associated with it. This topologically defined number may seem a rather arbitrary and obscure creature, but it is very important in algebraic geometry. It can, for example, be calculated arithmetically.

Mordell, in 1922, noticed that the only equations known to have infinitely many rational solutions are those of genus 0 or 1. For example the Pythagorean equation has genus 0; elliptic curves genus 1. Every equation treated by Diophantus has genus 0 or 1! Here at last was a semblance of order and generality. Mordell conjectured

that every equation of genus 2 or more has only finitely many rational solutions, possibly none. The Fermat equation has genus $(n\ 1)(n\ 2)/2$, which is greater than 1 when n is greater than 3. So *one single special case* of the Mordell Conjecture implies that for any $n > 3$, there are only finitely many solutions (if any) to the Fermat equation. This shows what a powerful result the Mordell Conjecture would be. The next step, after proving finiteness of the set of solutions, would be to put a definite bound on their size'make the theorem effective'. After that, in principle routine computation would show whether any solutions at all exist, and if so, find them. Of course this is all pie in the sky, but so was the Mordell Conjecture.

The evidence in favour of the Mordell Conjecture was very slight; so slight that André Weil, one of the world's leading number theorists, remarked that 'one cannot perceive a single good reason to bet for or against it'. Weil is a frequent critic of the overuse of the word 'conjecture' to dignify wild guesses that mathematicians *hope* might turn out to be true. Ironically, it was some conjectures due to Weil that opened up a path to a proof of the Mordell Conjecture.

The Fruits of Boredom

Weil tells us that in 1947, while in Chicago,

> I felt bored and depressed, and, not knowing what to do, I started reading Gauss's two memoirs on biquadratic residues, which I had never read before. The Gaussian integers occur in the second paper. The first one deals essentially with the number of solutions of equations $ax^4by^4 = 1$ in the prime field modulo p, and with the connection between these and certain Gaussian sums; actually the method is exactly the same that is applied in the last section of the *Disquisitiones* to the Gaussian sums for order 3 and the equations $ax^3by^3 = 1$. Then I noticed that similar principles can be applied to all equations of the form $ax^m + by^n + cz^r = 0$, and that this implies the truth of the so-called 'Riemann Hypothesis' for all curves $ax^n + by^n + cz^n = 0$ over finite fields. This led me to conjecture about varieties over finite fields .

The Weil Conjectures can be described as basic contributions to the following question: given a prime p, how many solutions does a given Diophantine equation have modulo p? This is important for ordinary integer solutions of Diophantine equations, because any solution in integers can be 'reduced' to give a solution modulo p. So, for example, if there are no solutions modulo p, there are no solutions at all! In practice there are problems in 'gluing together' the information coming from reduction modulo different primes; and Weil developed a very beautiful idea. It is known that for any prime power p^n there exists a *finite field* having exactly p^n elements. A field is a system with a self-consistent arithmetic permitting division. Given an equation, let a_n be the number of solutions that it has in this field. There is a gadget called the *zeta-function* of the equation which captures, in a single object, all the numbers a_n. Define an *algebraic variety* to be the set of solutions of not just one equation, but a whole system. Then Weil conjectured, on the basis of his investigations on curves, that for a general algebraic variety V, three things hold:

(1) The zeta-function of V can be computed in terms of a special finite set of algebraic integers.

(2) There is a 'functional equation' relating the zeta-functions of s and s/p^n.

(3) (The 'Riemann Hypothesis' for *V*) The algebraic integers in (1) all lie on a circle in the complex plane, whose centre is the origin, and whose radius is $p^{n/2}$.

All this may seem pretty technical, but the point is that you get very detailed information about the number of solutions to a system of Diophantine equations over a finite field.

A Surfeit of Conjectures

Ever since the nineteenth century algebraic geometers have exploited an analogy between algebraic number fields and 'function fields', whose elements satisfy an equation whose coefficients are not rational

numbers, but rational functions $p(x)/q(x)$ where p and q are polynomials. The first evidence that Mordell might be right came in 1963 when Yuri Manin proved the analogous result for function fields. H. Grauert obtained the same result independently in 1965. Meanwhile in 1962 Igor Shafarevich had made a conjecture about 'good reduction' for curvesthe process whereby an equation over the integers is reduced modulo a prime. (Instead of solving, say, $x^3 = 22$, one looks at equations such as $x^3 \equiv 22 \pmod 7$. This has the solution $x \equiv 2 \pmod 7$ and tells us something about the original x. By using many primes in place of the 7, further information can be gained.) His idea was that there should be only finitely many different types of curve, of given genus, having good reduction for all but a fixed finite set of primes. The important feature is the finiteness of the set of curves. And the pace hotted up in 1968 when A. N. Parshin proved that the Shafarevich Conjecture implies the Mordell Conjecture. As you may by now have noticed, algebraic geometry is rife with conjectures. In 1966 John Tate stated yet another, about 'abelian varieties', a generalization of elliptic curves to arbitrary dimensions. By the early 1980s experts in Diophantine equations were beginning to suspect that Mordell was right, that a proof was lurking not far away, and that Tate's conjecture was also involved. And in 1983 Gerd Faltings found a way to prove not only Mordell's Conjecture, but also those of Shafarevich and Tate. His method can be viewed as a very elaborate variation on Fermat's 'infinite descent'. Faltings first proves the Tate Conjecture. Combining that with Deligne's results on the Weil Conjectures, he establishes the Shafarevich Conjecture. And now Parshin's work on the Mordell Conjecture completes the proof. (A different proof was found very recently by P. Vojta.)

Regarding Fermat's Last Theorem, a gap still remained. Finitely many solutions is not the same as none. For all we can tell from Faltings's Theorem, the Fermat equation might have millions of solutions for any *n*! However, D. R. Heath-Brown soon proved that the proportion of integers *n* for which it has no solutions approaches 100 per cent as *n* becomes large, so Fermat's Last Theorem is 'almost always' true.

The Taniyama Conjecture

Time to go for the jugular. Can we drop that weasel word 'almost'?

We now know that the answer is 'yes', but its justification is highly technical. Wiles's proof of Fermat's Last Theorem is not the kind that can be written on the back of an envelope, and it takes a real expert to understand it in any detail. However, the general outline of the proof *is* comprehensible, so I'm going to try to give you some of the flavour of this radical development in number theory. At the very least, it will serve to drive home that whatever Fermat had in mind when he claimed to have a proof, it couldn't have been anything remotely like Wiles's argument.

The approach by way of ideals had come grinding to a halt, and it was clear that somebody must find a different entry point to the whole questionan entry point that would place Fermat's Last Theorem into a rich and well-understood context, an entry point that opened up the possibility of bringing some really powerful machinery to bear. And what context involved more powerful machinery than elliptic curves? Elliptic curves stimulated Mordell to his deep and influential conjecture, and over the years a very powerful theory has been developed. You could say that elliptic curves are the one area of Diophantine equations that we understand pretty well.

However, this understanding remains incomplete. Elliptic curve theory has its own big unsolved problems, and the biggest of all is called the *Taniyama conjecture*. Indeed it has various names, among them the TaniyamaShimura conjecture, the TaniyamaWeil conjecture, and the TaniyamaShimuraWeil conjecture. The surfeit of names reflects the history of the topic, for in 1955 Yutaka Taniyama asked some questions which were generalized and made more precise by Goro Shimura and by Weil. So we have a choice: use a simple name, or a historically accurate one. I've chosen to go for simplicity, so hereafter it's the Taniyama Conjectureeven though Taniyama asked something rather different.

We can grasp the general idea behind the Taniyama conjecture by thinking about one very special case. There is an intimate relationship

between the Pythagorean equation $a^2 + b^2 = c^2$, the unit circle, and the trigonometric functions 'sine' and 'cosine' (sin and cos). To obtain this relationship, write the Pythagorean equation in the 'dehomogenized' form $(a/c)^2 + (b/c)^2 = 1$. We can interpret this as saying that the point $x = a/c$, $y = b/c$ lies on the unit circle, with equation $x^2 + y^2 = 1$. The theory of trigonometric functions then provides a neat and simple way to *parametrize* the unit circleto represent it in terms of a single auxiliary variable. The fundamental relation between sines and cosines is

$$\cos^2 A + \sin^2 A = 1,$$

which holds for any angle A. This implies that if we set $x = \cos A$, $y = \sin A$, then the point with coordinates (x, y) lies on the unit circle. To sum up: solving the Pythagorean equation in integers is equivalent to finding an angle A such that both $\cos A$ and $\sin A$ are rational numbers (equal respectively to a/c and b/c). Because the trigonometric functions have all sorts of pleasant properties, this idea is the basis of a really fruitful theory of the Pythagorean equation.

The Taniyama conjecture says that the same kind of game can be played if the circle is replaced by any elliptic curve, but using more sophisticated functions than trigonometric ones, the so-called 'modular' functions. Specifically, it states that *every* elliptic curve can be parametrized by suitable modular functions, just as sin and cos parametrize the 'Pythagorean curve', the unit circle.

Frey's Elliptic Curve

Between 1970 and 1975 Yves Hellegouarch published a series of papers on a connection between Fermat curves $x^n + y^n = z^n$ and elliptic curves, and used them to deduce theorems about elliptic curves from known partial results about Fermat's Last Theorem. Jean-Pierre Serre suggested using the idea in the opposite direction, exploiting properties of elliptic curves to prove results on Fermat's Last Theorem. He found evidence suggesting that this line of attack had the potential to crack the whole problem wide open, and slowly the experts started to believe that Fermat's Last Theorem was on the

verge of yielding up its secrets. But it was to be a long, technical struggle.

In 1985, in a lecture at the international mathematical research centre at Oberwolfach, in the Black Forest area of Germany, Gerhard Frey made Serre's suggestion precise by introducing what is now called the *Frey elliptic curve* associated with a presumptive solution of the Fermat equation. The form chosen for this curve goes back to Hellegouarch, but Frey intended to use it in a different way. Suppose that there is a nontrivial solution $A^n + B^n = C^n$ of the Fermat equation. I've used capital letters A, B, C to show we're thinking of some *specific* solution in nonzero integers. For instance A might be 777235, B might be 84153, and C 28994except that those number *don't* satisfy the Fermat equation. We choose this presumptive solution, and from that moment on A, B, and C denote fixed (but unknown) integers. All we know about them is that $A^n + B^n = C^n$ for some $n \geq 3$, although we can also do some preliminary tidying and assume that A, B, and C have no common factor.

Now think about the specific (but unknown and possibly non-existent) elliptic curve whose equation is

$y^2 = x(x + A^n)(x B^n)$.

This is Frey's elliptic curve, and it exists if and only if Fermat's Last Theorem is wrong. So we want to prove that Frey's curve cannot exist. The way to do this it to assume that it does, and fight our way to *some* contradictory conclusion. It doesn't matter what the contradiction is.

Frey hacked away at his curve with the powerful general theory of elliptic curves, and discovered that if the Frey curve exists then it is a very curious beast indeed. So curious, indeed, that it seems highly unlikely that such a beast can exist at allwhich is exactly what we would like to prove. In 1986 Kenneth Ribet made Frey's idea precise, by proving that if the Taniyama conjecture is true then Frey's elliptic curve definitely cannot exist. Specifically, Frey's curve *cannot* be parametrized by modular functions. If the Taniyama conjecture is true, Fermat's Last Theorem follows.

This is a major, major link in the Fermat chain, for it tells us that

Fermat's Last Theorem is not just an isolated curiosity. Instead, it lies at the heart of modern number theory.

Seven-year Itch

As a child, Andrew Wiles had wanted to prove Fermat's Last Theorem. But when he became a professional mathematician he decided that it was just an isolated, difficult problemnice to prove, but not really *important* beyond its notoriety. Then he learned of Ribet's work, changed his view completely, and immediately decided to devote all of his research effort to a proof.

He realized that you don't need the full force of the Taniyama conjecture to make this approach work: you just need one particular special case of it, one that applies to a class of elliptic curves known as 'semistable'. Wiles broke the problem down into six pieces, and piece by piece he solved them, until finally only one held out. Then a lecture by Barry Mazur on something totally different sparked an idea that gave him the final clue. In a 200-page paper he marshalled enough powerful machinery to prove the semistable case of the Taniyama conjecture. This was enough for him to prove the following theorem. Suppose that M and N are distinct nonzero relatively prime integers such that $MN(M N)$ is divisible by 16. Then the elliptic curve $y^2 = x(x + M)(x + N)$ can be parametrized by modular functions. Indeed the condition on divisibility by 16 implies that this curve is semistable, so the semistable Taniyama conjecture establishes the desired property.

Now apply Wiles's theorem to Frey's curve, by letting $M = A^n$, $N = B^n$. Then $M N = A^n + B^n = C^n$, so $MN(M N) = A^nB^nC^n$, and we have to show this must be a multiple of 16. But this is easy. At least one of A, B, C must be evenbecause if A and B are *both* odd then C^n is a sum of two odd numbers, hence even, which implies that C is even. A tactical move is now in order: at this stage of the argument we can make life much easier for ourselves by taking $n \geq 5$, rather than $n \geq 3$, because Euler's proof establishes Fermat's Last Theorem for $n = 3$. Now we merely observe that the fifth or higher power of an even number is divisible by $2^5 = 32$, so $A^nB^nC^n$ is a multiple of 32, hence certainly a multiple of 16.

Therefore Frey's curve satisfies the hypotheses of Wiles's theorem, implying that it can be parametrized by modular functions. But Frey has proved that it can't be!

This is the contradiction that we have been seeking all alongbut this time it is signed, sealed, and delivered. The house of cards that we set up by assuming that there exists a nontrivial solution to the Fermat equation for $n \geq 3$ has collapsed in ruins. Therefore no such solution can existso Fermat's Last Theorem is true.

In summary: Wiles's strategy implies the semistable Taniyama conjecture, which implies that Ribet's argument proves that Frey's elliptic curve doesn't existand Fermat was right all along.

Wiles had a huge battery of powerful techniques to work with, but it took him seven years of hard effort to see how to fit the pieces together. He knew what strategy to use, but like any general fighting a major battle, he had to get his tactics right too. And as soon as he announced his proof, of course, there was a new question to ponder. Was it right? It is *so* easy to make mistakes

Right from the start, a surprisingly large number of experts were willing to commit themselves by saying, publicly, that they believed the proof was correct. The strategy made sense, they could see how the tactics were applied, they knew what the sticky points were and what ideas Wiles had invented to get over them. Mazur summed up the consensus view: 'It has the ring of truth.'

Eighth-year Glitch

Speculation was rife, but hard facts were thin on the ground. Unusually, Wiles did not release a 'preprint', or informal version, of his proof. Given the amount of interest, this was a reasonable move: coping with the likely demand would have laid low a forest or two. Instead he submitted his work to the world's leading mathematical journal, which sent it out to half a dozen experts to be refereed. Meanwhile, its hunger unsated, the mathematical world's communication lines hummedtechnical details gleaned from people who had been at the Newton Institute when Wiles made his historic announcement, comments, queries. Jokes, even. One widely circulated electronic mail message reported a spoof newspaper article

about the riotous behaviour that (allegedly) followed the announcement of the proof. (For the meaning of 'Bieberbach' see Chapter 11, and for the Four-Colour Theorem see Chapter 8.)

> 'Math hooligans are the worst,' said a Chicago Police Department spokesman. 'But the city learned from the Bieberbach riots. We were ready for them this time.'
>
> When word hit Wednesday that Fermat's Last Theorem had fallen, a massive show of force from law enforcement at universities all around the country headed off a repeat of the festive looting sprees that have become the traditional accompaniment to triumphant breakthroughs in higher mathematics.
>
> Mounted police throughout Hyde Park kept crowds of delirious wizards at the University of Chicago from tipping over cars on the midway as they first did in 1976 when Wolfgang Haken and Kenneth Appel cracked the long-vexing Four-Color Problem. Incidents of textbook-throwing and citizens being pulled from their cars were described by the university's math department as 'isolated'.

After a few weeks of such excitement, however, rumours of a mistake surfaced. On 6 December 1993 Wiles circulated his own e-mail message:

> In view of the speculation on the status of my work on the TaniyamaShimura conjecture and Fermat's Last Theorem, I will give a brief account of the situation. During the review process a number of problems emerged, most of which have been resolved, but one in particular I have not yet settled. The key reduction of (most cases of) the TaniyamaShimura conjecture to the calculation of the Selmer group is correct. However the final calculation of a precise upper bound for the Selmer group in the semistable case (of the symmetric square representation associated to a modular form) is not yet complete as it stands. I believe that I will be able to finish this in the near future using the ideas explained in my Cambridge lectures.

(Here Wiles's 'TaniyamaShimura conjecture' is my 'Taniyama conjecture' as explained earlier.)

It is not unusual for minor tactical errors to emerge when a long and difficult mathematical proof is subjected to the very close scrutiny that is appropriate to a major breakthrough. The question then is: can the errors be repaired, the gaps closedor is the mistake fatal?

Time passed. The promised repairs did not materialize.

Around Easter 1994 many mathematicians received a remarkable message in their electronic mail:

> There has been a really amazing development today on Fermat's Last Theorem. Noam Elkies has announced a counterexample, so that FLT is not true at all! He spoke about this at the Institute today. The solution to Fermat that he constructs involves an incredibly large prime exponent (larger than 10^{20}), but it is constructive I wasn't able to get all of the details, which were quite intricate

Eventually it emerged that this message had originated on April 1st.

Ninth-year Stitch

By the Autumn of 1994 even many experts were becoming pessimistic, estimating that stitching up the hole in Wiles's proof would take at least three years, maybe longerif at all. Fermat's Last Theorem seemed to be slipping away. Then, on 26 October 1994, Rubin circulated another e-mail message:

> As of this morning, two manuscripts have been released:
>
> *Modular elliptic curves and Fermat's Last Theorem*, by Andrew Wiles.
>
> *Ring theoretic properties of certain Hecke algebras*, by Richard Taylor and Andrew Wiles.
>
> The first one (long) announces a proof of, among other things, Fermat's Last Theorem, relying on the second one (short) for one crucial step.
>
> As most of you know, the argument described by Wiles in his Cambridge lectures turned out to have a serious gap, namely the construction of an Euler system. After trying unsuccessfully to repair that construction, Wiles went back to a different approach, which he had tried earlier but abandoned in favour of the Euler system idea. He was then able to complete his proof, under the hypothesis that certain Hecke algebras are local complete intersections. This and the rest of the ideas described in Wiles's Cambridge lectures are written up in the first manuscript. Jointly, Taylor and Wiles establish the necessary property of the Hecke algebras in the second paper.

By 1995 these two papers had been refereed, approved, and accepted. By the time this book is in print, they will be published. Barring yet another hiccup, the saga of Fermat has come to an end.

Or, more accurately, to a new beginning. Already the proof is

being simplified. Wiles's revised argument is shorter and simpler than the first incomplete attempt. Faltings, it is said, has already simplified parts of it. More importantly, we now have access to a whole heap of powerful new techniquesin particular the semistable case of the Taniyama conjecturewhich we can apply to other questions about elliptic curves and anything we can contrive to link to them.

There is a new power at the heart of number theory.

So: did Fermat really have a proof? There really is no likelihood that Fermat had anything similar in mind to the proof that Wiles has given the world. Big conceptual chunks were not known in Fermat's dayamong them elliptic curves and modular functions. The technical machinery that underlies Wiles's approach includes rather a lot of modern algebraic number theory and algebraic geometry. The twentieth-century viewpoint would have made no sense in the nineteenth century, let alone the seventeenth.

Which leaves two possibilities. Either Fermat was mistaken, or his idea was quite different. Like virtually all mathematicians, my money is on the first. But sometimes, deep in the night, I wake up from strange dreams; and I wonder

4
Parallel Thinking

The study of 'non-Euclidean' geometry brings nothing to students but fatigue, vanity, arrogance, and imbecility. 'Non-Euclidean' space is the false invention of demons, who gladly furnish the dark understandings of the 'non-Euclideans' with false knowledge. The 'non-Euclideans', like the ancient sophists, seem unaware that their understandings have become obscured by the promptings of the evil spirits.
Matthew Ryan

Sydney Smith tells of two disputatious women with a garden fence between them. 'Those women will never agree', said Smith; 'they are arguing from different premises.' This reflects the lesson, painfully learned by philosophers and logicians alike, that you can't get something for nothing. Any chain of logical argument has to start with unproved hypotheses. As Aristotle said, 'It is not everything that can be proved, otherwise the chain of proof would be endless. You must begin somewhere, and you start with things admitted but undemonstrable.' Euclid was aware of this, and when he wrote his famous geometry text the *Elements*, around 300 BC, he started out by listing his assumptions. Most of them are unobjectionable enough: things like 'objects equal to the same thing are equal to each other.' But among them is one whose intuitive appeal is much more debatable: 'If a straight line falling on two straight lines makes the interior angles on the same side less than two right angles, then the two straight lines, if produced indefinitely, meet on that side on which are the angles less than two right angles.' With a little work it can be seen that this assumption is equivalent to a more readily intelligible one: given a line, and a point not on the line, there is a unique

parallel to the line through the given point. A *parallel*, of course, is a line that does not meet the original line however far it is produced, that is, extended.

Commentators on Euclid generally regarded the need to make such an assumption as a blemish. Jean Le Rond d'Alembert called it a scandal. Proclus, in about AD 450, criticized it roundly: 'This ought to be struck out altogether; for it is a theorem involving many difficulties, which Ptolemy, in a certain book, set himself to solve. The statement that since the two lines converge more and more as they are produced, they will sometime meet, is plausible but not necessary.' It is clear that the Greeks had thought long and hard about the nature of the assumption. Innumerable attempts were subsequently made to prove that it follows from the rest of Euclid's assumptions: none was successful. Euclid's judgement was vindicated when it began to be clear that there exist consistent versions of geometry in which his *parallel axiom* does not hold. Instead of a single geometry there is a variety of geometries, in which different theorems are true. Their discovery took a long time, because everybody assumed that geometry somehow exists as an object of Nature, not just as a construct of the human mind. Eventually it was realized that, while it's perfectly sensible to ask 'what is the true geometry of Nature?', it's not a question that can be answered by mathematical reasoning alone.

Euclid Without Flaws

The earliest work on the problem has as its goal a *proof* of the property of parallels asserted by Euclid. A small number of mathematical problems seem to attract crackpots: squaring the circle, trisecting the angle, Fermat's Last Theorem, and the parallel axiom. As well as the lunatic fringe, there was some excellent work by people who *thought* they had found a proof, but who in reality had replaced Euclid's assumption by some logically equivalent, unproved one. These assumptions include:

Three points either lie on a line or on a circle.

The area of a triangle can be as large as we please.

The angles of a triangle add up to 180°.

Similar figures exist.

Others recognized that they had only found a logical replacement for Euclid's axiom, but felt their version was more intuitive.

The first serious attack was made by Girolamo Saccheri, who published a two-book work, *Euclid freed of every Flaw*, in 1733. Saccheri attempted a proof by contradiction, based on the following construction. Draw a straight line, erect two perpendiculars of equal length, and join them by a new line. Consider the angles (they are equal) made by this line with the perpendiculars. There are three cases:

(a) They are *right* angles.

(b) They are *obtuse* (greater than a right angle).

(c) They are *acute* (less than a right angle).

If you like, Saccheri's idea is that we try to build a rectangle and then ask whether we have succeeded. He showed that (a) is the same as the parallel axiom. The idea was to show that each of (b) and (c) leads to an absurdity, hence is false. Saccheri proceeded to make a series of deductions. Book 1 demolishes (to its author's satisfaction) the obtuse angle, and ends with the defiant statement that 'the hypothesis of the acute angle is absolutely false, being repugnant to the nature of the straight line'. Saccheri was whistling in the dark. That he wasn't convinced himself is suggested by the existence of Book 2, which attempts to shore up the argument. A crucial proposition, number 37, is given three proofs: all are fallacious. One involves infinitesimals; one, if correct, would prove that all circles have the same circumference; and one invokes physical ideas of time and motion. After this futile wriggling on his own hook, Saccheri comes to Proposition 38: 'The hypothesis of the acute angle is absolutely false because it destroys itself.' He then sums everything up by describing the refutation of the hypothesis of the obtuse angle as

being 'as clear as midday light', but notes that the falsity of the acute angle depends on that worrisome Proposition 37.

Fifty years later Lambert tried an approach very similar to Saccheri's, proving a large number of theorems which, although very strange to Euclidean eyes, do not give logical contradictions. Among his results on the hypothesis of the acute angle there is a formula relating the area of a triangle to the sum of its angles:

$k^2(\pi\ (A + B + C))$

where k is a certain constant. This is very similar to a theorem in spherical geometry, where the area of a triangle on a sphere of radius r is

$r^2((A + B + C)\ \pi)$.

Replacing r by $k\sqrt{1}$ Lambert hazarded the guess that the corresponding geometry is that of a sphere of imaginary radius. What he meant by thisif anythingis unclear; but we can detect in his work the germ of the idea that the parallel axiom may fail for the geometry of a surface.

Parallel Discoveries

All of this early work was motivated by a desire to demolish any alternatives to Euclid. But by the early nineteenth century a bolder viewpoint emerged: to devise self-consistent geometries that differ from Euclid's, in particular with regard to parallels. The main ideas seem to have been discovered independently by three people: János Bolyai, Nikolai Ivanovich Lobachevskii, and Gauss.

Bolyai was a dashing young artillery officer who reputedly fought, and won, thirteen consecutive duels in one day. Lobachevskii was a lecturer at Kazan University. Gauss was the leading mathematician of his (perhaps any) age. Bolyai's father Farkas was a friend of Gauss's, and had worked on the parallel axiom himself. He advised his son not to waste his time on such a poisonous problem. But in 1823 the son wrote to his father of his 'wonderful discoveries', saying 'I have created a new universe from nothing.' Meanwhile

Lobachevskii was busy: he lectured on parallels in 1826, and published a lengthy paper in Russian in 1829 which established, for the first time in print, an alternative to Euclid. It bore the unhappy name *Imaginary Geometry*. Bolyai published his work in 1832.

Gauss, in one of the more curious passages of his life, then wrote to the elder Bolyai to the effect that he was unable to praise the son's work because he, Gauss, had obtained identical results much earlier (but not put them into print). Gauss's refusal condemned Bolyai to a life of mathematical obscurity, and the merits of his work were not recognized until after his death. Lobachevskii fared little better: he died blind and in poverty. There is evidence to back up Gauss's claim: in 1824 he wrote to Franz Taurinus saying that the hypothesis of the acute angle 'leads to a peculiar geometry completely different from ours, a competely self-consistent geometry that I have developed for myself perfectly satisfactorily, so that I can solve any problem in it If at some time I have more leisure than now, I may publish my investigations.' In fact he never did, apparently worried that the public wouldn't understand: 'I fear the cries of the Boeotians '

Geometries Within Geometries

All three workers had convinced themselves that they had found an alternative, self-consistent type of geometry. They felt that way because everything seemed to fit together sensibly. But a complete proof that the system will *never* lead to a contradiction was lacking. It is doubtful if this lack was really noticed; certainly nobody worried too much about it. The final step was taken by Eugenio Beltrami in 1868. He was investigating the geometry of surfaces. On a curved surface, the analogue of a line is a *geodesic*the shortest curve between two points. Beltrami introduced a surface called the *pseudosphere*, and showed that the geometry of its geodesics satisfies Saccheri's hypothesis of the acute angle. It also satisfies the rest of Euclid's axioms. It is Bolyai/Gauss/Lobachevskii's non-Euclidean geometry in disguise. It follows that non-Euclidean geometry can never lead to a contradictionbecause, if it does, the pseudosphere

cannot exist. And the pseudosphere is a construction in *Euclidean* geometry! More accurately, what we now know is that the only way non-Euclidean geometry can contradict itself is for Euclidean geometry to contradict itself. If Saccheri's programme had succeeded, it would have demolished Euclidean geometry, not perfected it!

Beltrami had found what we now call a *model* for non-Euclidean geometry: a mathematical system whose objects, suitably interpreted, obey all of Euclid's axioms except that of parallels. The existence of such a model rules out any possibility of proving the parallel axiom from the others. For such a proof, interpreted within the model, will force the model to obey its interpretation of the parallel axiom as well. As David Hilbert said one day in the Berlin railway station: 'One must be able to say at all timesinstead of points, straight lines, and planestables, chairs, and beer mugs.' The idea behind this is that only those properties of geometrical objects that are expressed by the axioms may legitimately be used in a proof, so any particular interpretation of them in real terms is *in principle* irrelevant. In practice a good picture aids intuition, and it's a good bet that Hilbert's work on geometry was inspired by the usual mental picture of lines on a sheet of paper, not beer mugs on a table. He wasn't saying the traditional picture isn't useful, merely that it's not essential.

Beltrami's model was soon followed by others. A particularly beautiful one was devised by Henri Poincaré, who described a three-dimensional version thus:

> Suppose, for example, a world enclosed in a large sphere and subject to the following laws: the temperature is not uniform; it is greatest at the centre, and gradually decreases as we move towards the circumference of the sphere, where it is absolute zero. The law of this temperature is as follows: if R be the radius of the sphere, and r the distance from the centre, then the absolute temperature will be proportional to R^2r^2. Suppose that in this world the linear dilation of any body is proportional to its absolute temperature. A moving object will become smaller and smaller as it approaches the circumference of the sphere. Although from the point of view of our ordinary geometry this world is finite, to its inhabitants it will appear infinite. As they approach the surface of the sphere they will become colder, and at the same time smaller and smaller. The steps they take are also smaller and smaller,

> so that they can never reach the boundary of the sphere. If to us geometry is only the study of the laws according to which rigid solids move, to these imaginary beings it will be the study of the laws of motion of solids *deformed by the differences in temperature* alluded to.

Poincaré goes on to elaborate this model so that the paths traced by light rays become curvedin fact, arcs of circles meeting the boundary at right anglesand obey non-Euclidean geometry. It is actually a three-dimensional model: the analogous model using a disc instead of a sphere produces a two-dimensional non-Euclidean geometry. Felix Klein, who also devised models of non-Euclidean geometry, coined the name *hyperbolic* geometry for a model obeying Saccheri's hypothesis of the acute angle, and *elliptic* geometry for the obtuse angle. A model for elliptic geometry is very easy to find. Consider

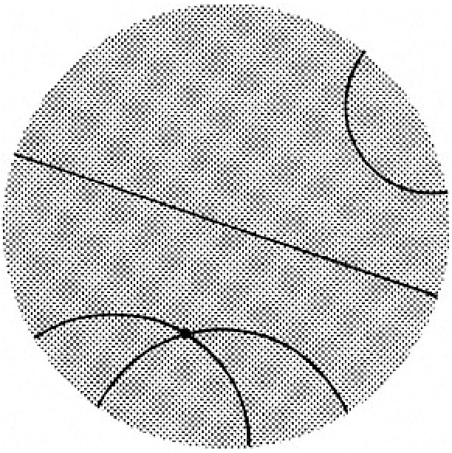


Fig. 3.
Poincaré's model of two-dimensional hyperbolic geometry is a disc whose boundary is considered to be 'at infinity'. Straight lines in hyperbolic geometry are modelled as arcs of circles meeting the boundary at right angles.

the surface of a sphere: interpret 'line' to mean 'great circle', and 'point' to mean 'pair of antipodal points'. (Great circles meet in two antipodal points: to obey Euclid's axiom that two lines meet in at most one point it is necessary to identify antipodal points in pairs. This is permissible by the Hilbert dictum about tables, chairs, and beer mugs.)

A Matter of Hindsight

To the modern mind it is astonishing how long it took for people to realize that geometries other than Euclid's are possible. The geometry of the sphere was already well developed, having important applications to astronomy, and hence navigation. Did nobody *notice* that in spherical geometry the parallel axiom is false? The answer is that they did; but they were unimpressed because the sphere is manifestly not the plane. Also, on a sphere any two lines, that is, great circles, meet at *two* points. This is clearly not what one would expect of a straight line. Only a person with a different logical point of view can get round this problem by pretending that antipodal points are the same! None of this would have been convincing at the time, because it was obvious that great circles aren't straight lines, and that two points aren't the same as one. To understand why it is convincing now, we must journey ahead in time and take a look at the logic of axiomatic systems.

The idea of an axiomatic system is that all assumptions are to be stated clearly at the start. In other words, the rules of the game are to be laid down precisely. You don't ask whether the axioms are 'true', any more than you ask whether the rules of chess are 'true'. However, you do expect to have a playable game. If the rules of chess were to insist on one page that white has the first move, and on another that black does, then it would be impossible even to start the game. In other words, we want the rules to be *consistent*. It is the same with axiomatic systems: the ultimate test is consistency, not truth. One way to prove that a system of axioms is consistent is to exhibit something that satisfies them. One example of a legal game of chess shows that the rules lead to a playable game; one

model for an axiom system shows that its assumptions do not contradict themselves. For example, we can show that Euclidean geometry is consistent by taking the usual coordinate geometry model of the plane as a set of points (x,y) where x and y are numbers, and checking that all of the axioms hold. There is one fly in the ointment, however: the question of which ingredients are permissible. The coordinate model of geometry establishes consistency *only* if the raw materialshere arithmeticare already known to be consistent. In other words, all consistency proofs are *relative*. Instead of asserting that system X is consistent, all we can show is that X is consistent provided some other system Y is. This viewpoint, now widely accepted, approaches the whole problem from a different direction. The original question was: 'Is Euclid's geometry the only true one?' But now even Euclidean geometry must come under scrutiny. Is *that* consistent? And the answer is that we have no idea. However, if arithmetic is consistent, then so is Euclid.

Now we ask: is *non*-Euclidean geometry consistent? And the answer is that again we don't know, but it is consistent provided either arithmetic, or Euclidean geometry, is. From the mathematical point of view, either Euclidean or non-Euclidean geometry is a reasonable thing to use. Which (if either) is the true geometry of physical space is revealed as a question for experiment, not mathematics. This seems so reasonable now that it's hard to appreciate how obscure it was then. The clarity is a matter of hindsight: without the unprecedented ideas of the nineteenth-century pioneers, everything would still be obscure.

Curved Space

The discovery of non-Euclidean geometries freed mathematics from its previous rigid stance, and a host of strange geometries proliferated. One might imagine that these oddities, while of theoretical interest, are of no importance for the behaviour of real space. But that also turned out to be untrue. In 1854 a young German, Georg Bernhard Riemann, was due to deliver his qualifying lecture for the title of *Privatdozent*, which translates loosely as 'assistant lecturer'.

The examiner was the great man himself, Gauss. Riemann offered several alternatives, some safe, some less so; and Gauss chose the most provocative, with the title 'On the Hypotheses which lie at the Foundation of Geometry'. Gauss later said he'd chosen that topic because he'd thought a lot about it himself, and wondered what the young man would have to say about it. Riemann, rising to the occasion, gave a brilliant lecture in which he imagined that the geometry of space might vary from point to point: hyperbolic here, Euclidean there, elliptic elsewhere. In two dimensions we can imagine this as the geometry of geodesics on some curved surface: the nature of the curvature determines the local geometry. Riemann extended this to multidimensional spaces, or *manifolds*. He ended prophetically by observing:

> It remains to resolve the question of knowing in what measure and up to what point these hypotheses about manifolds are confirmed by experience. Either the reality which underlies space must form a discrete manifold or we must seek the ground of its metric relations outside it, in the binding forces which act on it. This leads us into the domain of another science, that of physics, into which the object of our work does not allow us to go today.

In 1870 William Kingdon Clifford took up the prophecy. 'I hold that small portions of space are of a nature analogous to little hills on a surface which is on average flat That this property of being curved or distorted is continually passed on from one portion of space to another after the manner of a wave.' And he suggested that all physical motion was really just the ripples of these waves. In Albert Einstein's *General Theory of Relativity*, published some sixty years after Riemann, gravitational force is viewed as a curvature of space-time. Near a large mass, space is more bent than it is a long way away. Particles moving through the space follow curved paths because of this geometric distortion. The best experiments yet devised all confirm Einstein's picture, which explains a number of observations that would not accord with a Euclidean geometry for space-time; for example, irregularities in the motion of the planet Mercury, and the bending of light by a star. More recently, astronomers have found that entire galaxies can act as 'gravitational lenses',

focusing and distorting the light from distant sources. In one instance, what appeared to be two identical quasars close together in the sky were found to be a double image, in a distorted gravitational lens, of a *single* quasar on the far side of a massive galaxy. The galaxy was observed by 'subtracting' one image from the other. Still more recently, the study of solitary waves, or solitons, has suggested to some theorists that the behaviour of sub-atomic particles may resemble the moving hills imagined by Clifford. At any rate, we now believe that the geometry of space or space-time, on astronomical scales, is *not* Euclidean!

5
The Miraculous Jar

To see a World in a grain of sand,
And a Heaven in a wild flower,
Hold Infinity in the palm of your hand,
And Eternity in an hour.
William Blake

Pick up any mathematics book or journal and you probably won't get more than a page or two into it before you bump headlong against infinity. For example, selecting a few books from my shelf at random, the second *line* of the introduction to Harold Davenport's *The Higher Arithmetic* refers to 'the natural numbers 1, 2, 3, ' and the sequence of dots is intended to imply that the natural numbers are infinite in extent. Page 2 of *Numerical Analysis* by Lee Johnson and Dean Riess quotes the infinite series for the exponential function. B. A. F. Wehrfritz's *Infinite Linear Groups*need I say more? Page 1 of *Nonlinear Dynamics and Turbulence*, edited by G. I. Barenblatt, G. Iooss, and D. D. Joseph, refers to 'the NavierStokes equations or a finite-dimensional Galerkin approximation', correctly leading us to infer that mathematicians treat the full NavierStokes equations as an infinite-dimensional object. Page 434 of *Winning Ways* by Elwyn Berlekamp, John Conway, and Richard Guy talks of a game whose position is '∞, 0, ±1, ±4', where ∞ is the standard symbol for 'infinity'. You may feel that page 434 is stretching the point a bit, but it's the sixth page in volume 2, and I didn't check volume 1.

Infinity is, according to Philip Davis and Reuben Hersh, the 'Miraculous Jar of Mathematics'. It is miraculous because its contents are inexhaustible. Remove one object from an infinite jar and you

have, not one fewer, but *exactly the same number* left. It is paradoxes like this that forced our forefathers to be wary of arguments involving appeals to the infinite. But the lure of infinity is too great. It is such a marvellous place to lose awkward things in. The number of mathematical proofs that succeed by pushing everything difficult out to infinity, and watching it vanish altogether, is itself almost infinite. But what do we really mean by infinity? Is it wild nonsense, or can it be tamed? Are the infinities of mathematics real, or are they clever fakes, the wolf of the infinite in finite sheep's clothing?

Hilbert's Hotel

If you are laying a table and each place-setting has one knife and one fork, then you know that there are just as many knives as forks. This is true whether you lay out an intimate candlelit dinner for two or a Chinese banquet for 2,000, and you don't need to know how many places are set to be sure the numbers agree. This observation is the cornerstone of the number concept. Two sets of objects are said to be in *one-to-one correspondence* if to each object in one there corresponds a unique object in the other, and vice versa. Sets that can be placed in one-to-one correspondence contain the same number of objects.

When the sets are infinite, however, paradoxes arise. For example, Hilbert described an imaginary hotel with infinitely many rooms, numbered 1, 2, 3, One evening, when the hotel is completely full, a solitary guest arrives seeking lodging. The resourceful hotel manager moves each guest up a room, so that the inhabitant of room 1 moves to room 2, room 2 to 3, and so on. With all guests relocated, room 1 becomes free for the new arrival! Next day an Infinity Tours coach arrives, containing infinitely many new guests. This time the manager moves the inhabitant of room 1 to room 2, room 2 to 4, room 3 to 6, , room *n* to 2*n*. This frees all odd-numbered rooms, so coach passenger number 1 can go into room 1, number 2 to room 3, number 3 to room 5, and, in general, number *n* to room 2*n* 1. Even if infinitely many infinite coachloads of tourists arrive, everybody can be accommodated.

Similar paradoxes have been noted throughout history. Proclus, who wrote commentaries on Euclid in about AD 450, noted that the diameter of a circle divides it into two halves, so there must be twice as many halves as diameters. Philosophers in the Middle Ages realized that two concentric circles can be matched one-to-one by making points on the same radius correspond; so a small circle has just as many points as a large one. In Galileo's *Mathematical Discourses and Demonstrations* the sagacious Salviati raises the same problem: 'If I ask how many are the Numbers Square, you can answer me truly, that they be as many as are their propper roots; since every Square hath its Root, and every Root its Square, nor hath any Square more than one sole Root, or any Root more than one sole Square.' To this the seldom-satisfied Simplicius replies: 'What is to be resolved on this occasion?' And Salviati cops out with: 'I see no other decision that it may admit, but to say, that all Numbers are infinite; Squares are infinite; and that neither is the multitude of Squares less than all Numbers, nor this greater than that: and in conclusion, that the Attributes of Equality, Majority, and Minority have no place in Infinities, but only in terminate quantities.'

Infinity in Disguise

Galileo's answer to the paradoxes is that infinity behaves differently from anything else, and is best avoided. But sometimes it's very hard to avoid it. The problem of infinity arose more insistently in the early development of calculus, with the occurrence of infinite series. For example, what is

$$1+\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\ldots ?$$

It's easy to see that as the number of terms increases, the sum gets closer and closer to 2. So it's attractive to say that the whole infinite sum is *exactly* 2. Newton made infinite series the foundation of his methods for differentiating and integrating functions. So the problem of making sense of them must be faced. And infinite series are themselves paradoxical. For example, what does the series

1 1 + 1 1 + 1 1 +

add up to? Written like this

(1 1) + (1 1) + (1 1) +

the sum is clearly 0. On the other hand,

1 (1 1) (1 1)

is clearly 1. So 0 = 1, and the whole of mathematics collapses in a contradiction.

Calculus was much too important for its practitioners to be put off by minor snags and philosophical problems like this. Eventually the matter was settled by reducing statements about infinite sums to more convoluted ones about finite sums. Instead of talking about an infinite sum $a_0 + a_1 + a_2 +$ having value a, we say that the *finite* sum $a_0 + a_1 + + a_n$ can be made to differ from a by less than any assigned error ε, provided n is taken larger than some N (depending on ε). Only if such an a exists does the series *converge*, that is, is the sum considered to make sense. In the same way the statement 'there are infinitely many integers' can be replaced by the finite version 'given any integer, there exists a larger one'. As Gauss put it in 1831: 'I protest against the use of an infinite quantity as an actual entity; this is never allowed in mathematics. The infinite is only a manner of speaking, in which one properly speaks of limits to which certain ratios can come as near as desired, while others are permitted to increase without bound.' Today, in any university course on analysis, students are taught to handle the infinite in this way. A typical problem might be: 'prove that $(n^2 + n)/n^2$ tends to 1 as n tends to infinity'. Woe betide the student who answers '$(\infty^2 + \infty)/\infty^2 = \infty/\infty = 1$'. But also woe betide him who writes '$(n^2 + n)/n^2 = 1 + 1/n$, now let $n = \infty$ to get $1 + 1/\infty = 1 + 0 = 1$', although this is arguably correct. (Before you're allowed to write sloppy things like that you must prove your mathematical machismo by going through the tortuous circumlocutions needed to make it unobjectionable. Once you've learned, the hard way, not to be sloppy, nobody minds if you are!)

This point of view goes back to Aristotle, and is described as *potential infinity*. We do not assert that an actual infinite exists, but we rephrase our assertion in a form that permits quantities to be as large as is necessary at the time. No longer do we see the miraculous jar as containing a true infinity of objects; we just observe that however many we take out, there's always another one in there. Put that way, it sounds like a pretty dubious distinction; but on a philosophical level it avoids the sticky question: 'How much stuff is there in that jar?'

Sum Crisis!

However, there were still bold souls who continued to play about with the idea of 'actual' infinity; to think of an infinite set not as a process 1, 2, 3, which could in principle be continued beyond any chosen point, but as a completed, infinite whole. One of the first was Bernard Bolzano, who wrote a book called *Paradoxes of the Infinite* in 1851. But Bolzano's main interest was in providing solid foundations for calculus, and he decided that actually infinite sets aren't really needed there.

In the late nineteenth century there was something of a crisis in mathematics. Not fancy philosophical paradoxes about infinity, but a solid down-to-earth crisis that affected the day-to-day technique of working mathematicians, in the theory of Fourier series. A Fourier series looks something like this:

$$f(x) = \cos x + \frac{1}{2}\cos 2x + \frac{1}{3}\cos 3x + \ldots$$

and was developed by Joseph Fourier in his work on heat flow. The question is, when does such a series have a sum? Different mathematicians were obtaining contradictory answers. The whole thing was a dreadful mess, because too many workers had substituted plausible 'physical' arguments for good logical mathematics. It needed sorting out, urgently. Basically the answer is that a Fourier series makes good sense provided the set of values *x*, at which the function behaves badly, is not itself too nasty. Mathematicians were forced to look at the fine structure of sets of points on the real line. In 1874

this problem led Georg Cantor to develop a theory of *actually* infinite sets, a topic that he developed over the succeeding years. His brilliantly original ideas attracted attention and some admiration, but his more conservatively minded contemporaries made little attempt to conceal their distaste. Cantor did two things. He founded Set Theory (without which today's mathematicians would find themselves tongue-tied, so basic a language has it become), and he discovered in so doing that some infinities are bigger than others.

Cantor's Paradise

Cantor started by making a virtue out of what everyone else had regarded as a vice. He *defined* a set to be infinite if it can be put in one-to-one correspondence with a proper part (subset) of itself. Two sets are equivalent or have the same *cardinal* if they can be put in one-to-one correspondence with each other. The smallest infinite set is that comprising the natural numbers {0, 1, 2, 3, }. Its cardinal is denoted by the symbol $\aleph_0$ (aleph-zero) and this is the smallest infinite number. It has all sorts of weird properties, such as

$$\aleph_0 + 1 = \aleph_0, \aleph_0 + \aleph_0 = \aleph_0, \aleph_0^2 = \aleph_0$$

but nevertheless it leads to a consistent version of arithmetic for infinite numbers. (What do you expect infinity to do when you double it, anyway?) Any set with cardinal $\aleph_0$ is said to be *countable*. Examples include the sets of negative integers, all integers, even numbers, odd numbers, squares, cubes, primes, andmore surprisinglyrationals. We are used to the idea that there are far more rationals than integers, because the integers have large gaps between them whereas the rationals are densely distributed. But that intuition is misleading because it forgets that one-to-one correspondences don't have to respect the order in which points occur. A rational *p/q* is defined by a pair (*p, q*) of integers, so the number of rationals is $\aleph_0^2$. But this is just $\aleph_0$ as we've seen.

After a certain amount of this sort of thing, one starts to wonder whether *every* infinite set is countable. Maybe Salviati was right, and $\aleph_0$ is just a fancy symbol for ∞. Cantor showed this isn't true. The

set of real numbers is uncountable. There is an infinity bigger than the infinity of natural numbers! The proof is highly original. Roughly, the idea is to assume that the reals are countable, and argue for a contradiction. List them out, as decimal expansions. Form a new decimal whose first digit after the decimal point is different from that of the first on the list; whose second digit differs from that of the second on the list; and in general whose *n*th digit differs from that of the *n*th on the list. Then this new number cannot be anywhere in the list, which is absurd since the list was assumed to be complete. This is Cantor's 'diagonal argument', and it has cropped up ever since in all sorts of important problems. Building on this, Cantor was able to give a dramatic proof that transcendental numbers must exist. Recall that a number is transcendental if it does not satisfy any polynomial equation with rational coefficients. Examples include π = 3.14159 and the base of natural logarithms *e* = 2.71828 , although it took mathematicians many years to prove that suspicion. In 1873 Charles Hermite proved that *e* is transcendental. The degree of difficulty can be judged from a letter that he wrote: 'I do not dare to attempt to show the transcendence of π. If others undertake it, no one will be happier than I about their success, but believe me, my dear friend, this cannot fail to cost them some effort.' In 1882 Ferdinand Lindemann made the effort, and succeeded in adapting Hermite's approach to deal with π.

Cantor showed that you do not need these enormously difficult theorems to demonstrate that trancendental numbers *exist*, by proving in a very simple manner that the set of algebraic numbers is countable. Since the full set of reals is uncountable, there must exist numbers that are not algebraic. End of proof (which is basically a triviality); collapse of audience in incredulity. In fact Cantor's argument shows more: it shows that there must be uncountably many transcendentals! There are *more* transcendental numbers than algebraic ones; and you can prove it without ever exhibiting a single example of either. It must have seemed like magic, not mathematics.

Even Cantor had his moments of disbelief. When, after three years of trying to demonstrate the opposite, he proved that *n*-dimensional space has exactly the same number of points as 1-dimensional space,

he wrote: 'I see it but I do not believe it.' Others felt a little more strongly, for example Paul du Bois-Reymond: 'It appears repugnant to common sense.' There were also some paradoxes whose resolution did not just require imaginative development of a new but consistent intuition. For example, Cantor showed that, given any infinite cardinal, there is a larger one. There are infinitely many different infinities. But now consider the cardinal of the set of all cardinals. This must be larger than any cardinal whatsoever, including itself! This problem was eventually resolved by restricting the concept of 'set', but I wouldn't say people are totally happy about that answer even today.

