E. T. BELL

MEN OF MATHEMATICS

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VOLUME TWO

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CONTENTS OF VOLUME TWO

PREFACE TO VOLUME TWO

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THE general introduction in the first volume of this book need not be repeated here. However, a few points may be briefly recalled, as they are relevant for both volumes. We shall also take a quick glance ahead beyond the last of the men mentioned.

Mathematics as understood by mathematicians is based on deductive reasoning applied to sets of outright assumptions called axioms or postulates. It is sufficient here to describe deductive reasoning as the rules of common logic, although mathematical logic goes far beyond that. The postulates underlying a particular division of mathematics, such as elementary algebra or school geometry, may have been suggested by everyday observation of the world as it presents itself to our senses. Many of the propositions of geometry, for instance, such as that gem attributed to Thales in the sixth century B.C., 'The angle inscribed in a semicircle is a right angle', are evident to the eye. But however obvious and sensible they may seem, they are not a part of mathematics until they have been deduced from a set of postulates accepted without argument as self-consistent. The great but (to us) nameless mathematicians of Babylonia discovered, or invented, many beautiful things in both algebra and geometry, but, so far as is known, they proved none of them. It remained for the Greeks of about 600 B.C. to invent proof – deductive reasoning. With that epochal invention mathematics was born. But logic and proof are by no means the whole story. Intuition and insight are as freely used to-day in mathematics as they must have been by the Babylonians.

It was many centuries before the full significance of what those old Greeks had done was understood and applied to all mathematics, and thence to all reasoning. A notable instance is school algebra, first thoroughly understood and rigorously developed only in the 1830's by the British School, of whom George Peacock (1791–1858) is especially memorable. Unfortunately there is not space here to tell the lives of these little-known men who helped to prepare the way for the vast development of mathematics in the nineteenth and twentieth centuries.

As we pass from the eighteenth to the nineteenth century we are overwhelmed by a tidal wave of free inventiveness. New departments of mathematics were created and developed in bewildering profusion. The great mathematicians of the nineteenth century, some of whom are presented here, seem to be almost of a different species from their predecessors. The new men were not content with special problems, but attacked and solved general problems whose solutions yielded those of a multitude of problems which, in the eighteenth century, would have been considered one by one. A striking example has often been noted in the contrast between Gauss (1777–1855) and Abel (1802–29) in the theory of algebraic equations. There is a similar distinction in the matter of geometry between Gauss and his pupil Riemann (1826–66). It is no disparagement of Gauss, but merely a statement of historical fact, to say that he was content with the problem of finding the algebraic solution of binomial equations, and did not even mention the general problem, solved by Abel and Galois (1811–32), of determining necessary and sufficient conditions that any given algebraic equation be solvable by radicals. The nature of the general problem is explained in the accounts given here of Abel and Galois. Of course there is a certain loose continuity in all mathematics, clear back to Babylon and Egypt, but the interesting and fruitful points on the curve of progress are the discontinuities that appear when the curve is closely analysed as in that just noted of Gauss, Abel, and Galois. One such from the 1930's must suffice here as a current example.

The paradoxes of Zeno and the repeated attempts to establish the differential and integral calculus on a firm logical foundation exercised mathematicians as early as the seventeenth century, and continued to worry them all through the second half of the nineteenth. Among those who struggled at the task were three whom we shall meet later, Cantor, Dedekind, and Weierstrass. Dedekind admitted failure. But failure or not to achieve the desired end, the work of all three gave a tremendous impulse to the study of all mathematical reasoning. How was it to be decided that a certain theorem had really been proved? Might not there be concealed inconsistencies in the very foundations and postulate systems on which the whole elaborate structure had been reared? It began to appear that an exhaustive re-examination of everything from the ground up was demanded. The capital problem was to prove the self-consistency of mathematical analysis – the calculus and its numerous modern offshoots. Presently this programme turned out to be far more difficult than had been anticipated, and David Hilbert (1862–1943), the last of the giants from the nineteenth century, in 1898 proposed the more modest problem of proving the consistency of arithmetic. This led to the like for mathematical logic.

All was going well till 1931, when Kurt Gödel (1906–) showed that in any well-defined system of mathematical axioms there exist mathematical questions which cannot be settled on the basis of these axioms. But suppose we go to a more inclusive system in which, perhaps, the questions can be settled. The same difficulty appears in the new system, and so on indefinitely. There are thus specific purely mathematical 'yes–no' questions which will be forever undecidable by human beings.

This wholly unexpected conclusion has been called the most significant advance in logic since Aristotle. It does not mean that mathematics has gone to smash, but it does suggest that some of the claims made for mathematics in the past will have to be moderated. One philosophical die-hard who thoroughly misunderstood what Gödel had done, proudly proclaimed, 'I am an Aristotelian. The old logic is good enough for me', which sounded like an echo of the revivalist hymn 'The old-time religion, the old-time religion is good enough for me.' Aristotelian logic may be good enough for the old-timers, but it is not good enough for mathematics, nor has it been for at least three centuries. As one detail, Aristotle's logic makes no provision for variables and functions as they occur in mathematics. There is not space here to elaborate any of this, but those interested will find an elementary and lucid account by Alfred Tarski in his *Introduction to Logic and the Methodology of Deductive Sciences* (Oxford University Press, 2nd Edition, 1941).

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MEN OF MATHEMATICS

VOLUME TWO

CHAPTER SIXTEEN

THE COPERNICUS OF GEOMETRY

Lobatchewsky

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GRANTING that the commonly accepted estimate of the importance of what Copernicus did is correct, we shall have to admit that it is either the highest praise or the severest condemnation humanly possible to call another man the 'Copernicus' of anything. When we understand what Lobatchewsky did in the creation of non-Euclidean geometry, and consider its significance for all human thought, of which mathematics is only a small if important part, we shall probably agree that Clifford (1845–79), himself a great geometer and far more than a 'mere mathematician', was not overpraising his hero when he called Lobatchewsky 'The Copernicus of Geometry'.

Nikolas Ivanovitch Lobatchewsky, the second son of a minor government official, was born on 2 November 1793 in the district of Makarief, government of Nijni Novgorod, Russia. The father died when Nikolas was seven, leaving his widow, Praskovia Ivanovna, the care of three young sons. As the father's salary had barely sufficed to keep his family going while he was alive the widow found herself in extreme poverty. She moved to Kazan, where she prepared her boys for school as best she could, and had the satisfaction of seeing them accepted, one after the other, as free scholars at the Gymnasium. Nikolas was admitted in 1802 at the age of eight. His progress was phenomenally rapid in both mathematics and the classics. At the age of fourteen he was ready for the university. In 1807 he entered the University of Kazan (founded in 1805), where he was to spend the next forty years of his life as student, assistant professor, professor, and finally rector.

Hoping to make Kazan ultimately the equal of any university in Europe, the authorities had imported several distinguished professors from Germany. Among these was the astronomer Littrow, who later became director of the Observatory at Vienna, whom Abel mentioned as one of his excuses for seeing something of 'the south'. The German professors quickly recognized Lobatchewsky's genius and gave him every encouragement.