Mathematicians were divided on the importance of Cantor's ideas. Leopold Kronecker attacked them publicly and vociferously for a decade; at one point Cantor had a nervous breakdown. But Kronecker had a very restrictive philosophy of mathematics'God made the integers, all else is the work of Man'and he was no more likely to approve of Cantor's theories than the Republican Party is likely to turn the Mid-West over to collective farming. Poincaré said that later generations would regard them as 'a disease from which one has recovered'. Hermann Weyl opined that Cantor's infinity of infinities was 'a fog on a fog'. On the other hand Adolf Hurwitz and Hadamard discovered important applications of Set Theory to analysis, and talked about them at prestigious international conferences. Hilbert, the leading mathematician of his age, said in 1926: 'No one shall expel us from the paradise which Cantor created', and praised his ideas as 'the most astonishing product of mathematical thought'. As with other strikingly original ideas, only those who were prepared to make an effort to understand and *use* them in their own work came to appreciate them. The commentators on the sidelines, smugly negative, let their sense of self-importance override their imagination and taste. Today the fruits of Cantor's labours form the basis of the whole of mathematics.

The Continuum Hypothesis

After Cantor's insight, mathematicians rapidly came to realize that there are three fundamentally different types of domain in which they can operate: finite, countable, and uncountable. Styles of argument are available in the finite case that cannot be used anywhere else: for example the 'pigeonhole principle' that you can't put $n + 1$ objects into n boxes with at most one in each. Hilbert's hotel shows that this is untrue when $n = \aleph_0$, and the same goes for any infinite n. Countable sets again have their own special features; there are plenty of occasions where 1, 2, 3, is simpler than 1, 2, 3, , n. For example

$$1 + \frac{1}{4} + \frac{1}{9} + \ldots = \pi^2/6$$

exactly, whereas nobody has a formula for

$$1 + \frac{1}{4} + \frac{1}{9} + \ldots + 1/n^2.$$

The reason that 1, 2, 3, is simpler than 1, 2, 3, , n is plain: you don't have to worry about the n on the end! On the other hand, by counting 1, 2, 3, you eventually get to anything you want, so you can approach a countable set by way of a series of finite ones; whereas for the uncountable case nothing like this is possible. It's a very coarse but very important distinction; and you can tell a lot about a mathematician's likes and dislikes by asking him what size of set he feels happiest working with.

Cantor proved that the cardinal of the reals is larger than $\aleph_0$, but he left unresolved the question: is there anything in between? This problem became known as the *Continuum Hypothesis*. Every attempt to prove it failed dismally; but so did every attempt to construct a set with more elements than the integers but fewer than the reals. (Does this remind you of something? Read on.) Only in 1963 did Paul Cohen prove that the answer depends on your version of set theory. There are Cantorian set theories in which it is true; and non-Cantorian set theories in which it is false. It's Euclid's parallel axiom all over again. And we all ought to feel a lot less self-satisfied if we observe that it took modern mathematicians just as long to cotton on to the existence of non-Cantorian set theory as it did our predecessors to stumble across non-Euclidean geometry. 'The only lesson we learn from history is that we never learn from history.'

My Cardinal's Bigger Than Yours

Over the last twenty years or so, logicians have studied all sorts of alternatives to standard set theory, using the axiomatic approach that goes back to Euclid. They have discovered all sorts of straightforward-looking theorems whose truth or falsity depends on the precise choice of axioms. And they have managed to organize a lot of this material in terms of axioms for the existence of 'large cardinals'. For example an *inaccessible* cardinal is one not expressible in terms of a smaller number of smaller cardinals. Roughly, the type of cardinal possible in a set theory pretty much determines everything else it can do.

Mathematical logicians study the relative consistency of different axiomatic theories in terms of *consistency strength*. One theory has greater consistency strength than another if its consistency implies the consistency of the other (and, in particular, if it can model the other). The central problem in mathematical logic is to determine the consistency strength in this ordering of any given piece of mathematics. One of the weakest systems is ordinary arithmetic, as formalized axiomatically by Giuseppe Peano. But even weaker systems have been found useful. Analysis finds its place in a stronger theory, called second-order arithmetic. Still stronger theories arise when we axiomatize set theory itself. The standard version, *ZermeloFränkel Set Theory*, is still quite weak, although the gap between it and analysis is large, in the sense that many mathematical results require more than ordinary analysis, but less than the whole of ZermeloFränkel Set Theory, for their proofs. Beyond ZermeloFränkel, large cardinals hold sway. For example R. M. Solovay showed that the axiom 'there exists an inaccessible cardinal' implies that every set of reals is Lebesgue measurable (see Chapter 13); and subsequently Saharon Shelah proved the converse. There are other even larger types of cardinals: Mahlo, weakly compact, hyper-Mahlo, ineffable, measurable, Ramsey, supercompact, huge, n-huge. It's enough to make Kronecker turn in his graveinfinitely many times.

6
Ghosts of Departed Quantities

It will be sufficient if, when we speak of infinitely great (or, more strictly, unlimited) or of infinitely small quantities (i.e. the very least of those within our knowledge), it is understood that we mean quantities that are indefinitely great or indefinitely small, i.e., as great as you please, or as small as you please, so that the error that any one may assign may be less than a certain assigned quantity.
Gottfried Wilhelm Leibniz

In these profligate times it is hard to realize what a precious commodity paper used to be. Richard Westfall tells us that in 1612 the rector of North Witham, one Barnabas Smith, 'entered a grandly conceived set of theological headings' in a huge notebook, 'and under these headings a few pertinent passages culled from his reading. If these notes represent the sum total of his lifetime assault on his library, it is not surprising that he left no reputation for learning. Such an expanse of blank paper was not to be discarded in the seventeenth century.' I mention all this for one reason only: the treasure was seized by Smith's stepson, Isaac Newton, who called it his 'Waste Book', and it records the birth pangs of the calculus and mechanics.

In medieval times paper (rather, parchment) was even scarcer, and old manuscripts were habitually washed clean by monks seeking a surface on which to inscribe matters of high religious import. Imperfect erasure would leave faint traces of the original, known as a *palimpsest*. Where the medieval monks washed well, they destroyed works of enormous value; but more than one important document has been inadvertently saved for posterity by bad laundering. In

1906 the Danish scholar J. L. Heiberg heard of a mathematical palimpsest at Constantinople, tracked it down, and realized that it had originally contained works of Archimedes. These had been effaced in order to make room for a thirteenth-century Euchologiona collection of prayers and liturgies of the Eastern Orthodox Church. The text included *On the Sphere and Cylinder*, much of *On Spirals*, parts of the *Measurement of a Circle*, *Equilibrium of Planes*, and *Floating Bodies*. All of these works had been preserved in other manuscripts. But, sensationally, the palimpsest also contained the first copy ever found of a long-lost work, *The Method*. It was known from references elsewhere that Archimedes had produced a book with this title, but the contents were a mystery.

It has mathematical as well as historical significance, because *The Method* lets us in on how Archimedes thought of his ideas. Like most mathematicians, he first obtained his results by totally unrigorous methods, and then polished them up into a decent proof. And of course, only the polished version was ever displayed for public consumption. It's a habit that has deprived generations of researchers of the insights of their predecessors. Among the heuristic methods Archimedes used was a technique of slicing solids into infinitely many pieces of infinitesimal thickness, and hanging these on the arms of a notional balance, where their sum could be compared with some known object. He found the volume of a sphere this way. A great many problems are easier to solve rigorously if you know in advance what the answer is, so the method was of great value to Archimedes. Other mathematicians have made similar use of 'infinitesimal' arguments, and usually (but not always) apologized for their lack of rigour, waved their hands a bit, and asserted that 'of course it can all be made rigorous if you take a bit more trouble'.

Infinitesimals plagued seventeenth- and eighteenth-century analysis, although the philosophical arguments about them didn't prevent a lot of good work being done. By the nineteenth century the notion of an infinitesimal had been safely banished and replaced by that of a limit. It was generally agreed that infinitesimals were nonsense. Which only goes to show that you can't always trust a consensus view

Objections From Elea

As we have seen, the Pythagorean ideal of number as the basis for everything fell foul of the irrational. It also ran into philosophical trouble with the Eleatic school, the followers of Parmenides of Elea. The best known of these is Zeno, who lived around 450 BC. Zeno proposed four paradoxes about the nature of space and time. Two attack the idea that they are discrete; two that they are continuous. The story of the infinitesimal is much less straightforward than that of its cousin the infinite, and considerations such as Zeno's played a significant role. The 'dichotomy' paradox attacks infinite divisibility of space. Before an object can travel a given distance, it must first travel half as far. But before doing that, it must travel a quarter as far, and so on. Trying to do infinitely many things in the wrong order, it can never get started. 'Achilles and the tortoise' involves a similar scenario, with a speedy but late-starting Achilles unable to catch a more tardy tortoise because whenever Achilles reaches the place where the tortoise *was*, it has moved on. The 'arrow' attacks discreteness. An arrow in flight is in a fixed position at a fixed instantbut is then indistinguishable from a motionless arrow in the same position, so how do we know it's moving? The 'stade' is more involved, and attacks the idea that both space and time can only be subdivided a definite amount. Zeno's paradoxes are more subtle than they seem, and as assertions about physical space-time, rather than mathematical idealizations, still pose tricky questions. The Greeks found the paradoxes devastating, and this pushed them even further away from numbers and into the arms of geometry.

Venerable Beads

Can a line be thought of as some kind of sequence of points? Can a plane be sliced up into parallel lines, or a solid into planes? The modern view is 'yes', the verdict of history an overwhelming 'no'; the main reason being that the interpretation of the question has changed.

If a line can only be subdivided a definite amount, then points are

like little 'atoms of lineness', and follow each other like beads on a string. After each point there is a unique *next* point. So what real number corresponds to the point 'next to' the origin? Is it 0.001? No, because 0.0001 is closer. But 0.00001 is closer still, and 0.000001 closer than that, and one starts to feel sympathy for Achilles. What we want to do is write down 0.00000 and then put a 1 in the very last place, but there *is* no last place! There are two ways out of the dilemma. One is to assert that there really is a next number, but that it is *infinitesimally* larger than 0, which means it's smaller than anything of the form 0.00 01. The other is to accept that there is no 'next number' larger than 0. Then a line can be subdivided indefinitely, and there are no 'ultimate atoms' or *indivisibles*. Which means that you can't think of a line as being made up of points strung together in order. On the other hand it's obvious that any particular position on the line *is* a point: just draw another line crossing there, and use Euclid's axiom that 'two lines meet in a point'. It's a real bind, isn't it?

Our next observation about infinitesimals looks as if it has the game pretty well sewn up:

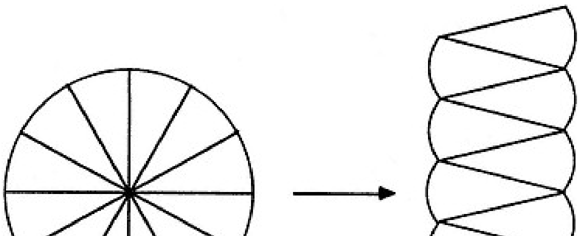
Infinitesimals Don't Exist

Suppose x is the smallest number greater than 0. Then $0 < x/2 < x$ so $x/2$ is also greater than zero, and smaller than x to boot. Contradiction.

However, we shouldn't be too easily satisfied, because another observation is:

Infinitesimals Are Useful

I've already mentioned Archimedes weighing infinitesimals to find the volume of a sphere. Nicholas of Cusa found a similar approach to the area of a circle, by slicing it up like a pie. Imagine a large number of very thin slices. Each is almost triangular, so its area is half the product of its height (the radius r of the circle) and the length of the base. The total length of the bases is near enough the circumference of the circle, which we know is $2\pi r$. So the area is



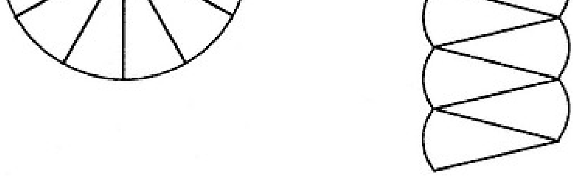


Fig. 4.
Dissecting a circle into a near-rectangle.

roughly 1/2 times r times $2\pi r$, that is, πr^2. If we could think of the circle as an infinite number of infinitesimally thin triangles, there would be no error at all. This is such a simple proof when compared to a Eudoxan style exhaustion that it's a pity it's nonsense. On the other hand, if it's nonsense, why does it give the right answer? And it's not the only example: for instance Democritus found the volume of a cone by piling up circular slices, an argument that attracted Archimedes' admiration.

From the time of Apollonius, about 250 BC, there was great interest in the 'problem of tangents': how to draw a tangent to a given curve. Apollonius solved it for conic sections, in particular the parabola, by purely geometric means. Newton and Leibniz, developing the calculus, attacked the problem using coordinate geometry. The equation of a parabola is $y = x^2$. The main problem is to find the *slope* of its tangent at x. Newton argued along the following lines. Let x increase slightly to $x + o$. Then x^2 changes to $(x + o)^2$. The *rate*

of change is therefore the ratio of the difference between the squares to the difference in the value of x, namely

$[(x + o)^2\ x^2]/[(x + o)\ x]$,

which simplifies to yield

$[2ox + o^2]/o = 2x + o$.

Let o approach zero; then the slope approaches $2x + 0 = 2x$. This is the slope of the tangent, or, as Newton put it, the *fluxion* of the *fluent* x^2. The answer agrees with Apollonius, too. Another coincidence? Newton's method, the wellspring of modern analysis, refreshes parts that Apollonius' cannot reach. It works just as well on $y = x^3$, giving a fluxion $3x^2$. Leibniz had a similar argument, but instead of o he used the symbol dx ('a little bit of x').

It may strike you that there's something fishy about all this. If so, you're in agreement with the arch-critic of calculus, Bishop George Berkeley. In 1734 he published *The Analyst, Or a Discourse Addressed to an Infidel Mathematician. Wherein It is examined whether the Object, Principles, and Inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than Religious Mysteries and Points of Faith. 'First cast out the beam in thine own Eye; and then shalt thou see clearly to cast out the mote out of thy brother's Eye.'* Berkeley objected that either o is not exactly zeroin which case the answers are wrong, albeit not by muchor it *is* zero, in which case you can't divide by it, and the calculation doesn't make sense. He suggested that the use of little o rather than big 0, while not necessarily exactly *cheating*, doesn't really improve matters.

Newton had defined a fluxion as the 'ultimate ratio of evanescent increments'. To this Berkeley reasonably responded: 'And what are these fluxions? The velocities of evanescent increments. And what are these same evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them ghosts of departed quantities?' Berkeley was objecting to the calculation of a fluxion as a *ratio* between two quantities ($2ox + o^2$ and o, in the example above) which both vanish. The ratio 0/0 makes

no sense. Since $1 \times 0 = 0$ we have $0/0 = 1$; but $2 \times 0 = 0$ so $0/0 = 2$. In fact you can similarly 'prove' that $0/0 = x$ for any x you please. So why does Newton's method seem to work? According to Berkeley, because of compensating errors.

The first thing to realize about Berkeley's objections is that he had the analysts dead to rights. It was a penetrating, informed criticism, and it revealed a horrible gap. He may have been grinding an axe about the difference (or lack thereof) between mathematical proof and religious belief, but he'd thought very hard about what he was criticizing. (Would that others might do the same.) Nobody had a *really* good answer to Berkeley. On the other hand it just won't wash to say that hundreds and hundreds of calculations, all confirmed in other ways, are due to 'compensating errors', without explaining why the devil the errors are perpetually conspiring to compensate! Indeed, if you can *prove* that the errors always compensate, that is tantamount to proving they aren't errors at all. Moreover, Newton and his followers knew that what they were doing made sense, because they had a slightly more subtle picture (albeit physical) of what was happening. Berkeley insisted on thinking like an algebraist, with o being some definite constant. Then o must be both zero and non-zero, an absurdity. But Newton thought of o as a *variable*, which can be made to *approach* 0 as closely as we please, without actually vanishing. Newton thought of the whole *process* whereby o shrinks to zero; Berkeley wanted Newton to fix its value at the instant before it disappeared forever, which he couldn't do. Newton knew that Achilles will catch the tortoise eventually because he is running faster; Berkeley was trying to make Newton isolate the instant immediately before the tortoise is overtaken, it being clear that there is no such thing. Berkeley won the logical argument hands down. Nobody took the slightest notice, because the method worked beautifully, logic or no.

Near Misses

Colin MacLaurin, in his *Treatise on Fluxions*, tried to prove the correctness of calculus by reinterpreting its methods within classical

geometry. He became such a dab hand at this that he persuaded several people to abandon calculus altogether in favour of geometry, which wasn't his intent. Leibniz sought refuge in the infinitesimal. 'Now these dx and dy are taken to be infinitely small . It is useful to consider quantities infinitely small such that when their ratio is sought, they may not be considered zero but which are rejected as often as they occur with quantities incomparably greater.' He also thought of an infinitesimal as a variable quantity; and he summed it all up in a *Law of Continuity*: 'In any supposed transition, ending in any terminus, it is permissible to institute a general reasoning, in which the final terminus may also be included.' In other words, what works for all non-zero os also works for $o = 0$. He supported this principle by examples, not proof. It needs caution in use: try it on the 'general reasoning' $o > 0$. John Bernoulli wrote part II of a calculus text in 1692, eventually published in 1742. Publication delays in mathematics are not unknown, but ones of this magnitude are unusual. Perhaps Bernoulli was trying to set a record: the first part, written in 1691, wasn't published till 1924. Seeking to clarify the mystery, he explained that 'a quantity which is diminished or increased by an infinitely small quantity is neither increased nor decreased'. Is it only the modern eye that finds this enigmatic? He also tried to define the infinitesimal as $1/\infty$.

Newton's ideas of 1704 got closer than Bernoulli's. 'In mathematics the minutest errors are not to be neglected. I consider mathematical quantities not as consisting of very small parts, but as described by a continual motion. Fluxions are, as near as we please, as the increments of fluents generated at times, equal and as small as possible.' He explicitly rejected the infinitesimal justification as being incoherent. He got very close indeed to the modern interpretation when, in the first and third editions of the *Principia*, he said: 'Ultimate ratios in which quantities vanish are not, strictly speaking, ratios of ultimate quantities, but limits to which the ratios of these quantities, decreasing without limit, approach, and which, though they can come nearer than any given difference whatever, they can neither pass over nor attain before the quantities have diminished indefinitely.' In other words, to find the limit which the ratio $2ox + o^2 : o$ approaches as

o diminishes towards 0, you do *not* set $o = 0$ to get the ratio $0 : 0$. Instead you keep o non-zero, simplify to get $2x + o : 1$, and then observe that as o *approaches* 0, this ratio *approaches* $2x : 1$. Newton knew exactly what he was doing; he just couldn't find a precise way to express it. From about this time a series of mathematicians, including John Wallis and James Gregory, missed the target by a hair's breadth.

The Plot thickens

Before calculus had been properly sorted out, a new complication arose: complex numbers. As we shall see in Chapter 11, the extension of analysis to complex-valued functions was itself a source of great (but creative) confusion. The foundational problems of calculus were soon to be settled, though. In the bible of complex analysis, his *Cours d'Analyse* of 1821, Augustin-Louis Cauchy based the theory on the idea of a *limit*, and defined it thus: 'When the successive values attributed to a variable approach indefinitely a fixed value so as to end by differing from it by as little as one wishes, this last is called the limit of all others.' As regards the infinitesimal, he was quite explicit: 'One says that a variable quantity becomes infinitely small when its numerical value decreases indefinitely in such a way as to converge to the limit 0.' Infinitesimals are *variables*, not constants. Similarly ∞ is not a constant, but a variable that becomes indefinitely large. What we have here is the Aristotelian notion of *potential* infinity, transmuted into potential infinitesimality. It is perhaps churlish to remark that nobody had defined what they meant by a variable. The omission was not serious, and went unnoticed.

There is a science-fiction story by Larry Niven, *Convergent Series*, in which a bright red, horned demon is raised using the traditional method, a pentacle, in which it appears spreadeagled. The protagonist is granted one wish, but valid for a mere twenty-four hours because of the usual small print in the contract. After that the demon reports to Hell, instantly reappears inside the pentacle, and exacts the time-honoured penalty. The victim does have the option of erasing the pentacle and redrawing it wherever he pleases. Niven's

hero scrawls his pentacle on the demon's bulging belly. When the demon reappears it finds itself shrinking rapidly, and before it can work out what's happening Are we left with an infinitesimal demon? No, says Cauchy: we are left with no demon at all. It is the *way it vanishes* that is infinitesimal.

Epsilontics

Finallyand frankly it's a relief to see itKarl Weierstrass sorted out the muddle in 1850 or thereabouts by taking the phrase 'as near as we please' seriously. How near *do* we please? He treated a variable, not as a quantity actively changing, but simply as a static symbol for any member of a *set* of possible values. A function (x) approaches a limit L as x approaches a value a if, given any positive number ε, the difference $(x)\ L$ is less than ε whenever $x\ a$ is less than some number δ *depending on* ε. It's like a game: 'You tell me how close you want (x) to be to L; then I'll tell you how close x has to be to a.' Player Epsilon says how near *he* pleases; then Delta is free to seek his own pleasure. If Delta always has a winning strategy, then (x) tends to the limit L. This epsilondelta definition of limit is perhaps a little cumbersome, but, like the Greek method of exhaustion, a competent professional soon gets accustomed to it, and can wield it with precision and occasionally dazzling virtuosity.

Notice how the physical ideas of motion have been replaced by a set of static events, one for each choice of ε. It is not necessary to think of a variable ε flowing towards 0; all we have to do is entertain all possible values (greater than 0) for ε, and deal successfully with each. The introduction of potential versus actual infinitesimality is a red herring; the whole problem can be formulated in purely finite terms. Weierstrass's definition of limit freed calculus from metaphysical considerations and modern analysis was born. Today every undergraduate beginning an analysis course is propelled through a week or two of what is colloquially called 'epsilontics'usually to his or her initial perplexity. It's the way the trade bloods its apprentices. If you want to *understand* analysis, and not merely calculate with calculus, this is the approved path.

Reeb's Dream

For all that, there's something intuitive and appealing about the old-style arguments with infinitesimals. They're still embedded in our language, in the way we think: 'instants' or 'moments' of time; 'instantaneous' velocities; the idea of a curve as a series of infinitely small straight lines. Engineers, chemists, and physicists happily introduce small increments δx, which miraculously transmogrify into $\mathrm{d}x$ at judicious points in the calculation, without worrying about full-blooded epsilontics. 'The mathematicians have justified this kind of thing anyway, let them beat their tiny brains out on it if they want to, I'm after bigger game.'

Georges Reeb puts it eloquently:

> The dream of an infinitesimal calculus worthy of the name, that is to say in which dx and dy are infinitesimal numbers. $\int_b^a f(x)\,\mathrm{d}x$ is a genuine sum of such numbers, and limits are attained, has always been dreamed by mathematicians; and such a dream deserves perhaps an epistemological enquiry. Some other dreams, lesser maybe if compared with the achievements of calculus, have haunted the mathematician's world and wishful thought: it is the idea of a world where integers can be classified as 'large', 'small', or even 'indeterminate' without loss of consistent reasoning, satisfy the induction principle, and where the successors of small integers would remain small; a world where concrete collections, fuzzy perhaps but anyhow not finite, could be gathered in a single finite set; a world where continuous functions would be approximated almost perfectly by polynomials of a fixed degree. In such a world, the finite realms could be explored either through the telescope or through the magnifying glass in order to gather entirely new

> pictures. Within such a world, the criteria of rigour set forth by Weierstrass, interpreted in a two-fold sense, would allow for phantasy and metaphor.

A footnote adds an example: 'There should be a finite chain linking some monkey to Darwin, respecting the rules: a monkey's son is a monkey, the father of a man is a man.'

Non-Standard Analysis

Between about 1920 and 1950 there was a great explosion of mathematical logic; and one of the topics that emerged was Model Theory. In model theory one contemplates some system of axioms,

and attempts to find and characterize mathematical structures that obey all the axioms of the system*models* of the axioms. Thus the coordinate plane is a model for the axioms of Euclidean geometry, Poincaré's universe is a model for the axioms of hyperbolic geometry, and so on.

The logicians found an important distinction between what they called first-order and second-order axiomatic systems. In a first-order theory the axioms can express properties required of all objects in the system, but not of all *sets* of objects. In a second-order theory there is no such restriction. In ordinary arithmetic, a statement such as

(1) $x + y = y + x$ for all x and y

is first order, and so are all the usual laws of algebra; but

(2) if $x < 1/n$ for all integers n then $x = 0$

is second order. The usual list of axioms for the real numbers is second order; and it has long been known that it has a *unique* model, the usual real numbers R. This is satisfyingly tidy. However, it turns out that if the axioms are weakened, to comprise only the first-order properties of R, then other models exist, including some that violate (2) above. Let R* be such a model. The upshot is a theory of *nonstandard analysis,* initiated by Abraham Robinson in about 1961. In non-standard analysis there are actual infinities, actual infinitesimals. They are constants, not Cauchy-style variables. And Reeb's Dreamcomes true! The violation of (2), for example, means precisely that x is a 'genuine' infinitesimal. And if ω is an infinite integer, then the sequence

1, 2, 3, , ω 3, ω 2, ω 1, ω

connects the monkeys 1, 2, 3, to the Darwins , ω 3, ω 2, ω 1, ω.

Infinitesimals Do Exist

It's not even a new game. How many times before has the system of 'numbers' been extended to acquire a desirable property? From

rationals to reals to allow $\sqrt{2}$; from reals to complexes to allow $\sqrt{-1}$. So why not from reals to hyperreals to allow infinitesimals? In non-standard analysis there are ordinary natural numbers N = {0, 1, 2, 3, } all right; but there is a larger system of 'unnatural', better 'non-standard natural' numbers N*. There are the ordinary integers Z, and the non-standard integers Z*. And there are the standard reals R, plus non-standard reals R*. And each pair is indistinguishable as far as first-order properties go, so you can prove first-order properties of R by working with R* if you want; but R* has all sorts of new goodies, like infinitesimals and infinities, which you can exploit in new ways.

First, a few definitions to give the flavour. A hyperreal number is *finite* if it is smaller than some standard real. It is *infinitesimal* if it is smaller than all positive standard reals. Anything not finite is *infinite*, and anything not in R is *non-standard*. If x is infinitesimal then $1/x$ is infinite, and vice versa. Now for the clever bit. Every finite hyperreal x has a unique *standard part* std(x) which is infinitely close to x, that is, x std(x) is infinitesimal. Each finite hyperreal has a unique expression as 'standard real plus infinitesimal'. It's as if each standard real is surrounded by a fuzzy cloud of infinitely close hyperreals, its *halo*. And each such halo surrounds a single real, its *shadow*.

Let's take a look at Newton's calculation of the fluxion (derivative) of the function $y = (x) = x^2$. What he does is take a small number o, and form the ratio $[(x + o)$ $(x)]/o$. This boils down to $2x + o$, which would be great if the o could be got rid of. Hence ultimate ratios, limits, and epsilontics, as we saw. But there's a simpler way. Take o to be infinitesimal, and instead of $2x + o$ take its *standard part*, which is $2x$. In other words, the derivative of (x) is defined to be

std{[$(x + o$ $(x)]/o$}

where x is a standard real and o is any infinitesimal. The Alexandrine sword that slices the Gordian Knot is the innocent-looking idea of the standard part. It's perfectly rigorous, because std(x) is uniquely defined. And it doesn't just *forget* about the extra o; it really does

get rid of it altogether. What's going on here? Basically, there are orders of magnitude for hyperreals, and you must work to the right order. Standard reals are order 0. Infinitesimals like o are mostly order 1, but o^2 is order 2, o^3 order 3, and so on. Meanwhile infinities such as $1/o$ are order 1, $1/o^2$ is order 2. The standard part picks out the lowest-order term in any finite hyperreal.

Non-standard analysis does not, in principle, lead to conclusions about R that differ in any way from standard analysis. It is the method, and the setting in which it operates, that are non-standard; but the results are true theorems of good old-fashioned analysis. So what's the point of all this rigmarole, if it can't prove anything new? Before discussing this important question, let's get some kind of feel for how non-standard analysis behaves.

Welcome to the Madhouse

A course in non-standard analysis looks like an extended parade of exactly those errors that we spend all our time teaching students to avoid. For example:

(1) A sequence s_n is bounded if s_ω is finite for all infinite ω.

(2) A sequence s_n converges to a limit L if s_ω L is infinitesimal for all infinite ω.

(3) A function is continuous at x if $(x + o)$ is infinitely close to (x) for all infinitesimal o.

(4) The function has derivative d at x if and only if $[(x + o)\ (x)]/o$ is infinitely close to d for all infinitesimal o.

(5) The Riemann integral of is an infinite sum of infinitesimal areas.

However, within the formal framework that non-standard analysis provides, these statements are not erroneous: they are rigorous truths!

Why Bother?

Back to my question: what's the point of all this rigmarole, if it can't prove anything new?

The question is an oversimplification. The correct statement is that any theorem proved by non-standard methods is a true theorem of standard analysis (and therefore must have a standard proof). But that doesn't tell us how to find the standard proof, nor does it give any idea whether the non-standard proof will be shorter or longer, more natural or more contrived, easier to understand or harder. As Newton showed in his *Principia*, anything that can be proved with calculus can also be proved by classical geometry. In no way does this imply that calculus is worthless. In the same way, the correct question is whether non-standard analysis is a more powerful piece of machinery. That's not a matter for mathematical argument: it has to be resolved by experience. Experience suggests quite strongly that proofs via non-standard analysis tend to be shorter and more direct. They largely avoid the use of complicated estimates of the sizes of things. Instead of worrying whether o^2 is less than $10^{-93.7}$ or whatever, we merely observe that it's a second-order infinitesimal, and so on. Such estimates form the bulk of the proofs of classical analysis. The main difficulty is that non-standard analysis requires a very different background from classical analysis, so a lot of initial effort must be put in before you come to the pay-off.

The Canard Unchained

New phenomena have been discovered by non-standard analysis, including some of importance in applications. One vast area of applied mathematics, perturbation theory, deals with 'very small' changes to equations. Why not make the perturbation infinitesimal, and see what you get? A few brave souls have embarked on this course in recent years. One example must suffice. There is a certain differential equation, which I won't write down, that involves a small parameter ε and an additional parameter k. It is known that for $k < 1$ there is a unique oscillatory solution, whereas for $k \geq 1$ no oscillations exist. How does the oscillation 'switch on', as k passes through 1? There is a mathematical gadget called a 'Hopf bifurcation' that acts as a prototype for the onset of an oscillation. The

oscillation just grows smoothly from a small one to a larger one. It's easy to calculate that a Hopf bifurcation occurs in the equation at $k = 1$. What more need be said?

Apparently, a great deal. As k decreases through 1 a small oscillation does indeed switch on by a Hopf bifurcation. But almost immediately, it explodes *very* rapidly into a big one. Because some of the intermediate pictures involve curves shaped like ducks, the French discoverers of this effect called it a *canard*. You need an accurate computer to bag a duck: for example when ε = 0.01 it is necessary to look at the sixtieth decimal digit of k. The first duck was found by Eric Benoit, Jean-Louis Callot, and Francine and Marc Diener. And they used no computer, but non-standard analysis, with an infinitesimal perturbation ε. The duck promptly surrendered of its own accord, overwhelmed by the power of the method.

The Infinitesimal Computer

One of the big problems in computer graphics is that the plane is continuous but the computer screen is discrete: a rectangular array of pixels, tiny squares that are coloured black or white (or red or green or other exotic colours) to represent shapes. Traditional geometric objects, such as circles and lines, live in the continuous plane; and so does the mathematical theory that describes them. The discrete lattice structure of the computer screen causes all sorts of difficulties: traditional continuous geometry is the wrong tool, but what else is there?

It is, for example, surprisingly hard to decide whether or not a given collection of black pixels represents a straight line. On a discrete lattice, two straight lines can cross without having any pixels in common; orif nearly parallelthey can meet in what appears to be a long segment. And while translating objects is relatively easy (at least through distances represented on the lattice), rotating them is somewhat fraught. If you rotate a square pixel through 45° it ends up diamond-shaped and overlaps *several* pixels. It's hard to handle points that start to multiply when you rotate them.

The French mathematician Jean-Pierre Reveillès has enlisted the aid of non-standard analysis to overcome this difficulty. The basic idea is to model the discrete but finite computer screen as a discrete but *in*finite lattice of infinitesimal pixels. From the philosophical viewpoint of non-standard analysis this is a finite object. It requires just one infinite integer, call it ω, and it provides an accurate model of the continuous plane of standard analysis. Computer programs for doing things in plane geometry can be rewritten using expressions in ω instead of real numbers, leading to a 'computer language' that is quite simple to use.

All very well, but of course a real computer doesn't have an infinite screen with infinitesimal pixels. This is where the clever bit comes in. Take the computer program, approximate the infinite integer ω by a moderately large finite one, say 10,000. The result is a program that runs on a real computer, using ordinary arithmetic, and which computes the desired geometric object to a high degree of accuracy. If you want more accuracy, just replace 10,000 by 1,000,000 or something larger; if you want a quick-and-dirty computation, reduce it to 1,000 or 100.

This approach is a remarkably 'clean' way to formalize discrete graphics. It handles difficulties such as those mentioned above with ease; it deals with rotations on the same basis as translations. As a consequence of its clean structure, programs written in this manner tend to be unusually efficient, often running in a fraction of the time taken by more conventional programs. Not only that: the method opens up new lines of attack in 'standard' combinatorics, and answers long-standing questionsfor instance, providing formulas for the number of lattice points inside a given triangle.

It's unlikely that an approach of this kind would ever have emerged from conventional computer science: it transports the problem into highly abstract regions, returning to the concrete world only at the very end of the analysis. Remarkably, it provides not only a practical method for making discrete graphics work, but arguably one of the best available. Non-standard analysis comes into its own when it does *not* try to mimic standard analysis, but is given its head and allowed to follow its own individual track.

Logic for Engineers?

Indeed there are reasons to expect that it is in applied mathematics that the most useful products of non-standard analysis might emerge. First, because applied mathematicians, engineers, and scientists already think in terms of infinitesimals like dx. Second, because so much attention is paid to small perturbations. And too much of perturbation theory is hopelessly unrigorous and disorganized, and reeks of rule-of-thumb. There's a good chance that non-standard analysis could bring some much-needed order into this area, and add some power to its elbow. The non-standard analysts have explored some of the possibilities, including polynomial equations, fastslow flows, geodesics, boundary value problems, boundary layer flow, stochastic differential equations, and spin systems in mathematical physics.

In scientific research, one might expect new ideas from elsewhere, new techniques, new phenomena, to be seized upon and exploited. Actually this only happens if they don't have to cross traditional boundaries between subjects. The 'Not Invented Here' syndrome is very strong, and some perturbation theorists appear to be suffering from it. For example, the reaction of one of them to the discovery of canards was to find a (rather complicated and delicate) proof from his own point of view, while expressing doubts (unjustified) as to the rigour of the non-standard approach. The pot calling the kettle black, in spades! It's easy to discover things with hindsight: I'd be more impressed by the perturbation theory proof if it had been found *before* the non-standard analysts had done the donkey-work.

But the reaction is symptomatic of a genuine problem, by no means confined to applied mathematics. The people most knowledgeable about the area of application are not, by training, equipped to handle a technique that developed out of mathematical logicespecially one which requires a distinctly different cast of mind, a new style of thinking that takes several years to get used to. They aren't impressed by the way it makes sloppy thinking rigorous, becausedare I say itthey'd rather stay sloppy. And so it will be the non-standard analysts who have to push ahead with the applied

development, until they finally come up with something *so* dramatic that even the least imaginative perturbation-theorist will begin to feel that maybe he's missing out on something. Of course, it may never happenbut I rather suspect it will, for the times they are a-changing. However, whether the engineers of the twenty-first century will have to study the metamathematics of model theory remains open to doubt.

7
The Duellist and the Monster

At the end of a session at the Engineering College, Cooper's Hill, a reception was held and the science departments were on view. A young lady, entering the physical laboratory and seeing an inverted image of herself in a large concave mirror, naively remarked to her companion: 'They have hung that looking glass upside down.'
Clement V. Durell

'Pistols at twenty-five paces!' On 30 May 1832 a young French revolutionary, duelling with a comrade over the honour of a lady, was hit in the stomach with a pistol-ball and died of peritonitis a day later. He was buried in the common ditch at the cemetery of Montparnasse. Just before the duel he wrote to his friends Napoleon Lebon and V. Delauney: 'I die the victim of an infamous coquette. It is in a miserable brawl that my life is extinguished. Oh! Why die for so trivial a thing, for something so despicable!' The circumstances are mysterious; indeed until recently the lady was known only as 'Stéphanie D.' until Carlos Infantozzi did some detective work and revealed her as Stéphanie-Felicie Poterin du Motel, the entirely respectable daughter of a physician.

The protagonist in the melodrama wrote another letter that night, to Auguste Chevalier, saying, 'I have made some new discoveries in analysis.' And he ended, 'Ask Jacobi or Gauss publicly to give their opinion, not as to the truth, but as to the importance of these theorems. Later there will be, I hope, some people who will find it to their advantage to decipher all this mess.' The mess was, according to Tony Rothman, 'instrumental in creating a branch of mathematics that now provides insights into such diverse areas as arithmetic,

crystallography, particle physics, and the attainable positions of Rubik's cube'. The tragic creator was Évariste Galois; the branch of mathematics was Group Theory.

What Shape Is an Equation?

Galois was working on a long-standing problem. As we saw in Chapter 1, equations of degrees 1, 2, 3, and 4 can be solved by an exact formula, an algebraic expression involving nothing worse then nth roots, or *radicals*. But the equation of degree 5, the *quintic*, resisted all efforts to find such a formula. By the 1800s attention had switched, not to finding a solution, but to proving that no solution by radicals exists. Paolo Ruffini made an abortive attempt in 1813, and the question was finally settled in 1824 by Abel. Abel's proof involved a complicated analysis of the degrees of auxiliary equations that might arise during the solution process, assuming there was one. Galois was more ambitious. Even though the general equation of degree 5 is insoluble, various special equations of equal or higher degree do possess solutions by radicals. Galois wanted a method to decide, for *any* given equation, whether or not it can be solved by radicals. And his answer was striking and original. Everything depends on the *symmetries* of the equation.

What is Symmetry?

'Symmetry', said Hermann Weyl in a famous series of lectures on the topic, 'is one idea by which man through the ages has tried to comprehend and create order, beauty, and perfection.' When mathematicians speak of symmetry they have something very specific in mind: a way of transforming an object so that it retains its structure.

For example, consider a square, and draw its outline around it. If you now move the square, it doesn't usually fit back into the outline. But for exactly eight different motions, it does. These motions are:

(1) Leave it alone.

(2) Rotate 90° (anticlockwise).

(3) Rotate 180°.

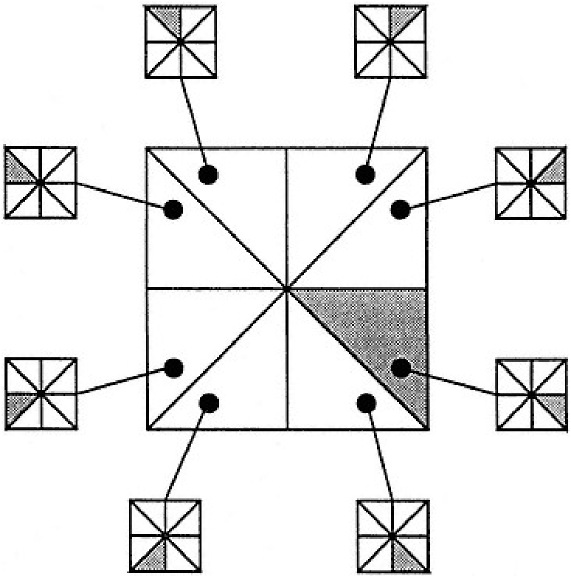


Fig. 5.
The eight symmetries of a square. The small squares show where the shaded triangle moves under each of the eight symmetry operations.

(4) Rotate 270°.

(5) Flip it over about a vertical axis (like a revolving door).

(6) Flip it over about a horizontal axis (like a cat-flap).

(7) Flip it about one diagonal.

(8) Flip it about the other diagonal.

Each of these operations is a *symmetry* of the square. If two symmetries are applied in turn, then the square obviously still ends up inside its outline; so the result is another symmetry. For example, 'rotate 90°' followed by 'rotate 180°' produces the same effect as

'rotate 270°'. We say that the set of symmetries has the *group property*, meaning that the 'product' of any two symmetries is a symmetry. The set of symmetries is called the *symmetry group* of the square. This particular group contains eight symmetry operations: we say it has *order* eight.

Consider now an infinite floor, covered with square tiles in the usual fashion. This tiling pattern has all of the eight symmetries of the square; but in addition it has infinitely many *translational* symmetries. The whole pattern can be displaced 1, 2, 3, units horizontally or vertically, up or down, left or right, as well as being rotated or flipped. So there are infinitely many symmetries, and the symmetry group has infinite order.

Any mathematical object, however abstract, can possess symmetries. These are now defined as transformations of the object that preserve its 'shape', that is, its basic structure. The symmetries again form a group; that is, if two are performed in turn the result is yet another symmetry. The way to study symmetry as a general concept is to look at the algebraic properties of this process of composition of transformations.

The Shape of an Equation

All this is much clearer now than it was in Galois's day; but I'll take advantage of the modern viewpoint to describe his ideas. So what shape is an equation? The mathematical object involved is the full set of its solutions. (An equation of the *n*th degree always has *n* real or complex solutions.) The transformations are the different ways to rearrange these among themselves, that is, their *permutations*. And the shapethe structure to be preservedis the system of algebraic relations that hold between them. The idea is most easily grasped using an equation whose solutions are easy to find:

$x^4 5x^2 + 6 = 0$.

The roots are $\sqrt{2}, -\sqrt{2}, \sqrt{3},$ and $-\sqrt{3}$: call them α, β, γ, δ. Clearly α and β form a 'matching pair', as do γ and δ. In fact there is no way to distinguish α and β from each other, just by using equations

with rational coefficients. Thus α satisfies $\alpha^2 2 = 0$ but so does β. And α + β = 0; but interchanging α and β gives β + α = 0, also true. Similarly γ and δ are algebraically indistinguishable (by equations over the rationals). On the other hand it is easy to tell α from γ, because $\alpha^2 2 = 0$ is true, but $\gamma^2 2 = 0$ is false. I don't mean that there is no difference at all between $\sqrt{2}$ and $-\sqrt{2}$: obviously they have different signs. But this difference of sign can't be detected using only polynomial equations with rational coefficients. If we write a list of the four roots, there are exactly four ways to rearrange them so that all true equations in the original order remain true in the new order. They are:

αβγδ | αβδγ | βαγδ | βαδγ.

This list of permutations is the *Galois group* of the equation. And you can tell, just by looking at the Galois group, whether the equation is soluble by radicals! It stares you in the face, doesn't it? No? Well, it does, if you are a budding Galois: the recipe *is* a little complicated to state. I'll do my best: the main point to grasp is that there is a definite computable criterion in terms of the internal algebraic structure of the Galois group.

Suppose the group is *G*. It may contain within it a smaller system of permutations *H*, which also has the group property. If so, call *H* a *subgroup*. A further technical condition on *H*, called *normality*, also comes into play: let's just agree that this means that *H* sits inside *G* in a particularly nice way. Now Galois proves that an equation is soluble by radicals precisely when:

(1) Its group *G* contains a normal subgroup *H* of smaller order;

(2) The order of *G* divided by that of *H* is prime;

(3) *H* contains a normal subgroup *K* of smaller order;

(4) The order of *H* divided by that of *K* is prime;

(5) And so on until we reach a subgroup containing only one element of *G*.

Such a group is said to be *soluble*. (The idea is that an *n*th root can be obtained from a series of *p*th roots, for various primes *p*. For example the sixth root is the square root of the cube root, since

6 = 2 × 3; and 2 and 3 are prime. Each *p*th root corresponds to one step in the chain of subgroups *G, H, K,*)

The Quintic

Obviously you can only use Galois's result if you have a well-developed method for analysing groups, subgroups, normal subgroups, orders of subgroups, and so on. Such methods now exist, inspired in part by Galois's work. One by-product is a satisfying explanation of what is so special about the quintic.

The *general n*th degree equation has no special relations between its roots, so its Galois group is the group of *all* permutations of its *n* roots. This is a very specific group and it's not too hard to find out a lot about it. In particular it can be shown that when *n* = 2, 3, or 4 it is a soluble group, so the equations of those degrees are soluble. In fact, the *methods* used to solve them can be interpreted and organized using group theory. However, when *n* = 5 the group *G* of all permutations is *not* soluble. *G* has order 120, and there is a normal subgroup *H* of order 60. The ratio 120/60 = 2 is prime; so you can get started all right. But, as Galois proved, the only normal subgroups of *H* have orders 60 and 1. The only possibility for *K* is the group of order 1; but then the ratio of orders is 60/1 = 60, which isn't prime. The group isn't soluble, and neither is the equation.

Genius Is Its Own Reward?

Galois worked out the basics of this by the time he was seventeen, and submitted some of his results to the Paris Academy of Sciences. The paper was rejected, the manuscript lost. He submitted his results twice more, and on each occasion the paper was lost. Galois blamed the politically oppressive Bourbon regime, and a society that condemned genius in favour of mediocrity. Actually it may well have been his own fault: he tended not to explain his ideas very clearly, and novelty *plus* obscurity is suicidal, especially from a complete unknown. At any rate, Galois took up revolutionary politics, was expelled from the École Normale, arrested, acquitted, arrested again,

and jailed for six months in Sainte-Pélagie. A cholera epidemic had him transferred to a hospital, and he was put on parole. Along with his freedom he experienced his first and only love affair, with the all too literal *femme fatale*, Stéphanie. His work might have been lost along with his life, were it not for Liouville, who later began to look through the papers that Galois had left after his death. On 4 July 1843 Liouville addressed the Academy: 'I hope to interest the Academy in announcing that among the papers of Évariste Galois I have found a solution, as precise as it is profound, of this beautiful problem: whether or not it is possible to solve by radicals '

Groups Galore

Galois's work was one of the clues that led to modern Group Theory. Several authors, notably Cauchy and Joseph-Louis Lagrange, worked on groups of permutations (or 'substitutions'). Camille Jordan systematized much of this in his 1870 *Theory of Substitutions and Algebraic Equations*. Meanwhile other objects, similar to permutation groups but not consisting of permutations, began to make their appearance. For example in 1849 Auguste Bravais used symmetries in three-dimensional space to classify the structure of crystals, leading Jordan to consider groups whose elements are not permutations, but linear transformations.

Another major source of inspiration was the theory of *continuous groups* of Sophus Lie. A square has only eight different symmetries, four rotations and four reflections, so its group of symmetries is *finite*, of order 8. But some objects, such as the circle and sphere, have a continuous range of symmetries. A circle can be turned continuously through any angle; so can a sphere, about any axis. So the group of symmetries of the circle or sphere contains a continuum of elements. A rough measure of the complexity of the group is its *dimension*: how many parameters are required to specify its elements. The symmetry group of the circle is 1-dimensional (specify one angle of rotation). That of the sphere is 3-dimensional (select an axis to rotate about; then use the longitude and latitude of the 'pole' of this axis, plus the angle of rotation). Methods from analysis, such

as differentiation and integration, can be applied to such groups. Lie developed a far-reaching theory which has become one of the central areas of modern mathematics: Lie groups.

Complex function theory produced yet another type of group, whose elements are *birational* transformations

$(z) = (az + b)/(cz + d)$

of the complex numbers (see Chapter 11). These are important in the theory of elliptic functions, which curiously also interested Abel and Galois. Number theory yielded its own store of 'groupish' objects: the integers modulo *n* under the operation of addition, the non-zero integers modulo a prime *p* under multiplication, Gauss's classes of quadratic forms

The Erlangen Programme

Another stimulus to the development of group theory was Felix Klein's programme to unify geometry, on which he lectured at Erlangen in 1872. At that time, geometry had split into a horde of related but distinct disciplines: Euclidean and non-Euclidean geometry, inversive and conformal geometry, projective and affine geometry, differential geometry, and the newly emergent topology. There were even geometries with only finitely many points and lines. Klein sought some semblance of order, and found it in the idea of geometry as the invariants of a group of transformations.

For example, consider Euclidean geometry. The basic notion is that of congruent triangles; and two triangles are congruent if they have the same shape and size. In other words, if one can be transformed into the other by a rigid motion of the plane. Now, said Klein: the rigid motions form a group. So we have a group of transformations of the plane; and the properties studied in Euclidean geometry are those, such as lengths and angles, that do not change under the action of this group. Similarly in hyperbolic geometry the group consists of rigid hyperbolic motions; in projective geometry it is the projective transformations; and in topology it is topological transformations. The distinction between geometries is at

base group-theoretic. Not only that, said Klein: sometimes you can use the groups to pass between geometries. If two apparently distinct geometries have basically the same group, albeit in disguise, then they are really the same geometry. He cited many examples. For instance, the geometry of the *complex* projective line is basically the same as the real inversive plane, and this in turn is the same as the real hyperbolic plane.

At a stroke, Klein's insight brought clarity and order to what had previously been a confused, if exciting, mess. (One major exception should be noted. Riemann's geometry of manifolds deprived Klein of a clean sweep.) It became possible to compare one geometry with another, to say that one was stronger or weaker than another, to use results from one type to prove theorems in another. Classical geometry is perhaps no longer a central area of mathematical *research*, largely because it was investigated so successfully in the nineteenth century; but it remains (as we shall see) central to *mathematics*. And Klein's Erlangen Programme still has an enormous influence. It is a measure of its success that its influence is not always explicitly perceived: the viewpoint has passed into common currency.