In 1811, at the age of eighteen, Lobatchewsky obtained his master's degree after a short tussle with the authorities, whose ire he had incurred through his youthful exuberance. His German friends on the faculty took his part and he got his degree with distinction. At this time his elder brother Alexis was in charge of the elementary mathematical courses for the training of minor government officials, and when Alexis presently took a sick-leave, Nikolas stepped into his place as substitute. Two years later, at the age of twenty-one, Lobatchewsky received a probationary appointment as 'Extraordinary Professor' or, as would be said in America, Assistant Professor.

Lobatchewsky's promotion to an ordinary professorship came in 1816 at the unusually early age of twenty-three. His duties were heavy. In addition to his mathematical work he was charged with courses in astronomy and physics, the former to substitute for a colleague on leave. The fine balance with which he carried his heavy load made him a conspicuous candidate for yet more work, on the theory that a man who can do much is capable of doing more, and presently Lobatchewsky found himself University Librarian and curator of the chaotically disordered University Museum.

Students are often an unruly lot before life teaches them that generosity of spirit does not pay in the cut-throat business of earning a living. Among Lobatchewsky's innumerable duties from 1819 till the death of the Tsar Alexander in 1825 was that of supervisor of all the students in Kazan, from the elementary schools to the men taking post-graduate courses in the University. The supervision was primarily over the political opinions of his charges. The difficulties of such a thankless job can easily be imagined. That Lobatchewsky contrived to send in his reports day after day and year after year to his suspicious superiors without once being called on the carpet for laxity in espionage, and without losing the sincere respect and affection of all the students, says more for his administrative ability than do all the gaudy orders and medals which a grateful Government showered on him and with which he delighted to adorn himself on state occasions.

The collections in the University Museum to all appearance had been tossed in with a pitchfork. A similar disorder made the extensive library practically unusable. Lobatchewsky was commanded to clean up these messes. In recognition of his signal services the authorities promoted him to the deanship of the Faculty of Mathematics and Physics, but omitted to appropriate any funds for hiring assistance in straightening out the library and the museum. Lobatchewsky did the work with his own hands, cataloguing, dusting, and casing, or wielding a mop as the occasion demanded.

With the death of Alexander in 1825 things took a turn for the better. The particular official responsible for the malicious persecution of the University of Kazan was kicked out as being too corrupt for even a government post, and his successor appointed a professional curator to relieve Lobatchewsky of his endless task of cataloguing books, dusting mineral specimens, and deverminizing stuffed birds. Needing political and moral support for his work in the University, the new curator did some high politics on his own account and secured the appointment in 1827 of Lobatchewsky as Rector. The mathematician was now head of the University, but the new position was no sinecure. Under his able direction the entire staff was reorganized, better men were brought in, instruction was liberalized in spite of official obstruction, the library was built up to a higher standard of scientific sufficiency, a mechanical workshop was organized for making the scientific instruments required in research and instruction, an observatory was founded and equipped – a pet project of the energetic Rector's – and the vast mineralogical collection, representative of the whole of Russia, was put in order and constantly enriched.

Even the new dignity of his rectorship did not deter Lobatchewsky from manual labour in the library and museum when he felt that his help was necessary. The University was his life and he loved it. On the slightest provocation he would take off his collar and coat and go to work. Once a distinguished foreigner, taking the coatless Rector for a janitor or workman, asked to be shown through the libraries and museum collections. Lobatchewsky showed him the choicest treasures, explaining as he exhibited. The visitor was charmed and greatly impressed by the superior intelligence and courtesy of this obliging Russian worker. On parting from his guide he tendered a handsome tip. Lobatchewsky, to the foreigner's bewilderment, froze up in a cold rage and indignantly spurned the proffered coin. Thinking it but just one more eccentricity of the high-minded Russian janitor, the visitor bowed and pocketed his money. That evening he and Lobatchewsky met at the Governor's dinner table, where apologies were offered and accepted on both sides.

Lobatchewsky was a strong believer in the philosophy that in order to get a thing done to your own liking you must either do it yourself or understand enough about its execution to be able to criticize the work of another intelligently and constructively. As has been said, the University was his life. When the Government decided to modernize the buildings and add new ones, Lobatchewsky made it his business to see that the work was done properly and the appropriation not squandered. To fit himself for this task he learned architecture. So practical was his mastery of the subject that the buildings were not only handsome and suited for their purposes but, what must be almost unique in the history of governmental building, were constructed for less money than had been appropriated. Some years later (in 1842) a disastrous fire destroyed half Kazan and took with it Lobatchewsky's finest buildings, including the barely completed observatory – the pride of his heart. But owing to his energetic cool-headedness the instruments and the library were saved. After the fire he set to work immediately to rebuild. Two years later not a trace of the disaster remained.

We recall that 1842, the year of the fire, was also the year in which, thanks to the good offices of Gauss, Lobatchewsky was elected a foreign correspondent of the Royal Society of Göttingen for his creation of non-Euclidean geometry. Although it seems incredible that any man so excessively burdened with teaching and administration as Lobatchewsky was, could find the time to do even one piece of mediocre scientific work, he had actually, somehow or another, made the opportunity to create one of the great masterpieces of all mathematics and a landmark in human thought. He had worked at it off and on for twenty years or more. His first public communication on the subject, to the Physical-Mathematical Society of Kazan, was made in 1826. He might have been speaking in the middle of the Sahara Desert for all the echo he got. Gauss did not hear of the work till about 1840.

Another episode in Lobatchewsky's busy life shows that it was not only in mathematics that he was far ahead of his time. The Russia of 1830 was probably no more sanitary than that of a century later, and it may be assumed that the same disregard of personal hygiene which filled the German soldiers in World War I with an amazed disgust for their unfortunate Russian prisoners, and which to-day causes the industrious proletariat to use the public parks and playgrounds of Moscow as vast and convenient latrines, distinguished the luckless inhabitants of Kazan in Lobatchewsky's day when the cholera epidemic found them richly prepared for a prolonged visitation. The germ theory of disease was still in the future in 1830, although progressive minds had long suspected that filthy habits had more to do with the scourge of the pestilence than the anger of the Lord.

On the arrival of the cholera in Kazan the priests did what they could for their smitten people, herding them into the churches for united supplication, absolving the dying and burying the dead, but never once suggesting that a shovel might be useful for any purpose other than digging graves. Realizing that the situation in the town was hopeless, Lobatchewsky induced his faculty to bring their families to the

University and prevailed upon – practically ordered – some of the students to join him in a rational, human fight against the cholera. The windows were kept closed, strict sanitary regulations were enforced, and only the most necessary forays for replenishing the food supply were permitted. Of the 660 men, women and children thus sanely protected, only sixteen died, a mortality of less than 2.5 per cent. Compared to the losses under the traditional remedies practised in the town this was negligible.