Abstract Groups

When mathematicians find themselves proving the same theorem fifteen times in fifteen different contexts, they usually decide that a bit of tidying-up is in order. In this case the tidying was begun by Arthur Cayley, who proposed a more general concept: a set of operators such that, if any two are performed in turn, the result is still in the set. This he called an *abstract group*. But Cayley still wanted his operators to operate *on* something. The final step, made explicit in 1902 by Edward Huntington but probably part of the general mathematical 'folklore', was to define a group axiomatically. This is the modern approach. A *group* is a set *G*, on which is defined a law of composition, or *product*. If *g* and *h* belong to *G*, the product is denoted *gh*. (The notation is not intended to imply that *gh* is *g* multiplied by *h* in the usual sense.) This product is required to satisfy four properties:

(1) *G* is *closed*: if *g* and *h* are in *G*, then so is *gh*.

(2) There is an *identity* 1 in *G* such that $1g = g = g1$ for all *g*.

(3) Every *g* in *G* has an *inverse* g^{-1} such that $gg^{-1} = 1 = g^{-1}g$.

(4) The *associative law* $g(hk) = (gh)k$ holds.

The permutation groups of Galois, the transformation groups of Bravais and Jordan, Lie's continuous groups, and Cayley's abstract groups, all satisfy these conditions. Condition (1) is Galois's 'group property'; the others express useful basic properties that automatically hold for transformations, but might not for more abstract objects. For example if *G* is the set of integers, and the 'product' *gh* is the sum $g + h$, then all four laws hold. The same goes if *G* is the integers modulo *n*. At a stroke a whole morass of special results for different realizations of the group concept was reduced to a few simple general propositions. And mathematics acquired a powerful language to describe and exploit ideas of symmetry.

Atoms of Symmetry

The analysis of the quintic equation leads to a group *H* whose only normal subgroups are trivial (order 1) or the whole of *H* (here order 60). Such a group is said to be *simple*. If you think of a group as a molecule, then the simple groups are its atoms. The key to understanding all groups is to understand the simple groups. Unfortunately, 'simple' here doesn't imply 'easy'.

The first major accomplishment in this area was the classification of simple Lie groups. Lie discovered four 'families' of simple Lie groups, defined in terms of linear transformations of *n*-dimensional space. The work was completed by Wilhelm Killing in 1888, and improved by Élie Cartan in 1894. The results deserve mention because simple Lie groups show up all over mathematics, and we shall soon encounter them again. First, there are the four families found by Lie. They have a satisfying and beautiful unified theory. But in addition there are five *exceptional* groups. Their dimensions are 14, 52, 78, 133, and 248, and as these numbers may suggest, they conform to no obvious pattern. Their existence has been described as a

'brutal act of Providence', but they are related to all kinds of strange and beautiful 'accidents' in mathematics, and they add some piquancy to what might otherwise be too bland a confection. They appear to be the visible signs of some deeper pattern that is beginning to make sense, but is still imperfectly understood. Simple Lie groups are currently fashionable in mathematical physics, and the exceptional groups appear to have important implications for the possible structures of space-time. An act of Providence they may be; but brutal they are not.

The Ten-Thousand-Page Proof

The more elementary problem of classifying the *finite* simple groups proved much less tractable. The additional analytic structure of Lie groups, while making it harder to discuss them without technicalities, provides a lot of mathematical leverage for proving theorems. Information on finite simple groups accumulated slowly during the 1900s, but then nothing much happened until 1957 when the floodgates burst.

The easiest simple groups to find are the 'cyclic' groups formed by the integers modulo a prime, under addition. Not only do they have no normal subgroups: they have no subgroups at all. Next are the alternating groups. The first of these has order 60 and is the group Galois discovered in his work on the quintic. Then there are finite analogues of the four classical families of simple Lie groups, obtained by replacing the complex numbers by a finite field. Some of these were known to Galois and Jordan. In addition there are five curiosities, found by Emile-Léonard Mathieu in 1861 and 1873, and associated with some exceptional 'finite geometries'. This was the state of play at the turn of the century.

For a long time very little progress was made, although Leonard Dickson managed to obtain analogues of the exceptional Lie groups of dimensions 14 and 78 over finite fields. Obviously the other three exceptional Lie groups were crying out for similar treatment; but nobody could find the right way to make this idea work. Then in 1957 Claude Chevalley found a unified approach which produced,

for any simple Lie group and any finite field, a finite simple group, said to be of *Lie type*. A variation, producing 'twisted' groups, was found by Robert Steinberg, Michio Suzuki, and Rimhak Ree. Suddenly order began to appear amid the chaos. There were the cyclic groups, the alternating groups, the groups of Lie typeand five *sporadic* groups, those found by Mathieu. This is very reminiscent of the list of simple Lie algebras: lots of nice families and a few black sheep. Would the pattern (or lack of it) repeat?

An important step towards the final solution was taken by Walter Feit and John Thompson in 1963, who verified a conjecture made in 1906 by William Burnside, to the effect that every finite simple group (other than cyclic) has even order. Their proof is 250 pages long and occupies an entire journal issue, and it set a pattern for work on finite simple groups (both in style and size). It opened up a completely new avenue of attack, because every group of even order has an element *g* such that $g^2 = 1$, and Richard Brauer had pioneered a method to exploit these special elements. Zvonimir Janko used this technique in 1965 to find a new sporadic simple group. Other sporadic groups made their entrance, associated with the names of Donald Higman, Charles Sims, John MacLaughlin, Michio Suzuki, John Conway, Dieter Held, Bernd Fischer, R. Lyons, Michael O'Nan, and Arunas Rudvalis. A few went through a brief period when it was believed that they existed, on circumstantial evidence, but nobody had actually constructed them. Lengthy computer calculations were often required to pin the existence problem down. The trickiest case was the biggest sporadic group, the *Monster*, whose order, expressed in prime factors, is

$$2^{46} \cdot 3^{20} \cdot 5^{9} \cdot 7^{6} \cdot 11^{2} \cdot 13^{3} \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 41 \cdot 47 \cdot 59 \cdot 71.$$

There was some doubt whether even a large computer would be able to handle such a brute. In 1982, to everyone's astonishment, Robert Griess slew the monster with his bare handsa feat that would do credit to St George. He constructed it as a group of rotations in 196883-dimensional space (the lowest dimension possible!).

With the successful completion of a programme of research laid down in the 1970s by Daniel Gorenstein, it is now known that there

are exactly twenty-six sporadic simple groups. Together with the known families, they exhaust the list of finite simple groups. The complete proof totals at least 10,000 printed pages, and required contributions from hundreds of mathematicians. It is a remarkable triumph.

Having settled this long-standing and absolutely central problem, the group theorists are moving on. One question that has not yet been given a full answer is: 'Which groups can occur as Galois groups?' The best guess is 'all of them' but nobody has been able to prove it. Shafarevich managed to prove that all soluble groups are Galois groups, so attention has switched to the simple groups. Recently Thompson proved that the Monster occurs as a Galois group!

Symmetry of Atoms

One of the applications of group theory is to crystal symmetry. The atoms in a crystal are arranged in a regular repetitive structure, or *lattice*, which has a great deal of symmetry. The symmetries include *point symmetries*, which measure how symmetrically the atoms are arranged around a given position in the lattice and *translational* symmetries, whereby the structure of the whole lattice repeats at fixed distances in particular directions. An infinite array of square tiles is a two-dimensional example. In fact, there are known to be exactly seventeen types of crystallographic symmetry group in the planeor, more vividly, seventeen basic types of wallpaper pattern. In three dimensions there are exactly thirty-two point groups, which combine with translations to yield 230 distinct types of crystal symmetry. This classification is fundamental to solid state physics. In four dimensions there are 4,783 types of crystal; results for higher dimensions are mercifully incomplete.

Beyond Groups

The classification of crystals shows that no crystal lattice can have 5-fold symmetry (like a pentagon): only 2-, 3-, 4-, and 6-fold symmetries can occur. But recently physicists have found a new

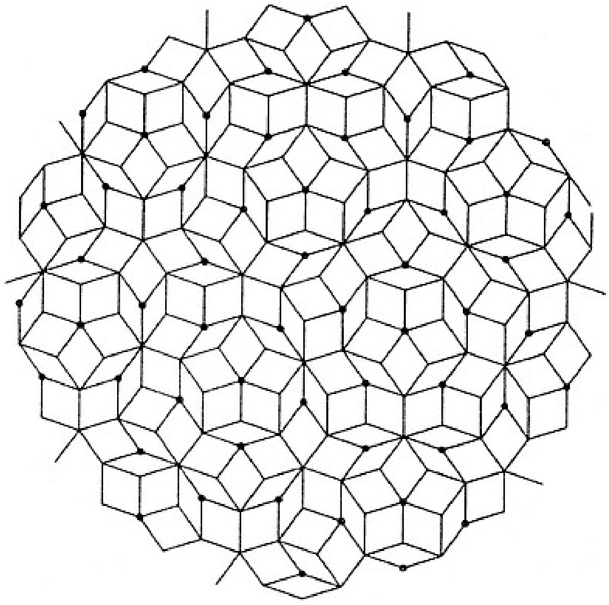


Fig. 6.
A quasicrystal tiling. (István Hargittai (ed.), *Quasicrystals, Networks, and Molecules of Fivefold Symmetry*, VCH Publishers.)

state of solid matter, called a *quasicrystal*, in which 5-fold symmetry does occur. The results suggest that group theory is not the ultimate description of symmetry in nature. A quasicrystal does not have the traditional lattice structure of a crystal. That is, if you move it sideways, it looks different in fine detail. However, there is still a kind of long-range order to the structure: there are a small number of possible directions in which the bonds between atoms can point, and

these are the same anywhere in the solid. Such patterns were originally discovered by Roger Penrose as a mathematical recreation. Penrose found two shapes of tile, kites and darts, which could cover the entire plane, but without any periodicity. Alan MacKay found three-dimensional versions of these tiles. The ideas were put on a firm mathematical basis by D. Levine and P. J. Steinhardt in 1984; and the predicted quasicrystal was observed in nature by Daniel Schechtman and others in an alloy of aluminum and manganese.

This discovery means that the traditional tool of crystallography, group theory, is not sufficient to deal with all 'symmetric' phenomena. Group theory has achieved dramatic successes, but it is not the last word. Mathematics must move with the times, it cannot rest forever on its laurels. The concept of symmetry may need to be broadened. Other 'forbidden' symmetries may arise in nature. And there are new challenges for mathematicians and physicists. 'Whatever the outcome,' says John Maddox, 'crystallographers will have to mug up some unfamiliar mathematics.' So will mathematicians.

8
The Purple Wallflower

If in any process there are a number of contradictions, one of them must be the principal contradiction playing the leading and decisive role, while the rest occupy a secondary and subordinate position. Therefore, in studying any complex process in which there are two or more contradictions, we must devote every effort to finding its principal contradiction. Once this principal contradiction is grasped, all problems can be readily solved.
Mao Tse-tung

In 1852 Francis Guthrie, a graduate student at University College London, wrote to his younger brother Frederick with a simple little conundrum that he had stumbled across while attempting to colour a map of the counties of England. 'Can every map drawn on the plane be coloured with four (or fewer) colours so that not two regions having a common border have the same colour?'

'Map' here is used in the geographical sense: a division of the plane into finitely many regions bounded by curves. 'Common border' refers to a definite line, and not just an isolated point, along which the regions meet. Frederick Guthrie couldn't solve it, so he asked his professor, the distinguished mathematician Augustus De Morgan. On 23 October 1852 De Morgan confessed in a letter to the even more distinguished William Rowan Hamilton that he couldn't make any headway. This is the earliest documented reference to the *Four Colour Problem*, which became notorious as the most easily *stated*, but most difficult to answer, open question in mathematics. It remained unsolved until 1976. Appropriately enough it has a colourful history, and presents subtle traps for the unwary. Hermann

Minkowski, a major figure of the nineteenth century, once told his students that the only reason it hadn't been solved was that only third-rate mathematicians had attacked it. 'I believe I can prove it', he announced. Some time later he told them sheepishly, 'Heaven is angered by my arrogance; my proof is also defective.' As Thomas Saaty and Paul Kainen remark: 'One of the many surprising aspects of the Four Colour Problem is that a number of the most important contributions to the subject were originally made in the belief that they were solutions.'

The eventual positive answer is unusual in at least a technological sense. The proof depends on showing that some 2,000 specific maps behave in a particular way. Checking all these cases is immensely tedious, and requires several thousand hours of time on a fast computer. If a full proof were to be written out, it would be so enormous that nobody could live long enough to read itlet alone verify it. Is such a monstrosity *really* a proof? Opinions are divided. Does a simpler proof exist? Nobody knows, although it *has* been shown that no substantially simpler proof can run along similar lines.

Despite its notoriety, the Four Colour Problem is not really in the mainstream of mathematics: its solution doesn't seem to open up any areas of major significance. It is more of a tidying-up exercise. But its solution introduced some novel ideas, requires little specialist technique to follow, and poses some intriguing philosophical questions which shed a lot of light on the way mathematicians feel about their subject. No discussion of the problems of mathematics would be complete without giving it a mention.

Comedy of Errors

De Morgan did manage to show that there is no way to arrange five countries so that any two are adjacent: this *suggests* that four is the maximum number of colours needed, but it isn't a proof because there might be other 'non-local' obstacles to 4-colourability, such as complicated chains of regions linking up in funny ways. In 1878 came the first reference to the problem in print, a letter by Cayley in the *Proceedings of the London Mathematical Society*, asking

whether the problem had been solved yet. In 1879 Arthur Kempe, a barrister and a member of the Society, published a proof, and the matter seemed settled. It was a minor curiosity, nothing more.

The proof involved an ingenious use of what are now called *Kempe chains*. Suppose for definiteness that the desired colours are red, blue, yellow, and green. Pick a pair, say red and yellow. Suppose that a portion of the map has successfully been coloured. Think of it as a number of red-yellow 'islands' in a blue-green 'sea'. (There may be lakes on islands and islands within lakes.) Observe that the colours red and yellow may be interchanged throughout any individual island without creating any forbidden adjacencies, because internal borders remain red-yellow, while external borders are either unchanged, or are on the 'coast', with sea (blue-green) one side and land (red-yellow) on the other. You can play this game with any pair of colours. Each island is a Kempe chain; and the possibility arises of colouring a map until you get stuck, and then changing colours in carefully chosen Kempe chains to get unstuck again and continue.

Minimal Criminals

More formally, we resort to a standard mathematical ploy, 'proof by contradiction'. If the Four Colour Theorem is *false*, then there must exist maps that require a fifth colourpurple, say. If such 'bad' maps exist, they can be incorporated into bigger maps in all sorts of ways, all of which will need purple passages too. Since there's no fun in making bad maps bigger, we go the opposite way, and look at the smallest bad maps. These *minimal criminals* need five colours; but any *smaller* map needs no purple regions. The aim is to use this information to restrict the structure of a minimal criminal, to exploit these restrictions to pin it down further, and so onuntil eventually it is shown that no such creature exists. No petty crime implies no big crime either so the theorem is true after all!

Kempe's idea was to take a minimal criminal, and shrink a suitable region to a point. The resulting map has fewer regions, so can be 4-coloured. Restoring and colouring the shrunken region may not be possible, because it may abut regions that between them already

use up all four colours. If it's a triangle (a region meeting only three others) that's not a problem. If it's a square, then a Kempe chain swap can change one neighbouring colour, which does the trick. If it's a pentagon, says Kempe, a similar argument works. And he could prove that *every* map must contain either a triangle, a square, or a pentagon.

But the Four Colour Problem wasn't going to succumb that easily. In 1890 Percy Heawood found a mistake in Kempe's treatment of pentagonal regions. A year later P. J. Tait published a different proof, but J. Petersen quickly found a mistake in that too. Heawood did notice that Kempe's method can be patched up to give a proof that *five* colours always suffice. (One extra colour makes the pentagon easier to restore.) He also studied analogues of the problem on surfaces other than the plane, giving a bound for the number of colours required, in terms of the number of holes in the surface. The *Heawood Conjecture*, that this number is the best possible bound, was proved in 1968 by G. Ringel and E. Youngs (using some beautiful and ingenious combinatorial ideas). There were two exceptions. One was the sphere (or plane) where the Heawood bound of 4 remained conjectural. The other was the Klein bottle, where the Heawood bound is 7 but the correct answer is known to be 6. So by 1968 the correct number was known for all surfaces *except* the plane! This illustrates a possibly undesirable tendency in mathematics: to do a lot of work on generalizations without solving the original problem. Of course, the generalizations might lead to an idea that solves everything: they often do. This time they didn't.

Shrink and Restore

Back to the original Four Colour Problem. In 1922 Philip Franklin proved that every map with 26 or fewer regions is 4-colourable. His method laid the foundations for the eventual successful assault, with the idea of a *reducible configuration*. A *configuration* is just a connected set of regions from the map, together with information on how many regions are adjacent to each around the outside. To see what reducibility means, let's take an example. Suppose a map contains a triangle. Shrink it to a point. Suppose the resulting map, which has one region fewer, can be 4-coloured. Then so can the original map. The triangle abuts only three regions, leaving a fourth colour spare when it is restored to the map. In general, a configuration is reducible if the 4-colourability of any map that contains it can be proved provided a smaller map is 4-colourable. (The 'shrink-colour-restore-extend' strategy can be varied in all sorts of ways, but is the basic way to prove reducibility.) A similar argument proves squares are reducible.

A minimal criminal cannot contain a reducible configuration. For suppose it did. Reducibility implies that, if a certain smaller map is 4-colourable, then so is the original. But minimality means that the smaller map *is* 4-colourable, so the minimal criminal is too. And that contradicts its nefarious nature. What we must do, then, is show that all minimal criminals must contain some reducible configuration. That way, we rule them all out. The most direct way to do this is to find a set of reducible configurations which is *unavoidable*, in the sense that any map must contain a configuration in this set. Kempe effectively tried to do this. He proved, correctly, that the set {triangle, square, pentagon} is unavoidable. But he made an error when proving reducibility of the pentagon. Nevertheless, the basic strategy of a proof now emerges: find an unavoidable set of reducible configurations. The list will have to be longer than Kempe's, because replacements for the pentagon will be needed.

Discharging

In 1950 Heinrich Heesch became the first mathematician to state publicly that he believed the Four Colour Theorem could be proved by finding an unavoidable set of reducible configurations. He estimated that about 10,000 configurations would be needed, each of moderate size. Heesch and his student Karl Dürre developed a computer program to test for reducibility. Like its more sophisticated successors, it wasn't foolproof. If the test was successful, the configuration was reducible; but if it failed, reducibility remained open.

Heesch also devised a method for proving unavoidability, based

on a loose electrical analogy. Suppose a quantity of electrical charge is applied to each region, and then allowed to move into neighbouring regions by following various rules. For example, we might insist that the charge on any pentagon is split into equal parts and transferred to any of its neighbours, except triangles, squares, and pentagons. By analysing the general features of charge distributions, it can be shown that certain specific configurations must occurotherwise charge 'leaks away'. More complicated recipes lead to more complicated lists of unavoidable configurations.

Man and Machine

In 1970 Wolfgang Haken found improvements to Heesch's discharging method and started thinking seriously about solving the Four Colour Problem. The main difficulty was the likely size of configurations in the unavoidable set. The *ring size* of a configuration is the number of regions that surround it. There was reason to believe that a ring size of 18 or more might be needed. As well as not being foolproof, reducibility tests grow exponentially with the ring size, increasing by a factor of about 4 for every extra region in the surrounding ring. While an 11-ring configuration isn't too bad, a 14-ring configuration will take 26 hours without doing any of the fancy extras likely to be needed. Even if that can be reduced to about half an hour on average, most 18-ring configurations will take 100 hours or moreand the calculations will use more memory storage than any computer possesses. With an estimated 10,000 regions in an unavoidable set, the whole computation could take a century. And if, at the end, just one configuration in the unavoidable set turns out not to be reducible, then the whole calculation is worthless! The computer chases its tail and succeeds only if it catches it. So between 1972 and 1974 Haken, together with Kenneth Appel, began an interactive dialogue with the computer, to try to improve the chances.

The first run of their computer program produced a lot of useful information. Some of it was positivethe likely ring size dropped to 16; the likely size of an unavoidable set was smaller too. Some was not: the quantity of computer output would be too big to handle,

and there were flaws in the method. Thus armed, they modified the program to overcome the flaws and tried again. More subtle problems emerged and were duly corrected. After some six months of this dialogue, Appel and Haken became convinced that their method of proving unavoidability had a good chance of success. It took another year to work out a proof of this. In 1975 they modified the program again. They write:

> At this point the program began to surprise us. At the beginning we would check its arguments by hand so we could always predict the course it would follow in any situation; but now it suddenly started to act like a chess-playing machine. It would work out compound strategies based on all the tricks it had been 'taught', and often these approaches were far more clever than those we would have tried. Thus it began to teach us things about how to proceed that we never expected. In a sense it had surpassed its creators in some aspects of the 'intellectual' as well as the mechanical parts of the task.

The Home Straight

By 1975 there seemed to be a real chance that the method would prove practicable, and the research programme moved from the exploratory phase to the final attack. The first step was to write an efficient program to prove reducibility, using assembly code on an IBM 360 at the University of Illinois. Much of the programming was done by John Koch, a computer scientist. The discharging procedure used to prove unavoidability had by now been modified so much that, rather than rewriting the program, it was decided to do that part of the work by hand. Jean Mayer, a professor of French Literature at Montpellier with a lifelong interest in the Four Colour Problem, suggested an important improvement in the procedure, and the likely ring size dropped to 14. In January 1976 construction of the unavoidable set began. Again it proceeded interactively, using the computer to check ideas and results. By June 1976 the work was complete. The computer duly reported that every configuration in Appel and Haken's unavoidable set was reducible. Red, blue, yellow, and green alone suffice to colour a planar map: the purple wallflower need never be invited to join the dance.

Is That a Proof?

To what extent can an argument that relies on an enormous and humanly uncheckable computation really be considered a proof? Stephen Tymoczko, a philosopher, wrote: 'If we accept the four-colour theorem as a theorem, then we are committed to changing the sense of "theorem", or more to the point, to changing the sense of the underlying concept of "proof".' The curious thing is that few mathematicians agree. This is so even though the correctness of the computer calculations requires a definite act of faith: it is not something that, even in principle, anybody could check.

There exist mathematical proofs that are so long and complicated that, even after studying them for a decade, nobody could put his hand on his heart and declare them to be totally unflawed. For example the classification theorem for finite simple groups is at least 10,000 pages long, required the efforts of at least a hundred people, and can be followed only by a highly trained specialist. However, mathematicians are generally convinced that the proof of the Four Colour Theorem is correct. The reason is that the strategy makes sense, the details hang together, nobody has found a serious error, and the judgement of the people doing the work is as trustworthy as anybody else's. Humans, by their nature, make mistakes. But serious mistakes in mathematics usually show up sooner or later, because the subject is constantly cross-checking results in one area against results in another. About a decade ago two teams of topologists, one Japanese and the other American, got exactly opposite answers to an important question. It took a while to sort out which team was wrong, and the Americans got annoyed because nobody really seemed very bothered; but eventually the mistake was found.

What mathematicians really like is an *illuminating* proof. That's a matter of taste rather than logic or philosophy. Davis and Hersh make a remark that must have flashed through many mathematicians' minds when the proof was announced:

> My first reaction was, 'Wonderful! How did they do it?' I expected some brilliant new insight, a proof which had in its kernel an idea whose beauty would transform my day. But when I received the answer, 'They did it by

> breaking it down into thousands of cases, and then running them all on the computer, one after the other,' I felt disheartened. My reaction was, 'So it just goes to show, it wasn't a good problem after all.'

There is nothing in the AppelHaken proof that is any less *convincing* than, say, the classification of simple groups; or indeed of the hosts of theorems published every day that are outside a particular mathematician's specialitywhich he will never check himself. It is also much less likely that a computer will make an error than a human will. Appel and Haken's proof strategy makes good logical sense and is sufficiently standard that no fallacy is likely there; the unavoidable set was in any case obtained by hand; and there seems little reason to doubt the accuracy of the program used to check reducibility. Random 'spot tests' and, more recently, repetitions of the computations using different programs, have found nothing amiss. I know of no mathematician who currently doubts the correctness of the proof.

Davis and Hersh also remark:

> To the philosopher, there is all the difference in the world between a proof that depends on the reliability of a machine and a proof that depends only on human reason. To the mathematician, the fallibility of reason is such a familiar fact of life that he welcomes the computer as a more reliable calculator than he himself can hope to be.

Perhaps we can leave the last word to Haken, who said in a newspaper interview:

> Anyone, anywhere along the line, can fill in the details and check them. The fact that a computer can run through more details in a few hours than a human could ever hope to do in a lifetime does not change the basic concept of mathematical proof. What has changed is not the theory but the practice of mathematics.

9
Much Ado About Knotting

The chief contortionist of Barnum and Bailey's circus slapped me on the back and hailed me so boisterously that the embarrassed young lady who accompanied me made the error of attempting to continue her stroll as if nothing unusual were happening. But by the time she finally entered the big top, her circle of acquaintances had increased by three and she proudly held a handful of photographs, inscribed and autographed by the Contortionist, the Tattooed Man, and the Expansionist. 'Chippet', the contortionist, was none other than ex-'Bender' of my own defunct-but-never-absorbed-or-amalgamated circus, whom Barnum had succeeded in teaching to tie himself in a perfect FIGURE-OF-EIGHT KNOT. *To this day I feel that P.T. had crowded me a bit.*
Clifford W. Ashley

Until the end of the eighteenth century it was a common belief in Europe that magic knots could bewitch a bridegroom and prevent consummation of a marriage. In 1705 two people in Scotland were executed for using nine magic knots to interfere with the wedded bliss of a Mr Spalding of Ashintilly. Knots are symbolic of mystery. Although Shakespeare did not commit our chapter title, he did say, in *Twelfth Night*:

> O time! Thou must untangle this, not I;
> It is too hard a knot for me t'untie.

Mathematicians have found the problems posed by knots pretty hard t'untie too, and their higher-dimensional analogues pose even knottier problems. Only with the aid of time have they begun to untangle the strands of a general theory. Even today some very simple basic

questionsan especially notorious one being the so-called Poincaré Conjectureremain unanswered.

The mathematician is primarily interested in *topological* properties of knotsfeatures that remain unchanged under continuous deformations. Knottedness itself is the most fundamental of these. Topology has been described as 'rubber sheet geometry', and this conjures up the right images even if it lacks technical accuracy. Knots are the most immediate topological features of curves in space. Beyond curves come surfaces; beyond surfaces come multidimensional generalizations called *manifolds*. Topologists study these, and a wild variety of other spaces. Topology is the study of the deeper influences of the notion of continuity, and considerations of continuity can turn up almost anywhere. As a result, topology has become one of the cornerstones of mathematics; and it is currently beginning to play a prominent role in applied science, especially mathematical physics (see Chapters 14, 15). To those with a long-term historical outlook this is not so surprising, because many of topology's roots are in applications; but during its main developmental period, about 192070, the subject became very abstract, and many people lost sight of its potential applicability.

Bridges and Bands

Topology itself is generally agreed to have begun in 1735 with Euler's solution to the problem of the Königsberg bridges. The river Pregelarme has two islands. These are linked by a bridge. One island has one bridge crossing from it to each bank; the other has two bridges to each bank. Can the citizens of Königsberg cross all seven bridges once only in a continuous walk? Euler showed that they can't, and he solved the most general problem of the same type. His method is based on the observation that it is the way the bridges connect, not their precise positions or sizes, that matters.

There are also a few earlier discoveries in the 'prehistory' of topology. In 1639 René Descartes knew that if a polyhedron has V vertices, E edges, and F faces, then $V E + F = 2$. Euler published a proof in 1751, and Cauchy gave another in 1811. Gauss said

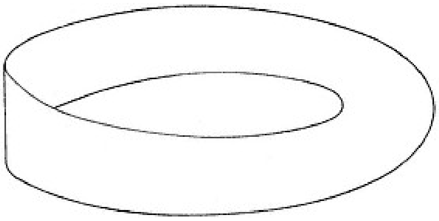


Fig. 7.
The Möbius band has a single side. If it is cut down the middle, it stays in one piece.

several times how important it was to study the basic geometric properties of figures, but he added little beyond a few observations on knots and links. A student of Gauss, Augustus Möbius, was the first person to define a topological transformation as a one-to-one correspondence between figures, such that nearby points in one correspond to nearby points in the other. He studied surfaces and their relation to polyhedra, which later became a central theme in the subject. In 1858 he and Johann Listing discovered the possibility of one-sided (or *non-orientable*) surfaces, the most famous being the Möbius band. Take a long strip of paper, give it a half twist, and join the ends. If you try to paint what look like its two sides with different colours, the twist causes these to merge somewhere. If you slice it down the middle it stays in one piece.

Topology is also the natural setting for the four-colour theorem, since the precise shapes of the countries on the map are irrelevantit's how they connect at the borders that matters.

The Importance of Holes

All this was amusing but of no clear significance. Topology became important for the rest of mathematics when Riemann introduced into complex analysis what are now called *Riemann surfaces*. Such a surface is equivalent to a sphere to which several handles have been added

(just like handles on a cup), or to put it another way, a torus with several holes. The number g of handles or holes is called the *genus*.

Klein discovered that the projective plane (a sphere with antipodal points identified, as in the standard model of elliptic geometry) is non-orientable, and invented another surface, the *Klein bottle*, with this property. Neither surface can be placed in three-dimensional space without crossing itself, but each can be defined abstractly by identifying, or gluing, the edges of polygons. As a simple example, imagine a square. If the two vertical edges are glued together it becomes a cylinder. If the top and bottom edges are now glued, the cylinder closes up into a torus. All properties of the torus can be studied using the *recipe* for the gluing, without actually going through the process of bending the square: it is sufficient to *pretend* that points due to be glued together are really the same. Now take a square; identify its two vertical edges; identify its two horizontal edges but in opposite directions. You've got a Klein bottle.

Conversely, given any surface whatsoever, you can slit it along some suitable system of curves and open the result up, like a peeled orange, to get a polygon. To reassemble the surface, you just imagine gluing the edges of the polygon back together. By studying how this ungluing and regluing procedure works, it is possible to classify completely the topological types of surfaces. The orientable ones are Riemann's g-handled spheres. The non-orientable ones are spheres in which g holes (with g at least 1) have been cut, and a Möbius band glued into each. This is possible in an abstract sense because the edge of a Möbius band is a single closed loop, that is, a topological circle. If you try to perform the construction in 3-dimensional space, the bands must cross themselves, forming hat-like objects called cross-caps. The surfaces just described are called the standard orientable and non-orientable surfaces of genus g. Any closed surface without edges is topologically equivalent to exactly one of them.

There is an important connection with Euler's Theorem. Although I didn't say so, the theorem that $V E + F = 2$ is only true for polyhedra which are topological spheresthat is, which don't have any holes. If you make a polyhedral torus, you'll find that $V E +$

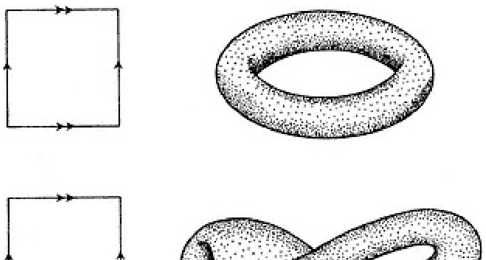


Fig. 8.
The torus and Klein bottle can be constructed by abstractly identifying the edges of a square.

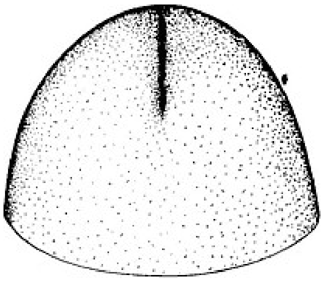


Fig. 9.
The cross-cap is a surface, topologically equivalent to a Möbius band with a circular edge.

F = 0. For example a three-sided picture-frame built from wood with a triangular cross-section has V = 9, E = 18, F = 9. Now you can take a surface that interests you, draw a polyhedron on it, and calculate $V E + F$. The fascinating thing is that the resulting value does not depend on how you draw the polyhedron: it depends only

on the topological type of the original surface. It is a *topological invariant*, the *Euler number* χ. The genus is also a topological invariant: two topologically equivalent surfaces have the same genus. So is orientability/non-orientability. In fact the genus and the Euler number are related. For an orientable surface we have $\chi = 2\ 2g$, and for a non-orientable one $\chi = 2\ g$.

Knots

Knots are trickier than surfaces, even though they are only 1-dimensional. The reason is that when studying a knot we are not concerned just with the topological type of the curve (which is always a circle), but also *how it is embedded* in 3-space. Let me enlarge on this remark, because it's exactly here that the 'rubber-sheet' notion is misleading. A topological transformation is more general than a continuous deformation in some surrounding space. All that is required is to set up a one-to-one correspondence that keeps nearby points nearby. No surrounding space enters into this. To transform a knotted circle into an unknotted one is easy: just make corresponding points correspond! Another way to say it is that we first *cut* the knotted circle, then unknot it, then rejoin the cut exactly as it was. Although points near to each other across the cut become far apart during this process, they end up close together again; and that's all we need.

Two knots are embedded in 3-space in topologically equivalent ways if we can find a topological transformation of the *whole* of 3-space that sends one knot to the other. This is a far stronger requirement than merely making the knots correspond topologically. Let me abbreviate this to 'the knots are equivalent' to save breath. The main problems of knot theory are:

(1) Decide whether a knot really is knotted.

(2) Given two knots, decide whether they are equivalent.

(3) Classify all possible knots.

Until the 1920s it wasn't even a theorem that knots existed at all! It was proved by Hans Reidemeister that the ordinary overhand knot,

or *trefoil*, is not equivalent to an unknotted loop. He did this by breaking up any deformation of a knot into a series of standard moves, and finding a property of the trefoil that is preserved by each such move, but which fails to hold for an ordinary circle. This was the first *knot invariant*, and we investigate it in more detail in Chapter 10.

A famous knot-theoretical Alexander, nicknamed 'The Great' was responsible in 333 BC for untying the immensely complicated *Gordian Knot*. He did this by a partial application of the 'cut and glue back' technique, only being a young man in a hurry he didn't bother about the gluing back, preferring to conquer the world rather than make his transformation topological. Another knot-theoretical Alexander, J. W., invented a beautiful knot invariant called the *Alexander Polynomial*. An unknotted loop has Alexander polynomial 1, the trefoil $x^2\ x + 1$, and these are different; *ergo* the knots are inequivalent. The figure 8 has polynomial $x^2\ 3x + 1$, so it's different from both. Sadly the reef knot and granny both have polynomial $(x^2\ x + 1)^2$, so the Alexander polynomial isn't powerful enough to yield a proof that generations of boy scouts can't be wrong. More sensitive invariants have recently been found: again see Chapter 10.

Conjurers often tie two separate knots in a rope, and manage to make the second 'cancel out' the first. Topologists know that this must be a trick, and I can't resist showing you a proof. Suppose knots K and L are tied in the same string (neatly separated from each other): call this the *connected sum* $K \# L$. Suppose a knot K has an 'inverse' K^* that cancels it out; and let 0 denote an unknotted loop. Tie infinitely many copies of K and K^* alternately on a single string, shrinking rapidly in size to keep the whole business finite. This gives an infinitely complicated knot

$$W = K \# K^* \# K \# K^* \# K \# K^* \#$$

Bracketing this expression one way, we have

$$W = K \# (K^* \# K) \# (K^* \# K) \#$$

$$= K \# 0 \# 0 \#$$
$$= K.$$

On the other hand,

$$W = (K \# K^*) \# (K \# K^*) \#$$
$$= 0 \# 0 \# 0$$
$$= 0.$$

So $K = 0$, that is, K is unknotted! Perhaps the biggest surprise is that this ridiculous argument can easily be made perfectly rigorous. Infinite knots like W are called *wild knots*. A mathematician called R. L. Wilder invented a particularly nasty class of wild knots: you can guess what these are called. The connected sum operation # produces a sort of knot arithmetic. Define a knot to be *prime* if it isn't the connected sum of two other knots (other than 0 and itself). Then every knot can be written *uniquely* as the connected sum of prime knots.

As well as knotted curves, it is possible to knot surfaces. As the Danish mathematician Piet Hein wrote in one of his 'grooks' (epigrammatic poems):

> There are doughnuts and doughnuts
> With knots and with no knots
> And many a doughnut
> So nuts that we know not.

Manifolds

The generalization of topology from surfaces to higher dimensions was initiated by Riemann, although he was thinking more in terms of rigid geometric properties. He introduced the idea of a *manifold*.

I'd better start by explaining what mathematicians mean by n-dimensional space. The idea is that the plane can be thought of as the set of points with coordinates (x, y) where x and y are numbers; and 3-space in terms of points (x, y, z). So 4-space is the set of points (x, y, z, u), 5-space (x, y, z, u, v) and so on. That's it: nothing could be simpler! And it only took 3,000 years or so to see how obvious it all is.

A surface, however curved, however complicated, can be thought of as lots of little round patches glued together; and topologically each patch is just like a patch in the ordinary Euclidean plane. It is

not this *local* patch-like structure that produces things like the hole in a torus: it is the *global* way all the patches are glued together. Once you see this, the step to n dimensions is easy: just assemble everything from little patches cut out of n-dimensional space instead of the plane. And that, aside from a few technical points which nobody sorted out until the 1930s, is exactly what is done. The resulting space is an *n-dimensional manifold*. Usually this is abbreviated to 'n-manifold'. There are good reasons to want to know about manifolds. For example the motion of three bodies under gravity is really a question about an 18-dimensional manifold, with three position coordinates and three velocity coordinates per body.

What topologists would like to do to manifolds is what they've already done for surfaces and knots. Namely:

(1) Decide when two manifolds are or are not topologically equivalent.

(2) Classify all possible manifolds.

(3) Find all the different ways to embed one manifold in another (like a knotted circle in 3-space).

(4) Decide when two such embeddings are, or are not, the same.

The first two problems are bad enough; the second two worse. But a great deal of progress has been made. I'll concentrate on (1) and (2); but I'd like to mention one result which shows that (3) must be pretty messy. The question is: when can two 50-dimensional spheres be *linked* in n-dimensional space? By 'link' I mean that they can't be separated by a topological transformation of the surrounding space. The answer is that they cannot link if n is 102 or more; they can link if $n = 101, 100, 99, 98$; buthere comes the surprisethey cannot link if $n = 97$ or 96! Then they can link again if $n = 95$, and so it goes down to $n = 52$. Weird. Topology is full of counter-intuitive things like that.

The Poincaré Conjecture

Until very recently progress on problems (1) and (2) above was stymied by a difficulty that Poincaré ran into in 1904, saying rather

offhandedly 'this question would lead us too far astray' before proceeding rapidly to other things. He was right: after eighty years of being led astray nobody knows the answer to his exact question, although higher-dimensional versions have been settled completely.

The problem lies in an area called *homotopy theory*. This is a part of *algebraic topology*. The idea in algebraic topology is to reduce topological questions to abstract algebra by associating with topological spaces various algebraic invariants. Poincaré was one of the fathers of this theory, and he invented a thing called the *fundamental group*. The idea is a cunning blend of geometry and algebra. Take a manifold M and select a point * in it. Consider loops that start and end at *. You can 'multiply' two loops by doing first one, then the other: the result is again a loop starting and ending at *. However, you can't 'divide' loops. This can be achieved, though, if you pretend that any loop is the same as a continuous deformation of itself. The reciprocal, or inverse, of a loop is then the same loop traversed in the opposite direction. The set of all loops, with this 'multiplication', is Poincaré's fundamental group. If M is a circle then the thing that distinguishes loops is how many times they wind round; and the fundamental group consists of the integers. The 'multiplication' is actually addition: if the first loop winds 5 times and the second 7, then the two together wind 12 times, not 35.

After Poincaré's lead, other mathematicians noticed that if you use k-dimensional spheres instead of 1-dimensional loops you get *higher homotopy groups*, one for each k. Each higher homotopy group is a topological invariant, in the sense that two topologically equivalent manifolds must have the *same* homotopy groups. Now Poincaré wondered whether these invariants are good enough to tell any two different manifolds apart. In particular he asked: if M has the same homotopy groups as the 3-dimensional sphere S^3, then is M topologically equivalent to S^3? His guess, 'yes', remains unproved to this day. The problem has also been generalized by later workers. The n-dimensional Poincaré Conjecture is that an n-dimensional manifold with the same homotopy groups as the n-dimensional sphere S^n is topologically equivalent to S^n. It is easy to check that this is true for $n = 2$, by using the classification of surfaces. But in higher

dimensions no classification of manifolds is known, which puts a different complexion on finding a proof.

Five

In 1961 Stephen Smale developed a method for breaking a manifold up into nice pieces, called *handles*. By cleverly moving handles around he proved an important result, called the h-cobordism theorem, in dimensions $n \geq 5$. Among its consequences is the Poincaré Conjecture for dimensions 5 or higher. In fact Smale's proof first worked only for $n \geq 7$. John Stallings dealt with the case $n = 6$ and Christopher Zeeman with $n = 5$. Then Smale extended his methods to cope with those cases too. However, an essential ingredient, the 'Whitney trick', fails completely in dimensions 3 or 4, and there seemed to be no way to extend Smale's proof to these cases. It became the conventional wisdom that topology is somehow different in dimensions 3 and 4, mostly because nobody could prove anything really useful in those dimensions.

Four

In 1982 Michael Freedman found a way to get round the failure of the Whitney trick, and showed that the conventional wisdom was wrong. In four dimensions, manifolds behave pretty much the way they do in five or more. In particular, the Poincaré Conjecture is true for 4-dimensional manifolds.

Three

This meant that the only unsettled case of the Poincaré Conjecture was the one originally posed by Poincaré, in three dimensions. This tantalizingly narrow gap happens because 2-dimensional space is too small to have room for any serious complexity, and 4- or higher dimensional space is so big that the complexities can be rearranged nicely. In three dimensions there is a creative tension: big enough to be complicated; too cramped to be easily simplified. What was needed

was a line of attack that exploited the special properties of 3-dimensional manifolds. But what are those special properties? Topologists joined the hunt, moving into 3-manifolds in droves.

Rigidity in Floppiness

A topological manifold is a very floppy thing, free to be kneaded and distorted virtually at will, like a lump of pizza dough. Traditional geometry, Euclidean and non-Euclidean, is much more rigid, like a cooked pizza crust. There is a metric, a distance structure, and distances cannot be altered. Stretching is not allowed. So it was a great surprise when in the mid-1970s William Thurston announced a programme to reduce the study of 3-manifolds to geometry!

The idea goes back to the nineteenth-century work on surfaces. One way to sum up the classical results on non-Euclidean geometry is to say that, up to a choice of scale, there are three basic geometries, each realizable as the geometry of a surface of constant curvature. The curvature distinguishes the three cases:

(1) the Euclidean plane, of curvature 0;

(2) the hyperbolic plane, of curvature 1;

(3) the elliptic plane or 2-sphere, of curvature +1.

Now it turns out that every topological type of surface is associated with, and can be constructed from, exactly one of these geometries. Specifically, each surface can be given a metric of constant curvature 0, 1, or +1. Which one depends on a topological invariant: the Euler number.

There are only two surfaces with positive Euler number: the sphere and the projective plane. The sphere already has curvature +1; and since the projective plane is obtained from the sphere by identifying antipodal points, this metric can be transferred to the projective plane. (In fact this is just the usual model of elliptic geometry.) There are also two surfaces with Euler number zero, namely the torus and the Klein bottle. We saw above how to get these by gluing the edges of a square. Now a square lives in Euclidean space, so inherits its zero-curvature metric; therefore the torus and Klein bottle inherit it too.

Finally there is an infinite family of surfaces with negative Euler number. These can all be constructed by gluing the edges of a polygon in the hyperbolic plane! So they inherit its metric of curvature 1.

Given this beautiful relation between geometry and topology, it ought to be natural to ask whether it generalizes to three dimensions. However, there are several complications. As Thurston says: 'Three-manifolds are greatly more complicated than surfaces, and I think it is fair to say that until recently there was little reason to suspect an analogous theory for manifolds of dimension 3 (or more)except perhaps for the fact that so many 3-manifolds are beautiful.' But he then adds, 'The situation has changed.'

Eight Geometries

One of the complications is that instead of the three geometries possible in two dimensions, there are *eight* possible geometries in three dimensions, as Thurston showed in 1983. Naturally, they include Euclidean 3-space, hyperbolic 3-space, and elliptic 3-space (or the 3-sphere); but there are five others. The proof that only these eight occur requires a lengthy analysis, based on group theory. This should be no surprise, given Klein's Erlangen programme: geometry is group theory! All sorts of familiar theorems make guest appearances in the proof, for example the classification of the seventeen wallpaper groups.

Just as surfaces can be given metrics modelled on the three types of two-dimensional geometry, so *some* 3-manifolds can be given metrics modelled on one of the eight geometries above. Each 3-manifold corresponds to at most one type of geometry. However, some 3-manifolds have no such geometric structure at all.

The way to get round this difficulty, according to Thurston's programme, is to break the manifold up into pieces, and put a geometric structure on each bit. The conjecture is that this can be done in only one way. First split it into prime manifolds (much as knots can be decomposed into prime knots via the connected sum #), then cut along specially chosen tori. There is a lot of evidence in favour of this

proposal, and none against. In fact, for seven of the eight geometries, the problem has been solved completely.

The Hyperbolic Phoenix

That leaves just one final case, which happens to be 3-dimensional hyperbolic geometry; but that seems to be a tougher nut to crack. It must be, because the still unproved 3-dimensional Poincaré Conjecture will appear as a minor detail if Thurston's programme can be completed. The reason people are excited about the programme is that it places the problem within a much richer framework, adding a lot of leverage. The older theory of knots and links is important for the study of 3-manifolds, because the complement of a knot (that is, the set of points *not* on the knot) gives rise to a 3-manifold. The hyperbolic geometry of knot complements is a rich and beautiful area.

One by-product of Thurston's programme is a proof of the Smith Conjecture, stated by Paul Smith in the 1940s. This is about transformations of a 3-sphere into itself, which are *periodic* in the sense that if you perform them a suitable number of times, everything ends up where it started. Some points never move at all, and the conjecture is that the set of all these *fixed points* (which is known to be a curve) isn't a knot. The proof, completed in 1978, assembles the results of a number of people, among them Thurston, Hyman Bass, William H. Meeks III, and Shing-Tung Yau.

One effect of all this is a sudden renewal of interest in hyperbolic geometry, which had been thought of as pretty much mined out. In fact little work was ever done on the 3-dimensional case, but only now does it appear sufficiently interesting and relevant to the mainstream to be worth doing. As the needs of research mathematics change, supposedly 'dead' subjects can rise like the phoenix from their own ashes.

Three Kinds of Glue

My remark above, that 4-dimensional manifolds are much like their higher-dimensional cousins, isn't completely true. It depends on what

brand of manifold you buy. Although I have concealed this fact so far, there are three kinds of manifolds: piecewise-linear, topological, and smooth. Up till now the difference hasn't really been important, but I want to talk about one of the most sensational discoveries ever made in topology, where the difference is the essence of the matter.

As I said above, you can think of a surface as being cut into a number of pieces, each of which can be 'flattened out' by a topological transformation to look like a piece of the plane. The original surface can be built up by overlapping all of these pieces. Indeed this is how mathematicians define a surface in the abstract: a lot of pieces of the plane, 'glued together' on their overlaps. A similar approach is used in *n* dimensions, but now the pieces are taken from Euclidean *n*-space. The way the gluing takes place determines what type of surface you have, and there areso to speakthree important types of glue. Real glue joins each point on one object to a point on the other. Mathematical glue just specifies, for each point on one, the corresponding point on the other (to which the real glue would attach it). That is, it is determined by a one-to-one correspondence between the overlapping parts. The type of glue is determined by what properties this correspondence preserves.

If you use topological glue, only notions of continuity are preserved. Points nearby on one piece must be glued to points that are nearby on the other piece; but you can bend the pieces however you like. If you use smooth glue, then smooth curves on one piece must be glued to smooth curves on the other. You can't, for example, put a sharp corner in one piece and glue that to a straight line in the other. It is this notion of 'manifold' that Riemann had in mind. Finally, if you follow in the footsteps of Poincaré and use piecewise-linear glue, then straight lines on one piece must glue to straight lines on the other. An irregular lump of dough is a sphere with a topological structure; an egg is a sphere with a smooth structure; and a wedge of cheese is a sphere with a piecewise-linear structure. The distinction is technical (essentially it lets you use different implements from the mathematical toolkit, according to your tastes), but the technicalities here are important, and in higher dimensions make a big difference. A theorem may be true for piecewise-linear

manifolds, but not for smooth manifolds, and so on. It's important to understand how these relations go, because it may be easier to prove theorems for (say) the piecewise-linear case, and then pass to the topological or smooth cases by some standard trick. (For example in the 1930s Henry Whitehead discovered how to give every smooth manifold a piecewise-linear structurethat is, how to bend an egg until it looks like a wedge of cheese. And in the 1950s it was shown that every topological 3-manifold has a *unique* smooth structurethat is, every lump of dough can be smoothed out into an appropriate egg shape. The 'egg' can have handles and holes and knots and so on, of courseit's the smoothness that's important.) In mathematical warfare, it's good strategy to choose the battleground that best suits your weaponry. A large part of the 1960s research effort in topology centred around these relationships, culminating in the results of Robion Kirby and Larry Siebenmann which sorted everything out in dimensions 5 or higherin a nice way, but with the odd surprise.

Fake 4-Space

One absolutely basic question is the uniqueness of the smooth structure on a given smooth manifold. In other words, is there only one consistent way to define 'smoothness' on a given space? Originally, everyone pretty much assumed the answer would be 'yes'; but Milnor's exotic 7-sphere (mentioned in Chapter 1) showed that it is 'no'. Indeed there are exactly 28 distinct smooth structures on a 7-sphere. However, it was still hoped that for good old *Euclidean* space of dimension *n* there should be a unique smooth structure, namely the standard one used in calculus. In fact this was known to be true for all values of *n* except 4. But at much the same time that Freedman was showing that 4-dimensional space is in most respects pretty much like five or higher dimensions, Simon Donaldson, in 1983, made the dramatic discovery of a non-standard smooth structure on 4-space.