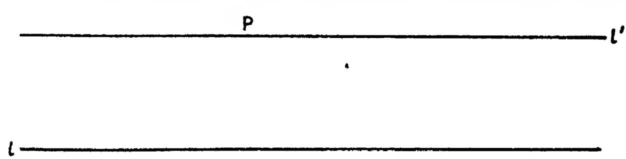
It might be imagined that after all his distinguished services to the State and his European recognition as a mathematician, Lobatchewsky would be in line for substantial honours from his Government. To imagine anything of the kind would not only be extremely naïve but would also traverse the scriptural injunction 'Put not your trust in princes'. As a reward for all his sacrifices and his unswerving loyalty to the best in Russia, Lobatchewsky was brusquely relieved in 1846 of his Professorship and his Rectorship of the university. No explanation of this singular and unmerited double insult was made public. Lobatchewsky was in his fifty-fourth year, vigorous of body and mind as ever, and more eager than he had ever been to continue with his mathematical researches. His colleagues to a man protested against the outrage, jeopardizing their own security, but were curtly informed that they as mere professors were constitutionally incapable of comprehending the higher mysteries of the science of government.

The ill-disguised disgrace broke Lobatchewsky. He was still permitted to retain his study at the University. But when his successor, hand-picked by the Government to discipline the disaffected faculty, arrived in 1847 to take up his ungracious task, Lobatchewsky abandoned all hope of ever being anybody again in the University which owed its intellectual eminence almost entirely to his efforts, and he appeared thereafter only occasionally to assist at examinations. Although his eyesight was failing rapidly he was still capable of intense mathematical thinking.

He still loved the University. His health broke when his son died, but he lingered on, hoping that he might still be of some use. In 1855 the University celebrated its semi-centennial anniversary. To do honour to the occasion, Lobatchewsky attended the exercises in person to present a copy of his *Pangeometry*, the completed work of his scientific life. This work (in French and Russian) was not written by his own hand, but was dictated, as Lobatchewsky was now blind. A few months later he died, on 24 February 1856, at the age of sixty-two.

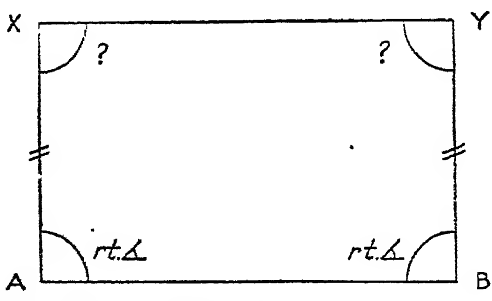
To see what Lobatchewsky did we must first glance at Euclid's outstanding achievement. The name Euclid until quite recently was practically synonymous with elementary school geometry. Of the man himself very little is known beyond his doubtful dates, 330–275 B.C. In addition to a systematic account of elementary geometry his *Elements* contain all that was known in his time of the theory of numbers. Geometrical teaching was dominated by Euclid for over 2,200 years. His part in the *Elements* appears to have been principally that of a co-ordinator and logical arranger of the scattered results of his predecessors and contemporaries, and his aim was to give a connected, reasoned account of elementary geometry such that every statement in the whole long book could be referred back to the postulates. Euclid did not attain this ideal or anything even distantly approaching it, although it was assumed for centuries that he had.

Euclid's title to immortality is based on something quite other than the supposed logical perfection which is still sometimes erroneously ascribed to him. This is his recognition that the fifth of his postulates (his Axiom XI) is a pure assumption.



The fifth postulate can be stated in many equivalent forms, each of which is deducible from any one of the others by means of the remaining postulates of Euclid's geometry. Possibly the simplest of these equivalent statements is the following: Given any straight line l and a point P not on l, then in the plane determined by l and P it is possible to draw *precisely one* straight line l' through P such that l' never meets l no matter how far l' and l are extended (in either direction). Merely as a nominal definition we say that two straight lines lying in one plane which never meet are *parallel*. Thus the fifth postulate of Euclid asserts that through P there is precisely one straight line parallel to l. Euclid's penetrating insight into the nature of geometry convinced him that this postulate had not, in his time, been deduced from the others, although there had been many attempts to *prove* the postulate. Being unable to deduce the postulate himself from his other assumptions, and wishing to use it in the proofs of many of his theorems, Euclid honestly set it out with his other postulates.

There are one or two simple matters to be disposed of before we come to Lobatchewsky's Copernican part in the extension of geometry. We have alluded to 'equivalents' of the parallel postulate. One of these, 'the hypothesis of the right angle', as it is called, will suggest two possibilities, neither equivalent to Euclid's assumption, one of which introduces Lobatchewsky's geometry, the other, Riemann's.



Consider a figure $AXYB$ which 'looks like' a rectangle, consisting of four straight lines AX, XY, YB, BA, in which BA (or AB) is the base, AX and YB (or BY) are drawn equal and perpendicular to AB, and on the same side of AB. The essential things to be remembered about this figure are that each of the angles XAB, YBA (at the base) is a right angle, and that the sides AX, BY are equal in length. *Without using the parallel postulate*, it can be proved that the angles AXY, BYX, are *equal*, but, *without* using this postulate, *it is impossible to prove that AXY, BYX are right angles*, although they look it. If we *assume* the *parallel postulate* we can *prove* that *AXY*, *BYX* are *right angles* and, conversely, if we *assume* that *AXY*, *BYX* are *right angles*, we can *prove* the parallel postulate. Thus *the assumption that AXY, BYX are right angles* is equivalent *to the parallel postulate*. This assumption is to-day called *the hypothesis of the right angle* (since both angles are right angles the singular instead of the plural 'angles' is used).

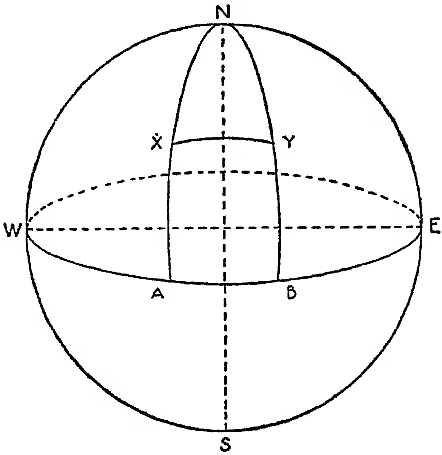
It is known that the hypothesis of the right angle leads to a consistent, practically useful geometry, in fact to Euclid's geometry refurbished to meet modern standards of logical rigour. But the figure suggests two other possibilities: each of the equal angles *AXY*, *BYX* is *less* than a right angle – *the hypothesis of the acute angle*; each of the equal angles *AXY*, *BYX* is *greater* than a right angle – *the hypothesis of the obtuse angle*. Since any angle can satisfy one, and only one, of the requirements that it be *equal to, less than*, or *greater than* a right angle, the three hypotheses – of the right angle, acute angle, and obtuse angle respectively – exhaust the possibilities.

Common experience predisposes us in favour of the first hypothesis. To see that each of the others is not as unreasonable as might at first appear we shall consider something closer to actual human experience than the highly idealized 'plane' in which Euclid imagined his figures drawn. But first we observe that neither the hypothesis of the acute angle nor that of the obtuse angle will enable us to prove Euclid's parallel postulate, because, as has been stated above, Euclid's postulate is *equivalent* to the hypothesis of the *right angle* (in the sense of inter-deducibility; the hypothesis of the right angle is both necessary and sufficient for the deduction of the parallel postulate). Hence if we succeed in constructing geometries on either of the two new hypotheses, we shall not find in them parallels in Euclid's sense.

To make the other hypotheses less unreasonable than they may seem at first sight, suppose the Earth were a perfect sphere (without irregularities due to mountains, etc.). A plane drawn through the centre of this ideal Earth cuts the surface in a *great* *circle*. Suppose we wish to go from one point A to another B on the surface of the Earth, keeping always *on* the surface in passing from A to B, and suppose further that we wish to make the journey by the shortest way possible. This is the problem of 'great circle sailing'. Imagine a plane passed through A, B, and the centre of the Earth (there is one, and only one, such plane). This plane cuts the surface in a great circle. To make our shortest journey we go from A to B along the shorter of the two arcs of this great circle joining them. If A, B happen to lie at the extremities of a diameter, we may go by either arc.

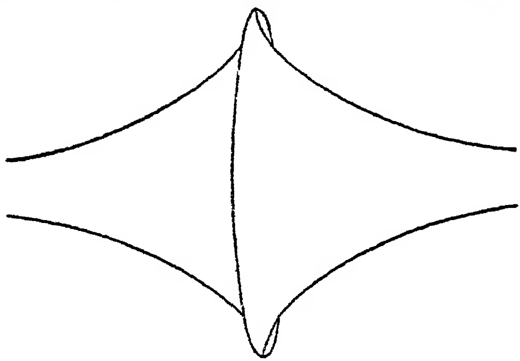
The preceding example introduces an important definition, that of a *geodesic on a surface*, which will now be explained. It has just been seen that the *shortest* distance joining two points on a sphere, the distance itself being measured *on the surface*, is *an* arc of the great circle joining them. We have also seen that the *longest* distance joining the two points is the *other* arc of the same great circle, except in the case when the points are ends of a diameter, when shortest and longest are equal. In the chapter on Fermat 'greatest' and 'least' were subsumed under the common name 'extreme', or 'extremum'. We recall now one usual definition of a straight-line segment joining two points in a plane – '*the shortest distance* between two points'. Transferring this to the sphere, we say that to *straight line* in the *plane* corresponds *great circle* on the *sphere*. Since the Greek word for the Earth is the first syllable ge ($\gamma\hat{\eta}$) of geodesic we call *all extrema joining any two points on any surface the geodesics of that surface.* Thus in a plane the geodesics are Euclid's straight lines; on a sphere they are great circles. A geodesic can be visualized as the position taken by a string stretched as tight as possible between two points on a surface.

Now, in navigation at least, an ocean is not thought of as a flat surface (Euclidean plane) if even moderate distances are concerned; it is taken for what it very approximately is, namely, a part of the surface of a sphere, and the geometry of great circle sailing is not Euclid's. Thus Euclid's is not the only geometry of human utility. On the plane two geodesics intersect in exactly *one* point *unless* they are parallel, when they do not intersect (in Euclidean geometry); but on the sphere *any* two geodesics always intersect in precisely *two* points. Again, on a plane, no two geodesics can enclose a space – as Euclid assumed in one of the postulates for his geometry; on a sphere, any two geodesics always enclose a space.

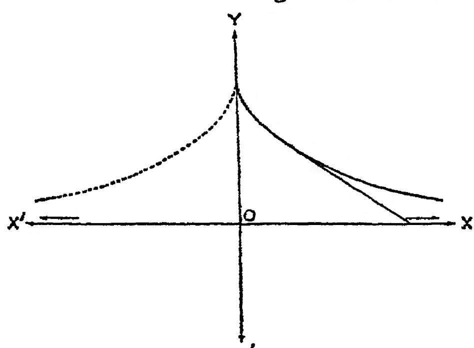


Imagine now the equator on the sphere and two geodesics drawn through the north pole perpendicular to the equator. In the northern hemisphere this gives a triangle with curved sides, two of which are equal. Each side of this triangle is an arc of a geodesic. Draw any other geodesic cutting the two equal sides so that the intercepted parts between the equator and the cutting line are equal. We now have, *on the sphere*, the four-sided figure corresponding to the $AXYB$ we had a few moments ago in the plane. The two angles at the base of this figure are right angles and the corresponding sides are equal, as before, *but each of the equal angles at X, Y is now greater than a right angle*. So, in the highly practical geometry of great circle sailing, which is closer to real human experience than the idealized diagrams of elementary geometry ever get, it is not Euclid's postulate which is true – or its equivalent in the hypothesis of the right angle – but the geometry which follows from the hypothesis of the obtuse angle.

In a similar manner, inspecting a less familiar surface, we can make reasonable the hypothesis of the acute angle. The surface looks like two infinitely long trumpets soldered together at their largest ends. To describe it more accurately we must introduce the plane curve called the *tractrix*, which is generated as follows.



Let two lines XOX', YOY' be drawn in a horizontal plane intersecting at right angles in O, as in Cartesian geometry. Imagine an inextensible fibre lying along YOY', to one end of which is attached a small heavy pellet; the other end of the fibre is at O. Pull this end out along the line OX. As the pellet

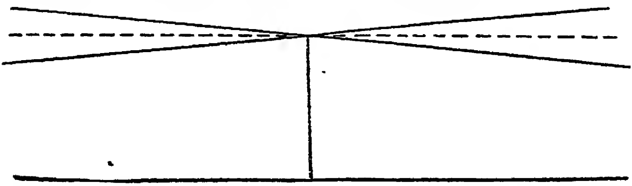


follows, it traces out one half of the tractrix; the other half is traced out by drawing the end of the fibre along OX', and of course is merely the reflection or image in OY of the first half. The drawing out is supposed to continue indefinitely – 'to infinity' – in each instance. Now imagine the tractrix to be revolved about the line XOX'. The double-trumpet surface is generated; for reasons we need not go into (it has constant negative curvature) it is called a *pseudo-sphere*. If on this surface we draw the four-sided figure with two equal sides and two right angles as before, using geodesics, we find that the hypothesis of the acute angle is realized.

Thus the hypotheses of the right angle, the obtuse angle, and the acute angle respectively are true on a Euclidean plane, a sphere, and a pseudosphere respectively, and in all cases 'straight lines' are *geodesics* or *extrema*. Euclidean geometry is a limiting, or degenerate, case of geometry on a sphere, being attained when the radius of the sphere becomes infinite.

Instead of constructing a geometry to fit the Earth as human beings now know it, Euclid apparently proceeded on the assumption that the Earth is flat. If Euclid did not, his predecessors did, and by the time the theory of 'space', or geometry, reached him the bald *assumptions* which he embodied in his postulates had already taken on the aspect of hoary and immutable necessary truths, revealed to mankind by a higher intelligence as the veritable essence of all material things. It took over 2,000 years to knock the eternal truth out of geometry, and Lobatchewsky did it.