His proof is difficult and indirect, making essential use of some new ideas cooked up between them by topologists and mathematical

physicists called *YangMills gauge fields* (see Chapter 10). These were invented to study the quantum theory of elementary particles, and were not intended to shed light on abstruse questions in pure topology! On the other hand, the topological ideas involved were not invented for the analysis of fundamental particles either. It is quite miraculous that two such disparate viewpoints should become united in such a tight and beautiful way. Ironically, many of Freedman's ideas are essential to the proof; and the starting-point is a curious group-theoretical coincidence. The Lie group of all rotations of *n*-dimensional space is simple, except when *n* = 4. In that case it breaks up into two copies of the rotation group in 3-space. That such a coincidence should have such stunning significance is yet another triumph for the Erlangen programme.

At any rate, we now know that 4-dimensional space is quite unusual, compared to any other dimension. In fact, not only is the smooth structure non-unique: it is *extremely* non-unique. Milnor's 7-sphere has only a finite number of smooth structures. But there is an *uncountable infinity* of distinct smooth structures on 4-space. However, things may not be quite as bad as they currently seem. Among these is one 'universal' structure which in a sense contains all the others. Perhaps the issues will eventually be resolved so that everything appears natural and inevitable; but right now there's still plenty of mystery.

Oodles of Handles

For example, until very recently nobody could answer a very straight-forward and obvious question. *What shape is a fake four-space?* How can we visualize such things? Donaldson's work provided only an existence proof: it was not constructive. In 1994 this gap was filledat least, to an extentby Zarko Bizaca. Starting from an idea of Selman Akbulut he derived an explicit geometrical description of one particular fake four-space. In keeping with the weirdness of fake four-spaces, the description verges on the bizarre.

Visualizing complicated geometry in four dimensions is not at all easy, and mathematicians have been forced to invent special tricks

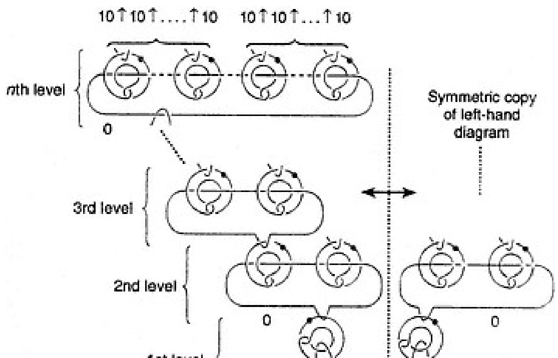
in this case 'handebody decomposition'. Imagine first that you are a Flatland topologist, a creature confined to a planar universe, who is desperately trying to comprehend the newfangled theory of three-space. You have mastered the description of a three-dimensional ball as the set of all triples (*x, y, z*) that lie at distance *r* or less from the origin. You are now trying to understand a torusa doughnut shape. One way to get a torus is to stick a handle on to sphere, just like the handle on a cup. A handle has two ends, which are 'glued' on to the ball at two patches. So in your mind you can think of the torus as a three-ball with two marked patches, a handle, and a rule for gluing them together. The great thing is that with practice you can work with the rule, and never actually do the gluing.

We Spacelanders use similar tricks to visualize four-dimensional objects, but of course everything gets more complicated: there are several types of handle, and the gluing rules can be a lot more intricate. Bizaca starts with a four-dimensional ball, whose surface is a three-dimensional sphere. This is a kind of 'curved three-space', and keen Spacelanders can get to grips with it fairly easily. Inside the three-ball he draws lots of wiggly loops, which look just like knots and links in ordinary three-space. Then he 'glues' certain kinds of handle on to the ball so that, in effect, their edges run along these links. Just like the Flatlander, you don't actually have to do the gluingyou just imagine that corresponding bits are adjacent. A systematic way to work with these links, called Kirby calculus, was introduced by Kirby in 1978. The main point is that you can define lots of four-dimensional spaces by drawing the corresponding linkswhich are things that Spacelanders feel comfortable with.

Bizaca's link is constucted so that it corresponds to a *fake* Euclidean four-space. The link is built from infinitely many closed loops, structured into levels as shown in the figure. (The link to the right of the dotted line is the same as that to the left.) At level one there is one 'kinky handle', the thing that looks like a little lifebelt. At levels two, three, and four there are two kinky handles. On level five there are two hundred kinky handles; on level six the number of kinky handles is the utterly huge number 2 × 10 10 10 10 where the arrow indicates forming a power. (It is *much* larger than the infamous

googolplex.) On higher levels the number of kinky handles is even vaster: on level *n* there are 2 × 10 10 of them, where the number of 10s is 8*n* 44.

Got that? Now exercise your imagination to 'glue on' an infinite system of handles, one for each loop in Bizaca's linkand the result, so he proves, is a fake Euclidean four-space. The prescription may seem curious, but that makes it all the more intriguing. Don't worry too much about what it *means*: just revel in its strangeness. Its importance is that it provides an explicit construction instead of mere existence. So at last we haveso to speaka handle on what fake four-spaces look like.



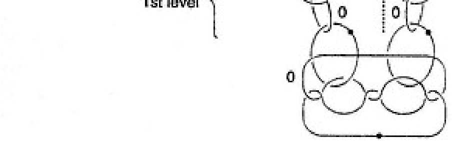


Fig. 10.
A link defining a fake 4-space.

10
More Ado About Knotting

To me the simple act of tying a knot is an adventure in unlimited space. A bit of string affords a dimensional latitude that is unique among the entities. For an uncomplicated strand is a palpable object that, for all practical purposes, possesses one dimension only. If we move a single strand in a plane, interlacing it at will, actual objects of beauty and utility can result in what is practically two dimensions; and if we choose to direct our strand out of this one plane, another dimension is added which provides opportunity for an excursion that is limited only by the scope of our own imagery and the length of the ropemaker's coil.
What can be more wonderful than that?
Clifford W. Ashley

Mathematics has a habit of waking up and biting people. Subject areas can lie dormant for decades, even centuries, then suddenly erupt in violent bursts of activity. The infusion of new ideas from another area, a chance observation, a new twist to an old trickalmost anything can trigger the eruption.

Topology has been a thriving area of research for most of this century; but a lot of its progress has been towards more esoteric questionsmultidimensional spaces, intricate algebraic invariants, big internal generalities. Some of the simpler parts of topology, its humble origins, have been neglected. Not deliberately, but because the remaining problems in those areas just seemed too difficult.

Low-dimensional topology has woken up with a vengeance, and is biting everything in sight. Classical knot theory has been turned upside down. The reverberations are being felt in molecular biology and in quantum physics. Indeed, it is physics that has supplied much

of the infusion of new ideas. The most curious feature of the whole business is that the discovery that triggered it *could* have been made at any time during the past fifty years. Are too many mathematicians just following fashion? Or does it take a special kind of genius to see something so simple that everybody else has overlooked it? A bit of both, I think. But ideas don't just arise of their own accord: there has to be an entry route, a natural line of thought that leads in the right direction, a bridge across the unknown. This particular bridge was so narrow, its entrance so well concealed by bushes and rocks, that a rare conjunction of circumstances was needed to reveal it.

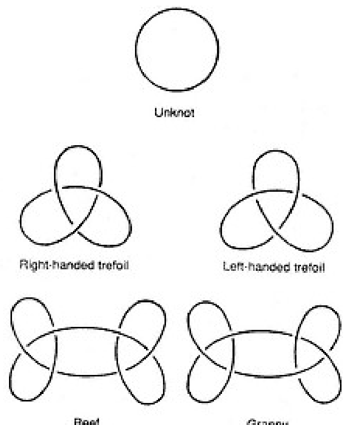
Within five years, it had become a six-lane highway.

Reef or Granny?

When you tie a knot, you use a length of rope. Real lengths of rope have ends. Indeed, without the ends, it would be impossible to tie the knots, for precisely the topological reason that we wish to investigate. The topologist, in contrast, has to get rid of the ends, otherwise the knot can escape by reversing the tying process. The simplest solution is to join the ends together, so that the rope forms an endless loop. Mathematical glue is used, rather than another knot, to avoid infinite regress. More precisely, a topologist defines a *knot* to be a closed loop embedded in ordinary three-dimensional space. A *link* is a collection of several such loops: the individual loops are its *components*. These component loops can be twisted or knotted, andas the name suggestsmay be linked together in any way, including not being linked at all in the usual sense. A knot is just a link with one component, so 'link' is a more general concept than 'knot'.

The picture shows some simple knots and links. Experiment will soon convince you that they are all different. The trefoil can't be deformed into its mirror image; the reef can't be deformed into the granny. All of these experimental observations are challenges to topology: *prove* it. As we shall see, this is far from easy; but the pay-off in terms of useful and intriguing mathematics is huge.

As already discussed in Chapter 9, two knots or links are *equivalent*topologically the sameif one can be deformed into the



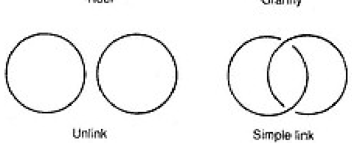


Fig. 11.
A variety of knots and links.

other by a continuous transformation. From the topological point of view it is unwise to judge knots by appearances: it is possible to deform a knot into an equivalent one that looks very different. Imagine taking an unknotted loop and scrunching it up into a horrible tangle. The tangle looks knotted, but really it isn't. The central problem in knot theory is to find efficient ways to decide whether two given links or knots are topologically equivalent. In particular we want to find out whether something that looks like a knot can be unknottedthat is, is equivalent to the *unknot*, a (manifestly unknotted) circleand whether a given link can be unlinked.

Knots Exist

Are there any knots at all? Perhaps every loop can be unknotted. In four dimensions, this is true; but in three dimensions, everyday experience strongly suggests otherwise. It is a reasonable mathematical conjecture that genuine knotsnot equivalent to the unknotexist. The first proof was given by Kurt Reidemeister in the 1920s. Why is it so hard to prove something that every boy scout learns at his scoutmaster's knee? Let's see how Reidemeister proved that the trefoil knot can't be untied.

We can represent a knot by its *diagram*: a projection or picture of it, laid flat, with the crossings neatly separated and tiny breaks introduced to show which part crosses over the other. The breaks divide the diagram up into pieces, its *parts*. At any crossing, three parts meet: an overpass and two underpasses. Actually it may sometimes be just two parts, because the overpass may loop round and join the underpass; but I'll continue to talk of three parts, bearing in mind that two of them may be the same.

The same topological knot may have many diagrams, depending on how it is deformed before being projected into the plane. Reidemeister's idea is to keep track of the changes that occur in the diagram when the knot is deformed in space, and try to find a topological invariant: a mathematical property that is immune to these changes. Any invariant will do, as long as it works. Knots with different invariants are necessarily inequivalent. (However, knots with the same invariants may or may not be equivalent, and the only way to decide is either to find an equivalence or to invent a more sensitive invariant.)

Every knot is determined by the crossings in its diagram and the way these join together. If you continuously deform the knot, then you also change its diagram. The pattern of crossings usually stays the same, although the shape of the curves joining them will change. But sometimes the crossings themselves change; and these are the important stages in the deformation. Reidemeister proved that all such changes can be reduced to three different kinds, which I'll call *basic moves*. The first, the *twist*, takes a U-shaped section of string and

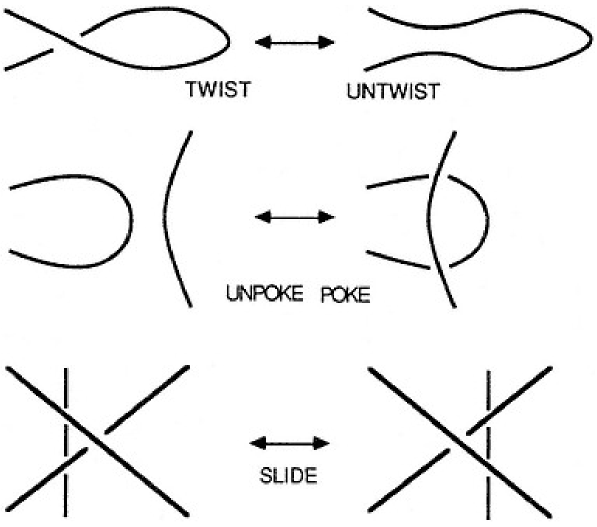


Fig. 12.
Basic moves.

twists it over itself to form a loop. It can also be reversed to *untwist* such a loop. The second, the *poke*, pushes a loop of string underneath (or over the top of) another one; and its reverse, the *unpoke*, pulls them apart again. Finally the *slide* just moves one string across the junction of two others. The reverse operation is just another slide in the opposite direction, so we don't need to talk about 'unslides'.

Every continuous deformation of a knot can be represented on its diagram as a sequence of basic moves. Therefore, in order to prove that something is a topological invariant, it is enough to show that each individual basic move leaves it unchanged. In other words, we have to show that it stays the same after a twist or untwist, a poke or unpoke, and a slide.

The property that Reidemeister invented is not at all obvious. It is that it should be possible to colour each part of the knot diagram with one of three colours, according to the following rules.

Rule 1 At any crossing, either the colours are all different, or they are all the same (that is, no crossing involves just two distinct colours).

Rule 2 At least two colours are used.

For example, the trefoil knot has a diagram with three parts, and if each is given a different colour, then both rules are obeyed. The unknot has only one part, so rule 2 cannot possibly be obeyed. In short, the trefoil is colourable, but the unknot isn't.

Now comes the cunning bit: the property of 'colourability' is invariant under each type of basic move. If you start with a colourable diagram, and apply a twist, an untwist, a poke, an unpoke, or a slide, then the resulting diagram remains colourable. (It's great fun to work out why. It's no harder than a crossword puzzle, and you don't need to know anything technical: try it.) Put everything together, and you have a proof that you can't unknot a trefoil.

That, in essence, is how Reidemeister solved the problem of the existence of knots. It's a long way removed from wiggling bits of string between your fingers and observing 'experimentally' that they don't come untied. The selection of basic moves, on the other hand, bears a close relation to such experiments: it's a matter of seeing what's important. Proofs are very different creatures from simpleminded experiments, but the germ of the idea must come from somewhere, and experiments often help.

Braid Upon the Waters

Reidemeister's trick can be generalized. Instead of three colours, you can use four, or five, or some larger number. The rules have to be modified, and it's easier to think of integers 0, 1, 2, rather than colours. A knot can be five-coloured, for example, if its parts can be labelled with integers 0, 1, 2, 3, 4, such that

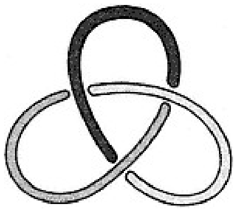


Fig. 13.
Colouring a trefoil.

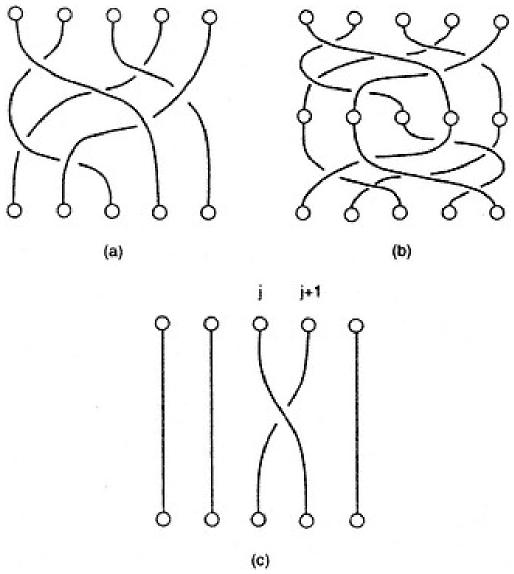


Fig. 14.
(a) A braid. (b) Combining two braids to get a third.
(c) Elementary braid with a single crossing.

Rule 1 At any crossing, three parts meet. If the overpass is numbered a and the two underpasses b and c, then we require $2a \equiv b + c$ (mod 5).

Rule 2 At least two numbers are used.

Colourability with a given number of colours is also invariant under basic moves, and hence a characteristic property of a knot. A figure-eight knot can be 5-coloured but not 3-coloured, and a trefoil can be 3-coloured but not 5-coloured, so the figure-eight is topologically different from the trefoil. This kind of game can be played with many different knots, but it has its limitations. It can't distinguish left- and right-handed trefoils; it can't tell reef from granny.

Notice how algebra is creeping into the problem. To begin with, everything was geometric. Now we're talking about numbers, and modular arithmetic. This is typical of topology, and we can here see how such changes of viewpoint occur. The geometry of knots is broken down into parts of a diagram and the way they link; the deformations of knots are replaced by basic moves; and the important feature then becomes the *combinatorics* of parts under the influence of basic moves. This problem in turn is reduced to algebraic questions about labels attached to the parts. This is the topologist's traditional strategy, played with simple ingredients (Reidemeister's colours) or sophisticated ones (the cohomology representation of the mapping class group, say).

Algebra entered in a big way in 1925 with Emil Artin's invention of the braid group. A *braid* is a system of curves, which start from a line of equally spaced points and end at a parallel line, but winding round each other on the way. Braids are equivalent if you can deform one continuously into the other, just like knots and links; but now the curves have ends, and the ends have to stay fixed; moreover, you're not allowed to push curves over the ends and undo them. They have to stay between the two parallel lines. The new feature is that two braids can be combined, by joining the end of the first to the start of the second. It turns out that braids form a group under this operation. The identity braid consists of parallel curves, not twisted in any way; and the inverse to a given braid is the same

braid upside down. Notice that you *only* get a group if deformations of a given braid count as the same braid: the way to cancel out a braid is to combine it with its inverse *and then straighten out the curves*.

Artin found a complete symbolic description of the braid group. Suppose, for example, that the braids have four strands. Every braid can be built up from *elementary braids* s_1, s_2, and s_3 which just swap adjacent points (s_j swapping strands j and $j + 1$). Their inverses are the same braids turned upside down: they look just the same except that overpasses become underpasses. We can symbolically express any braid as a sequence of powers of the *s*'s, using negative powers for inverses: say s_{13s2-} 4s17s35s2-8 and so on. Braids, like knots, may be topologically the same even though they look different, and Artin captured this in the *defining relations* of his braid group, satisfied by 'adjacent' elementary braids:

$s_1s_2s_1 = s_2s_1s_2$,

and so on. He proved that these relations correspond precisely to topological equivalence of braids. That is, suppose two braids are represented as symbol sequences. Then they are topologically equivalent if and only if you can pass from one symbol sequence to the other by applying the defining relations over and over again.

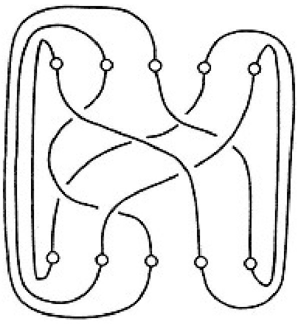


Fig. 15.
Converting a braid to a link.

Braids were around before Artin turned them into a group. In particular, in 1923 J. Alexander proved a fundamental connection with links. If you take a braid and join the top to the bottom, you get a link. Alexander proved that every link arises in this way. In short, you can use braids to represent links, and instead of asking questions about links, you can ask them about braids. However, the connection is not entirely straightforward, for many different braids give topologically equivalent links. In 1935 A. A. Markov extended Artin's ideas by finding an algebraic criterion for two braid symbols to yield the same link. There is an awkward extra subtlety: the possibility of changing the number of strands in the braid.

Alexander, as we've already mentioned, invented his own knot invariant, the Alexander polynomial. It's one of the central items in classical knot theory. Like Reidemeister's colouring numbers, it is defined in terms of the knot diagram. In 1936 W. Burau discovered how to find matrices that obey Artin's defining relations. It turns out that the Alexander polynomial of a knot is related to the Burau matrix of the corresponding braid. In short, the Alexander polynomial can be computed algebraically from the braid group. The Alexander polynomial isn't good enough to resolve the great classical questions, such as the inequivalence of left- and right-handed trefoils or the difference between the reef and the granny. However, it leads to more difficult and often highly technical methods that *do* resolve those questions. This 'classical' period of knot theory continued, with the development of more sophisticated invariants, until 1984.

Along Came Jones

In that year, a New Zealand mathematician named Vaughan Jones invented an entirely new knot invariant, also a polynomial, but apparently unrelated to anything classical. He published it a year later. It was a watershed in the subject, and we now divide knot theory into two eras: pre-Jones and post-Jones. The Jones polynomial can tell reef from granny as easily as falling off a log, and distinguish left-handed trefoil from right-handed with its hands tied behind its back.

Thing is, Jones wasn't a knot-theorist. He was working in analysis, studying creatures known as von Neumann algebras. If you think of them as groups with bells and whistles on you won't go too far astray, provided you understand that these are very elaborate bells and whistles. Now, just as groups are made from fundamental building-blocks called simple groups, so von Neumann algebras are made from fundamental building blocks called *factors*. Factors come in several kinds: types I, II, and III, with further subdivisions. Jones was thinking about type II_1 factors, which possess a numerical invariant, the *trace*. He focused on a particular set of von Neumann algebras. One of these, for example, has three generators, e_1, e_2, e_3. They satisfy some relations, which include things like

$e_2e_1e_2 = t/(1 + t)^2e_2$,

where t is a complex number. D. Hatt and Pierre de la Harpe pointed out that the relations in Jones's algebras are suspiciously similar to those in Artin's braid group; and Jones discovered that if he defined

$$s_j = \sqrt{t(te_j - (1 - e_j))}$$

then the relations become identical. He could then interpret a braid as an element of his algebra, and apply the trace to get a number that varied with t. In fact it's a polynomial in $\sqrt{t}$ and its inverse. The big surprise is that, because of Markov's results, this number is a topological invariant of the link associated with the given braid, a fact that emerged from long discussions between Jones and Joan Birman.

Knowing about the classical polynomial invariants of knots, Jones at first thought that his invariant must be some trivial variation on the Alexander polynomial. But he soon realized that it had to be new. For example, the Jones polynomial for a trefoil is

$t + t^3 t^4$,

whereas that for its mirror image is obtained by replacing t by $1/t$, giving

$$\frac{1}{t} + \frac{1}{t^3} - \frac{1}{t^4}$$

which is manifestly different. The Alexander polynomial is incapable of distinguishing these two knots, and no trivial variation on it can do any better. Similarly the Jones polynomial for the reef is obviously different from the Jones polynomial for the granny. Where classical knot theory had to push itself to the limit, the Jones polynomial worked effortlessly. It was totally new, and extremely powerful. The more people looked at it, the stranger it became: it wasn't the least tiny bit like classical knot invariants. What, precisely, *was* it? And was there anything else out there?

The race was on.

The HOMFLY Buzzes

Every so often a scientific discovery is made independently by several people at the same time. Few cases of multiple discovery are quite as striking as the one that happened in October 1984. Within

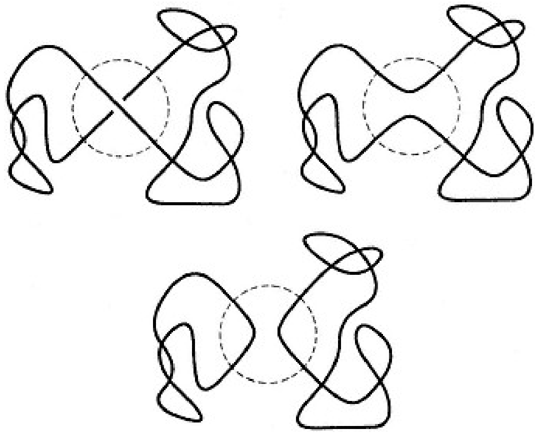


Fig. 16.
Surgery on a knot.

a period of a week or so, no less than four independent groups in Britain and the United States obtained the same generalization of the Jones polynomial, and submitted announcements to the same journal, the *Bulletin of the American Mathematical Society*. The papers were 'A new invariant for knots and links' by Peter Freyd and David Yetter; 'A polynomial invariant of knots and links' by Jim Hoste; 'Topological invariants of knots and links' by Raymond Lickorish and Kenneth Millett; and 'A polynomial invariant for knots: a combinatorial and an algebraic approach' by A. Ocneanu. As the editors of the journal explained in a half-page footnote, they felt that any attempt to decide priority would be rather pointless; moreover, 'there is enough credit for all to share in'. They persuaded the six authors to combine their results into a single paper, published in April 1985.

In 1970 John Conway had discovered a very quick way to calculate Alexander polynomials, totally different from any classical method. Suppose K, L, and M are three links, that agree except inside a small disk, and differ inside the disk in three possible ways: the strands cross like ×, run horizontally like =, or vertically like ||. Let their Alexander polynomials be $\Delta(K)$, $\Delta(L)$, and $\Delta(M)$, and let t be the variable in the polynomial. Conway proved that

$$\Delta(K) - \Delta(M) + \left(\sqrt{t} - \frac{1}{\sqrt{t}}\right)\Delta(L) = 0.$$

By using this relation repeatedly, it is possible to reduce the calculation of $\Delta(K)$ to combinations of Alexander polynomials of much simpler knots. In this way you can systematically build up a table of Alexander polynomials.

Jones found a very similar formula for his polynomial $V(K)$. The six topologists made the simultaneous discovery by starting from this property of the Jones polynomial, and introducing a new variable into the equation. The resulting polynomial involved two variables rather than one, but nobody minded that. There were technical snags that had to be overcome to obtain a proof that the new variation really worked. Each of the original four papers tackled these technical problems in a different way. The result is often called the *HOMFLY* polynomial (HosteOcneanuMillettFreydLickorish

Yetter). It was the first of many: today there are a dozen or more new knot polynomials, each with its own special features; and people are still looking for even bigger and better ones. The great puzzle, in retrospect, is why nobody had discovered any of these polynomials before. This doesn't detract from Jones's achievement: one of the greatest contributions a mathematician can make is to spot something so simple and powerful that everybody else has missed it.

State Models

This combinatorial approach to the Jones polynomial makes it look like a fortunate accident. However, something that beautiful can't possibly be just a coincidence. Is there a way to approach the Jones polynomial that makes its topological invariance inevitable and obvious? Can we find out what it *really* is?

Jones, early on, noticed an odd connection with statistical mechanics, a subject that on the face of it has nothing at all to do with knots. It arose from physicists' attempts to understand bulk matterthe nature of gases, liquids, and solids. Matter consists of huge numbers of atoms, rushing around at random and bouncing off each other. At low temperatures they stay pretty much in the same place, and combine together to form a stable solid; as they warm up, they start to move around more and you get a liquid; when they get really hot, they fly about all over the place and you get a gas. What is not obvious from this description is that the *phase transitions*changes of state from solid to liquid or from liquid to gasdo not occur gradually, but suddenly, at very specific temperatures. Imagine warming up a bucket of ice, very slowly, so that the ice has plenty of time to melt without the temperature changing much. The ice does not start to melt around 5°C, finishing the job by the time the temperature has reached +5°C: it changes completely from solid to liquid at a single temperature, 0°C.

One way to tackle this phenomenon mathematically is using state models. These are geometric arrangementsusually latticesof *sites*. Each site can exist in one of several states; and it affects the states of its neighbours according to specific rules. Another important phase

transition is magnetism: if you heat a magnet, it loses all magnetism at a well-defined temperature. State models are more easily understood if we think of magnetism. Imagine a checkerboard full of tiny magnets, some pointing up, some down, and each interacting with its close neighbours. Suppose the interactions are stronger at lower temperatures, so that neighbouring magnets tend to line up with each other. Then at low temperatures you will get a well-defined direction for the magnetism; but at high temperatures, the magnets point every which way and cancel each other out. There are many such models, and a small number have a remarkable property: you can solve the equations for them exactly and explicitly.

In 1971, long before Jones began his work, H. N. V. Temperley and E. H. Lieb found a connection between two different types of exactly soluble model in statistical mechanics, called Potts models and ice models. In order to establish the connection they had to find a collection of matrices that obeyed particular algebraic rules, determined by the structure of the models. The rules happen to be identical with those of the von Neumann algebras investigated by Jones. In statistical mechanics they are called TemperleyLieb algebras; and they provide a link between that subject and the new polynomials.

In particular, a knot diagram can be thought of as a state model. Its sites are its crossings, the interactions are determined by the geometry of the knot that joins them. The state of the knot at a crossing is the choice of local structure. This can be of several kinds: overpass on top, overpass underneath (analogous to switching the direction of magnetization at that site), or the overpass can be cut in half and joined to the underpasses to eliminate the crossing altogether. The strange coincidence of the TemperleyLieb algebras suggests that we may hope to interpret the Jones polynomial in statistical-mechanical terms.

By 1987, following this line of thought, Yasuhiro Akutsu and Miki Wadati did all this, and a lot more: they invented new knot invariants. They remark that 'In most cases where such an interaction between the two fields [of mathematics and physics] occurs, mathematics is usually applied to physics. In this paper, we shall reverse this order.' They then took an idea from physics, the Boltzmann

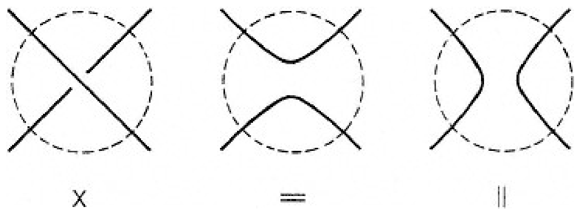


Fig. 17.
Breaking up a crossing.

weights of a soluble vertex model, and interpreted it in terms of links, to obtain a new topological invariant.

In the same year Louis Kauffman found a statistical-mechanical interpretation of the original Jones polynomial. It begins with yet another new polynomial, which he calls the *bracket polynomial K* of the knot *K*. This is defined by three rules:

Rule 1 The bracket of the unknot is 1.

Rule 2 If a separate unknotted component is added to a given link then the bracket multiplies by a variable *d*.

Rule 3 Select any crossing × in a knot and break it apart in two possible ways = or ||. Then the bracket of the original knot is *A* times that of the result on replacing × by =, plus *B* times that of the result on replacing

× by ||.

Rule 3 is yet another variation on Conway's formula for the Alexander polynomial. Briefly,

$\langle \times \rangle = A \langle = \rangle + B \langle || \rangle.$

Here *d*, *A*, and *B* are three independent variables.

The choices of 'splitting' at a crossing, ×, =, or ||, can be interpreted as the three 'states' of a statistical-mechanical state model. Kaufmann then derives a formula for his bracket polynomial, as the sum over all states of certain expressions in *A*, *B*, and *d*. In the statistical-mechanical analogy, it is the partition function of the

system, a physical quantity from which it is possible to compute thermodynamic variables such as free energy or entropy.

The trick now is to choose *A*, *B*, and *d* to get a topological invariant. By studying the effect of basic moves on the knot diagram, Kaufmann showed that the choice

$B = A^{-1}, d = A^2 A^{-2}$

makes his bracket polynomial invariant under pokes, unpokes, and slides. However, it is *not* invariant under twists or untwists: it multiplies by A^3 or by A^{-3}. As it happens, there is a second and independent quantity from classical knot theory, the *twist number* or *writhe*, that behaves in exactly the same way. It is a sum over all crossings of terms ±1, defined according to whether the overpass slants from top left to bottom right or from bottom left to top right. Kaufmann played the writhe off against the bracket to get something that is invariant under all basic moves. If the bracket is multiplied by $(A)^{-3w}$, *w* being the writhe, then the result is also invariant under twists and untwists. This modified bracket is thus a topological invariant of knots. It turns out to be a trivial variation on the Jones polynomial. So Kaufmann had found a statistical-mechanical interpretation, in terms of state models, of the Jones polynomial.

Biological Links

The new polynomials aren't just intellectual games for mathematicians: they're important in other areas of science, perhaps the most surprising one being molecular biology. Nearly four decades ago James Watson and Francis Crick discovered the 'secret of life', the double-helix structure of the DNA molecule, the backbone on which genetic information is stored and manipulated. The DNA helix is like a two-stranded rope, with each strand twisted repeatedly around the other. When a cell divides, the genetic information is transferred to the new cells by splitting the strands apart, copying them, and joining the new strands to the old in pairs. Anyone who has tried to separate the strands of a long piece of rope knows how tricky this process is: the strands tangle in lumps as you try to pull them apart.

In fact DNA is much worse: the helices themselves are 'supercoiled', as if the rope itself has been wound into a coil. Imagine several kilometres of fine thread stuffed into a tennis-ball and you have some idea of how tangled the DNA in a cell must be.

The genetic biochemistry must ravel and unravel this tangled thread, rapidly, repeatedly, and faultlessly; the very chain of life depends upon it. How? Biologists tackle the problem by using enzymes to break the DNA chain into pieces, small enough to investigate in detail. A segment of DNA is a complicated molecular knot, and the same knot can look very different after a few tucks and turns have distorted its appearance.

Until recently biologists had no systematic method for distinguishing these tangled structures, or for working out the associated chemical reactions. The Alexander polynomial, for example, just isn't effective enough. But the new polynomial invariants of knots have opened up new lines of attack, and the topology of knots is becoming an important practical issue in biology. A recent discovery is a mathematical connection between the amount of twisting in the DNA helix and the amount of supercoiling.

Jones's original motivation, in theoretical physics, had no apparent connection with knot theoryand even less with biology. Mathematics is the ultimate in 'technology transfer': a piece of mathematical insight, no matter how it is gained, can be exploited in any area of application that involves the appropriate structures. As the Jones saga shows, original mathematical ideas come from a rich and imaginative environment in which different points of view can interact; they tend not to arise from more specific 'goal-oriented' programmes.

YangMills Gauge Fields

Biology is not the only area of science to benefit from our renewed understanding of knots. Quantum physics is also a beneficiaryand a major source of insight. It is two-way traffic.

The connection between knots and quantum mechanics involves so-called *gauge fields*. The fundamental idea of a gauge field is *symmetry*.

For a long time, physicists have known that symmetries are immensely important in quantum theory. The biggest consumers of group theory today are quantum physicists. The idea is that symmetries of the physical system are reflected in its equations. For example, since any point in Euclidean 3-space looks the same as any other point, then the equations of physics should be invariants under translations, rotations, and reflectionsthe group of all rigid motions of 3-space.

But in addition there are non-geometric symmetries. If all the charges associated with a system of particles are reversed in sign, from positive to negative and vice versa, then the result is another physically possible system of particles. So here we have *charge-reversal* symmetry. Other symmetries have more bizarre effects, 'rotating' a neutron into a proton through some imaginary space of quantum numbers.

The geometric symmetries of space are *global* symmetries, which must be applied consistently throughout the entire physical theory. But the symmetry transformations can also be applied in different ways at different points. If the changes due to these transformations can be compensated by some physical field, we have a *local* *symmetry*. Suppose a number of electrically charged particles are distributed in space, and we compute the electric field. Now move the charges and compute the field again: the result does *not* respect any symmetry. But if we add in a magnetic field as well, then the combined system of electrical and magnetic fields can be transformed in such a way that the symmetry is maintained. So the system of two interconnected fields, described by equations discovered by Maxwell in 1868, has an exact local symmetry.

In quantum mechanics the quantities involved are complex numbers, not real numbers: they have a phase as well as a magnitude. If changing the phase (rotating the complex plane) is a local symmetry, then the system is said to be *gauge-invariant*. The name, which is not particularly evocative, goes back to Weyl in the 1920s. His idea was that physical theories should not depend on the unit of length: the original German can equally be translated as 'calibration-invariant', that is, independent of scale. Weyl's theory didn't work, but it would have if he'd used quantum-mechanical phase instead of scale. Yang

and Mills developed a gauge-invariant theory for the isotopic spin symmetry, analogous to Maxwell's electromagnetic theory. It is more complicated mathematically, because the group of local symmetries is not as nice as it is in electromagnetism.

During the 1970s and early 1980s physicists studied YangMills fields. Meanwhile the mathematicians, unaware of this, studied what they called 'connections' on 'fibre bundles'. Eventually it turned out that these were pretty much two names for the same thing, and neither subject has been the same since. There has been spin-off in physics, for example methods for solving the YangMills equations using algebraic geometry and topology; and in mathematics. Donaldson's work on 4-dimensional manifolds is just one of the astonishing results to have emerged; others include deep connections between classical algebraic geometry and partial differential equations. The entire area is undergoing an explosive expansion and is without doubt one of the most exciting developments in twentieth-century mathematics.

Atiyah's Challenge

Most people seem to think that once you have solved a problem, that's it. It's all over. Mathematicians, though, are seldom satisfied with mere answers; they have an ingrained habit of using the solutions as a springboard for asking new questions. If a calculation miraculously works out with a clean answer, instead of grinding to a halt in the usual morass, most people would breathe a sigh of relief and accept the miracle. Mathematicians, ever suspicious, always want to take the miracle to bits and see what makes it tick. There are good reasons for doing this. Sometimes you find out that it wasn't a miracle at all, but a mistake. Sometimes you find a better way of working miracles. And so it is with the Jones polynomial. You might imagine that mathematicians would be more than satisfied with the amazing series of discoveries that followed on Jones's heelsbut of course they aren't. They want more.

There is one curious feature of knot theory in the post-Jones era. A knot, by definition, is an intrinsically three-dimensional animal.

But the Jones polynomial and everything that followed it is obsessed with two-dimensional pictures of knots, diagrams in the plane, crossings, overpasses. A truly three-dimensional knot shouldn't 'know about' such things. If you were a knot, you wouldn't recognize that you possessed crossings, any more than we humans go around thinking 'my shadow is a funny shape today'.

At the Hermann Weyl Symposium in 1988, Sir Michael Atiyah laid down two challenges to physicists working in the area of quantum field theory. One was to give a physical interpretation of Donaldson's work on fake four-space. The other was to find an intrinsically three-dimensional approach to the Jones polynomial. Atiyah had chosen well. He knew that if any area of physics would provide answers to these questions, it had to be quantum field theory. There were clues, or bits of clues, all over the placein the physics, in the mathematics.

One of the important consequences of Donaldson's work is the discovery of new topological invariants for three- and four-dimensional manifolds. Mathematically they are highly unorthodox, but from a physical point of view they can be rationalized after the event as a natural extension of more classical ones. The classical invariants can be interpreted in terms of the solutions to Maxwell's equations for electromagnetism. Solve Maxwell's equations on a given manifold, then play games with those solutions, and out pop the classical invariants (the second homology group and its intersection form, if you want to know). Donaldson's central insight was to replace Maxwell's equations by the YangMills equations, and then play the same sort of game, only more cleverly. In this sense, Donaldson theory was always tied to physics. However, Atiyah was asking whether it is connected with one of the 'hottest' areas of physical research, quantum field theory.

Witten's World

In 1988 Edward Witten found just such a connection, which he called topological quantum field theory. By 1989 he had pushed the same ideas further, and the Jones polynomial was firmly in his sights.

From the start, the Jones polynomial was closely related to problems in two-dimensional physics (either two of space or one of space and one of time), such as state models, the YangBaxter equation, and TemperleyLieb algebras. The biggest problem was to impose some kind of order on this unruly area.

A quantum understanding of Donaldson theory involves constructing quantum field theories in which the observables are topological invariants. Witten suggests a loose analogy with general relativity which both explains this point, and reveals the differences involved in the quantum approach. Recall that in general relativity space-time is considered to be curved, and gravitation is a side-effect of this curvature. There are two ways to work with a curved space. One is to use concepts 'intrinsic' to the space, concepts (such as the curvature itself) that do not depend upon a choice of coordinates. The other is to introduce a system of coordinates, and insist that the only meaningful concepts are those that are independent of the coordinate system. Thus, in our own universe it is meaningless to say that Alpha Centauri is two light years east and three light years to the left of the Earth; but fine to say that its distance from Earth is four-and-a-bit light years.

In general relativity, the usual way to proceed is to choose some arbitrary coordinate system, and produce coordinate-free invariants by a kind of 'averaging' over all possible coordinate systems. The coordinate system itself becomes a dynamical variable in the theory. This viewpoint has become ingrained in physics. The mathematicians' approach to the Jones polynomial is identical in spirit. Begin with some two-dimensional projection of the knot ('choice of coordinates') and then show that the answers you get are the same no matter which projection you use (the 'choice of coordinates' makes no difference to the answer). The projection becomes a 'dynamic variable' in the mathematical theory, and your main attention is focused on the significant changes that occur to this variable.

A quantum field theory in which all observables are topological invariants is a quantum field theory that does not depend on any choice of coordinates. By analogy with general relativity and post-Jones knot theory, it would appear that the way to achieve this is to

start with some set of coordinates and 'average' over all possible choices. This is technically difficult, because the spaces used in topology are much more flexible than those of general relativity. In general relativity you can bend spaces, so to speak, but not stretch them; in topology, you can do both, and more. Witten's idea was to go against the conventional wisdom, and work intrinsically from the beginning.

I can't go into the details here: you need to be a physicist and a topologist to appreciate them. The main consequence is that there is a formulaonce again, a version of the partition function of statistical mechanics, but now in quantum dresswhich is defined using a quantum gadget known as a Feynman path integral. This formula is 'obviously' topologically invariant, because everything that you can sensibly write down in Witten's theory *has* to be topologically invariant. The invariance, you recall, is put in right at the beginning. Buried in the partition function of a link is a whole bunch of invariantsthe Jones polynomial and its generalizations. The formula works with the link itself, embedded in three-space, independent of any two-dimensional diagram or representation, so it answers Atiyah's challenge.

As a bonus, Witten's approach solves another awkward problem: how to generalize the Jones polynomial to knots that are tied not in ordinary three-space, but in an arbitrary three-dimensional manifold. Just use the same partition function, but define it on the three-manifold rather than on Euclidean three-space. At this point you can even throw the knots away! The result is a set of new topological invariants for three-manifolds, pure and simple.

Witten's theory wraps everything up in a single package: Jones polynomials, braid groups, state models, YangMills equations, Donaldson theoryeverything. It offers a totally new, 'intrinsic' way to think about topology in spaces of three and four dimensions. It also provides new insights into quantum field theory, whereby topological ideas can be used to solve physical problems. Within five years of Jones's discovery, the post-Jones era of knot theory has given way to the post-Witten era of low-dimensional topology. Nobody knows where all this will end, but it sure is exciting.

11
Squarerooting the Unsquarerootable

The Divine Spirit found a sublime outlet in that wonder of analysis, that portent of the ideal world, that amphibian between being and not-being, which we call the imaginary root of negative unity.
Gottfried Leibniz

The seventeenth century witnessed a new flowering of algebra, with the solution of cubic and quartic equations as the centrepiece. But still nobody was sure what a *number* was. Negative numbers were still causing headaches in Europe, even though the Hindu Brahmagupta had dealt satisfactorily with them in AD 628. In the mid-1600s Antoine Arnauld argued that the proportion 1 : 1 = 1 : 1 must be nonsense: 'How can a smaller be to a greater as a greater is to a smaller?' The dangers of verbal reasoning in mathematics could hardly be plainer; yet in 1712 we find Leibniz agreeing that Arnauld had a point. However, Thomas Harriot (15601621) worked with negative numbers, and Raphael Bombelli (152673) even gave clear definitions. Simon Stevin and Albert Girard treated negative numbers as being on a par with positive ones.

But even Brahmagupta couldn't swallow *square roots* of negative numbers; so the sparks were bound to fly when, as Morris Kline puts it, 'without having fully overcome their difficulties with irrational and negative numbers the Europeans added to their problems by blundering into what we now call complex numbers'. Thus in 1545 we find Girolamo Cardano, one of the great scoundrels of mathematics and a genius to boot, solving x(10 x) = 40 and emerging with the solutions $5+\sqrt{-15}, 5-\sqrt{-15}$. 'Putting aside the mental tortures

involved', says he, substitute the supposed answer in the equation. Lo and behold, you find that

$$\begin{aligned} x(10-x) &= (5+\sqrt{-15})(5-\sqrt{-15}) = 5^2-(\sqrt{-15})^2 \\ &= 25-(-15) = 40 \end{aligned}$$

as required. 'So', says Cardano, 'progresses arithmetic subtlety, the end of which is as refined as it is useless.'

A phrase that should haunt mathematicians is: 'it doesn't make sense but it works.' Coincidences like Cardano's are akin to the visible tracks left in the jungle by some passing creature: a sign that some wondrous discovery lies in wait for a sufficiently intrepid adventurer. So *does* 1 have a square root? If so, what manner of beast is it? Why should practical minds concern themselves with such otherworldly intellectual frippery?

Complex Calculations, Real Results

Cardano's manipulations occurred in his *Artis Magnae*, a celebrated work summing up Renaissance algebra, some of it pinched from others and published with due acknowledgement but without their permission. In the same book he noticed that Tartaglia's formula for the cubic (see Chapter 1) can lead into murky waters. Applied to the equation $x^3 = 15x + 4$ it yields the answer

$$x = \sqrt[3]{2+\sqrt{-121}} + \sqrt[3]{2-\sqrt{-121}}.$$

Since the obvious answer is $x = 4$, something is clearly rotten in the state of Bologna. Bombelli, who appears to have had an unusually clear head, observed that (again putting aside any qualms)

$$(2 \pm \sqrt{-1})^3 = 2 \pm \sqrt{-121},$$

so Cardano's expression becomes

$$x = (2 + \sqrt{-1}) + (2 - \sqrt{-1}) = 4,$$

and the idiotic formula is actually telling us something sensible after all. Bombelli had the first inkling that what we now call *complex* numbers can be useful for getting real results.

Descartes in 1637 called expressions involving square roots of negative numbers 'imaginary', and took their occurrence as a sign that the problem was insoluble. Isaac Newton shared the same opinion. So much for Bombelli. Nevertheless, mathematicians couldn't keep their sticky fingers off the goodies. Having floundered in the quagmire of algebra they plunged headlong into the flaming pit of analysis. John Bernoulli, trying to integrate functions like $1/(x^2 + 3x + 1)$, stumbled over *logarithms* of complex numbers. By 1712 Bernoulli and Leibniz were engaged in a ding-dong battle over a far simpler problem, logarithms of negative numbers. Bernoulli argued that since $d(x)/x = dx/x$ then integration shows that $\log(x) = \log(x)$. Leibniz countered that the integration only works for positive x. Euler said they were both wrong, because integration requires an arbitrary constant, so the correct conclusion is that $\log(x) = \log x + c$ for some constant c, obviously equal to log 1, whatever *that* is. All was grist to Euler's mill, which in 1748 ground out the miraculous formula

$$e^{i\theta} = \cos\theta + i\sin\theta$$

where $i = \sqrt{-1}$. Actually it wasn't new: Roger Cotes had found much the same result in 1714, but this had been overlooked. Putting $\theta = \pi$ we get $e^{i}\pi = 1$, whence $\log(1) = i\pi$. However, by cubing we also find that $e^{3i}\pi = 1$, so $\log(1) = 3i\pi$. A lesser man would have given up in disgust, but Euler found a loophole: log x has many values. If L is one of them, then the others are $L \pm 2\pi i$, $L \pm 4\pi i$, $L \pm 6\pi i$, and so on. The jungle creature was no longer breaking just the odd twig, it was laying waste to great swathes of forest; more a herd of rhino than a squirrel. But could it be trapped?

Plane Thinking

John Wallis, in his *Algebra* of 1673, interprets a complex number as a point in the plane, extending the usual picture of the reals as a line. In fact $x + iy$ is obtained by measuring a distance y at right angles to the real line, from the point x. The fact that 1 has no *real* square root is no longer an obstacle in Wallis's picture, because $\sqrt{-1}$ isn't

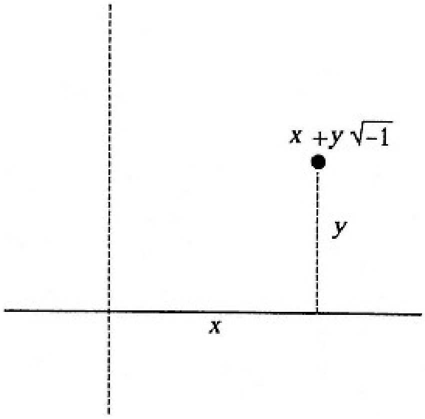


Fig. 18.
Wallis's proposal for visualizing complex numbers.

on the real line! It's one unit sideways into the plane. 'Lateral thinking' with a vengeance. But for some reason Wallis's proposal was ignored. In 1797 the Norwegian surveyor Caspar Wessel published a paper showing how to represent complex numbers in the plane. It was in Danish and nobody noticed it either, until a French translation appeared a century later. Meanwhile the idea became attributed to Jean-Robert Argand, who wrote about it independently in 1806. By 1811 Gauss was habitually thinking of complex numbers as points in a plane, and he published his ideas in 1831. Finally in 1837 William Rowan Hamilton took the last step, identifying $x + iy$ with its coordinates (x, y) and rewriting the geometric definitions in algebraic form. Thus $(x, 0)$ is identified with the real number x, $(0, 1)$ is i, and we have

$$(x, y) + (u, v) = (x + u, y + v)$$

$$(x, y)(u, v) = (xu\ yv, xv + yu).$$

Then

$$i^2 = (0, 1)(0, 1) = (1, 0) = 1$$

and everything falls sweetly into place. Whenever anyone made a discovery in the mid-1800s it turned out that Gauss had already thought of it, but omitted to tell anyone; and sure enough Gauss had anticipated Hamilton in 1832.