To use Einstein's phrase, Lobatchewsky *challenged an axiom*. Anyone who challenges an 'accepted truth' that has seemed necessary or reasonable to the great majority of sane men for 2,000 years or more takes his scientific reputation, if not his life, in his hands. Einstein himself challenged the axiom that two events can happen in *different places* at the *same time*, and by analysing this hoary assumption was led to the invention of the special theory of relativity. Lobatchewsky challenged the assumption that Euclid's parallel postulate or, what is equivalent, the hypothesis of the right angle, is necessary to a consistent geometry, and he backed his challenge by producing a system of geometry based on the hypothesis of the acute angle in which there is not *one* parallel through a fixed point to a given straight line but *two*. Neither of Lobatchewsky's parallels meets the line to which both are parallel, nor does any straight line drawn through the fixed point and lying within the angle formed by the two parallels. This apparently bizarre situation is 'realized' by the geodesics on a pseudosphere.



For any everyday purpose (measurements of distances, etc.), the differences between the geometries of Euclid and Lobatchewsky are too small to count, but this is not the point of importance: each is self-consistent and each is adequate for human experience. Lobatchewsky abolished the *necessary* 'truth' of Euclidean geometry. His geometry was but the first of several constructed by his successors. Some of these substitutes for Euclid's geometry – for instance the Riemannian geometry of general relativity – are to-day at least as important in the still living and growing parts of physical science as Euclid's was, and is, in the comparatively static and classical parts. For some purposes Euclid's geometry is best or at least sufficient, for others it is inadequate and a non-Euclidean geometry is demanded.

Euclid in some sense was believed for 2,200 years to have discovered an absolute truth or a necessary mode of human perception in his system of geometry. Lobatchewsky's creation was a pragmatic demonstration of the error of this belief. The boldness of his challenge and its successful outcome have inspired mathematicians and scientists in general to challenge other 'axioms' or accepted 'truths', for example the 'law' of causality, which, for centuries, have seemed as necessary to straight thinking as Euclid's postulate appeared till Lobatchewsky discarded it.

The full impact of the Lobatchewskian method of challenging axioms has probably yet to be felt. It is no exaggeration to call Lobatchewsky the Copernicus of Geometry, for geometry is only a part of the vaster domain which he renovated; it might even be just to designate him as a Copernicus of all thought.

CHAPTER SEVENTEEN

GENIUS AND POVERTY

Abel

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AN astrologer in the year 1801 might have read in the stars that a new galaxy of mathematical genius was about to blaze forth inaugurating the greatest century of mathematical history. In all that galaxy of talent there was no brighter star than Niels Henrik Abel, the man of whom Hermite said, 'He has left mathematicians something to keep them busy for five hundred years'.

Abel's father was the pastor of the little village of Findö, in the diocese of Kristiansand, Norway, where his second son, Niels Henrik, was born on 5 August 1802. On the father's side several ancestors had been prominent in the work of the church and all, including Abel's father, were men of culture. Anne Marie Simonsen, Abel's mother, was chiefly remarkable for her great beauty, love of pleasure, and general flightiness – quite an exciting combination for a pastor's helpmeet. From her Abel inherited his striking good looks and a very human desire to get something more than everlasting hard work out of life, a desire he was seldom able to gratify.

The pastor was blessed with seven children in all at a time when Norway was desperately poor as the result of wars with England and Sweden, to say nothing of a famine thrown in for good measure between wars. Nevertheless the family was a happy one. In spite of pinching poverty and occasional empty stomachs they kept their chins up. There is a charming picture of Abel after his mathematical genius had seized him sitting by the fireside with the others chattering and laughing in the room while he researched with one eye on his mathematics and the other on his brothers and sisters. The noise never distracted him and he joined in the badinage as he wrote.

Like several of the first-rank mathematicians Abel discovered his talent early. A brutal schoolmaster unwittingly threw opportunity Abel's way. Education in the first decades of the nineteenth century was virile, at least in Norway. Corporal punishment, as the simplest method of toughening the pupils' characters and gratifying the sadistic inclinations of the masterful pedagogues, was generously administered for every trivial offence. Abel was not awakened through his own skin, as Newton is said to have been by that thundering kick donated by a playmate, but by the sacrifice of a fellow student who had been flogged so unmercifully that he died. This was a bit too thick even for the rugged school board and they deprived the teacher of his job. A competent but by no means brilliant mathematician filled the vacancy, Bernt Michael Holmboë (1795–1850), who was later to edit the first edition of Abel's collected works in 1839.

Abel at the time was about fifteen. Up till now he had shown no marked talent for anything except taking his troubles with a sense of humour. Under the kindly, enlightened Holmboë's teaching Abel suddenly discovered what he was. At sixteen he began reading privately and thoroughly digesting the great works of his predecessors, including some of those of Newton, Euler, and Lagrange. Thereafter real mathematics was not only his serious occupation but his fascinating delight. Asked some years later how he had managed to forge ahead so rapidly to the front rank he replied, 'By studying the masters, not their pupils' – a prescription some popular writers of textbooks might do well to mention in their prefaces as an antidote to the poisonous mediocrity of their uninspired pedagogics.

Holmboë and Abel soon became close friends. Although the teacher was himself no creative mathematician he knew and appreciated the masterpieces of mathematics, and under his eager suggestions Abel was soon mastering the toughest of the classics, including the *Disquisitiones Arithmeticae* of Gauss.

To-day it is a commonplace that many fine things the old masters thought they had proved were not really proved at all. Particularly is this true of some of Euler's work on infinite series and some of Lagrange's on analysis. Abel's keen mind was one of the first to detect the gaps in his predecessors' reasoning, and he resolved to devote a fair share of his lifework to caulking the cracks and making the reasoning watertight. One of his classics in this direction is the first *proof* of the *general* binomial theorem, special cases of which had been stated by Newton and Euler. It is not easy to give a sound proof in the general case, so perhaps it is not astonishing to find alleged proofs still displayed in the schoolbooks as if Abel had never lived. This proof, however, was only a detail in Abel's vaster programme of cleaning up the theory and application of infinite series.

Abel's father died in 1820 at the age of forty-eight. At the time Abel was eighteen. The care of his mother and six children fell on his shoulders. Confident of himself Abel assumed his sudden responsibilities cheerfully. Abel was a genial and optimistic soul. With no more than strict justice he foresaw himself as an honoured and moderately prosperous mathematician in a university chair. Then he could provide for the lot of them in reasonable security. In the meantime he took private pupils and did what he could. In passing it may be noted that Abel was a very successful teacher. Had he been footloose poverty would never have bothered him. He could have earned enough for his own modest needs, somehow or other, at any time. But with seven on his back he had no chance. He never complained, but took it all in his stride as part of the day's work and kept at his mathematical researches in every spare moment.

Convinced that he had one of the greatest mathematicians of all time on his hands, Holmboë did what he could by getting subsidies for the young man and digging down generously into his own none too deep pocket. But the country was poor to the point of starvation and not nearly enough could be done. In those days of privation and incessant work Abel immortalized himself and sowed the seeds of the disease which was to kill him before he had half done his work.

Abel's first ambitious venture was an attack on the general equation of the fifth degree (the 'quintic'). All his great predecessors in algebra had exhausted their efforts to produce a solution, without success. We can easily imagine Abel's exultation when he mistakenly imagined he had succeeded. Through Holmboë the supposed solution was sent to the most learned mathematical scholar of the time in Denmark who, fortunately for Abel, asked for further particulars without committing himself to an opinion on the correctness of the solution. Abel in the meantime had found the flaw in his reasoning. The supposed solution was of course no solution at all. This failure gave him a most salutary jolt; it jarred him on to the right track and caused him to doubt whether an algebraic solution was possible. He *proved the impossibility*. At the time he was about nineteen. But he had been anticipated, at least in part, in the whole project.

As this question of the general quintic played a role in algebra similar to that of a crucial experiment to decide the fate of an entire scientific theory, it is worth a moment's attention. We shall quote presently a few things Abel himself says.

The nature of the problem is easily described. In early school algebra we learn to solve the *general* equations of the *first* and *second* degrees in the unknown x, say

$$ax + b = 0, ax^2 + bx + c = 0,$$

and a little later those of the *third* and *fourth* degrees, say

$$ax^3 + bx^2 + cx + d = 0, ax^4 + bx^3 + cx^2 + dx + e = 0.$$

That is, we produce *finite* (closed) formulae for each of these *general* equations of the first four degrees, expressing the unknown x in terms of the given coefficients a,b,c,d,e. A solution such as any one of these four which can be obtained by only a *finite number of additions, multiplications, subtractions, divisions, and extractions of roots*, all these operations being performed on the given coefficients, is called *algebraic*. The important qualification in this definition of an *algebraic solution* is 'finite'; there is no difficulty in describing solutions for *any* algebraic equation which contain no extractions of roots at all, but which do imply an *infinity* of the other operations named.

After this success with algebraic equations of the first four degrees, algebraists struggled for nearly three centuries to produce a similar *algebraic solution* for the general quintic

$$ax^5 + bx^4 + cx^3 + dx^2 + ex + f = 0.$$

They failed. It is here that Abel enters.

The following extracts are given partly to show how a great inventive mathematician thought and partly for their intrinsic interest. They are from Abel's memoir *On the algebraic resolution of equations.*

'One of the most interesting problems of algebra is that of the algebraic solution of equations. Thus we find that nearly all mathematicians of distinguished rank have treated this subject. We arrive without difficulty at the general expression of the roots of equations of the first four degrees. A uniform method for solving these equations was discovered and it was believed to be applicable to an equation of any degree; but in spite of all the efforts of Lagrange and other distinguished mathematicians the proposed end was not reached. That led to the presumption that the solution of general equations was impossible algebraically; but this is what could not be decided, since the method followed could lead to decisive conclusions only in the case where the equations were solvable. In effect they proposed to solve equations without knowing whether it was possible. In this way one might indeed arrive at a solution, although that was by no means certain; but if by ill luck the solution was impossible, one might seek it for an eternity, without finding it. To arrive infallibly at something in this matter, we must therefore follow another road. We can give the problem such a form that it shall always be possible to solve it, as we can always do with any problem.* Instead of asking for a relation of which it is not known whether it exists or not, we must ask whether such a relation is indeed possible. . . . When a problem is posed in this way, the very statement contains the germ of the solution and indicates what road must be taken; and I believe there

* '. . . *ce qu'on peut toujours faire d'un problème quelconque*' is what Abel says. This seems a trifle too optimistic; at least for ordinary mortals. How would the method be applied to Fermat's Last Theorem?

will be few instances where we shall fail to arrive at propositions of more or less importance, even when the complication of the calculations precludes a complete answer to the problem.'

He goes on to say that this, the true scientific method to be followed, has been but little used owing to the extreme complication of the calculations (algebraic) which it entails; 'but', he adds, 'in many instances this complication is only apparent and vanishes after the first attack.' He continues:

'I have treated several branches of analysis in this manner, and although I have often set myself problems beyond my powers, I have nevertheless arrived at a large number of general results which throw a strong light on the nature of those quantities whose elucidation is the object of mathematics. On another occasion I shall give the results at which I have arrived in these researches and the procedure which has led me to them. In the present memoir I shall treat the problem of the algebraic solution of equations in all its generality.'

Presently he states two general inter-related problems which he proposes to discuss:

'1. To find all the equations of any given degree which are solvable algebraically.

2. To determine whether a given equation is or is not solvable algebraically.'

At bottom, he says, these two problems are the same, and although he does not claim a *complete* solution, he does *indicate* an infallible method (*des moyens sûrs*) for disposing of them fully.

Abel's irrepressible inventiveness hurried him on to vaster problems before he had time to return to these; their complete solution – the explicit statement of necessary and sufficient conditions that an algebraic equation be solvable algebraically – was to be reserved for Galois. When this memoir of Abel's was published in 1828, Galois was a boy of sixteen, already well started on his career of fundamental discovery. Galois later came to know and admire the work of Abel; it is probable that Abel never heard the name of Galois, although when Abel visited Paris he and his brilliant successor could have been only a few miles apart. But for the stupidity of Galois' teachers and the loftiness of some of Abel's mathematical 'superiors', it is quite possible that he and Abel might have met.

Epoch-making as Abel's work in algebra was, it is overshadowed by his creation of a new branch of analysis. This, as Legendre said, is Abel's 'time-outlasting monument'. If the story of his life adds nothing to the splendour of his accomplishment it at least suggests what the world lost when he died. It is a somewhat discouraging tale. Only Abel's unconquerable cheerfulness and unyielding courage under the stress of poverty and lack of encouragement from the mathematical princes of his day lighten the story. He did, however, find one generous friend in addition to Holmboë.

In June 1822 when Abel was nineteen, he completed his required work at the University of Kristiania. Holmboë had done everything possible to relieve the young man's poverty, convincing his colleagues that they too should subscribe to make it possible for Abel to continue his mathematical researches. They were immensely proud of him but they were also poor themselves. Abel quickly outgrew Scandinavia. He longed to visit France, then the mathematical queen of the world, where he could meet his great peers (he was in a class far above some of them, but he did not know it). He dreamed also of touring Germany and meeting Gauss, the undisputed prince of them all.

Abel's mathematical and astronomical friends persuaded the University to appeal to the Norwegian Government to subsidize the young man for a grand mathematical tour of Europe. To impress the authorities with his worthiness, Abel submitted an extensive memoir which, from its title, was probably connected with the fields of his greatest fame. He himself thought highly enough of it to believe its publication by the University would bring Norway honour, and Abel's opinion of his own work, never more than just, was probably as good as anyone's. Unfortunately the University was having a severe financial struggle of its own, and the memoir was finally lost. After undue deliberation the Government compromised – does any Government ever do anything else? – and instead of doing the only sensible thing, namely sending Abel at once to France and Germany, granted him a subsidy to continue his university studies at Kristiania in order that he might brush up his French and German. That is exactly the sort of decision he might have expected from any body of officials conspicuous for their good hearts and common sense. Common sense, however, has no business dictating to genius.