It had now taken some three centuries to reach an answer that can be explained in three minutes. The answer had been *found* several times but not recognized as such. Moreover, everyone who *was* anyone already *knew* what the answer was. Morris Kline notes that obviously 'many menCotes, De Moivre, Euler, Vandermondethought of complex numbers as points in the plane' because when they tried to solve x^n 1 = 0 they imagined the solutions as the vertices of a regular *n*-sided polygon. As if to confirm that everybody knew the answer anyway, we observe the reaction to Hamilton's incisive solution. Did they cheer? Did they roast a hundred oxen? Did they at least buy him a drink? They did not. It was dull, boring, 'old hat'. But even if everyone had known the answer all along, it was only in about 1830 that they started to admit it, even to themselves. What caused the change of heart?

Evaporating Philosophy

All the while complex numbers were useless, people thought about them as a problem in philosophy. That meant they had to be invested with some deep and transcendent *meaning*. Thus we find Leibniz making statements like the one that decorates this chapter. For philosophers nothing is better than some obscure but mysterious idea that nobody really cares about and certainly can't test, because then you have plenty of room for clever arguments. Angels on the head of a pin, and so forth. But when something actually becomes useful, most people stop arguing about the philosophy and get on with the job instead. They don't care what the deep philosophical essence of the new gadget is; they just want to churn out as many results as they can in the shortest possible time by taking advantage of it. If

you can actually *see* the angels dancing on the head of the pin you stop trying to count them in favour of persuading them to dance on a microchip instead. And that's exactly what happened to complex numbers between about 1825 and 1850. The mathematicians discovered complex analysishow to do *calculus* with complex numbers. And that turned out to be so powerful that it would all have been dreadfully embarrassing had some ingenious but unwary philosopher proved that complex numbers don't really exist. Philosophical questions ('What *is* this stupid thing?') can sometimes be excuses for *not* getting on with the job of developing an elusive idea. Overnight the complex number concept became so useful that no mathematician in his right mind could possibly ignore it. So the question mutated slightly, into 'What can you *do* with complex numbers?', and the philosophical question evaporated. Unmourned, unnoticed, forgotten, buried. There are other cases of this nature in the history of mathematics but perhaps none more clear-cut. As time passes, the cultural world-view changes. What one generation sees as a problem or a solution is not interpreted in the same way by another generation. Today, when the 'real' numbers are seen as no less abstract than many other number systems, complex numbers included, it's hard to grasp how different it all looked to our forebears. We would do well to bear this in mind when we think about the development of mathematics. To view history solely from the viewpoint of the current generation is to court distortion and misinterpretation.

Complex Calculus

Unlike the gradual and painful emergence of complex algebra, the development of complex analysis seems to have been a direct result of the mathematician's urge to generalize. It was sought, deliberately, by analogy with real analysis. The theory of limits, differentiation, and power series carry over so easily that they can almost be set as an exercise for a student. The definition of integration carries over easily too; but its properties do not. There is a new, and fruitful, phenomenon. In real analysis, when one integrates a function between two

limits, then the limits fully specify the integral. But in the complex case, with the limits represented as points in the plane, there is more freedom. We have to specify a curve linking them, and then 'integrate along the curve'. Since there are lots of possible curves, we need to know to what extent the value of the integral depends on the curve chosen.

In a letter of 1811 to Friedrich Bessel, Gauss (who else?) shows that he knew the theorem that lies at the core of complex analysis. 'I affirm now that the integral has only one value even if taken over different paths provided *(x)* does not become infinite in the space enclosed by the two paths. This is a very beautiful theorem whose proof I shall give on a convenient occasion.' But the occasion never arose. Instead, the crucial step of publishing a proof was taken in 1825 by the man who was to occupy centre stage during the first flowering of complex analysis: Augustin-Louis Cauchy. After him it is called Cauchy's Theorem. In Cauchy's hands the basic ideas of complex analysis rapidly emerged. For example, a differentiable complex function has to be very specialized, much more so than in the real case: its real and imaginary parts must satisfy the so-called CauchyRiemann equations. Gauss's observation was given a proper proof. If an integral is computed along a path which violates the conditions of the theorem and winds round points where the function becomes infinite, then Cauchy showed how to calculate the integral using the 'theory of residues'. All that's needed is a certain constant, the *residue* of the function at the nasty point, together with a knowledge of how often the path winds round it. The precise route of the path doesn't actually matter at all!

Cauchy's definitions of analytic ideas such as continuity, limits, derivatives, and so on, were not quite those in use today. Full rigour was contributed by Weierstrass, who based the whole theory on power series. The geometric viewpoint was sorely lacking, and Riemann remedied this defect with some far-reaching ideas of his own. In particular the concept known as a *Riemann surface*, which dates from 1851, treats many-valued functions (such as Euler's logarithm) by making the complex plane have multiple layers, on each of which the function is single-valued. The crucial thing turns out to be the topological pattern whereby the layers join up.

From the mid-nineteenth century onwards the progress of complex analysis has been strong and steady with many far-reaching developments. The fundamental ideas of Cauchy remain, now refined and clothed in more topological language. The abstruse invention of complex numbers, once described by our forebears as 'impossible' and 'useless', has become the backbone of mathematics, with practical applications to aerodynamics, fluid mechanics, Quantum Theory, and electrical engineering. Space forbids a detailed survey, but we can capture some of the flavour by looking at two of the most notorious open problems in the area: the Riemann Hypothesis and the Bieberbach Conjecture.

The Riemann Hypothesis

Let $\pi(x)$ denote the number of primes less than x. This is a very irregularly behaved function. But Gauss and others noticed that for large x it is approximately equal to $x/\log x$. This Prime Number Conjecture combined the discrete (primes) and the continuous (logarithms) in an unexpected way, and it is no surprise that it gave rise to some spectacularly original mathematics. The answer turns out to depend on properties of the *zeta function*, defined by

$$\zeta(s) = 1 + 1/2^s + 1/3^s + \dots.$$

The relevance to primes was known to Euler, who noticed that

$1/\zeta(s) = (1 - p_1^{-s})(1 - p_2^{-s}) \ldots$

where p_n is the nth prime. This formula is not quite as remarkable as it might appear: it's the analyst's way of expressing the existence and uniqueness of prime factorization. But it brings the Big Gun of nineteenth-century mathematics, complex analysis, to bear.

By using the zeta function for real s, Pafnuti Chebyshev in 1852 showed that something close to the Prime Number Conjecture is true: the ratio of $\pi(x)$ and $x/\log x$ lies between 0.922 and 1.105 for large enough x. Bernhard Riemann showed how to define $\zeta(z)$ for all complex z in 1859, by a process called analytic continuation, and tried to prove the conjecture. He realized that the key was to

understand the *zeros* of the zeta function, the solutions of $\zeta(z) = 0$. The real zeros are easy to find; they are 2, 4, 6, and so on. Riemann's famous hypothesis, stated quite casually in a short note containing no proofs that sketched out most of the main ideas of the area, is that all the rest are of the form $\frac{1}{2} + iy$, that is, lie on the line 'real part = ½'. It is generally considered to be *the* outstanding problem in mathematics. It has been shown by J. van de Lune and Herman te Riele that the first 1.5 billion zeros lie on the right line, but for various reasons this is less than compelling evidence. A related conjecture due to Franz Mertens, with similarly strong evidence, was proved *false* in 1984 by Andrew Odlyzko and te Riele. Their proof involved computing the first 2,000 zeros of the zeta function to 100 decimal places. If the Mertens Conjecture had been true, it would have implied the Riemann Hypothesis. The experts weren't entirely surprised that it turned out to be false, because the

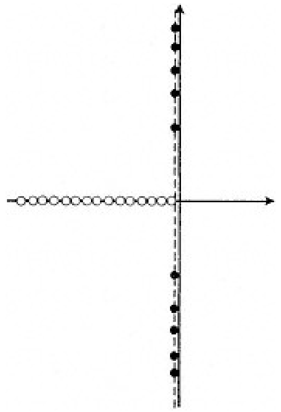


Fig. 19.
The first few zeros of the zeta function. Open circles show the 'trivial' zeros at 2, 4, 6, Black dots show the remaining zeros, whose real part is conjectured to always be ½.

proof of the Riemann Hypothesis that would result somehow looked 'too easy' to be plausible.

In 1896 Hadamard used the zeta function to prove the prime number theorem. De La Vallée Poussin independently did the same in the same year. If the Riemann Hypothesis is true, the results can be improved by giving a sharp bound on how big the *error* in replacing $\pi(x)$ by $x/\log x$ is. A great many details in the theory of the distribution of the primes depend on the Riemann Hypothesis.

The zeta function can be greatly generalized. For example, given an algebraic number field, you can use Euler's formula (with prime *ideals* in place of prime numbers) to define its zeta function. There is an appropriate analogue of the Riemann Hypothesis, which is important throughout algebraic geometry and Diophantine equations. In 1985 there was a flurry of publicity for an announced proof of the Riemann Hypothesis, along these more abstract lines. This announcement was premature, and the zeta function retains its secrets. Fame, fortune, and many sleepless nights await whoever uncovers them.

The Bieberbach Conjecture

Geometric function theory, as its name suggests, studies the geometry of transformations of the complex plane defined by analytic functions. It has its origins in the discovery that these transformations are *conformal*, that is, do not change the angles between curves. Let the *unit disc* be the set of all complex numbers z whose distance from the origin is less than 1, and let (z) be a complex function. The can be thought of as a way of deforming the disc, moving each z to (z). The deformed image of the disc may overlap itself, but there is an important class of functions for which it does not. Such a function is said to be *univalent* on the unit disc. At this point the hint of a connection between topological and metric properties arises. The larger the image of the disc, the larger in some sense the function is. The largest univalent function should transform the disc into the largest possible image, namely the entire complex plane. Does this statement make any kind of sense, and if so, what is this largest univalent function? An attractive guess is the Koebe function

$z/(1\ z)^2$, because this does have the largest possible image. Its power series expansion is

$z + 2z^2 + 3z^3 + + nz^n +$

and it occurred to Ludwig Bieberbach in 1916 that its extremality might be reflected in its power series. If so, the nth coefficient in the power series of a univalent function should be no larger than n. In other words, metric extremality follows from geometric extremality. It's an appealing, natural, and simple idea; the sort of thing that it ought to be possible to decide one way or the other without much trouble. In the event, the problem remained unsolved until 1984, when Louis de Branges obtained a miraculous proof.

Until that point the Bieberbach Conjecture was known to be true only for the first six coefficients in the power series. This was proved by Bieberbach himself for the second coefficient, by Karl Löwner in 1923 for the third, by P. R. Garabedian and M. Schiffer in 1955 for the fourth, by R. N. Pederson and M. Ozawa in 1968 for the sixth, and by Pederson and Schiffer in 1972 for the fifth.

De Branges's proof for *all* coefficients is obtained by way of two related conjectures. The first, due to M. S. Robertson in 1936, refers to power series univalent in the unit disc and containing only odd powers of *z*. The Robertson Conjecture implies the Bieberbach Conjecture. In 1971 I. M. Milin found (but couldn't prove) an inequality which implies the correctness of the Robertson Conjecture. It is this Milin Conjecture that de Branges proves: the Robertson and Bieberbach Conjectures follow immediately.

Löwner's successful attack on the third coefficient is based on a differential equation whose solutions approximate *any* univalent function. It describes how points in the disc 'flow' towards their images under the function, and it is possible to 'push information along the flow'for example, estimates of the size of power series coefficients. De Branges's proof introduced auxiliary functions to hold the desired information. If these can be shown to have certain properties, then the Milin inequality follows. However, the combinatorics of the sequence of auxiliary functions rapidly becomes unmanageable. Walter Gautschi verified by computer that the desired

properties hold for all coefficients up to the 26th (implying the truth of the Bieberbach Conjecture in this range). Later he noticed that the general result follows from a known theorem of Richard Askey and George Gasper about special functions. Askey and Gasper did not have applications to the Bieberbach Conjecture in mind when they proved their theorem: it just seemed an interesting result to them. Most mathematicians think of the field of special functions as an unfashionable and rather pointless dead-end: for this and other reasons that view now seems questionable.

De Branges originally obtained his proof as the climax of a lengthy typescript of the second edition of his book *Square Summable Power Series*. His announcement was greeted with some scepticism. The proof contained errors: it was unclear whether these could be repaired. He had previously claimed a proof of the Bieberbach Conjecture which had turned out to be fallacious, and this damaged his credibility. The conjecture has a history of leading mathematicians astray. As Carl FitzGerald says, 'The Bieberbach Conjecture is not difficult; I have proved it dozens of times.' Acceptance came only after de Branges visited the Steklov Institute in Russia in mid-1984, and his proof was confirmed by the members of the Leningrad Seminar in Geometric Function Theory. The conclusions were written up for the Russian Academy of Sciences and a preprint circulated. At this point mathematicians in the West studied the simplified proof (now translated from the Russian!), made some further simplifications, and agreed that de Branges really had solved the problem. The proof has now been simplified by de Branges, Christian Pommerenke, FitzGerald, and others, and has been made very short and direct: one version of it occupies only five typescript pages.

The Exhausted Goldmine

We now know that the Bieberbach Conjecture has a short and simple proof, based on standard techniques in geometric function theory. Something very close to the successful assault must have been attempted by dozens, if not hundreds, of mathematicians. Why wasn't the proof found earlier? Perhaps everyone was being stupid? It's

easy to criticize with hindsight, but not very helpful. A more likely reason is the subtlety of the problem. The precise approach proves to be very delicate here, and without a great deal of previous work, partial solutions, and 'experimental' results, it is unlikely that anyone would stumble upon the particular combination of conditions needed to make the proof work. Another possibility is that most of these attempts were a bit half-hearted, because nobody seriously thought that the standard approach would do the job. Mathematicians habitually talk of certain methods as being 'worked out', in the mining sense that everything of value that can be obtained from them is already known. It is usually considered to be a waste of time to attack difficult problems by 'exhausted' methods. Yet here, it succeeded.

The answer may be related to the structure of mathematical proofs. As an analogy, consider a game of chess. Chess openings have been analysed by generations of grandmasters, in the hope of finding a novel line of attack and springing a trap on an opponent who finds himself in unfamiliar territory and under time pressure. But there are so many possible variations in chess that a complete analysis even up to a dozen or so moves is not feasible. There is still room for an original line of development, only a little off the beaten track. Nimzovich caused his opponents plenty of headaches by opening with a knight, a move so ridiculous that nobody had bothered to take it seriously. Now there are many more possible 'opening moves' in a mathematical proof than there are in chess. A line of argument may seem to be a bad opening simply because everybody who tried to play the game along that line followed the same pattern of obvious but poor moves, missing a brilliant variation that would crack the game wide open. Certainly it's unwise to write off a plausible line of attack just because nobody has made it work yet.

12
Squaring the Unsquarable

Mad mathesis alone was unconfined,
Too mad for mere material chains to bind,
Now to pure space lifts her ecstatic stare,
Now, running round the circle, finds it square.
Alexander Pope

One of the more annoying things that mathematicians do is cast doubt upon things that we imagine we understand perfectly well. For example, we all know what areas and volumes are. Don't we? I'm going to try to convince you that there's far more to areas and volumes than we normally imagine. The tale begins somewhere in the prehistory of the human race, and culminates in the dramatic solution in 1988 of a problem posed in 1925 by the mathematical logician Alfred Tarski. He asked whether it is possible to dissect a circular disc into finitely many pieces, which can be reassembled to form a square. Tarski could prove, with some difficulty, that if such a dissection exists, then the resulting square must have the same area as the circle; but he wasn't sure whether such a dissection was possible.

Hang onisn't it *obvious* that the resulting square must have the same area? The total area won't be affected by cutting the circle into pieces, will it? And isn't it *obvious* that you can't cut a circle up to get a square, because the curved edges won't fit together properly? No, it's not. Indeed, one of those two 'obvious' statements is false. The other is true, but not at all obvious, and its three-dimensional analogue is false.

Squaring the Circle

Tarski's problem is often compared to the famous problem of 'squaring the circle', attributed to the ancient Greeks; but actually it's very different. In the classical formulation, you must construct a square of area equal to that of a circle using ruler and compasses. As we've seen, this was proved impossible by Ferdinand Lindemann in 1882. But Tarski asks for a dissection, not a construction, and you can use any methods you like.

There's a fascinating mathematical toy, which can be made from card but looks best in polished hardwood and brass. It's made by hinging together four oddly-shaped pieces, and attaching handles. If the hinges are closed in one direction, the result is a square; if they are closed the other way, you get an equilateral triangle. This is an ingenious example of a type of puzzle that was very popular towards the end of the nineteenth century, when a lot of people thought that Euclidean geometry was a real fun thing. Such dissection puzzles ask for ways to cut up a given shape or shapes, so that the pieces can be reassembled to form some other shape or shapes. For example, can you dissect a square into a regular pentagon? A regular octagon? Both answers are yes (see Fig. 21). What *can* you dissect a square into? For that matter, what can't you?

For simplicity, suppose we restrict ourselves to straight line cuts. Then we always end up with a polygona shape with straight edges. It also seems clear that when you dissect a square, its area can't change. Later we'll have to re-examine that belief more critically, but for polygonal dissections it's correct. So, if a square can be dissected to form a polygon, then the polygon must have the same area as the square. Are *all* such polygons possible? The answer is 'yes'. This could have been proved by the ancient Greeks, but as far as we know, it wasn't. It is usually called the BolyaiGerwien theorem, because Wolfgang Bolyai raised the question, and P. Gerwien answered it in 1833. However, William Wallace got there earlier: he gave a proof in 1807.

Briefly, the argument runs like this. Begin by dividing the polygon into triangles. Dissect each triangle into a rectangle of the same

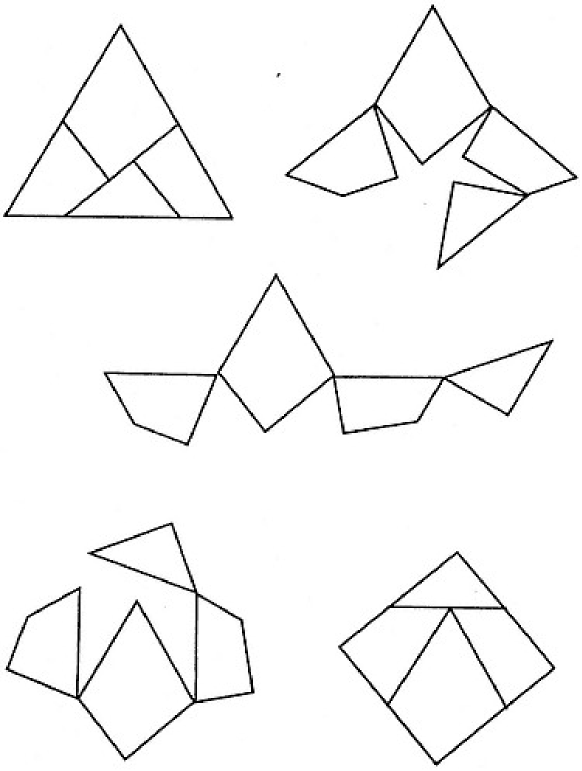
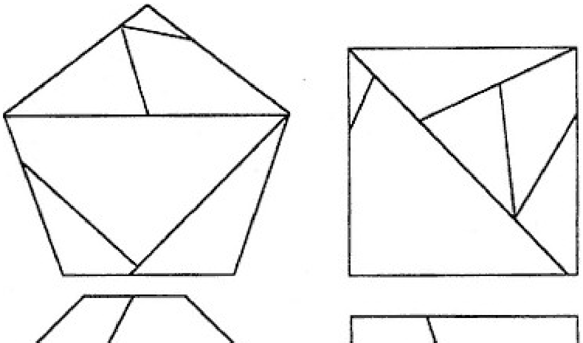


Fig. 20.
Dissecting an equilateral triangle into a square.



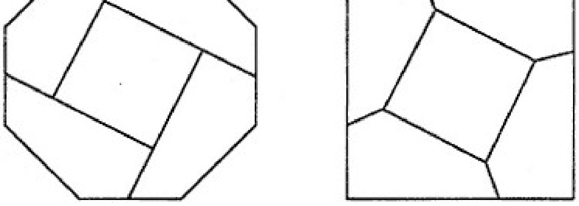


Fig. 21.
Dissecting a pentagon and an octagon into a square.

area. Convert each such rectangle into another rectangle, one of whose sides is that of the desired square (the square root of the area of the original polygon). Finally, stack the resulting rectangles together. The same method proves that any finite set of polygons can be dissected into any other finite set with the same total area.

Area, along with volume, is actually quite a subtle idea, as primary-school teachers know to their cost. It takes a lengthy process of experiment with scissors and paper, and vessels of various sizes, and water all over the floor, before children acquire the concept. In particular, the idea of conservationthat the amount of fluid you have doesn't depend upon the shape of the container that holds it

doesn't come naturally. Most adults can be fooled, by clever packaging, into thinking that they are buying more shampoo than they really are. One difficulty is that areas and volumes are most easily defined for rectangles and boxes: it's not so clear how they work for other shapes, especially ones with curves. We've all been taught that the area of a circle is πr^2; but have you ever wondered *why*? What does π, the ratio of circumference to diameter, have to do with 'how much stuff' there is inside a circle? How does that relate to 'how much stuff' there is inside a square? We saw in Chapter 5 that you can't do it with scissors.

Area and volume are such useful concepts that they really do deserve to be put on a sound logical basis. The BolyaiGerwien theorem isn't just a recreational curiosity: it's important because it justifies one possible method for defining area. Start by defining the area of a square to be the square of the length of its side, so that for instance a 3 cm square has an area of 9 cm^2. Then the area of any other polygon is defined to be the area of the square into which it can be dissected. This approach brings out two key properties of area:

Rigidity The area of a shape stays the same during a rigid motion.

Additivity If a number of shapes are joined without overlapping, the resulting area can be found by adding their individual areas.

Actually, there are two versions of additivity. The weaker one, Finite Additivity, applies to a finite number of pieces; the stronger one applies to infinitely many. The technique of integration, basic to the calculus, extends the property of additivity to shapes formed by joining together infinitely many pieces. The total area can then be defined as the sum of the infinite series formed by the areas of the pieces: this is Infinite Additivity. Because infinite series are tricky, this approach requires extreme caution. It is, however, unavoidable if we wish to assign areas to curved regions, such as a circle. A circle cannot be cut into finitely many triangles, but it can be cut into infinitely many. Pursuing a similar line of argument, Archimedes

proved that the area of a circle is the same as that of a rectangle whose sides are its radius and half its circumference. Because π is *defined* as the ratio of circumference to diameter, you can now see how it gets in on the act.

Hilbert's Hit-list

David Hilbert, one of the greatest mathematicians of all time, wondered whether an equally 'clean' approach to volume might be possible. Mathematicians already knew what volume *was*, of course: in particular they could calculate the volume of any pyramid (one-third the height times the area of the base). But again they wanted sound logical foundations. Hilbert asked whether any polyhedron (solid bounded by flat surfaces) can always be dissected into a finite number of pieces, and reassembled to form a cube of the same volume. In 1900, at the International Congress of Mathematicians in Paris, he listed the question among twenty-three major unsolved problems.

Unlike the other twenty-two, it didn't survive very long. In the following year Max Dehn, one of the founders of topology, found the answer. Surprisingly, it is 'no'. Dehn's proof uses the angles of the solid to define what is now called the *Dehn invariant*, a number that, like volume, remains unchanged under dissection and reassembly. However, solids of the same volume can have different Dehn invariants. For example, this happens for a cube and a regular tetrahedron. So you can't dissect a tetrahedron into a cube of equal volume. The Dehn invariant is the only new obstacle to dissectability in three dimensions, and the correct analogue of the BolyaiGerwien theorem is that polyhedra can be dissected into each other if and only if they have the same volume *and the same Dehn invariant*.

Though differing in this respect, area and volume share the basic features of rigidity and infinite additivity. A general theory of such concepts was developed by Henri Lebesgue, who called them 'measures', and devised the great grand-daddy of all such concepts, Lebesgue measure. Along the way, it transpired that sufficiently messy and complicated sets may not possess well-defined areas or volumes at all. Only the 'measurable' sets do. These include all of

the familar shapes from geometry, and a wide range of much more bizarre sets as well; but not everything. Measure theory is important for a variety of reasons: in particular, it is the foundation for probability theory.

Double Your Money

Tarski belonged to a group of Polish mathematicians who frequented the celebrated Scottish Café in Lvov. Another member was Stefan Banach. All sorts of curious ideas came out of bull sessions in the Scottish Café. Among them is a theorem so ridiculous that it is almost unbelievable, known as the BanachTarski Paradox. It dates from 1924, and states that it is possible to dissect a solid sphere into a finite number of pieces, which can be assembled, by rigid motions, to form two solid spheres *each the same size as the original*. (Banach and Tarski set no limit on the number of pieces required, but in 1928 John von Neumann stated without proof that nine pieces would do the job. By 1945 Wacław Sierpiński had reduced the number to eight. In 1946 Raphael Robinson reduced it to fiveone being a single point. The dissection cannot be performed with fewer than five pieces.)

This theorem sounds totally crazy, and even today many people refuse to believe it. At first sight it looks as if they ought to have a good case. What about the volume? It doubles. Surely that's impossible? The trick is that the pieces are so complicated that *they don't have volumes*. The total volume can *change*. Because the pieces are so complicated, with arbitrarily fine detail, you can't actually carry out this dissection on a lump of physical matter. A good job too, it would ruin the gold market.

Once we start thinking about very complicated pieces, we have to be more careful. For instance, the 'pieces' need not actually come as connected lumps. Each 'piece' might consist of many disconnected componentspossibly infinitely many. When we apply a rigid motion to such a set, we must not only keep each component the same shape: we must also preserve the mutual relationship of the parts. And it's not just the notion of rigid motion that needs to be made

precise. When we dissected a square into an equilateral triangle, for example, we didn't worry about the edges of the pieces. Do they abut or overlap? If a point is used in one edge, does that exclude it from the adjacent edge? The question wasn't raised before because it would have been distracting; and also because the edges of polygons have zero area and hence don't really matter. But with highly complicated sets, we need to be more careful: *every single point* must occur in precisely one of the pieces. Banach proved the BolyaiGerwien theorem is still true for this more careful definition of 'dissection'. He did so by inventing a method for 'losing' unwanted edges.

The spirit (but not the details) of how the BanachTarski Paradox works can be understood by thinking about a dictionary rather than a sphere. This is an idealized mathematician's dictionary, the *Hyperwebster*, which contains all possible wordssense or nonsensethat can be formed from the 26 letters of the English alphabet. They are arranged in alphabetical order. It begins with the words A, AA, AAA, AAAA, and only infinitely many words later does it get round to AB. Nevertheless, every word, including AARDVARK, BANACH, TARSKI, or ZYMOLOGY, finds its place. I'll show you how to dissect a *Hyperwebster* into 26 copies of itself, each maintaining the correct alphabetical orderwith a spare alphabet thrown in.

The first of the 26 copies, 'volume A', consists of all words that begin with A, apart from A itself. The second, 'volume B', consists of all words that begin with B, apart from B itself; and so on. Let's think about volume B. Like gentlemen songsters out on a spree, it begins BA, BAA, BAAA, and so on. Indeed, it contains every word in the entire *Hyperwebster* exactly once, except that a B has been stuck on the front of every one. BAARDVARK, BBANACH, BTARSKI, and BZYMOLOGY are in volume B. Moreover, they are in the same order as AARDVARK, BANACH, TARSKI, and ZYMOLOGY.

The same goes for volume A and volumes CZ. Each is a perfect copy of the entire *Hyperwebster*, with an extra letter stuck on the front of every word. Conversely, every word in the *Hyperwebster*, apart from those containing just a single letter, appears in precisely one of these 26 volumes. AARDVARK, for instance, is in volume A,

in the position reserved for ARDVARK in the *Hyperwebster* itself. BANACH is in volume B, and its position corresponds to that of ANACH in the original *Hyperwebster*. The cartoonist's snooze symbol ZZZZZ is in volume Z, in the position corresponding to the slightly shorter snooze ZZZZ.

In short, one *Hyperwebster* can be cut up, and rearranged, without altering the orders of the words, to form 26 identical *Hyperwebsters* plus a spare alphabet. Order-preserving dissections of *Hyperwebsters* are not 'volume -preserving. For 'Hyperwebster' now read 'sphere', for 'word' read 'point', and for 'without altering the order of' read 'without altering the distances between', and you've got the BanachTarski Paradox. In fact the analogy is closer than it might appear, because the BanachTarski Paradox is proved by using sequences of rigid motions in much the same way as I have used sequences of letters. The possibility of doing this was discovered by Felix Hausdorff, who showed that there exist 'independent' rotations, for which every different sequence of combinations leads to a distinct resultjust as every different sequence of letters leads to a distinct word in the *Hyperwebster*. In spirit, the BanachTarski Paradox is just the *Hyperwebster* Paradox wrapped round a sphere.

Banach and Tarski actually proved something much stronger. Take any two sets in space, subject to two conditions: they don't extend to infinity, and they each contain a solid sphere, which can be as small or as large as you wish. Then you can dissect one into the other. Squaring the Circle may be tough, but Banach and Tarski could Cube the Spheregetting any size of cube, to boot. They could dissect a football into a statue of Lady Godiva, a scale model of a rabbit three light years high, or a pinhead in the shape of the Taj Mahal.

Plane Speaking

The BanachTarski Paradox doesn't arise in two dimensions. You can't change areas in the plane by dissection. (You *can* change areas on the surface of a sphere.) The basic reason is that in the plane, the result of performing two rotations in succession is independent of

the order. Analogously, in the corresponding *Dyslexicon*, the order of the letters doesn't matter: SPOTTER means the same as POTTERS. Now the dissection trick goes haywire: if we put SPOTTER in volume S, corresponding to POTTER in the original, then volume P fails to contain anything corresponding to OTTERS. So the dictionary approach certainly won't work. Tarski proved that nothing else would either. Banach had proved the existence, in the plane, of what is now called a Banach measure. This is a concept of area, corresponding to the usual one for those sets that *have* areas in the usual sense, and defined for every set whatsoever, even those that *don't* have an area, which is rigid and *finitely* additive. It follows easily that no two-dimensional BanachTarski Paradox is possible.

That's the point Tarski had reached when he posed his circle-squaring problem. *If* a circular disc can be cut into a finite number of pieces, which can be reassembled to form a square, *then* the square must have the 'correct' area. However, it wasn't clear to him whether such a process is possible.

The problem proved difficult, so mathematicians concentrated on special cases: restricting the nature of the pieces, or the type of rigid motion allowed. In 1963 L.

Dubins, M. Hirsch, and J. Karush proved that you can't cut up a circle along continuous curves and reassemble the pieces to form a square. You can't market a jigsaw puzzle whose pieces can be put together one way to form a circle and another way to form a square. If the circle can be squared by dissection, then the 'pieces' have to be far more complex than anything that can be cut by the finest jig-saw.

In 1988, at a conference in Capri, an American mathematician named Richard Gardner gave a lecture. In it he proved that, in any number of dimensions and not just two, there can be no solution to a version of Tarski's circle-squaring problem in which a limited system of rigid motions is permitted (selected from any 'discrete group'). He conjectured that the same is true if a rather broader system of rigid motions is allowed ('amenable group'). In the audience was Miklos Laczkovich, a Hungarian mathematician from Eötvös Lorand University in Budapest. Laczkovich had been making a careful study of dissection problems in *one* dimension, and during

Gardner's talk he realized that his conjecture is *false* in the one-dimensional setting.

Marriage Made in Heaven

The discovery triggered a whole series of new ideas on Tarski's circle-squaring problem itself, and within two months Laczkovich had a complete solution. You *can* square the circle by finite dissection! Even more suprisingly, the pieces do not even have to be rotated. The same goes for any shape whose boundary is composed of smoothly curving pieces, such as an ellipse, or a crescent, or the outline of an amoeba.

Laczkovich's method requires about 10^{50} pieces, so it's not something you can draw. The proof occupies about forty pages, and involves a whole series of novel ideas: naturally, it gets rather technical. One trick worth mentioning is a version of the Marriage Theorem. A (rather old-fashioned) dating agency has a rule that clients may date only clients of the opposite sex to whom they have been properly introduced. Under what conditions can all clients secure a date simultaneously? Obviously the numbers of men and women have to be equal, but that's not enoughfor instance, some men may not have been introduced to any women at all. Even if each man has been introduced to at least one woman, there can still be problems: two hundred men trying to date one woman, who has been introduced to them all. The more you think about it, the trickier it gets; but there's a very elegant solution. According to the Marriage Theorem, a complete set of dates is possible if and only if every subset of men collectively has been introduced to at least as many women, and conversely. In Laczkovich's proof the 'men' are possible candidates for pieces of the square, the 'women' are candidates for pieces of the circle, and 'being introduced' is the same as 'related by a rigid motion'. The Marriage Theorem lets Laczkovich 'marry' a suitable collection of pieces from the square to a corresponding collection from the circle. Much of the remainder of the proof tackles the construction of those candidate sets.

Despite Laczkovich's brilliant breakthrough, the last word has not

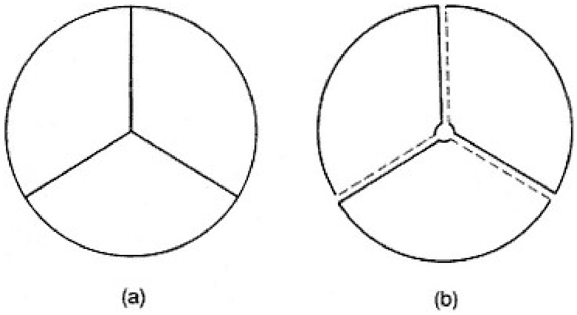


Fig. 22.
Trisecting a disc (a) with overlaps, (b) minus its central point.

been said. Here's a (deceptively) simple example of the questions that remain unanswered. *Can a circular disc be trisected?* That is, can you cut a circular disc into three pieces which, apart from rigid motions, are *identical*? It's easy to do this if overlaps at edges are allowed, or for a disc with its central point missing. The central difficulty is to deal with that one tricky point.

13 Strumpet Fortune

Out, out thou strumpet, Fortune! All you gods,
In general synod, take away her power,
Break all the spokes and fellies from her wheel,
And bowl the round knave down the hill of heaven,
As low as to the fiends.
William Shakespeare

Not a nice thing to say about a lady, but a time-honoured judgement on the character of Lady Luck. But does the Lady deserve her scarlet reputation?

At the end of the eighteenth century the 'system of the world' was a clockwork-like machine, set going by the Hand of Creation but thereafter left to impose its own precision on the universe. The Laws of Nature were sharp-edged and predictable; nothing was left to chance, not a leaf fell nor wavelet rippled without exact and meticulous cause. Chance confined herself to sleazy pursuits, such as gambling. In the twentieth century, we find ourselves in a world ruled by chance, in which the very stuff of Nature is a random cloud of probabilities. Clockwork has given place to a Cosmic Lottery. But randomness is no longer synonymous with patternless chaos and capriciousness. Chance too has its patterns. Demurely obedient the Lady is not, but for all that she is no wanton. She creates her own precedents, follows her own paradigm; but there is an inner order to her conduct. And that inner order has given rise to a new kind of mathematics: the theory of probabilities.

The Fair Coin

It is not inappropriate that the earliest writings on probabilities were by Girolamo Cardano, the Gambling Scholarand that they had to do with those most practical of human activities, games of chance. Probability theory, and its applied arm, statistics, has had a sometimes uneasy relationship with gambling ever since. For example life insurance: you bet that you will die, the insurance company that you will not. In order to win, you have to lose! The particular problem that interested Cardano was how to divide the stakes for a game of dice that is interrupted. The same question was asked of Blaise Pascal by his friend the Chevalier de Meré in 1654. A player is given eight tries to throw a one with a die, but the police raid the joint after three throws have been completed: how should the stakes be shared out after the hue and cry has died down? The question prompted Pascal to write to Fermat, and their thoughts were published by Huygens in 1657 in the first book devoted to probability theory: *On Reasoning in Games of Dice*. The investigation led Pascal to his famous arithmetical triangle

1
1 1
1 2 1
1 3 3 1
1 4 6 4 1

in which each number is the sum of that directly above it and that above and to the left. For example, consider three tosses of a coin. There are eight possible outcomes in all, which I'll arrange like this:

HHH	HHT	HTT	TTT
	HTH	THT	
	THH	TTH	

Of the eight series of tosses, three heads occur in one case, two heads in three, one head in three, and no heads in one. The sequence of numbers is 1, 3, 3, 1, just like the fourth row of Pascal's triangle; and their sum is 8. The chances of throwing exactly one head in

three trials are 'three out of eight', or to put it another way, the probability of throwing exactly one head in three trials is 3/8. Assuming, of course, that all possibilities are 'equally likely', or that the coin is *fair*.

The arrival of probability theory as a subject in its own right may be traced to Pierre-Simon de Laplace's *Analytic Theory of Probabilities* of 1812. Here he defines the probability of a particular event as the ratio of the number of ways in which it can happen, compared to the total number of possible outcomesagain on the assumption that all outcomes are equally likely. What do we mean by 'equally likely'? To answer that the probabilities are all the same is to invoke circular reasoning! This foundational question caused a lot of trouble in the early days of probability theory: we shall see later how it has been resolved. Laplace's definition reduces questions of probability to combinatorial counting problems. For example, to find the chances of getting a full house (three cards of one value and two of another, say three kings and two queens) in a game of 5-card stud, we work out

(1) The total number of different poker hands (2598960)

(2) The number of full houses (3744)

and form their ratio

3744/2598960 = 0.00144.

Great Expectations

In 1994 the British government introduced a National Lottery, similar to those run by several other countries including Germany. The proceeds are divided between the company that runs it, the government, various worthy causes such as charities, sport, and the arts, and the winners. The lottery predictably proved controversialnot because of any irregularities in its running, but for political and ethical reasons. (It was, for example, dubbed a 'desperation tax' by the Methodist Church.) Setting politics and ethics aside, let's try out the methods of probability theory on this topical example to get some insight into its mathematical structure.

The rules are simple. For £1, a player buys a lottery card and chooses six distinct numbers between 1 and 49 inclusivefor example 5, 6, 17, 31, 32, 47. Every week six random numbers (plus a bonus number which I will largely ignore) are generated by a machine that mixes up plastic balls with numbers on and blows them one by one into a tube. In order to win prizes, players must match the numbers marked on their cards to those selected by the machine. The order in which the numbers are chosen or drawn is irrelevant.

The top prize, the jackpot, is typically worth about £8 million, and goes to those players who get all six numbers correct. Successively smaller prizes are given for getting five numbers out of the six main ones, plus the bonus number; five numbers out of the six main ones; four numbers out of the six main ones; and three numbers out of the six main ones.

In the first draw for the lottery nobody won the jackpot, which was therefore 'rolled over' to the next weekmeaning that the prize money for the second week's jackpot was added to that for the first. In week two a factory worker from the North of England duly won £17,880,003, with the choice of numbers 26, 35, 38, 43, 47, 49, and Lottery Fever struck. For example, an insurance company offered a policy to protect employers against a key member of staff winning the jackpot and leaving. How worthwhile would such a policy be to a company? How do the odds of winning the jackpot compare with those for being killed in a road accidentagainst which few companies insure their employees, however important?

What *is* your chance of winning the jackpot? Assume that the machine that chooses the numbers is statistically random. All the evidence supports this assumption, and indeed there are good mathematical reasons to expect it to be true, based on the idea that the machine 'does not know' which numbers are written on the balls. That is, in principle it treats all balls equally. So the probability of any particular number being chosen is 1/49. A human player may think that the numbers 3 and 7 are 'lucky' and 13 is 'unlucky', but statistical analysis of the numbers produced over many years in other countries shows that the machine isn't superstitious.

Assume you've chosen 5, 6, 17, 31, 32, 47. What is the probability that the machine will produce those numbers?

The probability that the first number drawn by the machine matches one of your six is clearly 6/49, because there are six possible numbers to choose out of a total of 49. If that number *is* one of yours, then the probability that the second number drawn by the machine matches one of your other five choices is clearly 5/48, because now there are only five possible numbers left to choose out of a total of 48. Similarly the probability of the machine choosing one of the remaining four numbers on the next try is 4/47, and so on with successive probabilities 3/46, 2/45, and 1/44.

According to probability theory, then, the probability of all six numbers matching those you chose can be found by multiplying all of these fractions together:

$$\frac{6}{49} \times \frac{5}{48} \times \frac{4}{47} \times \frac{3}{46} \times \frac{2}{45} \times \frac{1}{44} = \frac{1}{13983816}$$

So the odds of winning the jackpot are precisely 1 in 13,983,816 each week.

Notice that this calculation produces the same answer for *any* set of six numbers between 1 and 49. All choices of six numbers, no matter what patterns do or do not occur, have exactly the same chance of being chosen by the machine.

The typical jackpot is around £8 million, and you bet £1. On the simplifying assumption that at most one person wins the Jackpotwhich we'll shortly see is not the caseyour *expected* winningswhat you win on average if you make a very large number of betsare $£\left(\frac{8000000}{13983816} - 1\right)$, that is, a *loss* of 43p.

Of course there are the other prizes, but the expected 'winnings' of any player still amounts to a substantial losswhich is *why* the Lottery can pay the company that runs it, the government, charities, sport, and the arts, as well as the winners. In gambling terms, the lottery is a sucker bet.

This doesn't mean that it's not worth your while to play. The company, the government, charities, sport, and the arts, are effectively

betting huge numbers of times and thus can expect to *receive* their expectations. The typical player, however, bets only a small sum every week, and for them the expectation gives an incorrect picturejust as the statement 'the average family has 2.4 children' fails to apply to any individual family. The pattern for most players is that they pour small sums down the draintypically about £100 per yearhoping for numbers that, for them, never turn up. Occasionally they get three numbers right and win £10, but that's just a psychological sop to keep them interested, and it's more than cancelled out by their losses. What they buy is £100 worth of excitement, and a possibilityalbeit remoteof instant riches.

The pattern for the very few who hit the jackpot is utterly different. What they buy is a total change of lifestyle.

Should a 'rational' person play? There are many considerations, among them 'do I really *want* a total change of lifestyle?' But even if we focus solely on mathematical issues, probability theory offers guidelines, not proofs. There are many cases where it is rational to bet when your expectation is negative: the clearest example is life insurance. The mathematics of life insurance is very similar to that for the lottery. The insurance company makes huge numbers of bets with all of its clients, but each client's bet is small. The expectations are similarly weighted in the insurance company's favour. The main difference is that normally the players hope *not* to hit the jackpot. It is entirely rational to play the life insurance game, negative expectation notwithstanding: players pay an affordable premium in order to purchase financial security in the event of an unlikelybut possibledisaster.

Should a company insure against key personnel winning the jackpot and heading off for a lifetime of indulgence in warmer climes? The chance of being killed in a car crash in the UK is about 1 in 500,000 per week. This is about 28 times as likely as winning the lottery. So it is definitely irrational to insure against an employee winning the lottery if you aren't insuring against far more likely, and more prosaic, disasters. Advertising for the lottery emphasizes that 'it could be YOU', but your chances of being mugged, or of contracting AIDS heterosexually, are a lot higher. However, in any

given year you are about two hundred thousand times more likely to win the jackpot than to be hit by a meteorite.

How To Win the Lottery

Now we've finally reached the bit you bought this book for. A mathematician is going to tell you how to win a fortune on the lottery. Some amazing system based on a mixture of probability, chaos theory, and your great aunt's cat's initials.

Come on, get real. If I knew how to win, do you think I'd give the secret away for the price of a paperback? The same goes for all the other people who try to sell you their systems. Because the expectation is negative, most systems have the same fundamental flaw: they are just ways to lose your money faster. Buying ten thousand tickets raises your chance of winning the jackpot to one in 1398, but your average loss goes up to around £4,300. You'd do better putting your £10,000 on a four-horse accumulator with each horse at 11 to 1.

It's true that if you can lay hands on really big sums, you can guarantee winning. For a mere £13,983,816 you can bet on every combination. If you are the only winner then you will usually win about £8 milliona neat way to throw away £6 million. If several other people get the same winning numbers as you, then you throw away a lot more. The one case where this calculation fails is when there is a roll-over, in which case the jackpot becomes about twice as big as usual. Then you might come out aheadbut only if nobody else gets the numbers right. This danger made itself apparent on 14 January 1995, when a roll-over jackpot of £16,293,830 was shared between 133 people, who chose the numbers 7, 17, 23, 32, 38, 42.

Statistically, only about four people should have chosen this sequence if everybody picked numbers at random, so there must be some curious effects of human psychology at work. For example, people often think that because the lottery machine draws sequences at random, they must always *look* random. A sequence like 1, 2, 3, 4, 5, 6 doesn't look random, so it will never come up. Not true. On

average it will come up once every 13,983,816 timesexactly the same odds as those on the sequence that *did* come up on 14 January 1995. Odds are about the potential, not about the actual; and a random *process* does not always produce a result that *looks* irregular and patternless. To compound the misunderstanding, most of us have the wrong intuition of what a random sequence looks like. If you ask somebody to write down a random sequence, they usually space the numbers apart too much. Randomly generated sequences are often 'clumpy', but we human beings don't expect them to be. Compare the nice, evenly but not *too* evenly spaced sequence 7, 17, 23, 32, 38, 42 that 133 people picked, with the clumpy sequence 26, 35, 38, 43, 47, 49 that won £18 million for the only person who chose it.

And here, finally, is a system that might just work, and your faith in mathematicians is justified. If you want to play the Lottery, the smart move is to make sure that if you do win, then you win a bundle. There are at least two ways to achieve this aim, and the best strategy is to combine them.

The first is *only bet when there is a roll-over*. Paradoxically, if everybody did that, the roll-over wouldn't be worth anything, because the company running the Lottery wouldn't receive any stake money. So you just have to hope most punters aren't as smart as you arewhich, if you're reading this advice and following it, is of course true.

The second tactic was made dramatically clear when those 133 disappointed people won a jackpot of £16 million and got only a miserable £122,510 each. It is *never bet on numbers that anybody else is betting on*. It's not so easy to do this, because you have no idea what numbers the other 25 million punters are choosing, but you should avoid too many numbers under 31 because of all those birthdays that everybody falls back on when asked to choose numbers. Avoid 3, 7, 17, and other numbers people think are lucky. You could sprinkle in a few 13s: many people think they're unlucky, but those little plastic balls aren't superstitious. I personally recommend something *really* stupid, like 43, 44, 45, 46, 47, 48. But not exactly that, you understand, because a lot of other people might have read this book too.

'But surely a sequence like that never comes up?' Not so. It comes up, on average, once every 13,983,816 timesonce every half a million years, to put the chances in perspective. The sequence 43, 44, 45, 46, 47, 48 is exactly as probable as the sequence 7, 17, 23, 32, 38, 42 that came up on 14 January 1995. It is, dear reader, exactly the same as the odds on the six numbers your sweaty little paws have just marked on your Lottery ticket as you handed over your hard-earned £1 coin.

Problems With the Infinite

Back to more overtly mathematical issues. The approach to probability that we used to calculate lottery odds is 'combinatorial'. It boils down to *counting* how many ways various things can happen. This is fine when there are only finitely many possibilities. But many practical problems involve infinitely many possible outcomes. For example, throw three darts at a dartboard: what is the probability of getting three treble-20s? Modelling the board as a circular disc and the position of the dart as a point, there are infinitely many possible outcomes, of which infinitely many are favourable. Laplace's recipe leads to ∞/∞ which doesn't get us very far. It is also not clear what we should mean by 'equally likely' here. For each individual point the probability of hitting it is, again by Laplace, $1/\infty$, which is 0. The chance of hitting any given point is exactly zero. So all points are equally likely. But the chances of hitting some set of points should be the sum, over that set, of the chances of hitting individual points. The sum of lots of 0s is 0. How can this be reconciled with the fact that, on any throw, *some* point is definitely hit?

The simplest approach is to assume that the chance of hitting a given *region* of the board is proportional to its *area*. Laplace's ratios of numbers are replaced by ratios of areas. For simplicity choose units making the total area 1. The area of a single point is zero, and so are the chances of hitting it; but the area of the whole board is 1, and we are therefore bound to hit *something*. The area of the treble-20 is roughly 0.0005; the probability of three-in-a-bed is the cube of this, about one chance in eight billion. Of course a good darts player does better, since he does *not* distribute darts over the

board at random, but concentrates them near the target zone. The area should be appropriately 'weighted' to take this into account.

During the eighteenth and nineteenth centuries mathematicians grappled with such problems, but failed to find a really precise formulation of the general notion of probability. Laplace said hopefully that 'at bottom, the theory of probabilities is only common sense, reduced to calculation', but unfortunately there are situations in which common sense, applied in equally reasonable ways, gives contradictory answers. As Morgan Crofton put it in 1868, 'It is true that in a few cases differences of opinion have arisen as to the principles, and discordant results have been arrived at', but added 'all feel that this arises, not from any inherent ambiguity in the subject matter, but from the weakness of the instrument employed.'

A Stronger Instrument

The next significant step apparently had nothing to do with probabilities, but arose in the calculation of lengths, areas, and volumes. The way to do this is to use the integral calculus. The area under a curve is the integral of the function that defines it; volumes and higher-dimensional analogues thereof are obtained by multiple integrals. There are technical difficulties in the manipulation of multiple integrals that led mathematicians to seek a more general notion of the basic concepts of length, area, and volume. The hope was that the greater generality would resolve the technical snags.

The key concept is length. If a and b are two points on the real line, then the length of the interval joining them is $b a$. Given a set of points on the real line built up from a finite number of intervals, then it is reasonable to define its length to be the sum of the lengths of its component intervals. But more complicated sets can be envisaged. Is there a way to define the 'total length' of *any* set of real numbers? In 1882 du Bois-Reymond defined the *content* of a set by 'approximation from the outside' by finite unions of intervals. Peano improved the idea in 1887 by approximating from inside as well. For example, to find the area of a region in the plane, he calculated the *outer content* as the limit of the areas of polygons containing the set, and the *inner content* as the limit of the areas of

polygons contained within the set. If these are equal, their common value is defined to be the content, or area, of the set. (If not, the set is considered not to have an area.) This is a direct generalization of Archimedes' method for finding the area of a circle. Put a polygon outside, another inside, then increase the numbers of sides of the polygons so that the area between them shrinks to zero. The advantage of Peano's concept is that it is *additive*: to find the area of a set that splits up into finitely many pieces, you just add up the individual areas.