Abel dallied a year and a half at Kristiania, not wasting his time, but dutifully keeping his part of the contract by wrestling (not too successfully) with German, getting a fair start on French, and working incessantly at his mathematics. With his incurable optimism he had also got himself engaged to a young woman – Crelly Kemp. At last, on 27 August 1825, when Abel was twenty-three, his friends overcame the last objection of the Government, and a royal decree granted him sufficient funds for a year's travel and study in France and Germany. They did not give him much, but the fact that they gave him anything at all in the straitened financial condition of the country says more for the state of civilization in Norway in 1825 than could a whole encyclopaedia of the arts and trades. Abel was grateful. It took him about a month to straighten out his dependents before leaving. But thirteen months before this, innocently believing that all mathematicians were as generous-minded as himself, he had burned one of his ladders before ever setting foot on it.

Out of his own pocket – God only knows how – Abel had paid for the printing of his memoir in which the impossibility of solving the general equation of the fifth degree algebraically is proved. It was a pretty poor job of printing but the best backward Norway could manage. This, Abel naïvely believed, was to be his scientific passport to the great mathematicians of the Continent. Gauss in particular, he hoped, would recognize the signal merits of the work and grant him more than a formal interview. He could not know that 'the prince of mathematicians' sometimes exhibited anything but a princely generosity to young mathematicians struggling for just recognition.

Gauss duly received the paper. Through unimpeachable witnesses Abel heard how Gauss welcomed the offering. Without deigning to read it he tossed it aside with the disgusted exclamation 'Here is another of those monstrosities!' Abel decided not to call on Gauss. Thereafter he disliked Gauss intensely and nicked him whenever he could. He said Gauss wrote obscurely and hinted that the Germans thought a little too much of him. It is an open question whether Gauss or Abel lost more by this perfectly understandable dislike.

Gauss has often been censured for his 'haughty contempt' in this matter, but those are hardly the right words to describe his conduct. The problem of the general equation of the fifth degree had become notorious. Cranks as well as reputable mathematicians had been burrowing into it. Now, if a mathematician to-day receives an alleged squaring of the circle, he may or may not write a courteous note of acknowledgement to the author, but he is almost certain to file the author's manuscript in the waste-basket. For he knows that Lindemann in 1882 proved that it is impossible to square the circle by straight-edge and compass alone – the implements to which cranks limit themselves, just as Euclid did. He knows also that Lindemann's proof is accessible to anyone. In 1824 the problem of the general quintic was almost on a par with that of squaring the circle. Hence Gauss' impatience. But it was not quite as bad; the impossibility had not yet been proved. Abel's paper supplied the proof; Gauss might have read something to interest him intensely had he kept his temper. It is a tragedy that he did not. A word from him and Abel would have been made. It is even possible that his life would have been lengthened, as we shall admit when we have his whole story before us.

After leaving home in September 1825, Abel first visited the notable mathematicians and astronomers of Norway and Denmark and then, instead of hurrying to Göttingen to meet Gauss as he had intended, proceeded to Berlin. There he had the great good fortune to fall in with a man, August Leopold Crelle (1780–1856) who was to be a scientific Holmboë to him and who had far more weight in the mathematical world than the good Holmboë ever had. If Crelle helped to make Abel's reputation, Abel more than paid for the help by making Crelle's. Wherever mathematics is cultivated to-day the name of Crelle is a household word, indeed more; for 'Crelle' has become a proper noun signifying the great journal he founded, the first three volumes of which contained twenty-two of Abel's memoirs. The journal made Abel, or at least made him more widely known to Continental mathematicians than he could ever have been without it; Abel's great work started the journal off with a bang that was heard round the mathematical world; and finally the journal made Crelle. This self-effacing amateur of mathematics deserves more than a passing mention. His business ability and his sure instinct for picking collaborators who had real mathematics in them did more for the progress of mathematics in the nineteenth century than half a dozen learned academies.

Crelle himself was a self-taught lover of mathematics rather than a creative mathematician. By profession he was a civil engineer. He early rose to the top in his work, built the first railroad in Germany, and made a comfortable stake. In his leisure he pursued mathematics as something more than a hobby. He himself contributed to mathematical research before and after the great stimulus to German mathematics which his *Journal für die reine und angewandte Mathematik* (Journal for pure and applied Mathematics) gave on its foundation in 1826. This is Crelle's greatest contribution to the advancement of mathematics.

The Journal was the first periodical in the world devoted exclusively to mathematical *research*. Expositions of old work were not welcomed. Papers (except some of Crelle's own) were accepted from anyone, provided only the matter was new, true, and of sufficient 'importance' – an intangible requirement – to merit publication. Regularly once every three months from 1826 to the present day 'Crelle' has appeared with its sheaf of new mathematics. In the chaos after the World War 'Crelle' tottered and almost went down, but was sustained by subscribers from all over the world who were unwilling to see this great monument to a more tranquil civilization than our own obliterated. To-day hundreds of periodicals are devoted either wholly or in considerable part to the advancement of pure and applied mathematics. How many of them will survive our next outburst of epidemic insanity is anybody's guess.

When Abel arrived in Berlin in 1825 Crelle had just about made up his mind to start his great venture with his own funds. Abel played a part in clinching the decision. There are two accounts of the first meeting of Abel and Crelle, both interesting. Crelle at the time was holding down a government job for which he had but little aptitude and less liking, that of examiner at the Trade-School (*Gewerbe-Institut*) in Berlin. At third-hand (Crelle to Weierstrass to Mittag-Leffler) Crelle's account of that historic meeting is as follows.

'One fine day a fair young man, much embarrassed, with a very youthful and very intelligent face, walked into my room. Believing that I had to do with an examination-candidate for admission to the Trade-School, I explained that several separate examinations would be necessary. At last the young man opened his mouth and explained [in poor German], "Not examination, only mathematics".'

Crelle saw that Abel was a foreigner and tried him in French, in which Abel could make himself understood with some difficulty. Crelle then questioned him about what he had done in mathematics. Diplomatically enough Abel replied that he had read, among other things, Crelle's own paper of 1823, then recently published, on 'analytical faculties' (now called 'factorials' in English). He had found the work most interesting he said, but –. Then, not so diplomatically, he proceeded to tell Crelle that parts of the work were quite wrong. It was here that Crelle showed his greatness. Instead of freezing or blowing up in a rage at the daring presumption of the young man before him, he pricked up his ears and asked for particulars, which he followed with the closest attention. They had a long mathematical talk, only parts of which were intelligible to Crelle. But whether he understood all that Abel told him or not, Crelle saw clearly what Abel was. Crelle never did understand a tenth of what Abel was up to, but his sure instinct for mathematical genius told him that Abel was a mathematician of the first water and he did everything in his power to gain recognition for his young protégé. Before the interview was ended Crelle had made up his mind that Abel must be one of the first contributors to the projected *Journal*.