The definitive version of the theory was devised by Henri Lebesgue in 1902. In Lebesgue's theory, the notion of content is replaced by *measure*. The approximating sets are not unions of finitely many intervals, but of an infinite sequence of intervals. The total length is still defined provided the infinite series formed by the individual lengths converges to a well-defined sum. And Lebesgue's measure is not just additive: it is *countably additive*. If a set is built up from a sequence of disjoint pieces, then its measure is the sum of the measures of the pieces (interpreted as an infinite series). With this definition, a great many complicated sets can be shown to possess lengths, areas, and so on. For example, the set of rational numbers between 0 and 1 has measure 0; the set of irrationals between 0 and 1 has measure 1. As far as measure theory goes, there are a lot more irrationals than rationals (a judgement with which Cantor, for other reasons, would have agreed). But even with this much broader definition, there remain sets which do not have measures, at least in the usual formulations of set theory as we saw in Chapter 12. Despite this, the range of *measurable* sets is enormous.

Lebesgue's ideas (arrived at independently by William Young) led to a greatly improved theory of integration, and have become a cornerstone of analysis. But they also solve the problem of continuous probability theory.

Kolmogorov's Axioms

As on many previous occasions, the answer was to stop worrying about what probability 'really' is, and instead to write down a list of

properties that it ought to have. In other words, to axiomatize the concept. The final step was taken in about 1930 by Andrei Kolmogorov, using the idea of a *sample space* due to Richard von Mises: the set of possible *outcomes* of an experiment. For example, if a coin is tossed once, the sample space is the set {Heads, Tails}. If a dart is thrown, it is the set of points on the dartboard. If three coins are tossed, it is the set of eight possible sequences of heads and tails. Now comes the tricky step. Define an *event* to be not just a single outcome, but a *set* of outcomes. For example, when tossing three coins, the event 'two heads and one tail' corresponds to the set {HHT, HTH, THH} of outcomes. In the case of a dart being thrown, the event 'treble-20' corresponds to a particular region of the dartboard, in the treble ring and the 20 sector; and this region is again a set (this time infinite) of points of the sample space.

We now select a suitable set of events (I'll explain what 'suitable' means in a moment) and define a *probability function p*, which assigns to each event *E* in the set a number *p(E)*. This should have the following properties:

(1) *p(E)* always lies between 0 and 1.

(2) If *E* is the empty event 'nothing happens' then *p(E)* = 0.

(3) If *E* is the universal event 'some possible outcome happens' then *p(E)* = 1.

(4) *p* is *countably additive* in Lebesgue's sense.

'Suitable' just means that given any sequence of events *E* for which *p(E)* is defined, they can be lumped together into a single event *F* (their union) for which *p(F)* is also defined. The set of events considered must be 'closed under countable unions'.

How Does This Help?

For finite probability everything is very straightforward. The set of events to be considered contains *all* sets of outcomes. Assign probabilities to individual outcomes so that the total is 1; then the probability *p(E)* of an event (set of outcomes) *E* is the sum of the probabilities of its members. For example in the tossing of three

coins, each of the eight possible sequences is *assigned* probability 1/8; and this is the *definition* of a fair coin. The meaning of the phrase 'equally likely' doesn't enter into the formulation; but of course it provides the motivation for the values chosen. For the dartboard example, we must be more careful. The events considered are now those *regions* of the dartboard that possess an area, in the sense of measure theory. The probability *p(E)* is the proportion of the total area filled up by *E*. Individual points have area zero, hence probability zero; but the treble-20 region R has a definite non-zero area, and hence a non-zero probability. Now R can be broken up into infinitely many 1-point subsets, each of probability zero. So why does not property (4) imply that *p*(R) is the sum of these, hence itself zero? The answer is that R is made up from an *uncountable* infinity of points, so we cannot appeal to (4), which asserts only countable additivity. It is on this delicate distinction that the formulation hinges, and the apparent paradoxes are resolved.

In an application of probability theory, it is up to the end user to decide for himself what the sample space, the set of events, and the probability function should be. Only then does the machinery of probability theory swing into action. But this is entirely right and proper. The laws of mechanics, for example, tell us how to analyse the motion of the Solar System; but they do not of themselves prescribe the masses or shapes of the planets, or their initial positions in orbit. This modelling step must be made first, before the mathematics can be used: it is not part of the mathematics.

The Multidimensional Drunkard

Among the things probabilists study are *stochastic processes*. Here a system of some kind sits in some particular state, and at each instant the state changes randomly according to some specified set of probabilities. One possible application is to the motion of a molecule under random collisions with other molecules; that is, the diffusion of a chemical through some medium. Now it's possible to treat diffusion effects in the time-honoured way, and develop a continuum-mechanical model based on partial differential equations. We

may now ask whether this is in agreement with the predictions of a molecular theory. The mathematics for molecular diffusion is complicated, and it makes sense to start with simple models and work our way up to something more realistic.

The simplest such process is the one-dimensional *random walk*. Imagine a line, marked off in units. At time 0 a particle starts at position 0. At time 1 a coin is tossed: if the coin comes up heads the particle moves one unit to the right; if tails, one unit to the left. At time 2 this process is repeated, and so on. Assuming the coin to be fair, the *transition probabilities*, for motion right or left, are 1/2 at each stage. We ask what the long-term behaviour of this system looks like. Will the particle wander off along the line, or hover about near the origin, or what? Since it has equal chances of going either way, it doesn't seem especially likely that it will wander away for good; on the other hand, there's little to prevent it drifting quite a way along the line, because every so often a run of unusually many heads or tails will occur. In other words, we expect its position to fluctuate in a pretty irregular fashion, not moving too fast on average, but with a fair chance of eventually getting to pretty much any point on the line. And indeed that's what happens. Given *any* point, however far away from the origin, the probability that the particle will eventually reach that point is 1. Or, putting it another way, the chance that it *never* reaches the chosen point is zero. Indeed, with probability 1 it returns infinitely often to the point. The motion is smeared into 'uniform' fluctuations over the whole line.

So, if you are lost in a 1-dimensional desert, and go East or West by repeatedly tossing a coin, you will eventually reach any point you wish. But there's a price to pay: it takes a very long time. In fact the time is so long that the *average* time between successive returns to the same point is infinite. If you perform a similar random walk, but now move in the plane, with probabilities ¼ of going East, West, North, or South, the results are very similar. If you are lost in a 2-dimensional desert, you still expect to reach every point eventually by moving at random in each of the four directions. What about three dimensions? Now you can go up and down as well, with all transition probabilities being 1/6. In 1921 George Pólya proved that the results

are very different: there is a non-zero probability (about 0.65) that you *never* reach your chosen position, however long you walk.

These examples are obviously very special, although they already make the point that the dimension of the space in which the walk takes place has a strong influence. A great many generalizations suggest themselves. There is no reason to make all of the transition probabilities the same. How do the results depend on the values chosen? What happens if we allow moves in 'diagonal' directions? Along a specified set of directions? The random walks discussed above are *discrete* in that the positions are specified by *integer* co-ordinates and the distance moved at each stage is the same (namely 1). What about *continuous* random walks? What if the discrete time-steps are replaced by a continuous motion?

The Molecular Speculator

In 1827 the botanist Robert Brown was staring down his microscope at a drop of fluid. There were tiny particles in the fluid, and he noticed that they weren't sitting still: they were jumping about in a very erratic fashion. The particles weren't alive, moving under their own volition; and the fluid was absolutely still. What was the cause? Brown suggested that this movement is evidence for the molecular nature of matterwhich at the time was itself a highly speculative theory. The fluid is composed of tiny molecules, whizzing about at high speed and colliding randomly with each other. When the molecules bump into a particle, they give it a random push.

It is possible to model Brownian motion by a 'continuous' random walk. This is defined by taking a discrete version, with small time-steps and small individual movements in many directions, and then passing to the limit. William Feller describes what this achieves:

> In the limit the process will occur as a continuous motion. The point of interest is that in passing to the limit our formulas remain meaningful and agree with physically significant formulas of diffusion theory which can be derived under much more general conditions by more streamlined methods. This explains why the random-walk model, despite its crudeness, describes diffusion processes reasonably well.

What we have is a direct mathematical connection between the motions of individual molecules, and the bulk properties of the fluid they make up. The statistical features of molecular motion, and the continuum mechanics of diffusion, agree.

The first person to discover the connection between random walks and diffusion was Louis Bachelier, in a doctoral dissertation of 1900. The examiners, damning it with faint praise, gave it a *mention honorable* at a time when only a *mention très honorable* was considered worth having. Bachelier wasn't studying Brownian motion, however: he had his eye on something closer to the gambling origins of probability theorythe random fluctuations of the Paris stock-market. It's perhaps no surprise that a failed thesis on stocks and shares attracted little attention from theoretical physicists. In 1905 Albert Einstein laid what everyone thought were the foundations of the mathematical theory of Brownian motion; and Norbert Wiener developed these extensively. Only much later was it discovered that Bachelier had anticipated many of their main ideas!

Random walks are still important in mathematical physics, and are now appearing in quantum theory. Among the unsolved problems is the behaviour of *self-avoiding* random walks, in which the particle is not permitted to visit the same point twice. Meanwhile the mathematics of probability theory, in innumerable forms from statistical mechanics to ergodic theory to stochastic differential equations, has diffused throughout the whole of the sciences.

The Mathematics of Nature

And new Philosophy calls all in doubt,
The Element of fire is quite put out,
The Sun is lost, and th'earth, and no mans wit
Can well direct him, where to looke for it.
John Donne

In 1642 in the village of Woolsthorpe, a widow who ran the family farm was blessed with a son. Born prematurely, he was not expected to live beyond a week, but he survived. He went to ordinary schools and was an ordinary little boy, save perhaps for a knack for making gadgets. A stay at Grantham is described thus by Richard Westfall:

> While the other boys played their games, he made things from wood, and not just doll furniture for the girls, but also and especially models. A windmill was built north of Grantham while he was there. Only the schoolboy inspected it so closely that he could build a model of it, as good a piece of workmanship as the original and one which worked when he set it on the roof. He went the original one better. He equipped his model with a treadmill run by a mouse which was urged on either by tugs on a string tied to its tail or by corn placed above it to the front. He called the mouse his miller. He made a little vehicle for himself, a four-wheeled cart run by a crank which he turned as he sat in it. He made a lantern of 'crimpled paper' to light his way to school on dark winter mornings, which he could simply fold up and put in his pocket for the day. The lantern had other possibilities; attached to the tail of a kite at night, it 'wonderfully affrighted all the neighbouring inhabitants for some time, and caused not a little discourse on market days, among the country people, when over their mugs of ale.'

Growing older, the young man studied at Trinity College, Cambridge, making no special impact. In 1665 Europe was under the

scourge of a disease transmitted to man by rat-fleas and causing swelling of the lymph glands: the bubonic plague. The young man returned to the safety of his home village. In rural solitude he almost single-handedly created mechanics, optics, and the calculus. His name was Isaac Newton.

Why did the dormant genius awaken so explosively? The best explanation we have is Newton's own: 'In those days I was in the prime of my life for invention, & minded Mathematicks & Science more than at any time since.' He published none of this work. According to De Morgan the reason was that 'a morbid fear of opposition from others ruled his whole life'. In *The Sotweed Factor* John Barth's fictional Newton was 'so diffident about his talents 'twas with great reluctance he allowed aught of his discoveries to be printed; yet so vain, the slightest suggestion that someone had antedated him would drive him near mad with rage and jealousy. Impossible, splendid fellow!' And so it may well have been. Finally in 1672 Newton's work on optics appeared in print, and he promptly found his fears justified, running into a united wall of opposition from scientists such as Robert Hooke and Christiaan Huygens.

This was hardly encouraging. But, after much urging by Edmund Halley, Newton did eventually publish his theories of mechanics and gravitation, the *Philosophiae Naturalis Principia Mathematica*; probably *the* most significant book in history, for it explained the 'System of the World'. Unlike most attempts at this, it succeeded. It was a difficult book, and deliberately so; lesser men than Newton have also deflected criticism with a competent snow-job. As far as proofs go, the *Principia* is relentlessly geometrical, in the classical mould. But there is a strangeness to Newton's geometry. A competent mathematician can follow it and verify its correctness; but never in a month of Sundays could he answer the only important questionhow the devil did Newton think of it? Many years later Gauss, apropos of proofs, remarked 'When one has constructed a fine building the scaffolding should no longer be visible', and Newton was apparently of like mind. Jacobi called Gauss 'the fox of mathematics' because he erased his tracks in the sand with his tail. The tracks Newton was busily erasing were those of the calculus. He had in his

possession a devastatingly powerful weapon. He made sure it stayed a concealed weapon; either to make his methods seem more familiar to his contemporaries, or to avoid more controversy, or possibly because of his deep admiration for classical Greek geometry. His successors, less reticent and able to spot a worthwhile dodge when they saw it, adopted the calculus explicitly. The result was a new paradigm for the description of Nature: *differential equations*.

Vive la Differential

Galileo Galilei had investigated the motion of bodies and derived mathematical laws from his observations, saying prophetically:

> It has been pointed out that missiles and projectiles describe a curved path of some sort; but no one has pointed out the fact that this path is a parabola. But this and other facts, not few in number or less worth knowing, I have succeeded in proving; and what I consider more important, there have been opened up to this vast and excellent science, of which my work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners.

Newton was acute enough to seek general laws governing all kinds of motions, and this problem led him to invent calculus. Here's a modern summary. Let x be a quantity depending on time. Then it has an instantaneous rate of change, or *derivative*, written x'. For example if x is position, then x' is velocity. Repeating the process we get x'', which here is acceleration; and so on. Newton called these objects *fluxions*, and the original quantities they were derived from he called *fluents*.

Newton's basic Law of Motion states that the *acceleration* of a body is proportional to the force acting on it, that is,

$$mx'' = F$$

where m is its mass, x its position, and F the force acting. This is a *differential equation* because it specifies not x, which we want to know, but a derivative (here the second). Its solution depends on how F varies with time, and is by no means evident. The variety of possible differential equations is endless; the number of natural

phenomena they can describe is scarcely less. These new types of equations demand new methods of solution. Newton favoured one above all: power series. He assumes that the variation of x with time t is of the form

$$x = a_0 + a_1t + a_2t^2 + ,$$

then substitutes this in the equation, and solves it in turn for a_0, a_1, Others, such as Leibniz, preferred to find solutions in 'closed form' that is, as a formula in t. Neither approach is really satisfactory. 'Closed form' solutions seldom exist, though they're useful when they do, and many an equation has been torn limb from limb until some mutilated corpse has a solution in closed formto what purpose is debatable. Power series solutions often only work for small values of the variables, and sometimes not even thenthough an important idea, the theory of asymptotic series, makes sense of some series even when they don't converge.

Sun-God and Crystal Spheres

The immediate impact of Newton's work was greatest in that time-honoured subject, celestial mechanics: the motion of the heavenly bodies. The astronomers of ancient Babylon made many accurate observations and understood the heavens well enough in empirical terms to predict eclipses. Their theoretical cosmology was less satisfactory, being compounded of two earlier ones, those of Eridu and Nippur. In Eridu cosmology water is the origin of all things. The world arises from the sea and is encircled by it; beyond the ocean-stream the sun-gold pastures his cattle. In that of Nippur the world is a mountain. The Babylonians held that the heavens were a solid vault with its foundations on the ocean. Above are the upper waters; above these live the gods, and the 'sun-illuminated house' from which the Sun emerges every morning through one door and to which it returns every evening through another. Under the mountain lies the abode of the dead. Old Testament cosmology echoes these ideas.

The Egyptians held that the Earth is a long box, running northsouth (like the Nile). The sky is the ceiling; stars hang on lamps by

cords or are carried by deities. During the day they are switched off. The ceiling is supported by four mountains at the cardinal points. On a ledge below their peaks a river runs round the Earth, the Nile being one of its branches. The sun-god Ra is carried round this river in a boat, and when he is attacked by a serpent, there is an eclipse. The Nile was the source of all life in Egypt, and it consequently loomed large in Egyptian cosmology.

The Ionian school of Greek philosophers made no significant advance on this picture. Anaximander held that the Earth is flat; Anaximenes that the celestial vault is solid, with stars attached like nails. Thales did predict an eclipse in 585 BC, but he probably got the method from the Egyptians or Chaldeans. Parmenides, in about 500 BC, realized that the Earth is a sphere, but only the Pythagoreans believed him, and they probably for the wrong reasons. He stated that the Earth is immobile because there is no good reason why it should fall one way or another. The Pythagoreans elaborated the theory, embellishing it with their characteristic number mysticism. There are ten heavenly bodies, they said, since 10 = 1 + 2 + 3 + 4 is the source of all number. These are the Earth, Moon, Sun, Venus, Mercury, Mars, Jupiter, Saturn, the Starry Sphere, and Antichthon (counter-Earth). All encircle a celestial fire, with Antichthon perpetually hidden from us by the Earth (which also conveniently hides the fire). The Earth is a sphere; the Moon does not rotate. The Sun takes a year to encircle the central fires, the Earth only 24 hours: this explains day and night. The planets emit harmonious tones (the 'music of the spheres') which we don't hear because we are so used to them.

According to Plato, the Earth is round and lies at the centre of the universe, with everything else turning about it on a series of hollow spheres, rotating on their axes. Eudoxus modified this model to incorporate reversals of direction that had been observed in the motion of planets, and the inclination of the planetary orbits. He mounted the poles of one sphere on another, the poles of this on the next, and so on, using 27 spheres in all. Kalippus improved the fit with observation by extending the system to 34 spheres. Aristotle accepted Eudoxus' spheres but rejected celestial harmony. He treated the

spheres as actually existing, and hence had to add new ones to prevent the motion of one set being transmitted to the next, getting 56 in all. Herakleides (370 BC) held that the Earth rotates, the stars are fixed, Venus moves round the Sun, not the Earth, and so does Mercury. Aristarchus (281 BC) suggested that the Earth goes round the Sun. When it was observed that the Moon varies in size, hence cannot go round the Earth in a circle, a small eccentric circle was added. This still wasn't satisfactory, so Apollonius (230 BC) used *epicycles*. The Earth goes round a circle whose centre goes round another circle. Claudius Ptolemaeus (Ptolemy) lived in Alexandria in AD 100160. He proved that anything you can do with an eccentric can be done using an epicycle, so eccentrics lapsed into disuse. He refined the system of epicycles until they agreed so well with observations that nothing supplanted them for 1,400 years.

Nicolas Copernicus, in 1473, noticing that in the Ptolemaic system there are many *identical* epicycles, realized that they can all be eliminated if the Earth is made to go round the Sun. They are merely traces, superimposed on all the other bodies, of the Earth's motion. In other words, a heliocentric theory is *simpler* and thus should be preferred. As is well known, this suggestion did not meet with approval from the religious authorities. Copernicus wisely withheld publication until after his death. But later a more foolhardy Galileo was hauled up before the Inquisitor in Florence:

> *Propositions to be Censured*. Censure made in the Holy Office of the City, Wednesday, February 24, 1616, in presence of the undersigned Theological Fathers. *First*: The sun is the centre of the world, and altogether immovable as to local movement. *Censure*: All have said that the said proposition is foolish and absurd in philosophy, and formally heretical, in as much as it expressly contradicts the opinions of Holy Scripture in many places and according to the common explanation and sense of the Holy Fathers and learned theologians. *Second*: The earth is not the centre of the world and is not immovable, but moves as a whole, also with a diurnal motion. *Censure*: All have said that this proposition must receive condemnation in philosophy; and with respect to theological truth is at least erroneous in faith.

Tycho Brahe (15461601) didn't believe the heliocentric theory either, but his interests in astronomy quickly became almost entirely

experimental. He made extensive and remarkably accurate observations over a lengthy period, and eventually produced his own version of Copernicus's theory. On his deathbed he begged Johannes Kepler to sort out cosmology. Kepler felt that the Ptolemaic theory, even as revised by Copernicus, was dissatisfyingly arbitrary and complicated. He sought patterns in planetary motion. Not all of his patterns would meet with modern approval. For example in the *Mysterium Cosmographicum* he explains the spacing of the planets in terms of regular solids fitted between their spheres, thus:

Mercury
    Octahedron
Venus
    Icosahedron
Earth
    Dodecahedron
Mars
    Tetrahedron

Jupiter
Cube
Saturn

One advantage of this delightful theory is that it explains why there are only five planets (apart from the Earth which, of course, is different): there are only five regular solids to go round. At the time it seemed at least as sensible as anything else Kepler suggested (and it reminds me uncomfortably of modern theories of elementary particles in terms of representations of groups). Almost buried in Kepler's work are three fundamental laws, which have withstood the ravages of time much better.

He selected Mars to work on, a lucky choice as we shall soon see; and began by assuming that its orbit is an eccentric circle. Eventually, on this hypothesis of a circular orbit, he got his *Second Law*: a planet sweeps out equal areas in equal times. Then he decided that a circle wouldn't do, but some sort of oval might. Eventually the observations forced him, somewhat unwillingly, to his *First Law*: a planet orbits the Sun in an ellipse, with the Sun at one focus. It's

here that the luck comes in: Mars has an especially non-circular orbit, so the elliptical shape shows up well in observations. Kepler then found that the second law still works for elliptical orbits, a discovery that impressed him no end. What about the distances of the planets from the Sun? As well as the regular solids, Kepler tried musical analogies, but with no better success. Finally he hit on the *Third Law*: the cube of the distance is proportional to the square of the orbital period. Kepler was over the Moon:

> It is not eighteen months since I got the first glimpse of light, three months since the dawn, very few days since the unveiled sun, most admirable to gaze, burst upon me. Nothing holds me; I will indulge my sacred fury; I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians to build up a tabernacle for my God far away from the confines of Egypt. If you forgive me, I rejoice; if you are angry, I can bear it; the die is cast, the book is written, to be read either now or by posterity. I care not which; it may well wait a century for a reader, as God has waited six thousand years for an observer.

Although his three laws are almost buried in a mass of metaphysical speculation, Kepler had struck gold, and he knew it.

Equations for Everything

Newton explained all this with his theory of universal gravitation. Every particle in the universe attracts every other particle by a force proportional to their masses and inversely proportional to the square of the distance between them. The 'inverse square' prescription is what gives Newton's theory its precision; but the universalitythat it holds for *every* particleis what gives it its power. It can be applied to any configuration of particles and bodies whatsoever. At least, in principle: it's easier to state an equation than to solve it. For two particles the equation can be solved, yielding elliptical orbits that obey the three laws discovered empirically by Kepler. Conversely, Kepler's laws show that if a force is the cause of planetary motion, then it must obey an inverse square law. Very fine effects in the motion of the Moon can also be explained by Newton's theories. Another, later triumph was the explanation of a grand cosmic dance

in which Jupiter and Saturn alternately fall behind and ahead of their expected places. Even so, Newton's ideas at first met with a lot of opposition because the notion of a force acting at a distance was felt to be discredited.

More generally, people began to make use of *partial differential equations*, involving rates of change with respect to more than one unknown quantity. In 1746 d'Alembert applied them successfully to the vibrations of a violin-string. Daniel Bernoulli and Euler extended the ideas to water waves, sound waves, fluid flow, and the elastic deformation of solids. The *wave equation* became a standard item in the mathematical repertoire. Legendre and Laplace developed Newtonian gravitation into Potential Theory, studying such problems as the gravitational attraction of ellipsoids, it being known by then that the Earth was not a perfect sphere, but flattened at the poles. New astronomical discoveries, such as the asteroids, Uranus, and Neptune, fitted Newton's laws perfectlyand were predicted using them. Joseph Fourier's *heat equation* paved the way for thermodynamics and Fourier analysis. James Clerk Maxwell summed up all of electricity and magnetism in his *Maxwell equations*. Soon almost all of physics became formulated in terms of partial differential equationsto such an extent that even today, many scientists find it hard to conceive of any other way to model reality.

The Oscar Award

As a result of all this activity, people began to think of Newton's description of planetary motion as some kind of Ultimate Truth. The Solar System formed a kind of infinitely accurate clock, set going when the planets were hurled by the Hand of Creation. It was a clock that could never run down. But could it run wild? For instance, could the other planets resonate with the Earth like a child's legs pumping a swing, to make it collide with Mars, or run off into the cold and empty interstellar wastes? For imaginary solar systems simpler than ours, this kind of thing can happen. Could it happen to *us*? In 1887 King Oscar II of Sweden offered a prize of 2,500 crowns for an answer.

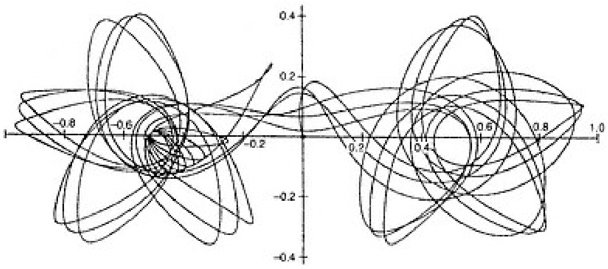


Fig. 23.
Orbit in a three-body system.

The state of the art at the time was far from adequate. It gave an exact solution in closed form for *two* bodies: Keplerian ellipses. A solar system having one sun and one planet could not run wild; all it could do was repeat the same motions over and over again forever. The general problem of *three* bodies seemed totally intractable. As far as we know today, no closed form solutions exist; at any rate, the general behaviour is enormously complicated. A few special solutions are known; for example Joseph-Louis Lagrange found that all three bodies can move in synchrony in elliptical orbits. But this type of motion is an accident of the mathematics, unlikely to occur in practice. Power series methods à la Newton help a bit, but are best on problems where most of the bodies have a mass small enough to be neglected: systems like the Sun, Jupiter, and an asteroid. Lagrange and Laplace between them did manage to show that the total departure from circularity of the orbits of the planets in the Solar System is constant; and that this total, even if concentrated on the Earth, will not switch it to an orbit that escapes the Sun altogether, like that of a comet. But this didn't show that the Earth might not slowly gain energy from other planets, and drift out by bigger and bigger almost-circles until we become lost in the silence and the dark. Proof was still lacking that we will neither leave the hearth nor fall into the fire, and for that the king offered his crowns.

The Last Universalist

Possibly the last person to understand fully *all* of the mathematics of his day was Poincaré. Naturally, he had a go at King Oscar's problem; and, characteristically, he invented a new field of mathematics to do it: *analysis situs*, now better known as topology. To see how it might apply, suppose that we could show that at some specific time from now, all of the planets would be in exactly the same relative positions as they now are. Then we would conclude from Newton's Law that the motion will thereafter repeat indefinitely. Poincaré saw that the essence of this idea is geometrical. According to Newton's dynamics, the motion of a system can be described if we know, at any instant, the positions and velocities of every particle in it. One way to imagine this is to invent an enormous multidimensional space, with six dimensions for each particle: three of position, three of velocity. The motion of a system of N bodies (say $N = 10$ for the Solar System, neglecting moons, or about 40 if moons are included, or billions if every particle in Saturn's rings is taken into account) corresponds to the motion of a single point in a $6N$-dimensional *phase space*. If the track in phase space joins up with itself to form a *closed* loop, then King Oscar's question is answered. No planet can ever wander off; the representative point, and hence the entire system, is *periodic*, going through the identical motions forever.

We thus see the importance of very simple-looking questions, such as 'does the path traced in phase space close up?' Poincaré's contribution was to find ways to tackle such questions *directly*, without first solving the equations or calculating the numbers involved. He did not settle the question of the stability of the solar system: that had to wait until the 1960s. But he made such a dent in it that in 1889 he was awarded his coveted Oscar, and he thoroughly deserved it. He had invented a whole new kind of mathematics. He showed how to reduce various physical problems to topology, and in some cases solved the resulting topological problems too. For instance, he proved that in the motion of three bodies there are always infinitely many distinct periodic motions, provided a certain topological

theorem holds. In 1913, the year after Poincaré died, George Birkhoff proved the theorem.

The Great Escape

If the motion of the Moon can be predicted accurately, it is possible to navigate by observing its motion against the background of stars. So practical is *this* problem that a substantial sum of money was on offer for its solution. For this and other less mercenary reasons a lot of effort was devoted to the three-body problem: the motion of a system consisting of three point masses (such as Sun, Earth, Moon) moving under Newtonian gravitation. It's easy enough to write down the appropriate equations of motion; but immensely harder to solve them. A simple closed form solution like Keplerian ellipses doesn't exist. One practical method is to approximate the true motion by series. This can be done effectively, but the formulae get very complicated. Charles Delaunay devoted an entire *book* to one formula for the Moon's motion. Another method is to solve the equations numerically on a computer, but that's best for special problems, like landing on the Moon, and less helpful for general understanding.

As an aside: it has been said that one can gauge the progress of science by the value of n for which the n-body problem *cannot* be solved. In Newtonian mechanics the 3-body problem appears to be insoluble. In Relativity, it is the 2-body problem that causes trouble. Quantum Theory gets hung up on the 1-body problem (a particle); and Relativistic Quantum Field Theory runs into trouble with the 0-body problem (the vacuum)!

Messy calculations get answers, but often provide little insight. The modern approach to the 3-body problem is topological: its aim is to derive the main qualitative features. It is now known that these can be extremely complicated. For example a light body orbiting a pair of heavy ones can perform virtually any sequence of turns round first one, then the other.

Just how complicated the problems are is demonstrated by recent work on *singularities* in the n-body problem: events beyond which the motion cannot be continued within the mathematical model

provided by Newton's laws. The simplest singularities are collisions, where several bodies meet. In 1897 Paul Painlevé proved that for three bodies, the only possible singularity is a collision. A two-body collision can be 'regularized' by making the bodies bounce elastically off each other: in this way a solution valid for times after the collision may be obtained. A simultaneous collision of three bodies cannot be regularized in this way: as far as the Laws of Motion go, such a collision might as well be the end of the universe, for there is no way to predict how the system will behave afterwards. Painlevé suggested that with more than three bodies there may exist much nastier types of singularityfor example bodies escaping to infinity in finite time, or oscillating ever more violently.

In 1984 Joseph Gerver (following earlier work by H. J. Sperling, Donald Saari, John Mather, and R. McGehee) gave a non-rigorous, but convincing, argument for the existence of configurations of five point masses which, under their mutual Newtonian gravitational attraction, fling themselves out to infinity in a finite time. The proposed mechanism is ingenious. Imagine three massive stars. (The astronomical terminology is just to make it easier to remember the orders of magnitude of the masses: I repeat that the bodies are *points*.) Arrange all three stars in a triangular constellation with an obtuse angle at the heaviest one, and start them out in such a way that the triangle expands at a uniform rate while maintaining its shape. Add a tiny asteroid which orbits round the outside of all three, approaching them very closely. As the asteroid passes each star it undergoes the same 'slingshot' effect that was used by the Voyager spacecraft as it journeyed through the Solar System. As a result, it gains a small amount of kinetic energy from one star, which it can then share with the other two to increase their speeds (and its own). If only there were a way to increase the energy of the first star as well, it would be possible to make the triangle expand at an ever-increasing rate, disappearing to infinity in a *finite* time.

Of course, the law of conservation of energy forbids this. But there's a way out. Add a fifth body, a planet orbiting the heaviest star. Now make the asteroid gain energy at the expense of the planet, and transfer some of it to its star via a zig-zag slingshot. Otherwise, proceed as before. Now, as time

increases, the constellation expands ever faster but maintains its shape; the asteroid whizzes round ever more rapidly in its triangular orbit; and the orbit of the planet descends ever closer to its star. As a result, *all five bodies* escape to infinity in a finite time.

It's a nice idea, but it involves a very delicate balancing act. You've got to arrange for an infinite sequence of slingshots to take place without disturbing the general scenario. Gerver's original arguments on this point lacked rigouras he himself pointed outand the detailed calculations became so messy that the proof couldn't be pushed through. However, stimulated by Gerver's new ideas, Zhihong Xia proved in 1988 that the 5-body problem really does have a solution in which all five bodies escape to infinity in finite time. His scenario is different from Gerver's, and requires three dimensions of space rather than the two that suffice to set up Gerver's system. It can most simply be described as a game of cosmic tennis, played by two binary star systems which bat a planet to and fro between them (Fig. 24 below). At each 'hit' in the cosmic baseline rally the planet moves faster and faster, while the 'court' lengthens rapidly as the binary pairs move outwards along the axis. The gain of energy experienced by the planet when it passes near a binary pair is compensated by a loss of energy of the pairjust as a grunt transfers energy from a tennis-player to the balland the stars in each pair move closer together, 'dropping into lower orbit'. As the

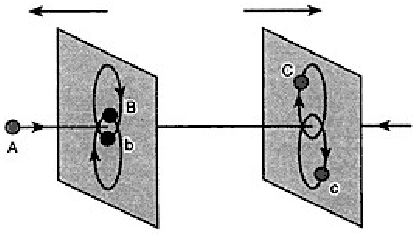


Fig. 24.
Xia's game of cosmic tennis. A is a planet, and Bb, Cc are pairs of binary stars, all having the same mass.

rally continues, the planet oscillates more and more wildly, while the binary stars head outwards at a faster and faster rate. The speeds increase exponentially, and the entire system of five bodies disappears to infinity after a finite period of time. The difficult part of the proof is to arrange the initial distances and speeds so that the celestial rally goes on forever. Xia achieves this with the aid of 'symbolic dynamics', a method that provides approximate information about the motion by representing some of its features as a string of symbols.

Xia's system requires three dimensions of space to pull off the trick. By 1989 Gerver had come up with yet another approach, not requiring a third dimension. He used a symmetry argument suggested by Scott Brown, to prove that the 3*n*-body problem permits an escape to infinity for large enough *n*. Symmetry makes the calculations tractablethough still far from easy. The configuration required is a descendant of the triangle of stars. Take *n* pairs of binary stars, of equal mass. Each pair rotates around its mutual centre of gravity in a nearly circular orbit, and the centres of mass of the pairs lie at the vertices of a regular *n*-gon. A further *n* planets, whose masses are equal, but much smaller than the mass of the stars, follow orbits that approximate the edges of the polygon. Whenever a planet approaches a binary star, it gains kinetic energy via the slingshot effect, while the binary star loses kinetic energy and moves to a tighter orbit. The planet also transfers momentum to the binary pair, causing it to move outwards, away from the centre of the polygon. Because of symmetry, all pairs are affected in exactly the same manner at exactly the same time. At each slingshot, the polygon grows, the planets speed up, and the binary stars close up into tighter orbits.

By suitably adjusting the number of bodies, their masses, and their initial positions and velocities, infinitely many slingshot events can be made to occur during a finite time. The polygon expands to infinity, while the encircling binary stars oscillate more and more wildly as their orbits shrink to zero radius.

Symmetry effectively reduces the problem from 3*n* bodies to three. Once we have determined the positions and velocities of one binary star and one planet, the *n*-fold rotational symmetry determines those of the remaining 3*n* 3 bodies. Thus we can keep track of just three

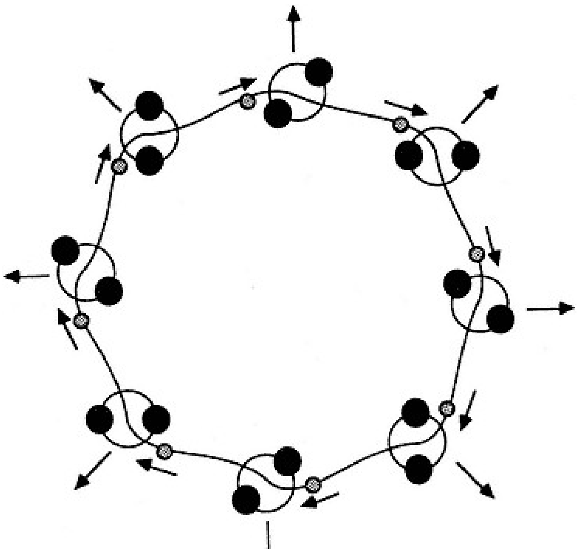


Fig. 25.
Scenario for the Great Escape.

bodies, the rest being taken care of by symmetry. In other words, the problem reduces to a different problem about three disconnected 'bodies' (each an n-gon of point masses) moving under rather complicated forces. This problem is tractable, at least for large enough n, although the calculations remain far from easy.

The Great Escape is purely a mathematical phenomenon. In the real world particles aren't points, so the binary stars would eventually collide. Moreover, real velocities are limited by the speed of light, so nothing can *ever* escape to infinity in finite time.

Is *that* why the deity made the universe relativistic?

The Stratosphere of Human Thought

After its impressive beginning, with Birkhoff's proof of Poincaré's conjecture on the 3-body problem, topology rather faded from the view of the scientific public. This happened because topological problems are *not* always easy to solve. Calculus got off to a good start because there are plenty of functions that you can easily differentiate or integrate, and plenty of differential equations that *can* be solved, even if the most interesting ones can't. Topology, picking up at the point where calculus got stuck, was deprived of easy conquests. For example you can give a nice topological proof that a pendulum oscillates periodically; but since this is already known (even though the usual proof is a fudge) nobody except topologists will be impressed by it. Because it was so difficult to solve the topological problems posed by physics, mathematicians were unable to act as short-order cooks for the physicists, with the motto 'bring us your problem in topological form and we'll tell you the answer'. So mathematicians had to start over, and develop a whole new chestful of tools for handling topological problems.

I don't intend to suggest that topology was isolated from other branches of *mathematics*: quite the contrary. In the 1930s and 1940s Lefschetz applied it to algebraic geometry, and Marston Morse to differential geometry (in work that has since become important in scientific applications). But topology wasn't widely seen as What Every Young Physicist Should Know. Indeed, by the 1950s, all of this activity had got pretty far removed from physics. It had to: that's the direction topology led. So topology turned inward upon itself, to solve its internal problems. Physicists promptly lost interest in it: to them it looked about as relevant as the sage Gautama sitting cross-legged under a tree meditating on the nature of suffering. To a non-specialist, the degree of introspection required to sort topology out appears extreme. Thus Alexander Solzhenitsyn, who had enough mathematical training to recognize this self-absorption, but lacked the sense of history to understand the reasons for it, wrote in *The First Circle*:

> Nerzhin, his lips tightly drawn, was inattentive to the point of rudeness: he did not even bother to ask what exactly Verenyov had written about this arid branch of mathematics in which he himself had done a little work for one of his courses. Suddenly he felt sorry for Verenyov. Topology belonged to the stratosphere of human thought. It might conceivably turn out to be of some use in the twenty-fourth century, but for the time being
>
> '*I care not for the sun and the stars,*
> *I see but man in torment.*'

But the modern prophet of Mother Russia has been proved wrong within his own lifetime. Almost everything interesting being done in modern dynamics involves a more or less heavy dose of topology, which has brought a semblance of order to what looked, twenty years ago, to be a hopeless muddle and a baffling enigma.

The World in a Hairy Ball

To see the way in which topological insight can illuminate a wide variety of apparently unrelated things, consider one of its minor triumphs, which says (in suitably refined language) that you can't comb a hairy ball to make it smooth everywhere. Here are some direct consequences:

(1) At any moment, there is at least one point on the Earth's surface where the horizontal windspeed is zero. (If not, comb along the wind.)

(2) A spherical magnetic bottle, as used in 1950s fusion reactor experiments, must leak. (It leaks where lines of magnetic force run perpendicular to the surface of the bottle, i.e. where the horizontal component is zero.)

(3) Dogs, bears, cheetahs, and mice must have a parting in their fur. (Self-explanatory.)

(4) Every polynomial equation has a complex root. (Technical proof based on the fact that the complex numbers, plus a point at infinity, form a sphere.)

(5) A chemical gradient on the surface of a cell must have a critical point (where there is no local 'downhill' direction).

This is a relatively simple case, and the 'applications' are not necessarily burning issues at the frontiers of science. But recent uses of more difficult and technical topological theorems most certainly are, notably in the current search for a way to unify the four fundamental forces of Nature. Think of it merely as an accessible example of the way topological reasoning can be all-embracing.

The Advantages of Navel-Gazing

The subjects that originally provoked the creation of topology are no longer fundamental physics. The stability of a Newtonian Solar System is beside the point when the currently interesting one obeys Einstein's laws, not Newton's. And anyway, atomic theory says that the Sun will eventually blow up, making the long-term future of heliocentric planetary motion, either Newtonian or Einsteinian, moot. All of these far-reaching changes to science occurred over a period of some five decades, during which topology did little save contemplate its own navel. (Mind you, what subject is better fitted to do this?) Since the whole self-indulgent exercise's sole practical motivation was in Newtonian physics, it looks as though, while the doctor debated hypothetical points of treatment, the patient died. But paradoxically topology, now honed to a fine cutting edge, remains highly relevant to fundamental physics and other sciences. For example (2) is about nuclear engineering, (5) biochemistry. Not only did Poincaré not design topology with applications to these subjects in mind: the subjects themselves did not exist when topology was born.

How can this be? Was Poincaré a better prophet than Solzhenitsyn? Well, very likely, but that's beside the point. The current success of topology, some examples of which we examine in detail in later chapters, owes little to Poincaré's abilities at crystal-gazing, but an enormous amount to his mathematical imagination and good taste. Topology is a success precisely *because* it forgot the details of those original problems, and instead concentrated on their deeper structure. This deep structure occurs in any scheme of mathematical investigation that makes use of continuity. In consequence, topology

touches on almost everything. Of course, the extent to which this fact helps depends on the question you want to answer.

Beware the Galactic Mouse!

We'll take a further look at topology's contribution in the next few chapters; but it would be unreasonable to close this one without giving the modern answer to King Oscar's problem. In 1963, using extensive topological arguments, Kolmogorov, Vladimir Arnol'd, and Jurgen Moser were able to respond to the question 'Is the Solar System stable?' with a resounding and definitive answer: 'Probably'. Their method (usually called KAM Theory) shows that *most* of the possible motions are built up from a superposition of periodic ones. The planets never *exactly* repeat their positions, but keep on almost doing so. However, if you pick an initial set-up at random, there is a small but non-zero chance of following a different type of motion, whereby the system *may* lose a planet (or worse)though not terribly quickly. It all depends on exactly where, and at what velocity, the planets are placed by the Hand of Creation. The fascinating point is that there is no way to tell by observation which of these two types of behaviour will occur. Take any configuration that leads to almost periodic motion; then there are configurations as close as you like where planets wander off. Conversely, adjacent to any initial configuration from which a planet wanders off, there are others for which the motion is almost periodic. The two types of behaviour are mixed up together like spaghetti and bolognese sauce. If a mouse on a planet a billion light-years away were to twitch one whisker, it could switch the Solar System from a stable configuration to an unstable one, or vice versa. It would be difficult to confirm this theory by direct observation, just as it is hard to confirm the universality of Newtonian theory. But KAM Theory does make predictions that can be tested observationally, for example about the spacing of asteroids round the Sun, and Saturn's rings. It also applies to Quantum Theory, for example the instabilities of particles in accelerators. Topology has revealed complexities that King Oscar never dreamed of; but it has also given us the power to tame them and exploit them.

15 The Patterns of Chaos

Chaos of thought and passion, all confused;
Still by himself abused, or disabused;
Created half to rise, and half to fall;
Great Lord of all things, yet a prey to all;
Sole judge of truth, in endless error hurled;
The glory, jest, and riddle of the world!
Alexander Pope

'Rough winds', said Shakespeare, wondering aloud whether to compare thee to a Summer's day, 'do shake the darling buds of May.' We get the rough winds most years; but May 1985, for example, will be remembered mostly for being *cold*. In 1984 our ancient apple-tree produced over 400lb. of apples, ending up as 18 gallons of apple wine, dozens of pies, pounds of purée, and finally compost. A year later an Easter snowstorm damaged either buds, or bugs, or both, and we got a miserable 20lb. I'd hate to guess about this year's crop: British weather is notoriously unpredictable. But what do we *mean* by that?

The Age of Reason, which was Newton's legacy, viewed the System of the Worldthat is, *everything*as some gigantic and intricate clockwork machine. If you knew enough about the way the machine fitted together, you could in principle say what was going to happen from now till the crack of doom. In practice nobody could predict what would happen to a mere three bodies under the action of gravity, but grand philosophical positions are not renowned for taking such material facts into account. The mathematical foundation of this view was that the motion of everything in the universe is described by some differential equation; and if you know the

'initial conditions' then all subsequent motion is fixed uniquely. So why can't you predict the result of rolling a pair of dice? Because you don't know enough about the initial conditions. If you rattle the dice in a cup, you have no idea what's happening to them. But once they hit the table, it would be possible to take a series of rapid snapshots, work out the positions and velocities, stick the whole thing on a supercomputer, andif the computer were fast enoughwork out which faces would end up on top, *before* the dice worked it out for themselves.

Determinism

Laplace stated the viewpoint clearly.

> An intellect which at any given moment knew all the forces that animate Nature and the mutual positions of the beings that comprise it, if this intellect were vast enough to submit its data to analysis, could condense into a single formula the movement of the greatest bodies of the universe and that of the lightest atom: for such an intellect nothing could be uncertain; and the future just like the past would be present before its eyes.

This is the classical view of *determinism*. In principle, the entire history of the universe, past and future, is determined once you know exactly where every particle is going, and how fast, at one single instant of time. This leads to a delicate ethical and moral problem, that of free will. As Voltaire pointed out, 'it would be very singular that all nature, all the planets, should obey eternal laws, and that there should be a little animal, five feet high, who, in contempt of these laws, could act as he pleased, solely according to his caprice.' Does such a thing as free will exist? As a practical matter, even if it does not, its non-existence appears to constrain us *to act as if it did*. That is, when all the atoms in my body, following nature's immutable laws, cause me to mug an old lady, then those same immutable laws cause the magistrate to send me down for a five-year stretch.

A non-deterministic, or stochastic, system behaves quite differently. The same initial conditions can lead to different behaviour on

different occasions. For example imagine a pendulum swinging in a random breeze. The equation describing the motion will have explicit random terms in it; and even if you set everything going from exactly the same starting-point, interference by the breeze will soon change the behaviour. Let me emphasize that in some sense the distinction is a modelling one. If we add deterministic equations describing the behaviour of the breeze, then the initial conditions for *that* also come into play, and the whole game becomes deterministic again. Indeed, it is impossible to prove any event is really random by repeating an experiment exactly: either time or place must differ, otherwise it's the *same* experiment. This again emphasizes the need to discuss the difference between determinacy and randomness within an agreed context, a specific model.

The extent to which practice fails to measure up to the deterministic ideal depends on how accurately the initial conditions can be observed (and, of course, on whether you're using the right differential equation). So, if you want to predict the weather mathematically, you need two ingredients:

(1) As accurate as possible a system of differential equations to describe the motion of the Earth's atmosphere.

(2) The best possible observations, using ground stations, ships, aircraft, meteorological balloons, and satellites.

After that, it's just a matter of computation, right? By now you won't be surprised to find it isn't quite that straightforward. For a start, if you make the differential equations *too* accurate, you end up modelling sound waves, not weather; moreover, these resonate with your numerical method, blow up, and swamp everything interesting. The solution is deliberately to coarsen the accuracy of the equations! But there's a much more serious problem. Topologists, following the lead of Poincaré in qualitative dynamics, asked whether a typical differential equation behaves predictably. To their surprise, Smale discovered that the answer is 'no'. Determinism is not the same as predictability. Indeed a perfectly deterministic equation can have solutions which, to all intents and purposes, appear random.

I'm not sure whether this solves the moral problem of free will,

but it certainly puts it in a new light. You may recall how Douglas Adams, in *The Hitch-Hiker's Guide to the Galaxy*, tells of a race of hyperintelligent pan-dimensional beings who build a supercomputer called Deep Thought. When asked for the answer to 'Life, the Universe, and Everything', it announces that it can find it, but the program will take some time to run: about seven and a half million years. At the end of that time it announces the answer: 'forty-two'. Which leaves just one further problem: to work out what the question was. Laplace's hypothetical intellect looks about as practical a proposition to me; and its answers would probably be equally intelligible.

Familiar Attractors

Topologists naturally think of a differential equation in a more geometric way than analysts do. They think of it as a flow. The state of the system is described by a point of a multidimensional phase space; and as time flows, so does the point. The path traced out in phase space by a moving point represents the evolution of the state of the system from a particular initial condition. The system of all such pathsthe *phase portrait*gives a qualitative picture of what happens to *every* possible initial condition. So the question is: what are the typical kinds of phase portrait?

The simplest systems are those that settle down to a steady equilibrium. Here all the paths converge on to a single point; that is, all histories end up in the same unchanging state. Topologists say that the point is an *attractor*. More generally an attractor is some region of phase space such that all nearby points eventually move close to it. The attractors describe the long-term evolution of the system.

The next simplest attractor is a *limit cycle*, where paths converge on to a closed loop. This corresponds to a periodic oscillation of the state of the system; periodic because the representative point keeps tracing out exactly the same sequence of events as it goes round and round the loop.

Finally there is *quasiperiodic* motion, in which a lot of periodic motions with different periods are superimposed. The Earth going round the Sun, the Moon round the Earth, a spacecraft orbiting the

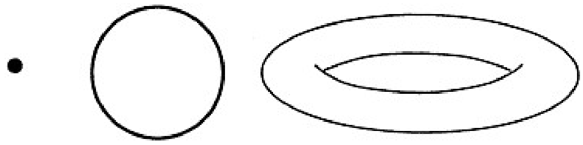


Fig. 26.
Classical attractors: point, circle, and torus.

Moon, and an astronaut swinging a cat inside itassuming there's room to swing a cat in a spacecraft. The cat's motion is compounded from four different periodic motions, with different periods. Over millions of years the cat will come very close to repeating its previous movements, but (unless all four periods are exact integer multiples of some fixed time interval) it will never repeat exactly. Topologically we put together the four separate circles corresponding to the four periodic motions, getting a 4-dimensional torus as the attractor.

These are the three familiar types of behaviour recognized in classical work on differential equations, though of course their discoverers didn't think in terms of topological attractors in phase space. Each is deterministic, in the sense that the corresponding equations have unique solutions for unique initial conditions; and also predictable in the sense that, if you know the initial conditions accurately enough, the system will always stay close to what it would have done if those initial conditions were infinitely accurate. It's never possible in practice to measure anything exactly: there are bound to be limitations on the precision of measurement. So practical predictions have to be able to work with slightly inaccurate data; and the hope is that the predictions will also be only slightly inaccurate. For the classical attractors, this is so.

Strange Attractors

So what the topologists asked themselves was: is every typical attractor classical? The restriction to typical cases occurs for the

same reasons as in many other branches of mathematics: there are millions of infinitely rare accidents that complicate the picture impossibly if you try to describe them too, but which only occur in practice if some special constraint is forcing them to. In which case you'd be better off with a specially tailored theory to suit that constraint.

There are two main kinds of dynamical system, known as *discrete* and *continuous*. The difference is that in the first, time flows in definite steps 0, 1, 2, 3, , whereas in the second it flows continuously. The discrete case is mathematically more tractable, especially for constructing and analysing fancy examples. And you can turn any discrete system into a continuous one, by 'interpolating' the behaviour between successive time instants. All that happens is that the phase space grows one extra dimension to give the interpolation somewhere to live. The technical name for the process is *suspension*. Conversely, you can often understand the behaviour of a continuous system by watching how representative points flow through some lower dimensional cross-section. This forms a discrete system since there is a definite interval between returns to the chosen slice. The upshot of all this is that for theoretical purposes 2-dimensional discrete systems are much like 3-dimensional continuous ones, 3-dimensional discrete like 4-dimensional continuous, and so on. I can now describe a 3-dimensional discrete system having an attractor that is typical, and not classical. Its suspension will be a 4-dimensional continuous system with the same properties. The phase space is a *solid* torus. At successive instants of time, its points move as follows. Stretch the torus out to ten times its length and one tenth the thickness, and wrap it round *ten times* inside itself, in a tenfold loop. (Actually, two will do in place of ten, but it complicates life later.) At the next instant it will stretch more, shrink more, and wrap a hundred times; then a thousand, ten thousand Any initial point will move into successive tori in this sequence, so the attractor is the set of points common to all of the multiply wrapped subtori. It's pretty clear that this set is not a classical point, circle, or torus. The topologists call it a *strange attractor*. It is strange because, unlike the classical attractors, it has 'structure on all scales'. If you magnify a segment

of a circle, it looks pretty much flat; and the same goes for a torus. But if you magnify the strange attractor, it retains its detailed structure of nested tori. A set with structure on all scales is called a *fractal*. So the distinction between the newfangled strange attractors and the familiar classical ones is geometric: the old ones are manifolds, the new ones fractals. The term *fractal attractor* is gaining popularity. I'll have more to say about fractals in the next chapter.

Let Your Digits Do the Walking

To see where the element of unpredictability comes in, consider an arbitrary initial point, and ask: where does it go? You get a solid torus by pushing a disc round a circle, which forms a 'core'. You can specify a point in a solid torus using three coordinatestwo to specify a point in the original disc, and one for the angle the disc has to be pushed through. To keep the discussion simple, consider just this angle θ. Since we stretch the torus to ten times its length and wrap ten times, each step takes θ to 10θ.

It's convenient to measure θ so that the full 360° circle corresponds to one unit. We can tell roughly where a point is by dividing the circle into ten sectors labelled 0, 1, , 9. Take an arbitrary starting-point, say at angle 0.37528 units. On the first wrap it goes to ten times this, namely 3.7528; but angle 1 is the full 360° and this is the same as angle 0. So we have to chop off the part in front of the decimal point, getting, 0.7528. By similar reasoning this goes in turn to 0.528, 0.28, 0.8, and then settles at 0. This is striking. You can predict the entire future of the point by using the successive decimal digits of the angle representing it. At the *n*th step, it ends up in the sector indicated by the *n*th decimal digit.

This particular initial point eventually reaches 0 and stops, but that happens only because its decimal expansion terminates. Out to infinity it is really 0.375280000000 . If instead we use 0.37528000 0063291 (say), with twenty-nine billion zeros where the dots are, its history is quite different. It also goes to sectors 3, 7, 5, 2, 8, and then sits in sector 0. But instead of staying there forever, as 0.37528 did, it only sits there for the next twenty-nine billion steps. On the

twenty-nine billion and first, it hops to sector 6, then 3, then 2, then 9, then 1. And by extending the sequence of digits you can make it do whatever you wish. The initial points coincide for twenty-nine billion and five decimal places (far more accurate than any practical measurement could possibly be); but their eventual histories are *completely independent of each other*. This effect is called 'sensitivity to initial conditions'. Now in any interval, however small, you can find real numbers whose sequence of decimal digits, from some point on, follows any pattern you wishincluding completely random series of digits. In fact *most* sequences of digits are random, see Chapter 22. So as close as you wish to any initial condition you can find initial conditions leading to completely random motion.

Fractal Weather

If strange attractors are typical, how come nobody spotted them earlier? There are two reasons. One is the classical obsession with analytical or closed-form solutions: these are too nicely behaved to lead to strange attractors. Numerical solution on a computer, only recently a viable option, produces strange attractors just as easily as classical ones. And in fact people *did* observe strange attractors in numerical studies. But, with very few exceptions, they assumed something had gone wrong, either with the theory, the calculation, or the experiment; so they threw the results away. Publication was never even contemplated. Random behaviour in a deterministic system was not expected, therefore it was not believed when it was seen. Only after the topologists showed that it *was* expected did people start to publish papers saying that they'd found it.

A gallant exception was a paper by the meteorologist E. N. Lorenz in 1963. Appropriately, Lorenz was modelling the weather. However, his equations are a very rough-cut approximation to anything happening in real weather. The Lorenz system is an oscillator: it wobbles to and fro like a pendulum. But, quite unlike a pendulum, it doesn't oscillate regularly. In fact, its wobbles look pretty much random. The Lorenz system has a strange attractor.

Lorenz did publish his results, but (strangely!) they attracted little attention. So, when the topologists were developing their theories, they didn't know what Lorenz had done, and they rediscovered strange attractors in their own way. Only later was everything put together. And because the topologists hadn't known of the Lorenz attractor when they were setting up their general theory, it turned out to be an even more complicated attractor than the ones they could comfortably handle. Everyone who has ever solved Lorenz's equations on a computer has obtained the same general picture of the Lorenz attractor: a two-lobed fractal surface somewhat like the Lone Ranger's mask, which has become one of the most famous icons of Chaos Theory. However, computers do not store numbers correct to infinitely many decimal placesthey use 'finite precision' arithmetic, which introduces round-off errors. The fine detail of the Lorenz attractor therefore looks different on different types of computer. This raises the question whether the Lorenz attractor is just an artefact of the finite-precision approximation, or whether it is genuinely present in the exact solution to the original equations. The answer is not at all obvious, and a few diehards have even stated that the irregular behaviour found by Lorenz is entirely due to computer error.

A rigorous proof that Lorenz's system is chaotic had to wait until 1995, and even that didn't pin the chaotic nature of the Lorenz attractor down completely. The proof was devised by Konstantin Mischaikow and Marian Mrozek, and like the proof of the four-colour theorem it is 'computer assisted'. A different proof, also computer assisted, has also been given by Brian Hassard and colleagues, but it is slightly less specific. Computer assisted proofs are just as rigorous as those done with pencil and paper, because in such proofs the role of the computer is merely to perform certain lengthy but routine calculationswhich could in principle be done by hand. Mischaikow and Mrozek set up a mathematical famework that corresponds to the finite-precision calculations of the computer; and their main effort goes into devising a theory (of so-called 'finitely representable multivalued maps') that can link the finite-precision calculations to the infinite precision required in conventional mathematics. Those calculations are then fed into a topological argument

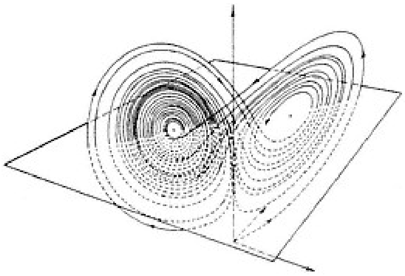


Fig. 27.
The Lorenz attractor, which produces 'random' dynamics in a deterministic equation derived from a model of the weather. (From John Guckenheimer and Philip Holmes, *Nonlinear Oscillations, Dynamical Systems, and Bifurcations of Vector Fields*, Springer, New York, 1983.)

(known as the 'Conley index') which establishes the occurrence of chaos. Although this approach does not yet prove that the chaos comes from anything definitely known to resemble the two-lobed surface in the computer pictures, it seems only a matter of time before this final step is taken.

Three Theories of Turbulence

One of the most classical parts of applied mathematics is the dynamics of fluids. These are governed by various differential equations, used for different purposes, and among the most general are the *NavierStokes Equations*. These have an excellent track record of agreement with experiment, and apply equally well to fluid flowing through channels and pipes, round obstacles, and over dams. They describe the motion of waves; and according to recent computer studies they even describe how waves break. Despite these successes, there is a type of fluid flow that has baffled hydrodynamicists for a century or more: *turbulence*. The theories work well on smooth flows, such as a tap turned on gently; but they have little to say

about the irregular frothing flow that occurs when a tap is turned on full. On the whole, a fluid becomes turbulent when its speed becomes too great. Turbulence is both regular and irregular. In laboratory experiments, it usually sets in at much the same point in the experiment, in much the same way, and with much the same general appearance. It may appear suddenly or gradually, or take on distinct forms, but it is repeatable. On the other hand the actual motion is chaotic, random, and unpredictable. Its statistical features do tend to have regular patterns, and engineers often model turbulence with random terms in the equations. Even though a great deal of work has been done on turbulence, its fundamental *mathematical* nature is still a puzzle.

The Leray Theory of turbulence, dating from 1934, asserts that when turbulence occurs the NavierStokes equations break down as an adequate description of the fluid. In other words, turbulence is a fundamentally different phenomenon from smooth flow. The HopfLandau theory of 1948 disagrees, and holds that turbulence is a quasiperiodic flow in which infinitely many different period motions combine to give the appearance of randomness. In 1972 David Ruelle and Floris Takens criticized the Hopf-Landau theory on the grounds that it is topologically unlikely. It requires the fluid to undergo a series of changes which from the topological viewpoint should never be able to get off the ground. An infinite series of coincidences is required for the Hopf-Landau mechanism to work. Ruelle and Takens suggested instead that turbulence is the formation of a strange attractor in the dynamics of the fluid. This is with hindsight a plausible idea: random behaviour and sensitivity to initial conditions are characteristic of turbulence and strange attractors alike. The physicist Mitchell Feigenbaum had a similar idea with a rather different topological scenario. The proposal was highly controversial and did not appeal to many classical hydrodynamicists.

How do you find out what a fluid is doing? The classical method is to eject a stream of dye, or insert probes or hot wires at various points. The development of the laser makes it possible to obtain much better results, more quickly, and with less disturbance to the fluid. The idea is to suspend tiny particles in the fluid, and bounce a laser beam off them. Their speed can be found using the Doppler effect, a shift in the wavelength of the reflected light. Experiments by Harry Swinney and his collaborators, using laser Doppler velocimetry, have done much to confirm that, at least in special laboratory systems, the onset of turbulence conforms to a strange attractor theory. The experiments yield precise measurements of how the fluid velocity at a single point varies over time. But how can we get from the velocity at a point to the dynamics of the entire fluid? Quantitatively, there's not much chance. But qualitatively, it's not hard. New techniques, known as *phase space reconstruction*, make it possible to deduce the topology of the corresponding attractor. These techniques are currently being developed into entirely new tools for analysing experimental data and extracting patterns.

What Use Is Chaos?

A fair question. There are several ways to answer it.

The simplest is the 'what use is a new-born baby?' approach. New ideas have to mature before they affect company balance-sheets.

Another is to compare it with the question 'what use are shock-waves?' The mathematics of compressible fluid flow is enormously complicated by their presence. They are the reason behind the 'sound barrier', which caused severe problems in the development of supersonic flight. You don't *use* shock-waves so much as overcome them. The point is, they're *there*. The existence of shock-waves is a mathematical discovery; but you don't make aerodynamics work any better by 'undiscovering' it. The shock-waves don't go away if you avoid the mathematics that proves their existence. In the same manner, chaos doesn't go away if you ignore it. It is a phenomenon that affects the whole of nonlinear dynamics, and hence almost all areas of applied science. Even if your only interest in chaos is to get rid of it, you'd better understand how it comes about. *Know your enemy*.

But the best answer is to tackle the question head on. Chaos *is* useful. It helps us to understand complicated phenomena, such as epidemics, or irregularities of the heartbeat. More than that: you can exploit chaos to attain a desired end. Let me offer four examples

the fourth being rather speculative, to stimulate your imagination rather than to prove any particular debating point.

The first example is mixing. Many industrial processes require ingredients to be mixed together thoroughly and uniformly. However, you don't want your mixing equipment to be incredibly complicated and 'random': you want something nice and simple that your engineers can maintain in good condition. In other words, you want a simple deterministic system that produces random behaviour. Mixing-machines are chaos generators, virtually by definition. Think of an egg-beater, for instance. Two interlocking paddles rotate at constant speed: the egg goes all over the place. A deterministic process produces a chaotic result.

Chaos has led to new insights into the mathematics of mixing. In particular it turns out that many simple mixing devices tend to leave 'dead areas' that are not well mixed. The reason is that the materials being mixed are (approximately) incompressible, and the dynamics is rather special: it is an instance of what's called a *Hamiltonian system*. The dynamics of the solar system is Hamiltonian; and we've already seen how counter-intuitive some features of celestial mechanics can be. Hamiltonian chaos has a very special flavour. Uniformly chaotic flow is hard to achieve in Hamiltonian systems: one of the basic phenomena is the tendency for 'islands of stability' to occur, even when surrounded by a sea of chaos. These islands create the 'dead areas' of poor mixing.

A second application is to noise reduction. A typical problem is to extract meaningful conversation from a recording made in a room with a *very* noisy air-conditioner. The traditional approach is to think of the conversation as a 'signal' and the air-conditioner as a source of 'noise'. The noise is treated as a random disturbance to a regular signal. Provided the signal-to-noise ratio is large enough, much of the noise can be filtered out by appropriate mathematical procedures. But when the air-conditioner noise exceeds the level of conversation by a large amount, there's no hope for the traditional method.

Let's think chaos, however. From this viewpoint, it is the *air-conditioner* that is the signalit is produced by a deterministic mechanical process. The conversation is non-deterministicyou can't

hope to predict what the people will sayand should be thought of as *noise*. Our perception of what is noise and what is signal is now reversed. Instead of the noise being much larger than the signal, the signal is much larger than the noise. Admittedly, it's the wrong sort of signal for the traditional approachit's not regular enough. The new methods of phase space reconstruction, however, are designed to handle chaos, and they let you make short-term predictions of the chaotic signal, which filter away the noise. Great: now you have a cleaned-up recording of an air-conditioner, with the conversation removed. The final step is to subtract this from the original recording, air-conditioner plus conversation. The air-conditioner sounds cancel out, leaving only the conversation. This method works: the conversation is 95 per cent intelligible even when the air-conditioner is 25 decibels louder.

The third application is a piece of work in which I've had a lot of personal involvement: it is having a 'nuts and bolts' effect in an area of manufacturing industryand a 'dollars and cents' effect too, for it is estimated that it will save British industry up to £12 million per year.

In the UK, the spring and wire industries are composed of numerous small companies. They have joined forces to create a research and development organization, the Spring Research and Manufacturers' Association (SRAMA) in Sheffield. The wire-makers buy steel and produce wire, the spring-makers coil that wire into springs, and the springs are put into beds, car engines, ballpoint pens, and electric fires. The wire used to make springs must pass several stringent tests, but despite these, about 10% will not make good coils.

In 1991 Len Reynolds, an engineer at SRAMA, telephoned me to ask about chaos-theoretic methods of data analysis. SRAMA had devised a coiling test, but was having trouble extracting useful quantitative information out of it. An experienced eye could *see* when the wire was coiling badly, but in order to introduce a new test, there had to be a quantitative measurement, not just the opinion of one alleged expert. Reynolds had tried out the simplest version of the technique of phase space reconstruction (originally invented to detect chaos in turbulent flow) on his coiling dataand found that the

results made it possible to characterize good or bad wire in a quantitative manner.

Serious work on this idea began when Reynolds and his boss Derek Saynor secured a grant from the Department of Trade and Industry under its new Carrier Technology programme. It was the first such grant to be awarded, and it provided the wherewithal to build a machine that would automatically coil a test wire into a long spring, measure the spacings of successive coils with a laser micrometer, feed the resulting data to a computer, and analyse it using chaos theory.

The full story is a long one. In brief: we built it, and it works. My colleagues Mark Muldoon and Matt Nicol found that chaos theory is just the right mathematics to detect short-term variability in the wire's coiling properties. Traditional statistical methods can detect overall variability: marry the two together, and you have a simple and effective way to characterize the coilability of wire. The savings arise because without such a test, the spring-maker has to run a batch of test springs right through the entire manufacturing process and then measure them by hand (to see if they are the right length, for example). If the results are unacceptable then the settings on the coiling machine must be changed, and the process is repeated until either the springs come out right or the wire is declared uncoilable. This process can often tie up an expensive coiling machine for an entire working day.

The chaos theory test takes three minutes.

These are technological uses of chaos. What about 'natural' uses? Surely living organisms have no use for chaos?

Maybe they do. The most obvious example, our fourth application of chaos, is flight from a predator. The motions of the prey should be unpredictable, to confuse the predator. But they have to be generated by the prey's nervous system. The simplest way to generate unpredictable movement is to put some neural circuit into a chaotic state, and let that take charge. Some houseflies have a habit of flying along paths that look like random polygons, with sudden changes of direction at irregular intervals. Presumably they're doing this to confuse predators, such as humans armed with fly-swatters. But these

'random' changes have to be controlled by the fly's nervous system, which is not the most complicated of computational devices. Low-dimensional chaos would seem an attractive option. I know of no experimental data on this particular form of behaviour, but it would be interesting to take a look.

If prey do indeed use chaos to help them escape, then it becomes evolutionarily worthwhile for predators to evolve neural circuitry for some type of 'phase space reconstruction', to predict where the prey will go next. This sets up suitable conditions for what Richard Dawkins calls an evolutionary 'arms race'. The result should be increasingly sophisticated chaos-generators from the prey, and better phase space reconstruction from the predators. But perhaps the speculation should stop here.

Symmetric Chaos

Traditional dynamics deals with regular patterns, whereas chaos is the science of the irregular. Its aim is to extract new kinds of pattern from apparent randomness, pattern that would not be recognized as such in a traditional approach. This description makes it tempting to think that order and chaos are two opposite polarities. However, some fascinating computer experiments have revealed that some systems manage to combine order and chaos at the same time.

The traditional source of pattern is symmetry. Anything with a high degree of symmetry is automatically patterned. A few years ago, Pascal Chossat and Martin Golubitsky asked themselves what would happen if you took a symmetric dynamical system and made it operate in the chaotic regime. The answer is that you get strange attractorswith symmetry.

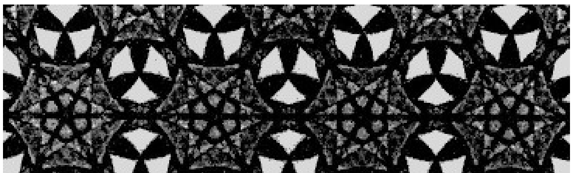
The method is simple. Choose a group of symmetries, say transformations of the plane, such as the symmetry group of a regular polygon. Invent a mapping from the plane to the plane that 'preserves' these symmetries. This means that if you start with two symmetrically related points and apply the mapping to them both, then the two resulting points must be related by the same symmetry. For example, suppose one symmetry is rotation through 72° (five-

fold symmetry). Take a point A, and rotate it through 72° to get another point B. Suppose the mapping sends A to A' and B to B'. Then B' must be A', rotated through 72°.

For any given group of symmetries, it is just a technical calculation to find such symmetry-preserving transformations. Usually there are huge numbers of them, with lots of adjustable coefficientsnumbers that you can choose at will. Having done so, you just iterate the mapping millions of times. That is, you choose a point, apply the mapping, and repeat. The resulting set of points (with the first few hundred omitted to remove 'transients') is the attractor for your chosen mapping.

You get a different attractor for each choice of coefficients. Most of them are symmetric. Many are also chaotic. Different choices of symmetry group yield different symmetries in the attractors. There are 'icons' with the symmetries of a starfish. Indeed, one closely resembles the creature known as a sand-dollar, a fact that is presumably a visual pun. And a different choice of symmetry group produces a chaotic 'quilt' design with a pattern that repeats in a lattice. Patterns of this kind have been investigated by Golubitsky and Mike Field.

There's more to this idea than just pretty computer pictures. Symmetry is fundamental in nature. So, we now realize, is chaos. Therefore the two in combination should be especially interesting. Symmetric chaos is a new area of research, but it has already progressed beyond computer experiments to proving theorems. It appears to offer an explanation of some puzzling phenomena involving 'patterns on the average'. One example is the Faraday experiment, in which a shallow dish of water is vibrated rapidly in the vertical direction. The vibrations create intricate patterns of waves on the surface of the liquid. At certain speeds and amplitudes of vibration, these patterns become irregular, and the waves look 'dislocated', breaking up and merging in a constantly changing manner. The theory of symmetric chaos predicts that if a series of such patterns are 'time averaged' byin effectstacking them on top of each other, then the average picture should reveal regular, symmetric patterns. This surprising prediction has been verified in experiments (Fig. 29).



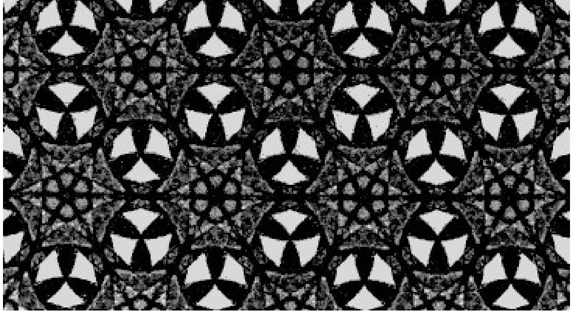


Fig. 28.
Symmetric chaos. A 'quilt' attractor for a system with lattice symmetry (Mike Field and Martin Golubitsky).

The attractors created by symmetric chaos also raise interesting questions about the nature of complexity. The amount of 'information' required to *de*scribe the points in a chaotic attractor is huge. The attractor looks very complicated. But very little informationa single formula plus a few numerical coefficientsis needed to *pre*scribe the mapping that generates the attractor. You could transmit the formula and coefficients over a phone line in a few seconds. To transmit the end result, coordinate by coordinate, would take hours.

This implies that the visual simplicity or complexity of an object may be misleading. What is more important, but far deeper, is the complexity of the *process* that generates it. This in turn means that at least some of the enormous complexity that we see in the world

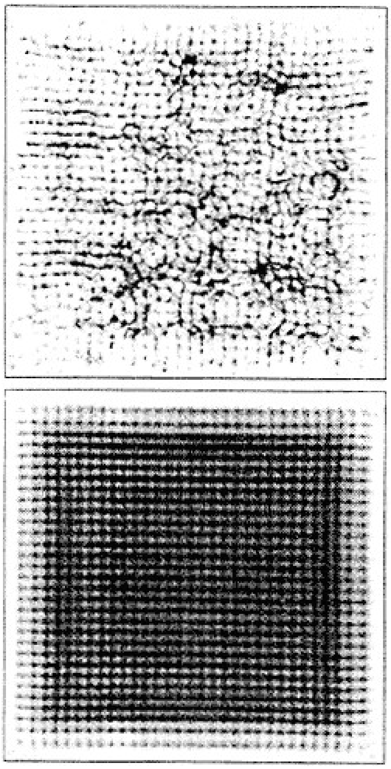


Fig. 29.
(a) Instantaneous and (b) time-averaged images of chaotic surface waves in a square cell of width 10.5 cm, with vertical excitation 50% beyond that required to produce waves by the Faraday instability. Dark regions correspond to the positions of small surface slope (i.e. the wave antinodes). Images courtesy of B. J. Gluckman and J. P. Gollub.

of living creatures may have simple causes. The mathematical development of this point of view is called algorithmic information theory: see Chapter 17.

When Einstein's Theory of Relativity achieved public recognition, most people interpreted it as saying that 'everything is relative', a comfortable philosophy that, for example, justifies the rich ignoring the poor on the grounds that others are yet poorer. However, that's *not* what Einstein was saying: he was telling us that the speed of light is *not* relative, but absolute. It should have been named the Theory of Non-relativity. Something similar is happening with Chaos Theory. Some people are taking it to mean that 'everything is random', and using that to justify economic or ecological mismanagement. How unfair to blame the Chancellor of the Exchequer for not controlling inflation or unemployment, when 'everybody knows' these are chaotic! But Chaos Theory offers no such excuse. Its message is that *some* things that we think we understand may behave in very funny ways, *some* things that appear random may obey laws we haven't yet spotted, and *most* things don't fit into any of these categories at all. Indeed Chaos Theory has opened up new methods for *controlling* systems that appear to behave randomly: the Chancellor must shoulder more, not less, of the blame when the economy goes awry.

Rich Mixture

Fifty years ago the difference between determinism and randomness was straightforward. Equations without random terms were deterministic, and that meant predictable. Which in turn meant that if you were observing something that seemed to behave randomly, then you needed a random model. But this black-and-white picture has given way to a complete spectrum, with varying shades of grey. A system can be deterministic but unpredictable, have no random terms but behave randomly. Many irregular phenomena may perhaps be modelled by simple deterministic equations. On the other hand, simple equations are no longer assumed to imply simple behaviour. Examples other than turbulence include the Earth's magnetic field,

with its random reversals; electronic oscillators; the growth of insect populations; and properties of liquid helium.

A typical dynamical system is usually neither wholly random, nor wholly predictable. Its phase space has regions corresponding to each, often intertwined to form an intricate and rich mixture. And while we now understand that this is so, we can only guess where this observation may lead. It will certainly require new kinds of experimental method; one is described in the next chapter. It may well alter the accepted view of experimental 'predictability' as a criterion for acceptance of a theory.

16
The Two-and-a-Halfth Dimension

Clouds are not spheres, mountains are not cones, coastlines are not circles, and bark is not smooth, nor does lightning travel in a straight line.
Benoit Mandelbrot

How long is the coast of Britain? It depends on how carefully you measure it. There are large wiggles (the Wash and the Bristol Channel), medium-sized wiggles (Flamborough Head, Orford Ness), small wiggles (gaps between rocks on the foreshore at Dover), diminutive wiggles (irregularities in a single rock) The closer you look, the more wiggles you find. So the more carefully you measure the coast, the longer it appears to become. What is the area of the human lung? The same problem arises, because the surface of the lung is folded and branched in a very complicated way, in order to squeeze a large oxygen-gathering capacity inside a small human chest.

The traditional models of theoretical science are objects such as curves and surfaces. Usually these are assumed to be smoothso that calculus can be used to study themwhich means that on a large enough scale they appear flat. For example the Earth, on a rough approximation, is a sphere; and to its inhabitants it appears flat, because not enough of it is visible to notice the very gentle curvature. But the coastline of Britain and the surface of the lung do not look flat, even under a powerful microscope. They remain crinkled. Such objects, possessing detailed structure on many scales of magnification, abound in nature; but only recently have they been identified by scientists and mathematicians as something worthy of study in its own right. The result is a new breed of geometric figure,

called a *fractal*. The name, derived from the Latin for 'irregular', was coined by Benoit Mandelbrot, an IBM Fellow at the Watson Research Center in New York State. Mandelbrot has been the prime mover in the recognition of fractals as a new kind of mathematical object, well suited for the modelling of natural phenomena involving irregularity on many scales.

We generally say that we live in a space having three dimensions (north, east, up). A plane has two dimensions, a line one, and a point zero. Mathematicians have devised spaces with any whole number of dimensions. It turns out that the sensible way to assign a dimension to a fractal leads to *fractional* values, such as 1.4427. This bizarre property is very basic and reflects the fractal's behaviour under changes of scale. The very *existence* of such forms suggests an enormous range of new physical and mathematical questions, by directing our attention away from the classical obsession with smoothness. What happens when light rays pass through a medium whose refractive index varies fractally? If they reflect off a fractal mirror? Or, for the more practically (?) minded: how will the passage of a laser beam through a turbulent atmosphere affect its ability to destroy a nuclear missile? What happens to a radar beam when it bounces off an irregular landscape? What sort of noise will a drum with a fractal rim make? Once our eyes have been opened to the distinctive character of fractals, we observed that the world is full of them. As yet, we know very little about their deeper nature.

Gallery of Monsters

Around the turn of the century a number of mathematicians introduced some novel geometrical forms: highly 'irregular' curves and surfaces. They were seen as 'pathological' objects whose purpose was to exhibit the *limitations* of classical analysis. They were counterexamples serving to remind us that mathematics' capacity for nastiness is unbounded. Curves that fill an entire square, curves that cross themselves at every point, curves of infinite length enclosing a finite area, curves with no length at all. For instance, during the eighteenth and early nineteenth centuries it was assumed that a

continuous function has a well-defined tangent (that is, is differentiable in the sense of calculus) at 'almost all' points. In a lecture to the Berlin Academy in 1872, Weierstrass showed that this is untrue, exhibiting a class of functions which are continuous everywhere but differentiable nowhere. The graph of the Weierstrass function has no gaps, but is so irregular that it has no tangent at any point. There are other continuous but nowhere differentiable functions, including one creature aptly called the *blancmange function*, its graph having multiple peaks.

A classic pathological case is the *Cantor set*, obained from an interval by throwing most of it away. Delete the middle third, to leave two smaller intervals; delete the middle third of each of these, and repeat forever. The total length removed is equal to the length of the original interval; yet an uncountable infinity of points remain. Cantor used this set in 1883; it was known to Henry Smith in 1875. In 1890, Peano constructed a curve passing through *every* point of the unit square. This demolished the current definition of the 'dimension' of a set as the number of variables required to specify its points. The *Peano curve* lets us define the points in the two-dimensional square by using a single variable: how far along the curve we must travel to reach the chosen point. In 1906 Helge von Koch found a curve of infinite length containing a finite area: the *snowflake*. It is obtained from an equilateral triangle by erecting on each side a triangle one-third as large, and repeating this process indefinitely. Like Weierstrass's curve, the snowflake is continuous but has no tangents. However, it is not the graph of a function.

Most mathematicians of the period accepted that these strange sets did demonstrate that there were limits to the applicability of classical analysis. But they were perfectly prepared to operate *within* those limits, and few saw any reason to study the 'pathologies' in their own right. They were seen as artificial objects, unlikely to be of any importance in science or mathematics. Poincaré called them a 'gallery of monsters'. Hermite wrote of 'turning away in fear and horror from this lamentable plague of functions with no derivatives', and he tried to prevent Lebesgue from publishing a paper on non-differentiable surfaces. As we saw in Chapter 13, Lebesgue

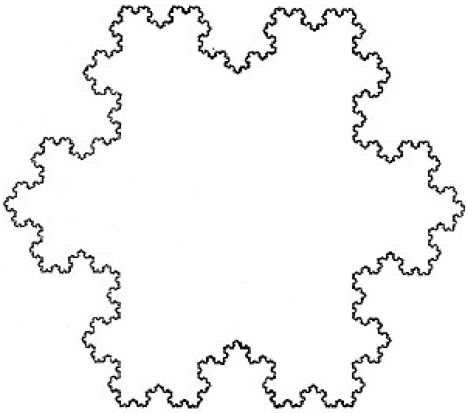


Fig. 30.
The von Koch snowflake curve has infinite length and encloses a finite area. It was one of the earliest fractals to be invented.

invented the enormously important method of integration that still bears his name; he was no intellectual lightweight. He later wrote of Hermite's rejection of his paper: 'He must have thought that those who make themselves dull in this study are wasting their time instead of devoting it to useful research.' And he also said: 'To many mathematicians I became the man of the functions without derivatives. And since the fear and horror which Hermite showed was felt by almost everybody, whenever I tried to take part in a mathematical discussion there would be an analyst who would say, "This won't interest you; we are discussing functions having derivatives."' Lebesgue's experience shows that we must be careful to distinguish between the mathematical mainstream and the purely conventional or fashionable.

To be fair, it is clear that the undue proliferation of such sets, without any clear purpose, is a pretty futile exercise. But with hind-

sight, we observe mathematics starting to grapple with the structure of fractals.

Topological Dimension

There are many different ways to define the dimension of a space. If it is a smooth manifold (the multidimensional analogue of a surface) then the dimension is the number of variables in any *smooth* coordinate system. (Requiring smoothness excludes the Peano curve but restricts the range of spaces to which the definition applies.) For example the surface of a sphere is 2-dimensional, because the two variables latitude and longitude provide a coordinate system. Obviously the dimension in this sense must be a positive integer. Engineers will recognize it as the number of 'degrees of freedom'.

Poincaré generalized this definition to arbitrary topological spaces by insisting that:

(a) The empty set has dimension 1.

(b) If the boundaries of small neighbourhoods of all points in the space are (n1)-dimensional, then the space is n-dimensional.

This is called an *inductive* definition: it defines 0-dimensional spaces in terms of 1-dimensional spaces, then 1-dimensional in terms of 0-dimensional, and so on. Let us call the result the *topological dimension*. By its nature it is a topological invariant, and again must be a positive integer.

In the theory of fractals, something stronger than topology is required. A snowflake curve is topologically the same as a circle! It turns out that the *metric* structurenotions of distancemust also be taken into account. It is necessary to seek a definition of dimension that extends the usual one for manifolds, but reflects metric, not topological, properties.

Fractal Dimension

Take a piece of string. Take another the same length, and place them end to end: the result is, of course, twice the size. Starting with a

square of paper, however, you need *four* copies to make a square twice the size. With a cube of cheese, it takes *eight* copies to double the size. If four-dimensional cheese existed, you would need *sixteen* copies. In other words, to double a d-dimensional hypercube you need $c = 2^d$ copies. Therefore the dimension is given by solving for d:

$d = \log c/\log 2$.

For example, if we can find an object whose size doubles if three copies are stuck together, then it would have dimension

$d = \log 3/\log 2 = 1.4427$

More generally, to multiply the size of a d-dimensional hypercube by a factor a you need $c = a^d$ copies, and now

$d = \log c/\log a$.

It is easy to see that (one side of) the snowflake curve is made up of four copies of itself, each one-third the size. So $a = 3$, $c = 4$, and

$d = \log 4/\log 3 = 1.2618$

The dimension thus defined may appear a trifle unorthodox (like the blancmange functionjoke) but it has a serious purpose: it reflects the *scaling* properties of the curve. And that's what makes the *similarity dimension* that we have defined so basic. A set to which it can be applied is said to be *self-similar*. The idea can be modified to apply to other sets too; the final version is called HausdorffBesicovitch dimension, or, more simply, *fractal dimension*.

In 1961 Lewis Richardson took a look at the coastline-measuring problem for a number of countries. Take a ruler of fixed length e and move along the coast in steps of size e: add the results to get a total length $L(e)$. Because shorter rulers detect smaller wiggles, $L(e)$ must get bigger as e gets smaller. Richardson found experimentally that there is a number D such that $L(e)$ behaves like e^{1-D}. For Britain, D is about 1.25. The same value holds for Australia; for the land-frontiers of Germany and Portugal it is closer to 1.15; for South Africa roughly 1. By looking at the snowflake's length, measured the same way, we see that D is just the fractal dimension. So Britain's

coastline is a fractal curve of dimension about 1.25. For some reason South Africa is much smoother. Indeed you can think of D as a measure of the degree of roughness. The snowflake curve is pretty much as rough as the coast of Britain. It won't make a very good model of a coastline, though, because it has much too definite a pattern. (It's not really a good idea to describe a fractal as being 'irregular'; 'rough' is better.) There is an idea of statistical self-similarity that can be applied to random curves; and a random fractal of dimension 1.25 looks just like a piece of coastline. The self-similarity means that if you take a section of coastline and magnify it, then the result is equally plausible as a stretch of coastline. To put it another way, if you are given a map of the coastline with no scale marked, you won't be able to work out the scale by analysing the map.

Nature's Fractals

Structures resembling fractals are common in Nature. Of course, a natural object won't have structure on *all* scales, from the infinitely large to the infinitely smallor at least, if it does, we can never observe all of itso a fractal model is only an approximation. But when an object possesses detailed structure on a wide range of scales, then a fractal may be a better model than a smooth curve or surface.

William Harvey, discoverer of the circulation of the blood, noted that close to almost all points of the body there will be found both an artery and a vein. Modelling this by a logical extreme, we can insist that there should be both an artery and a vein infinitely near every point. Or, in different words, every point of the body not already inside a vein or artery should lie on the boundary between the two blood networks. In addition, for efficiency the total volume of arteries and veins should be small compared to that of the body (or else living creatures would be merely large bags full of blood). Mandelbrot puts it this way: 'From a Euclidean viewpoint, our criteria involve an exquisite anomaly. A shape must be topologically two-dimensional, because it forms the common boundary to two shapes that are topologically three-dimensional, but it is required to

have a volume that not only is non-negligible compared to the volumes of the shapes that it bounds, but is much larger!' A simple fractal construction resolves the paradox. All criteria can be met provided the blood networks branch infinitely often, with rapidly decreasing diameters for the blood vessels at each stage. Body tissue then becomes a fractal surface; it has topological dimension 3, but fractal dimension 2. We are not walking blood-bagswe are fractals incarnate! Fanciful though this idea may seem, it has a compelling intuitive quality for anyone familiar with fractals. The paradoxical properties of fractal geometry can liberate our intuition and save us from unwarranted assumptions.

For example, consider Saturn's rings. Until recently these were thought of as something akin to flat discs of matter. Telescopic observations suggested the presence of five or six annular zones, separated by clearly defined gaps (supposedly due to, and explained by, gravitational resonances with some of Saturn's moons). When the two Voyager spacecraft actually went to Saturn and took a look, they found something more complicated: a fine structure of thousands if not millions of rings. New gaps in the rings failed to fit the 'resonance' theories. Saturn's rings look much more like a fractal than a flat disc. Now the gravitational properties of fractally distributed matter have not been studied; but they are unlikely to be exactly the same as those of flat discs. It's an uncharted area of research; and it may be that the fractal model could explain the failure of the resonance calculations. Simplifying hypotheses such as smoothness are not always appropriate; and an awareness of the fractal alternative could help researchers from making such an assumption in the wrong context.

Most plants have a fractal structure. Just think of a tree with its ever-decreasing hierarchy of limbs: trunk, bough, branch, twig and the bog down in the valley-oh! Indeed Aristid Lindenmayer and colleagues have shown that combinatorial fractal schemes known as *L-systems* can generate realistic plant forms, and these can be linked to actual plant growth patterns. Imagine an insect living on a leaf with a fractal surface. The smaller the insect, the more tiny dents it will be able to get into. So the effective surface area of the leaf is

much larger for smaller insects, just as the coastline of Britain seems longer if you use a finer ruler. New 'ecological niches' arise for ever tinier creatures. Experiments conducted in 1985 by D. R. Morse. J. H. Lawton, M. M. Dodson, and M. H. Williamson have confirmed this relation between insect size, insect population, and fractal dimension of the host plant. The fractal nature of plants may have influenced the evolution of insects.

Brownian Motion

Another example of fractals in Nature is Brownian motion, mentioned in Chapter 13. Small particles suspended in a fluid undergo rapid irregular movements, caused by thermal agitation of molecules in the fluid. The molecules bump into the particles, and give them a shove. In 1906 Jean Perrin wrote:

> The direction of the straight line joining the positions occupied at two instants very close in time is found to vary absolutely irregularly as the time between the two instants is decreased. An unprejudiced observer would therefore conclude that he is dealing with a function without derivative, instead of a curve to which a tangent should be drawn. At certain scales and for certain methods of investigation, many phenomena may be represented by regular continuous functions. If, to go further, we attribute to matter the infinitely granular structure that is in the spirit of atomic theory, our power to apply the rigorous mathematical concept of continuity will greatly decrease.

Perrin went on to discuss the idea of density of a gas as a limiting value of the mass-to-volume ratio for a small region. He pointed out that this concept, the basis of the continuum mechanics of gases, makes no physical sense in an atomic theory, where the limiting value is 0 except at isolated points (the atoms) where it becomes infinite. In the atomic regime, fluctuations dominate. Perrin realized that the 'pathological' continuous but non-differentiable curves of Weierstrass and von Koch occur in Nature, in processes as fundamental as molecular motion. In the 1930s, Norbert Wiener formulated a mathematical model of Brownian motion possessing exactly this feature of non-differentiability of the paths of particles.

Mandelbrot Set

One particular fractal has attracted enormous public attention. It has become virtually synonymous with 'Chaos Theory'the popular conception of chaotic dynamics and fractals. A group of hoaxers even created a 'corn circle' in its image in the Cambridge countryside. (We know it must have been hoaxers for a variety of reasons: mostly, they got the shape *wrong*.) It was invented by Mandelbrot, and has become known as the Mandelbrot set.

The Mandelbrot set is a test-bed for the theoretical investigation of fractal structures that come up in an area known as *complex dynamics*. This is not just dynamics with bells and whistlesthough it often looks like it. It is the dynamics of mappings of the complex plane under iteration. The subject's 'boost phase' was the work of Pierre Fatou and Gaston Julia in the 1920s. The 'pre-launch phase' goes back at least as far as Gauss, who made some basic discoveries in his theory of the arithmetico-geometric mean. We are now in the flight phase.

Suppose that c is a complex constant, and consider the mapping $z \rightarrow z^2 + c$ where z is also complex. What happens to z? In detail, there are many possibilities; one of the simplest is that z disappears to infinity. The set of initial points z that do this is called the *basin of attraction* of the point at infinity; and its boundary is the *Julia set* corresponding to c. With modern computer graphics it is easy to draw Julia sets. They can take a remarkable variety of forms: circles, pinwheels, rabbits, sea-horses, starry skies. To bring some order, we ask only whether the Julia set is connected. Is it all in one piece, or does it fall apart into dust? Note that this is a question about the parameter c, not the variable z. The points c in the complex plane for which the Julia set is connected comprise the Mandelbrot set.

One method of drawing the Mandelbrot set, developed by John Hubbard and exploited to great effect by Heinz-Otto Peitgen and Peter Richter, involves plotting colour-coded contours that home in on its intricate boundary. It is these pictures that feature on the postcards, posters, and coffee-mugs of shops such as Kensington's *Strange Attractions*. If you take any region near the boundary of the Mandelbrot set and zoom in on it, you see an ever-changing scene

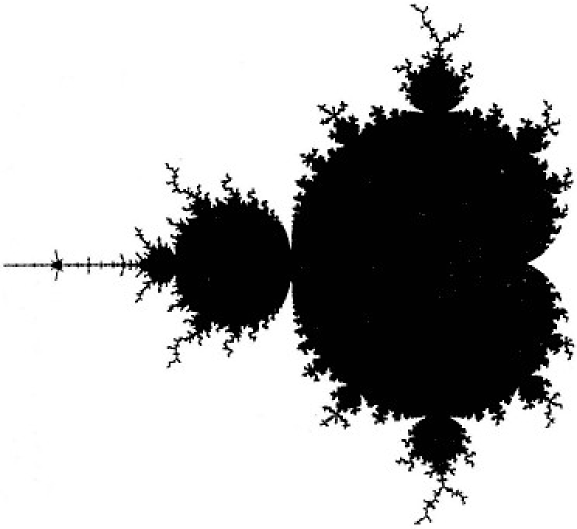


Fig. 31.
The notorious Mandelbrot set (H.-O. Peitgen and D. Saupe (eds.), *The Science of Fractal Images*).

of complex geometric structures. If you look in just the right place, you see tiny Mandelbrot sets, complete in every detailthough each has its own characteristic external decoration. If you zoom in towards the point c, then the picture begins to resemble, ever more closely, the Julia set for c. In short, the Mandelbrot set is all possible Julia sets, glued together.

Because of this complexity and depth of detail, the Mandelbrot set has been described as 'the most complicated object in mathematics'. It is perhaps worth explaining in just what sense this clichéd statement is true. It is extremely complicated to *describe* the Mandelbrot set in detail, point by point. However, the *process* that generates it

($z \to z^2 + c$) is remarkably simple. So the Mandelbrot set isn't *really* complicated: it just looks that way. This is precisely the distinction that I made in the previous chapter, between description and prescription, process and result. There are plenty of things in mathematics that are much, much more complicated than the Mandelbrot set.

It is, however, complicated enough to pose some really tricky problems for mathematicians. One basic one is to prove that it really is a fractal. Yes, it certainly looks like one; but can you prove it? In 1991 Mitsuhiro Shishikura did so. Indeed, he proved that the boundary of the Mandelbrot set has fractal dimension 2, the largest possible value. This is the same value that would be attained by a space-filling curvea curve that fills some region of space of non-zero areaand raises the question whether the boundary of the Mandelbrot set might actually be such a curve. However, different curves can have equal fractal dimensions, so this particular value of the dimension need not imply that the curve fills space. The result does mean, however, that the boundary of the Mandelbrot set is just about as wiggly as it is possible to be while living in a plane.

Recall that locally, near a point c, the Mandelbrot set looks like a Julia set for that value of c. Shishikura's proof relies on finding a sequence of Julia sets whose fractal dimensions approach 2 as the parameter c that defines them approaches the boundary of the Mandelbrot set. He doesn't define the sequence explicitly: he proves that it exists. His techniques are of considerable interest, and open up new possibilities in complex dynamics.

Digital Sundial

Fractals have revitalized a classical area of mathematics known as geometric measure theory. This is where A. S. Besicovitch originally defined his notion of dimension, now renamed 'fractal dimension'. One problem in that area, solved by a fractal construction, asks what relation there is between the different projectionsshadowsof a set in three-dimensional space. The answer, proved by Ken Falconer in 1986, is 'none whatsoever'. Roy Davies proved the same result for a restricted case, subsets of the plane, in 1952.

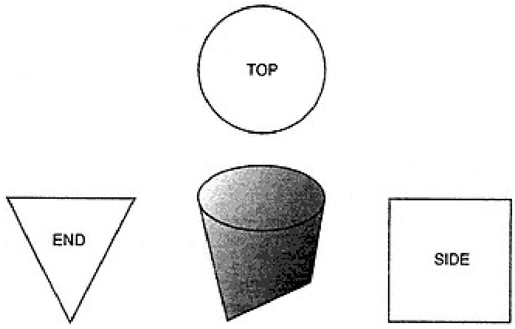


Fig. 32.
Traditional puzzle: a cork with three shadows.

Imagine some set, possibly very complicated, hovering in space. On one side of it place a lamp (infinitely distant so that its rays are parallel) and on the other place a flat screen. You get a shadow. Move the lamp and screen round: you get another shadow. How does the shadow change? That the answer may be surprising is shown by an ancient puzzle: make a cork that will fit a round hole, a square one, or a triangular one. The shadow of the cork, from three distinct directions, has to be round, square, or triangular. It's not hard to concoct such a shape.

Falconer proves that, given any prescribed list of shadows corresponding to directions in space, there exists some set that produces 'almost' those shadows when illuminated from those directions. 'Almost' here means that the actual shadows may differ from those prescribed by a set of zero area. His construction is a kind of iterated venetian blind. A venetian blind lets light through in one direction, but not in others. What we want is the opposite: something that casts a solid shadow in one direction, and nothing in others. To this end, we modify the venetian blind. If its slats are replaced by tiny venetian blinds pointing in some other direction, it now lets light through in

two directions. By repeating this process you can get a set that lets light through in nearly all directions, with one exception. Cut such a blind to the shape of the shadow that you want to be produced in that direction. Now repeat with a new blind, a new direction, and a new shape. Combine all the resulting sets into one: it has arbitrarily prescribed shadows. The 'zero area' differences arise when you try to fill in the technical details in this approach. Falconer describes the ideas as the 'digital sundial'. You can make a shape which, when hung in sunlight, produces a moving shadow; and at each moment, the shape of the shadow tells you the time *in digits*. For example, at 11.17 the shadow will form the same shape as the digits 11.17.

Through Fractal Eyes

The contribution of fractals to our understanding of the natural world is not so much one of 'technology' as of what used to be called natural philosophy. Instead of being a computational tool, it is a clue towards an integrated world-view, an organizing principle. The recognition of fractals as basic geometric forms of a new kind, with their own mathematical structure, opens our eyes to a new range of phenomena and sensitizes us to new points of view.

At the moment, fractal models are descriptive rather than explanatory. We can *see* that fractal mountains of the right dimension look just like real mountains; but we lack any real idea of the reasons, in terms of erosion processes or fracture properties of materials, that are responsible for their fractal nature. We cannot yet compute even as basic an item as the fractal dimension of a model mountain from physical principles. Of course, a good descriptive framework is important. If cellular tissue is best modelled by a fractal, then there's not much point in a theory that treats the cell as a rectangular slab of homogeneous goo, or a living creature as a uniform sphere. What are the effects of fractal structure on a mathematical problem? When are they important and when do they make little difference? What are the causes of fractal forms? There are many such questions, but as yet few answers. They hint at a deeper structure of which fractal geometry is the outward sign.

Fractals are important, not because they have solved vast open problems in mathematics, or helped us to build a better mousetrap, or unlocked the secrets of rotating black holes. They are important because they suggest that, out there in the jungle of the unknown, is a whole new area of mathematics, directly relevant to the study of Nature. The great strength of mathematics is its ability to build up complex structures from a few simple key ideas. Once the skeleton of such a structure appears, each new piece can be attached in its rightful place. Without awareness of the skeleton, the pieces lie scattered and unappreciated. We now have the skeleton of a theory of fractals. The challenge to the mathematicians of the next century will be to mould the flesh on to those already fascinating bones.

17
Dixit Algorizmi

You say you got a real solution,
Well, you know,
We'd all love to see the plan.
John Lennon and Paul McCartney

Muhammad ibn Musa abu Abdallah al-Khorezmi al-Madjusi al-Qutrubulli was born about AD 810 in Khorezm (now Urgench in Uzbekistan). He served as astronomer to the caliph Abd-Allah al-Mamun (The Trustworthy), who inherited the Bayt al-Hikmah (House of Wisdom, or Academy of Sciences) at Baghdad from his father Harun ar-Rashid (The Righteous), and brought it to its peak. Al-Khorezmi's known works are ten in number, and include *Kitab azzij al-sindhind*, a set of astronomical tables including the first sine and cotangent tables; *Kitab hisab al-'adad al-hindi*, an arithmetic text; and *Kitab al-muhtasar fi hisab al-gabr w'al-muqabalah*, a compact text on the solution of equations. The third book, Latinized as *Ludus algebrae et almucgrabalaeque*, gave us the word 'algebra'. Face it, it could have been worse. The second, Latinized as *Algorithmi de numero Indorum*, brought the Hindu number system to the Arab world, and the resulting HinduArabic number system to Europe. That is, our system of 'hundreds, tens, and units' with a symbol for zero and nine other digits whose value depends on their place. In medieval times, arithmetic was identified with his name, rendered as 'Algorismus'. The formula *dixit Algorizmi* (thus spake al-Khorezmi) was a hallmark of clarity and authority. His text on arithmetic included all the basic arithmetical processes by which numbers could be added, subtracted, doubled, halved, multiplied, divided, and their

square roots extracted. It also had a chapter on business calculations. Today his name survives as the description of any clear and precise procedure for solving a given problem: an *algorithm*. It is a concept at the heart of practical and theoretical computer science, and the mathematics of computability. And you may have noticed that computers, too, have their business uses.

Existence and Constructibility

There are many different styles of mathematics. At one, rather nebulous extremity is the pure existence proof, asserting that an object with certain properties must necessarily *exist*, but giving no method to find it. In the middle are more or less constructive techniques which provide a more explicit description of the desired results or objects. At the hard core is the fully constructive algorithm, a perfectly definite procedure guaranteed to calculate exactly what is required, if only you wait long enough.

The problem of transcendental numbers illustrates all three styles well. Cantor's existence proof, mentioned in Chapter 5, exhibits not a single transcendental. It merely observes that since there are more reals than algebraic numbers, transcendentals must exist. In the same way, if 349 passengers are booked on to a 348-seater Boeing, they all know that one of them is going to get bumped; but the argument does not prescribe whom. At the intermediate level are transcendence proofs for specific numbers, such as e and π. These exploit special properties of the individual numbers involvedjust as we can predict that the father travelling with five young children, or the movie star, isn't the one who will be transferred to another flight. An algorithm for transcendence would be a general method of deciding, for any number whatsoever, whether it does or does not satisfy an algebraic equation. No such technique is known; probably no such technique exists. No general 'bumping rule' appears to exist on airliners, either.

There have been extensive philosophical arguments about the value and nature of these types of result. Does a pure existence proof really convey any useful information? One school of modern mathematics,

the Constructivists, founded by Errett Bishop in 1967, takes a very restrictive position and refuses to consider any non-constructive arguments at all. My own thinking on this question changed when I was working on applications of computers to algebra. For a particular object (a so-called Cartan subalgebra) there is a classical existence proof, but no algorithm. By approaching the problem from another angle I was able to find a definite algorithm. But my *proof* that the algorithm worked depended on the classical existence theorem! So here an existence theorem was necessary to justify the algorithm. This state of affairs is actually very common. *To calculate something, it is often useful to know in advance that it exists*. Ever since, I've felt that the Constructivists have missed one of the subtleties of our subject.

In fact the waters become murkier still. How should we react to proofs of the *existence of algorithms* that don't actually specify what the algorithm is? I can imagine an algorithm to find existence proofs; or proofs of the existence of algorithms that find existence proofs for algorithms that well, you get the idea.

Algorithms Before Algorismus

Algorithmic thinking pre-dates al-Khorezmi by millennia. One of the first significant algorithms is to be found in Euclid's Book Seven, and it illustrates several of their most important features. The problem is to calculate the highest common factor (hcf) of two numbers; that is, the largest number dividing both exactly. Conceptually the simplest solution is to resolve both into primes and retain the smallest power of each prime; and this is the method commonly taught in schools today. For example to find the hcf of 60 and 280 we write $60 = 2^2 \cdot 3 \cdot 5$ and $280 = 2^3 \cdot 5 \cdot 7$ and retain $2^2 \cdot 5$, namely 20, as hcf. But this method is hopelessly inefficient for numbers even of four or five digits, because of the difficulty of factorizing into primes. (Further, the proof that it works *depends* on the Euclidean algorithm!) Euclid's improved method runs as follows. Let the two numbers be m and n, with m the smaller. Then:

(1) Divide n by m with remainder r.

(2) Replace n by m, and m by r.

(3) Repeat from step (1) until the remainder becomes zero.

Then the final divisor m is the required hcf. For instance, to find the hcf of 50983 and 34017 we proceed thus:

50983/34017	leaves remainder 16966
34017/16966	leaves remainder 85
16966/85	leaves remainder 51
85/51	leaves remainder 34
51/34	leaves remainder 17
34/17	goes exactly.

So the hcf is 17. The proof that the algorithm always works is quite easy, and is based on the fact that any number dividing m and n must also divide r, and conversely.

The Good-Natured Rabbit-Breeder

The numbers that occur in the Euclidean algorithm decrease rapidly. How rapidly? How many steps does the algorithm take? To answer this we seek the worst case, where the decrease is smallest at each step. Then m must divide n exactly, once at each step. Working backwards we find that m and n should be consecutive terms in the sequence

1, 2, 3, 5, 8, 13, 21, 34, 55, 89,

where each number is the sum of the previous two. This sequence has a long history: it was invented by Leonardo of Pisa, nicknamed Fibonacci ('son of good nature') in a problem about rabbit-breeding. The nickname, incidentally, was invented by Guillaume Libri in the nineteenth century. Leonardo's *Liber Abaci* of 1202 introduced HinduArabic arithmetic to Europe, another link with al-Khorezmi. How big is the nth Fibonacci number? There's an exact formula, but a cruder estimate yields the necessary insight. The number of steps in the Euclidean algorithm is roughly five times the number of digits

in the larger of the two numbers involved. Even for 100-digit numbers, no more than 500 steps will be required. We have found a measure of the efficiency of the algorithm, a topic hinted at in Chapter 2. Here the 'speed' is proportional to the number of digits. This is very efficient indeed. In comparison, the length of the 'prime factors' method, using trial division, grows exponentially with the number of digits. For a 100-digit number about 10^{20} steps would be needed.

The Malthusian Connection

Suppose we decide on a formal model of computation in which we can define precisely the size of input data and the running time. (One such method is described in the next chapter, but it would be distracting to introduce it here. Informally, think of the input size as the total number of digits involved, and the running time as the number of computational steps.) Then we can measure the efficiency of an algorithm by the way the running time varies with the size of input data. In 1965 J. Edmonds and A. Cobham proposed that the two crucial cases, roughly corresponding to what experience would describe as 'good' and 'bad' algorithms, are polynomial and exponential time. If the running time for input size s grows like a fixed power, say s^3 or s^8, then the algorithm runs in *polynomial time*. If it grows like 2^s or faster then it runs in *exponential time*. Rates of growth sandwiched between these can occur, but are rare in practice. One advantage of this classification is that is does not depend on the chosen model of the computational process. Notice that the 'good' Euclidean algorithm runs in *linear* time (the first power of s), whereas the natural but 'bad' factorization method is exponential.

In 1798 Thomas Malthus wrote a celebrated essay on population pressure, in which he distinguished between the linear growth of food supplies and the exponential growth of the population. It's basically the same distinction, and the crucial point is that in the long run exponential growth will win, however slowly it starts. These crude but effective measures are for theoretical purposes, and must be tempered in practice with other considerations. An algorithm with

running-time $10^{500}s^{120}$ is by our definition 'good', but in practice it's useless. One with running-time $\exp(10^{-500}s)$ is 'bad', but might work reasonably for a wide range of values s. But I know of no sensible examples of either of these weird concoctions.

P = NP?

Next, let's look at a few typical problems that it would be interesting to find algorithms for:

(1) [Sorting Problem] Given a set of integers, arrange them in ascending order.

(2) [Routing Problem] Work out the best way to route garbage trucks through a city, minimizing mileage subject to constraints such as collecting all the garbage in a working week using no more trucks than the city authorities possess.

(3) [Assignment Problem] Given data on courses, teachers, and students, design a timetable with no clashes.

(4) [Bin-Packing Problem] Given a set of objects of various sizes, fit them into the smallest number of fixed-size bins.

(5) [Travelling Salesman Problem] Given a list of cities, each to be visited exactly once, find the shortest route.

These are all *combinatorial optimization problems*. For each situation X there is a value $c(X)$ that must be minimized. In (1) it is the total number of integers out of order; in (2) the total mileage; and so on. In this context the polynomial/exponential distinction takes on a new and important twist.

Let P denote the class of problems that can be solved by an algorithm running in polynomial time: the *easy* problems. A (presumably) more general class, containing most of the interesting problems is called NP, the problems soluble in *non-deterministic polynomial time*. This is more complicated to describe. Suppose for each situation X we are trying to minimize $c(X)$, as above. Suppose for a given number B it is possible to check in polynomial time whether a *solution* X to the problem has $c(X) < B$. Then the problem

is NP. Note that the presumably hard part*finding* the solutionis not involved.

For example, consider a jigsaw puzzle. Solving a puzzle with a large number of pieces is hard. But verifying that an attempted solution *is* a solution is very easy. The human eye can do it at a glance; a computer could do it just by checking each piece against its neighbours. Jigsaw puzzles are NP.

Clearly every problem in P is also in NP. Is the reverse true? In other words, if you can *check* a solution in polynomial time, can you *find* it in polynomial time? It sounds unlikely! As we've just seen, you can check the solution of a jigsaw puzzle very rapidlybut nobody expects that to imply that you can also *solve* it rapidly! So we expect to have P ≠ NP. It looks the sort of thing that it should be easy to decide. It's not. It's an unsolved problem, a major challenge that may well have to be bequeathed to the mathematicians of the twenty-first century.

NP-Completeness

What makes the P ≠ NP problem so hard is that it is very difficult to prove that a problem *cannot* be solved in polynomial time. You must envisage *all possible algorithms* for it, and show that each is inefficient. Non-existence proofs are often hard, witness squaring the circle, solving the quintic, proving the parallel axiom, or establishing the continuum hypothesis. Another curious feature is that all of the problems that one might hope are in NP but not in P are very much on a par with each other, making it difficult to know where to start. Specifically, say that a problem is *NP-complete* if it belongs to NP, and if it can be solved in polynomial time, then *every* problem in NP can be solved in polynomial time. In other words, if it's not a counterexample, nothing is. Virtually *every* problem anyone can think of in NP that isn't obviously already in P turns out to be NP-complete. Thousands of problems have been studied. And it is also known that the most plausible methods for showing either that P = NP (simulation) or that P ≠ NP (Cantor's diagonal argument) cannot be made to work.

Is it true? Is it false? Is it neither, like the continuum hypothesis? Is its truth independent of formal set theory? Nobody has the foggiest idea. It's one of the biggest unsolved mysteries in mathematics.

Bins and Boots

Suppose you are given a number of objects, of various sizes, and have to pack them into identical bins, each capable of holding only a certain total size. How many bins do you need? There are a few obvious remarks.

Each object must be smaller than the bins, otherwise there is no solution. You can't get a quart into a pint pot. Similarly the total size of objects, divided by the bin capacity, places a lower bound on the number of bins needed. Aside from these simple observations (which don't help very much) it's hard to think of anything except trying all possible packings and selecting the best. This is, of course, slow and inefficient, and indeed the problem is NP-complete. In the absence of optimal solutions, one can ask for less: is there a practical way to find a 'reasonably good' packing? In 1973 Jeffrey Ullman showed that the 'first fit' algorithm (number the bins, and fill as much of each bin as you can before using one with a higher number on it) gives no worse than 17/10 of the optimal number of bins. Moreover, 17/10 is as good as you can get, in general. If the objects are dealt with in order of decreasing size, the 'first fit decreasing' algorithm, the ratio can be improved to 11/9, so you'll never be more than 20 per cent wrong. Anyone packing the boot (USA: trunk) of the family car for a holiday knows that it's best to fit the largest suitcases in first.

A Simpler Simplex and a Torn Knapsack

The theory of algorithms is a very new branch of mathematics, and it is changing at high speed as more emphasis is placed on the practical cost of computational methods. One important subject that makes everyday use of computations is *linear programming*, which solves problems like this: how to allocate resources between the

production of different items, subject to various price constraints, in order to maximize profits. Obviously such a method has great commercial value. The traditional algorithm was invented by George Dantzig in 1947 and is called the simplex method. It usually works pretty quickly in practice, but for some pathological examples it runs very slowly. The topologist Stephen Smale proved a theorem a few years ago that made this statement precise. In 1979 a lot of publicity was given to a new algorithm devised by L. G. Khachianpossibly because the Press confused the linear programming problem with the much harder travelling salesman problem. Khachian solved the problem of linear programming in polynomial time (the fifth power of the input size, to be precise) by exploiting geometric properties of ellipsoids. As if to emphasize that the distinction between exponential and polynomial time is a little academic, it turned out that in practice Khachian's method is slower than the simplex methodalmost all the time! However, in 1985 Narendra Karmarkar managed to combine the best features of both, into a truly efficient polynomial-time algorithm. This is of serious practical importance on the dollars-and-cents level.

Another recent breakthrough involves the knapsack problem. Suppose you have a knapsack, of fixed size, and a collection of objects whose sizes are given. Does any subset of the objects fill the knapsack exactly? In 1982 Ralph Merkle set up a public-key cryptosystem, of the type discussed in Chapter 2, based around the knapsack problem; and he bet $1,000 that the resulting code was uncrackable. Adi Shamir promptly broke one version of the code, but that wasn't enough to win the bet, althoughto Merkle's irritationthe Press seemed to imagine it was. In 1985 Ernest Brickell broke the whole thing, by developing a fast algorithm to solve the knapsack problem.

18
The Limits of Computability

Achilles: Hmm there's something just a little peculiar here. Oh, I see what it is! The sentence is talking about itself! Do you see that?
Tortoise: What do you mean? Sentences can't talk.
Achilles: No, but they REFER *to thingsand this one refers directlyunambiguouslyunmistakablyto the very sentence which it is!*
Douglas Hofstadter

For centuries mathematicians beat their brains out trying to prove Euclid's parallel axiom. The discovery of non-Euclidean geometries put the problem in a different light. There are geometries different from Euclid's that are equally consistent: if Euclid's geometry is consistent, so are the others. Which raised a much deeper question: *is* Euclid's geometry consistent? Having started from the belief that Euclidean geometry is the true geometry of Nature and that no other geometries can make sense, and being proved wrong on both counts, mathematicians now began to doubt even Euclidean geometry. Is *that* consistent? It was reasonably easy to see that this is so, provided that the real number system is consistent, and to work on the foundations of mathematics after Cantor managed to reduce the consistency problem for the reals first to that for the integers, and then to mathematical logic and formal set theory. Within set theory, and using logic, you can model the integers; with integers you can model the reals; with the reals you can model Euclidean geometry; with Euclidean geometry you can model non-Euclidean geometry. All very wellbut what about set theory and logic themselves? It was

all a dreadful anticlimax: mathematicians set out to conquer the Universe and ended up doubting the very ground they stood on.

At the turn of the century David Hilbert devised a research programme whose end result was to be a rigorous proof of the consistency of logic and set theory, or equivalently of arithmetic. But before it really got going, an unknown called Kurt Gödel threw a spanner in the works and the programme collapsed in ruins. Gödel showed that there are true statements in arithmetic that can never be proved, and that if anyone finds a proof that arithmetic is consistent, then it isn't! At much the same time, Alan Turing was working in mathematical logic, with a view to clarifying the notion of computability. He too discovered that certain very natural questions *have no answer whatsoever*. By this I do not mean that nobody is clever enough to find the answer: I mean that it can be shown in all rigour that no answer exists at all!

Turing Machines

To make our ideas preciseand in this area that's mandatorywe need a precise concept of 'computability'. We might, for example, take an IBM Personal Computer (with an ideal unlimited memory, that is, as many floppy discs as you like), running in assembly language. That would be a bit messy for theoretical purposes, because we'd have to understand the entire operating system of the computer, how its circuits work, and so on. The usual choice is an invention of Turing, who together with John von Neumann provided the theoretical impetus for the creation of the modern computer: it is called a *Turing machine*. It's an idealized computer with the most rudimentary structure possible. Imagine an infinite *tape* divided into square cells, passing under a *head* which can be in a finite number of internal *states*. The head can read what's on the tape, or write symbols 0 and 1 to it. Only 0 and 1 are required, since any information whatsoever can be encoded using just 0s and 1s. For example, use Morse code with 0 representing a dot and 1 a dash. (A standard binary code would be more likely in practice.) The machine's behaviour is controlled by a kind of computer program, called a

Turing-Post program. This consists of numbered steps, each step being one of seven commands:

Print 1 in the current cell.

Print 0 in the current cell.

Move one cell right.

Move one cell left.

Go to program step *k* if the current cell holds 1.

Go to program step *k* if the current cell holds 0.

Stop

As well as the program, there should be some input data on the tape, for the program to operate on. The machine processes the data according to the program, and then stops.

For example, suppose the initial data is a block of 1s, terminated by 0s, such as

0111111110

where the arrow points to the current cell. Assume that we want the machine to change the terminating 0s to 1, and then stop, thus lengthening the block by two. The following program achieves this:

1. Move right

2. If scanning 1, go to step 1

3. Print 1

4. Move left

5. If scanning 1, go to step 4

6. Print 1

7. Stop

The head moves right until it finds the first 0, and replaces it by a 1; then it moves left until it finds a 0, replaces that with a 1, and stops. You might imagine that such a simple device has only a limited repertoire of tricks. Not so, as Turing showed. Anything that a computer can calculate, so can a Turing machine. Slowly, of course, but that won't bother a theorist seeking insight into the nature of computability.

A Turing-Post program can itself be encoded on the tape, using for example the following codes:

000	Print 0
001	Print 1
010	Move right
011	Move left
1010001	Go to step *k* if 0 scanned
1101110	Go to step *k* if 1 scanned
100	Stop

Here theindicates *k* repetitions. The program above then encodes as

0101101000101111011110001100,

and you can reconstruct any program uniquely from its code. Turing showed in 1936 how to construct a *universal* Turing machine, call it *U*, capable of simulating the action of *any* program in *any* Turing machine. It has a fixed program. Its input data are a sequence of 0s and 1s of the form code (*M*) · *d* for a machine with program *M* and input data *d*. The program for *U* scans the code, decodes the actions for *M*, and carries them out on the data *d*. Here's another way to say it. Any computer, given enough time and memory, can be programmed to simulate any other computerperhaps very slowly. The program will include a complete coded description of the machine being simulated and the way it works. So, for theoretical purposes, we may as well think of all computations as being performed by Turing machines, and *define* an algorithm to be a Turing-Post program.

The Halting Problem

An important property of an algorithm is that it should eventually *stop* with a definite answer. A calculation that may go on forever isn't much use, because until it's finished you don't know what the answer is. This raises a natural question. Suppose we are given a Turing-Post program. For some finite strings of input data, it will eventually stop. For others it may not. (For example, 'move right

looking for 1s' won't stop if there are no 1s to the right of the head.) The *Halting Problem* is to decide which inputs do or do not cause the machine eventually to stop. In the spirit of the enterprise, we would like to solve this by an algorithm. In other words, we want a Turing-Post program which, given a fixed program *P* (encoded in 0s and 1s) and variable data *d*, calculates 0 if *P* eventually stops for input *d*, and 1 if not.

Turing was musing along these lines in 1936, and he had a brilliant idea. Suppose such a Halting Algorithm exists, and apply it to the universal Turing machine *U* with data *d*. In fact, consider a slightly more complicated program:

(a) Check whether *d* is the code for a Turing-Post program *D*. If not, go back to the start and repeat.

(b) If *d* is the code for *D*, double the string to get *d* · *d*.

(c) Use the Halting Algorithm for *U* with input data *d* · *d*. If it stops, go back to the start of (c) and repeat.

(d) Otherwise, stop.

Call this program *H*. You'll have noticed how *H* gets 'hung up' at stage (a) if *d* isn't a code of a program, or at (c) if input *d* · *d* causes *U* to halt. Now *H* is a program, so it has a code *h*. Does *H* halt for input *h*? It certainly doesn't get hung up at (a), because *h* by definition is a code (of *H*). It gets past (c) if and only if *U* does *not* halt with input *h* · *h*. So, to sum up:

H halts with input data *h*
if and only if
U does *not* halt with input data *h* · *h*.

But now consider how the universal machine U simulates a program P. It starts with input data code(P) · d, and behaves just like P with input data d. In other words we have a second 'poem':

P halts with input data d
if and only if
U halts with input data code(P) · d.

Finally put $P = H$, so code(P) = code(H) = h; and put $d = h$. Then the second poem becomes:

H halts with input data h
if and only if
U halts with input data $h \cdot h$.

But this exactly contradicts the first poem!

This has proved the simplest and most basic undecidability theorem. A problem is *undecidable* if there exists no algorithm to solve it. The undecidability of the Halting Problem places definite limits on the applicability of algorithms. The proof is really an application of Cantor's diagonal argument, like many in undecidability theory. It has a very similar logical structure to a card, on one side of which is written:

> THE STATEMENT ON THE OTHER SIDE OF THIS CARD IS TRUE

and on the other:

> THE STATEMENT ON THE OTHER SIDE OF THIS CARD IS FALSE.

The Used Axiom Salesman

Turing's theorem, although the most basic undecidability theorem, wasn't the first of its kind. In 1930 Gödel sent for publication a paper which left Hilbert's programme to prove the consistency of arithmetic an unsalvageable wreck. It also scuppered another of Hilbert's most cherished beliefs, that *every* problem can in principle be solved. 'We *must* know, we *shall* know', proclaimed Hilbert. But Gödel proved that there are more things than are dreamt of in Hilbert's philosophy. The two devastating theorems were:

(1) If formal set theory is consistent then there exist theorems that can neither be proved nor disproved.

(2) There is no procedure that will prove set theory consistent.

Legend has it that Hilbert blew his top when he heard of Gödel's results. At first it was thought that the cause lay with the particular

formalization of set theory used by Gödel. But as people came to understand how the proof worked, it became clear that the same ideas will do the trick for *any* formalization of arithmetic. It is not any particular axiomatization that is to blame: it is arithmetic itself. With Gödel's theorems the hope of proving beyond any doubt the unsullied purity of mathematics vanished forever. Mathematics was forced to face an ugly fact that all other sciences had come to terms with long before: it is impossible to be absolutely certain that what you are doing is correct. With hindsight this was no bad thing, since the goal of total perfection was obviously a chimera: how can any system demonstrate its own infallibility? Would you believe someone who said 'I am infallible', and justified it by saying 'because I say so'? (If so, I've got a super used car you'd just *love*, very low mileage; owned by an old lady who only drove it to mathematics seminars on Sundays)

Hilbert's Tenth Problem

In his address of 1900 to the International Congress of Mathematicians, Hilbert posed twenty-three problems which he felt were of the highest importance. The continuum hypothesis and the consistency of arithmetic were among them. Also included was the search for an algorithm to decide the solubility of arbitrary Diophantine equations. That stayed open until 1970, when Yuri Matijasevich (following earlier work of Martin Davis, Hilary Putnam, and Julia Robinson) showed that no such algorithm can exist. James Jones found an explicit example, a system of eighteen equations of maximum degree 5^{60} in thirty-three variables. Maybe when the counterexamples look like that we're asking the wrong question. For example, what if we insist that the degree be small? Carl Ludwig Siegel found an algorithm in 1972 for equations of degree 2. It is known that no algorithm exists for degree 4, so that leaves cubics as an open problem. Matijasevich's work produced some unexpected spin-offs, notably a polynomial formula whose position values are precisely the primes. It is questionable whether it will have much influence on the theory of primes, though, for it is even more complicated than

the system above. Essentially what he did was to find a way to simulate a Turing machine using a Diophantine equation. Consequently anything you can do with Turing machinescompute primes, fail to solve the Halting Problemcan be done with Diophantine equations.

Random Sequences

Undecidability theories were pushed to the limit by Gregory Chaitin in 1965 as a by-product of a successful attempt to define what is meant by a *random* sequence of 0s and 1s. A random sequence is of course one that has no pattern. Let's take this oft-made remark seriously, and analyse it. First consider a case with a lot of pattern: a million 1s in a row. You could sit down and write it out in full; but you don't need to, because I've given you a short recipe: 'a million 1s in a row'. With a little more work you could use my definition to set up a Turing machine to output this sequence, with a program which could be encoded as a sequence with far fewer than a million digits. So we could say that a sequence has a *pattern* if it can be computed by a *shorter* sequence, interpreting the latter as the code for a Turing machine. 'A sequence with a pattern has a compact description.' A random sequence is then one that *cannot* be given a compact description. The shortest way to define it is to write it out in full.

Ray Solomonoff anticipated Chaitin's ideas in a less formal way in 1960. Instead of a Turing-Post program, he specified the sequence by a phrase in the English language, and counted the characters (letters, spaces, etc.). Then the sequence

11111111111111111111

can be defined as 'twenty 1s', using 9 characters; whereas

11111111110000000000

is 'ten 1s plus ten 0s', requiring 18. Unless more compact descriptions exist, the second sequence is about twice as complicated as the first. Chaitin derived a more formal version of this idea from

Information Theory, which was invented by Claude Shannon in the 1940s to study message transmission. It leads to the idea that a given string, or sequence, of 0s and 1s has *complexity* or *information content c* if there is a Turing-Post program which computes the string from suitable data, the total length of program code and data being no more than c. It's now easy to see that there are very few strings having much smaller complexity than their length. Suppose for example the complexity of a string is n 5, where n is the length. Then it can be computed by a machine whose code, plus data, had length n 5 or less. There are at most

$1 + 2 + + 2^{n-5} = 2^{n-4}\ 1$

such machines, hence at most 2^{n-4} (let's keep it easy) strings of length n and complexity less than or equal to n 5. Since there are 2^n strings of length n, the proportion having complexity $\leq n$ 5 is at most $2^{n-4}/2^n = 1/16$. Only one string in 16 has complexity only 5 less than its length. Similarly only one string in 500 or so will have complexity $\leq n$ 10. Most strings are random or, at least, pretty close to random.

Chaitin used these ideas to obtain a strong generalization of Gödel's theorems. One consequence can be stated roughly thus. There is a definite number K such that it is impossible to prove that any particular string of 0s and 1s has complexity K. Another consequence is utterly bizarre. While almost all long strings of 0s and 1s are random, by Chaitin's definition, it is impossible to write down an arbitrarily long string and *prove* it to be random. To 'write down' an arbitrarily long string you have to state a general rule for its entries, but then this rule is shorter than suitably large sections of the string, so the string can't be random after all! One way to think of all this is to apply the Solomonoff approach, and consider the phrase 'the smallest string not definable in fewer than 62 characters'which by its definition is definable in a phrase of 61 characters! This 'Richard Paradox' is at the heart of Chaitin's results.

Another of Chaitin's discoveries is the occurrence of randomness in arithmetic, dramatically extending the results on Hilbert's tenth

problem. He has constructed a Diophantine equation requiring 17,000 variables, one of which is a parameter n. Define a number r whose binary expansion has a 0 in the nth place if Chaitin's equation has finitely many integer solutions, but a 1 if it has infinitely many. Then r is random. That is, the question 'does Chaitin's equation have finitely many solutions?' depends on the parameter n in a completely random fashion. No finite proof, for example, can answer that question for infinitely many distinct values of n.

Deciding the Undecidable

Another mathematician who has thought a lot about issues of computability is Roger Penrose, whose book *The Emperor's New Mind* attempts to relate them to quantum physics and the nature of the brain. The central thesis of the book is that nature produces harnessable non-computable processes, but at the quantum level only; and Penrose goes on to speculate that human intelligence may be such a process.

Recent work of Newton da Costa and F. A. Doria casts doubt upon Penrose's thesis, and explores the limits of computability in chaotic classical dynamical systems. They show that undecidability extends to many basic questions in dynamical systems theory. These include whether the dynamics *is* chaotic, whether a trajectory starting from a given initial point eventually passes through some specific region of phase space, and whether the equations are integrablethat is, possess an explicit solution defined by formulas built up from classical functions such as polynomials, sines, and exponentials. Indeed virtually any 'interesting' question about dynamical systems isin generalundecidable.

This does not imply that it cannot be answered for specific systems of interest: on the contrary, an important recent breakthrough has been the proof by Michael Benedicks and Lennart Carleson that the Hénon system possesses chaotic attractors. And I have already mentioned a similar result of Mischaikow and Mrozek for the Lorenz equations. However, it does demonstrate the absence of any general

formal technique. Another recent 'uncomputability' result in dynamics is Christopher Moore's example of a deterministic system with uncomputable trajectories.

Steven Smale has initiated a systematic theory of computation using real numbers rather than integers. The idea is to model the procedures followed by numerical analystsfor example, solving an equation by finding a sequence of successively better approximations. Smale's set-up corresponds to a computer that can handle real number arithmetic to infinite precision. No existing computer can actually do this, but theoretical analyses of numerical methods generally assume such a facility. Together with Leonora Blum and Mike Shub, Smale has proved that Julia setsand, assuming one plausible conjecture in complex analysis, the celebrated Mandelbrot set itselfare *uncomputable*. That this may be so, despite the walls of the planet being papered with computer pictures of Julia and Mandelbrot sets, hinges upon the infinite precision required in the theoretical computations.

A totally different approach also indicates the possibility of curious 'computational' behaviour in classical dynamical systems. You can represent the behaviour of an electronic computer as a dynamical system: just write down idealized but realistic equations for its electronic components. Alternatively, write down equations for a 'real' mechanical Turing machine, with paper tape. Because classical mechanics poses no upper bound upon velocities, it is possible to 'accelerate' time in these equations, so that infinite 'real' time passes within a finite period of 'fake' time. Thus you can concoct a system of classical equations that corresponds to an infinitely fast computera Turing machine with a tape that accelerates so rapidly that it can complete an infinite number of operations in one second. Such a machine can in principle compute functions which, in the usual sense of mathematical logic, are uncomputable. For example, it can solve the Halting Problem by running a computation for infinite 'real' time and throwing a particular switch if and only if the program stops. In 'fake' time this entire procedure is over within a fixed period: one then merely inspects the switch to see whether it has been thrown. Additions to conventional mathematical logic that decide

undecidable questions are quite respectable things: they are called oracles. So we've invented a classical mechanical oracle for the halting problem. The same oracle can prove Fermat's Last Theorem by checking all possible sums (infinitely many) of two *n*th powers. More ambitiously, it can prove *all possible theorems* in a finite period of time, by pursuing *all* logically valid chains of deduction from the axioms of set theory. It is clear from this that some 'classical' dynamical systems simulate bizarre computations. The reason is that perfectly respectable equations have a natural interpretation as models of physically impossible devices. However, it's not easy to implement this informal scenario in a logically impeccable mannerfor example, you must actually *write down* the equations, or at least indicate how this could be done; and, more seriously, you have to deal with the potentially infinite-dimensional space of positions on the Turing machine's tape.

Da Costa and Doria use a quite different approach. Their basic idea is inspired by a remark of B. Scarpellini that it may be possible 'to build an analog computer that can simulate functions $f(x)$ so that the predicate $[\varphi(f(x))]$ isn't decidable, while the analog machine itself decides it'. That is, he conceived the existence of an analogue oracle. Developing this idea, da Costa and Doria construct a series of functions $\theta_m(x)$, obtained from classical functions and integrals that involve them. These functions are either identically 0 or identically 1, but their value is formally undecidable. Da Costa and Doria then define a family of classical (that is, non-quantum) dynamical systems, one for each *m*, each being either a free particle or a simple harmonic oscillator, depending upon the value of θ_m. Since the value of θ_m is undecidable, *there is no formal way to compute which is the case*. However, one can imagine a thought experiment in which an analog device simulates the dynamics for any given *m*. It will then be immediately apparent to an observer whether or not the motion is simple harmonic or freein one case the particle oscillates along an interval, in the other it whizzes off to infinityso the analog machine can effectively decide the undecidable.

This shows that even *classical* systems can go beyond the computational capabilities of Turing machines. The thrust of Penrose's

thesis is thus directed elsewhere: if it is to be valid, then it must be posed for 'real' classical systems rather than mathematical ones, a concept that is hard to handle rigorously in a world that is 'really' quantum.

19
The Ultimate in Technology Transfer

One factor that has remained constant throughout all the twists and turns of the history of physical science is the decisive importance of mathematical imagination. In every century in which major advances were achieved the growth in physical understanding was guided by a combination of empirical observation with purely mathematical intuition. For a physicist mathematics is not just a tool by means of which phenomena can be calculated; it is the main source of concepts and principles by means of which new theories can be created.
Freeman Dyson

Can we can learn any lessons about the direction in which mathematics seems to be heading as we tend towards the tail-end of the twentieth century and plough relentlessly into the twenty-first? Is mathematics changing? Or is it just the same tawdry old stuff in a shiny new wrapper?

A bit of both, I think. There is an enormous historical continuity to mathematics: it would be hard to think of another subject in which work done four thousand years ago retains the same freshness that it had on the day it was conceived. The shelf-life of good mathematics is virtually infinite. Once a problem is solved, it's *solved*; and successive generations can continue to learn from that solution.

Despite which, the manner in which mathematicians go about their work is definitely changing in important respects. Let me single out two items for discussion. One is the advent of the computer and the mathematician's new-found ability to perform 'experiments'. The other is the interaction between mathematics and the rest of science. In the first edition of this book, the final chapter ('A Tour of the

Minefield') was a lengthy diatribe on the relation between 'pure' and 'applied' mathematics. Happily, this is a distinction that increasingly makes no sense at all. As one reviewer put it: 'the minefield does not exist.' Fair enough; but it sure looked as if it did when I wrote that chapter. Maybe it was just a parochial view from one side of the Atlantic. Fortunately, the minesweepers have been busy, the explosives have been defused, the burning oil-wells have been quenched, and sweetness and light reign where once departments fought last-ditch battles over every appointment, every lecture course, every examination question

No, wait a minute, the cease-fire hasn't been as effective as *that*

I can see I'm going to get myself into trouble, so I'll abandon this particular line of enquiry immediately and focus solely on the two issues just introduced. Because this chapter is purely personal opinion (as, I freely admit, was its lengthy predecessor), I'll also keep it short.

Experimental Mathematics

Until about the 1970s, computers were seen as glorified calculating machines, 'number-crunchers' whose role was to perform huge calculations. The kind of problems they were useful for were very specific questions in engineering. How does this design of bridge stand up to gales? What are the flight characteristics of a particular wing design on a jet aircraft? How does water flow around the Dutch canal system?

These are eminently important questions in a technological society. However, they hold little to interest the more theoretically minded mathematician, who is interested in general ideas and methods rather than in specific answers. Answers are ten a penny; they only tell you what you wanted to know. The true mathematician always wants to know *more*. Unfortunately, even the simplest general questions can easily lead to calculations far beyond the capabilities of the kind of computer that is entirely happy with supersonic air flow or the gross national product of Lithuania.

The advent of a new generation of computers, with huge memory

capacity, very quick arithmetic, and above all precise, high-resolution graphics, has changed all that. It has become possible to use the computer as an experimental tool. Mathematicians can run through large numbers of examples of phenomena that interest them, and see what answers the computer comes up with. Thenthe crucial step that turns fun and games into mathematicsthey can *think* about the patterns and regularities revealed by those experiments, and see whether they can *prove* that such patterns occur in general.

For example, two minutes with a calculator will convince you that the square of any odd number, minus one, is always a multiple of eight. Thus 3^2 1 = 8, 5^2 1 = 24 = 3.8, 7^2 1 = 48 = 6.8, , 1001^2 1 = 1002000 = 125250.8. But while virtually any mathematician would be convinced by such evidence that there was something interesting to be proved, none of them would consider any such list of numbers, however lengthy, actually to *be* a proof. Here the proof is not far away: $(2n + 1)^2$ 1 = $4n(n + 1)$ and either n or $n + 1$ is even. But many experimental 'facts'such as the Riemann hypothesisremain unproven. OthersFermat's Last Theorem, the Bieberbach conjecturehave succumbed only after a lengthy struggle.

There are good reasons for the mathematician's scepticism. Experiments, even extensive ones, can often be misleading. For example, if you count the number of primes of the form $4k + 1$ and $4k + 3$ up to a given limitsay a billion or sothen you will find that those of the form $4k + 3$ always come out ahead. This might seem compelling evidence; but it has been proved that eventually the $4k + 1$ primes catch up. In fact, the lead changes infinitely often. An early theorem along these lines showed that the primes of the form $4k + 1$ gain the lead for numbers no larger than 10^{1034}. Innocent enough, you may thinkbut there isn't enough room in the universe to write this number out in normal decimal place notation. Certainly it would never be found by direct trial-and-error experiment.

There is nothing intrinsically radical about the experimental approach. Gauss's notebooks reveal that he performed innumerable calculations trying to guess the answers to tricky problems in number theory. Isaac Newton's papers are also littered with experimental calculations, and so presumably are those of most research

mathematiciansafter all, their ideas have to come from somewhere. It may be true that occasionally an idea comes fully formed, 'out of thin air'; but most of the time it is the result of a lot of pencil-and-paper 'experiment'.

Two things, however, are new. The first is the computer, which makes it possible to perform large numbers of experiments, or very complicated experiments, to an extent that exceeds the capacity of the unaided human brain. Computer algebra programs make it possible to perform symbolic calculationsto do algebra just like the brain does, in symbols, not with specific numbersand to do this with far greater accuracy than the brain, and with much more complicated expressions than the brain can handle. There are 'computer-assisted proofs'rigorous proofs of theorems that require the computer to do certain boring donkey-work. Appel and Haken's proof of the four-colour theorem was one of the first really spectacular examples.

The second new development is that mathematicians are becoming much more explicit about their use of experiments. They are now being published even when rigorous proofs are lacking. This doesn't represent a drop in standards: everybody agrees that experiments aren't proofs. But the emphasis has shifted, and evidence that may motivate new ideas has become interesting in its own right. In Gauss's day it was all right to perform experiments, but onlyso he believedif you kept them to yourself. He never published his experiments: only the polished proofs that he concocted on their basis. You will recall his justification from Chapter 14: 'when one has constructed a fine building, the scaffolding should no longer be visible.' And you will also remember that Jacobi likened Gauss to the fox who erases his tracks with his tail.

Nowadays, quite a few people are more interested in the scaffolding than in the building, and they want to see in which directions the fox ran. Indeed, a new journal of experimental mathematics has been started, to publish precisely this kind of material. The modern taste is to take a look at the scaffolding as well, in the hope of learning how to make buildings of one's own. Anyone interested in the mental processes of mathematicians is driven to despair by misguided

attempts to conceal them. If you are going to train research mathematicians, you need to *show* them the scaffolding, otherwise they get totally the wrong idea of what research is about.

Storm in a Mandelbrot Set

As I write, a minor controversy is raging about experimental mathematics. A few peoplelet me call them the anti-experimentalistsdislike the approach intensely. They believe that it is damaging the integrity of mathematics. A sample of the genre is the statement that fractalsone of the major beneficiaries of the trend towards computer experimentshave added nothing new to mathematics. The anti-experimentalists argue that the theoretical side of fractals is just geometric measure theory or complex analysis, while the applications are not mathematics at all. As for the role of experiment, the anti-experimentalists point out that pictures of the Mandelbrot set make it appear disconnected, whereas in fact it has been proved to be all in one piece.

How naïve of the experimental mathematicians to make such a silly mistake.

Except, of course, that they didn't. When Mandelbrot first drew his set, on a rather ancient line printer, he did notice a few tiny specks scattered around, and wondered whether they were dirt or Mandelbrot set. Dirt was soon ruled out. The specks certainly did not appear to be connected to the rest of the set. Did anyone announce that the Mandelbrot set is disconnected? Not at all. As anyone who works with computer graphics soon learns, there may be invisible fine structure below the limits of resolution of the pictures. Instead, Mandelbrot conjectured that the set was *connected*, and asked for proof or disproof. It became a well-known *problem* in the field.

Other experimental evidence came from a rather different method of representing the set, by multicoloured contours that focus in upon it. Those contours do *not* surround the specks, as they would if the specks really were disconnected from the rest of the set. Instead, they seem to nestle around thin filaments that run between the specks and the main body of the Mandelbrot set. The filaments themselves

do not show up in computer pictures whose pixels are much larger than the filaments are wide, which explains why Mandelbrot didn't see them. Nevertheless, this particular piece of experimental evidence was in their favour. Far from being misleading, it pointed directly at the truth. However, nobody interpreted *those* pictures as proof either. The problem remained open, until John Hubbard and Adrien Douady proved, in the conventional sense of the word, that the Mandelbrot set is indeed connected.

Hubbard has explained the role that the computer pictures played in suggesting a proof. After staring at them for hours, he decided that the set must in fact be connected. Despite naïve appearances to the contrary, if you think about what the pictures represent, *and their limitations*, you can convince yourself that the Mandelbrot set must be in one piece. The proof did not emerge directly from the computer pictures, it wasn't even computer-assistedbut the computer experiments played a key inspirational role.

The main plank in the anti-experimentalist platform is the danger of making unwarranted assumptions. In this particular case, however, what actually happened was the exact opposite. It seems to me that their position reduces to the assertion that, if you perform computer experiments badly, or leap to naïve conclusions by forgetting the things that computer pictures do *not* show, then you can come up with nonsense. I agree entirely, but I think it's irrelevant. It's true of everything, not just experimental mathematics. If you do it badly, or stupidly, then you get in a mess. In this case, the *only* people who seem to have assumed that the computer pictures indicate that the Mandelbrot set is disconnected are the anti-experimentalists.

Indeed it's hard to see quite what the anti-experimentalists think they are trying to achieve. Most mathematicians are perfectly happy with the use of computers as tools for suggesting interesting theorems. They accept, as do most if not all experimental mathematicians, that the experiments aren't proofs. They remain happy to go out looking for the proofs that the experiments suggest might exist. They neither reject the computer, nor do they believe everything that it seems to be telling them. This is an entirely healthy attitude. It offers experimental mathematics a secure position *alongside*

conventional approaches to the subject. As in all branches of science, theory and experiment go hand in hand. Why ever not?

Relations with Applied Science

One common view of mathematics divides it into two camps, labelled 'pure' and 'applied'. As the century draws to a close, this division is looking increasingly artificial, increasingly dated, and increasingly unhelpful. Applied science has motivated some very important new mathematics, ideas that can if necessary stand on their own as 'pure' mathematics of the highest order. Conversely, innumerable areas of allegedly 'pure' mathematics, invented for their own sake, are turning out to be useful all over the place. The interactions are often remarkable, and sometimes totally unexpected. Only someone with an unusually broad grasp of science and blessed with an unusually fertile imagination might have been able to see them coming.

Virtually every chapter of this book contains examples of interactions in both directions. The application of number theory to secret codesand the stimulation of new discoveries in number theory. The physical inspiration behind calculusand the mathematical imagination that turned it into complex analysis. The application of group theory to crystallographyand the new mathematical challenges of quasicrystals. Chaos. Fractals. Knots.

What we see developing here is not new. It is the way mathematics used to be done, before it got so complicated and so huge that it began to focus itself around specialities, before the specialities became so enmeshed in their own internal problems that they lost sight of their roots in the outside world. And, to the credit of the global mathematical community, it is the way mathematics is starting to face up to the challenges of the next century. Very little of the mathematics discovered over the past fifty years is rendered useless by this trend: on the contrary, almost all *good* mathematicsby which I mean mathematics that is non-trivial and that relates creatively to the rest of the subjectis finding new roles in relation to applied science.

Huge amounts of new mathematics are discovered every year, accompanied by a growing number of new applications, many of them unexpected both in the ingredients that are used and in the areas to which they are applied. They include the use of number theory to study the efficiency of telephone systems, of graph theory to see how best to extract ore from mines, of symmetry to understand the inner workings of electric arc furnaces, and of geometry to improve the production of chemicals. These applications involve new mathematical concepts, new ideas, new theoriesthey're not just the result of 'putting problems on the computer'. It's very hard to put a problem on the computer without first understanding it in some detail, and computer scientists are increasingly resorting to mathematics to help them sort out how best to use their electronic machinery.

What of the future? Here is my guess at the shape of twenty-first century mathematics. It will be more closely connected to applied science, more explicitly reliant on computer experiments, more aware of its own motivation, less interested in contemplating its own logical navel. It will remain vigorous and innovative. This new *style* of mathematics will continue to be the common language of all branches of science. It will grow organically from mathematics as we know it, it will adapt to meet new challenges, but its pivotal role will be unchanged.

Jovian Javelins

Why is mathematics such a powerful and central tool for science? Eugene Wigner wrote about the 'unreasonable effectiveness' of mathematics, and wondered how something that at source was just an artificial construct of the human mind could provide insights into the nature of the universe. Part of his answer was that mathematics is one end of a two-way flow of ideas, with the natural world at the other. I think this is true, but there is an additional feature of mathematics that makes it unusually responsive to the needs of science. That is its universality. Mathematics is about ideas; about how certain facts follow inevitably from others; about how certain structures automatically imply the occurrence of particular phenomena. It lets us build mental models of the world, and manipulate those models in ways that would be impossible in a real experiment.

You want to know how far a javelin would travel in Jupiter's gravity? Plug in the equations, set g to 27 m $\sec^{-1}$, and work it out. *You don't have to go to Jupiter and throw one*. (With reasonable assumptions, the current Olympic record of 85.9 metres would become about 33.5 metres on Jupiter.)

Mathematics is about ideas *per se*, not just specific realizations of those ideas. If you learn something about differential equations by analysing Jovian javelins, you may be able to apply that insight to the population dynamics of giraffes or the design of better bicycles. Mathematics doesn't care how you use it, or what problem you apply it to. Its very abstractionwhich many people admittedly find repellinggives it a unity and a universality that would be lost if it were made more concrete or more specialized.

Because many people do find abstraction repelling, it's no good mathematicians thinking that their job is finished when the tool is built. A sales job remains. The world will not beat a path to the mathematical door unless it knows about the improved mathematical mousetrap. Contrary to what many mathematicians appear to assume, the world is not full of people scouring technical journals for a new piece of mathematics that they can put to use in their own area. They need to be told what's available, in their own language, and in the context of their own concerns. I'm not suggesting we all become salespeople; but I am saying that the health of Mathematics Plc depends upon the provision of an adequate sales force.

In short: for the effective transfer of abstract mathematical ideas into science it helps to have an interpreter, a go-between, someone with a foot in both camps. In many areas of science you will find the same underlying ideasthe use of matrices to represent linear equations, saybut bearing the names of many different scientists, one or more per discipline. This is the person who discovered how to use that particular piece of mathematics in that areaor, given the general level of historical accuracy in such attributions, some prima donna who used the idea and attracted attention to it. Because of the lack

of communication between disciplines, he or she may well have been reinventing the mathematical wheel at the time the work was first done; nevertheless, it *was* done, and it opened the way to a more organized movement of ideas. There is a lot of talk these days about 'technology transfer', mostly in the sense of taking established technologies and using them in other applications. Mathematics, as I have said before, is the ultimate in technology transfer. It transfers general ideas between *different* technologies.

As a result, mathematics pervades our society. Most of us don't notice this, because it mostly stays behind the scenes. If everything that relied upon it carried a big red label saying MADE USING MATHEMATICS, then red labels would take over from CFCs as a major environmental pollutant. For example, take something as innocuous as a jar of jam. The container is made in a computer-controlled factory. Its label is printed by machinery designed according to mathematical principles of the strength of materials and the behaviour of lubricants. Its ingredients are transported in ships that navigate using satellites, whose orbits are analysed mathematically to give the ship's position. The price is determined by the rules of mathematical economics. Now start thinking about how you make the computers, launch the satellites, build the ships, even make the paper for the label

I've forgotten something oh, yesthe fruit. What could be simpler than raspberries? But even the plants you grow in your garden aren't as straightforward as you may think. Raspberries are disease-prone: they have to be destroyed every five or ten years and replaced by plants imported from colder regions, such as Scotland. The breeders are constantly seeking new varieties, and a great deal of probability theory and combinatorics goes into the statistical tests used to check the progress of breeding. What about genetic engineering, looming on the horizon? Or the weather forecasting that warns the grower of an early frost?

We live in a complicated world, where nothing is as simple as it once was, nothing is as simple as it seems to be. Mathematics knits that world together.

Don't you forget it.

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