Abel's account differs, but not essentially. Reading between the lines we may see that the differences are due to Abel's modesty. At first Abel feared his project of interesting Crelle was fated to go on the rocks. Crelle could not make out what the young man wanted, who he was, or anything about him. But at Crelle's question as to what Abel had read in mathematics things brightened up considerably. When Abel mentioned the works of the masters he had studied Crelle became instantly alert. They had a long talk on several outstanding unsettled problems, and Abel ventured to spring his proof of the impossibility of solving the general quintic algebraically on the unsuspecting Crelle. Crelle wouldn't hear of it; there must be something wrong with any such proof. But he accepted a copy of the paper, thumbed through it, admitted the reasoning was beyond him – and finally published Abel's amplified proof in his *Journal*. Although he was a limited mathematician with no pretensions to scientific greatness, Crelle was a broad-minded man, in fact, a great man.

Crelle took Abel everywhere, showing him off as the finest mathematical discovery yet made. The self-taught Swiss Steiner – 'the greatest geometer since Apollonius' – sometimes accompanied Crelle and Abel on their rounds. When Crelle's friends saw him coming with his two geniuses in tow they would exclaim 'Here comes Father Adam again with Cain and Abel.'

The generous sociability of Berlin began to distract Abel from his work and he fled to Freiburg where he could concentrate. It was at Freiburg that he hewed his greatest work into shape, the creation of what is now called Abel's Theorem. But he had to be getting on to Paris to meet the foremost French mathematicians of the day – Legendre, Cauchy, and the rest.

It can be said at once that Abel's reception at the hands of the French mathematicians was as civil as one would expect from distinguished representatives of a very civil people in a very civil age. They were all very civil to him – damned civil, in fact, and that was about all that Abel got out of the visit to which he had looked forward with such ardent hopes. Of course they did not know who or what he was. They made only perfunctory efforts to find out. If Abel opened his mouth – when he got within talking distance of them – about his own work, they immediately began lecturing about their own greatness. But for his indifference the venerable Legendre might have learned something about his own lifelong passion (for elliptic integrals) which would have interested him beyond measure. But he was just stepping into his carriage when Abel called and had time for little more than a very civil good-day. Later he made handsome amends.

Late in July 1826 Abel took up his lodgings in Paris with a poor but grasping family who gave him two bad meals a day and a vile room for a sufficiently outrageous rent. After four months of Paris Abel writes his impressions to Holmboë:

Paris, 24 October 1862.

To tell you the truth this noisiest capital of the Continent has for the moment the effect of a desert on me. I know practically nobody; this is the lovely season when everybody is in the country. ... Up till now I have made the acquaintance of Mr *Legendre*, Mr *Cauchy* and Mr *Hachette*, and some less celebrated but very able mathematicians: Mr *Saigey*, editor of the Bulletin des Sciences, and Mr *Lejeune-Dirichlet*, a Prussian who came to see me the other day believing me to be a compatriot of his. He is a mathematician of great penetration. With Mr *Legendre* he has proved the impossibility of solving $x^5 + y^5 = z^5$ in whole numbers, and other very fine things. *Legendre* is extremely polite, but unfortunately very old. *Cauchy* is mad. ... What he does is excellent, but very muddled. At first I understood practically none of it; now I see some of it more clearly. ... *Cauchy* is the only one occupied with pure mathematics. *Poisson*, *Fourier*, *Ampère*, etc., busy themselves exclusively with magnetism and other physical subjects. Mr *Laplace* writes nothing now, I believe. His last work was a supplement to his Theory of Probabilities. I have often seen him at the Institut. He is a very jolly little chap. *Poisson* is a little fellow; he knows how to behave with a great deal of dignity; Mr *Fourier* the same. *Lacroix* is quite old. Mr *Hachette* is going to present me to several of these men.

The French are much more reserved with strangers than the Germans. It is extremely difficult to gain their intimacy, and I do not dare to urge my pretensions as far as

> that; finally every beginner has a great deal of difficulty in getting noticed here. I have just finished an extensive treatise on a certain class of transcendental functions [his masterpiece] to present it to the Institut [Academy of Sciences], which will be done next Monday. I showed it to Mr *Cauchy*, but he scarcely deigned to glance at it. And I dare to say, without bragging, that it is a good piece of work. I am curious to hear the opinion of the Institut on it. I shall not fail to share it with you. ...

He then tells what he is doing and continues with a rather disturbed forecast of his prospects. 'I regret having set two years for my travels, a year and a half would have sufficed.' He has got all there is to be got out of Continental Europe and is anxious to be able to devote his time to working up what he has invented.

> So many things remain for me to do, but so long as I am abroad, all that goes badly enough. If I had my professorship as Mr *Kielhau* has his! My position is not assured, it is true, but I am not uneasy about it; if fortune deserts me in one quarter perhaps she will smile on me in another.

From a letter of earlier date to the astronomer Hansteen we take two extracts, the first relating to Abel's great project of re-establishing mathematical analysis as it existed in his day on a firm foundation, the second showing something of his human side. (Both are free translations.)

> In the higher analysis too few propositions are proved with conclusive rigour. Everywhere we find the unfortunate procedure of reasoning from the special to the general, and the miracle is that after such a process it is only seldom that we find what are called paradoxes. It is indeed exceedingly interesting to seek the reason for this. This reason, in my opinion, resides in the fact that the functions which have hitherto occurred in analysis can be expressed for the most part as powers. ... When we proceed by a general method, it is not too difficult [to avoid pitfalls]; but I have had to be very circumspect, because propositions without rigorous proof (i.e. without any proof) have taken root in me to such an extent that I constantly run the risk of using them without further examination. These trifles will appear in the journal published by Mr *Crelle*.

Immediately following this he expresses his gratitude for his treatment in Berlin. 'It is true that few persons are interested in me, but these few are infinitely dear to me, because they have shown me so much kindness. Perhaps I can respond in some way to their hopes of me, for it must be hard for a benefactor to see his trouble lost.'

He tells then how Crelle has been begging him to take up his residence permanently in Berlin. Crelle was already using all his human engineering skill to hoist the Norwegian Abel into a professorship in the University of Berlin. Such was the Germany of 1826. Abel of course was already great, and the sure promise of what he had in him indicated him as the likeliest mathematical successor to Gauss. That he was a foreigner made no difference; Berlin in 1826 wanted the best in mathematics. A century later the best in mathematical physics was not good enough, and Berlin quite forcibly got rid of Einstein. Thus do we progress. But to return to the sanguine Abel.

> At first I counted on going directly from Berlin to Paris, happy in the promise that Mr *Crelle* would accompany me. But Mr *Crelle* was prevented, and I shall have to travel alone. Now I am so constituted that I cannot endure solitude. Alone, I am depressed, I get cantankerous, and I have little inclination for work. So I said to myself it would be much better to go with Mr *Boeck* to Vienna, and this trip seems to me to be justified by the fact that at Vienna there are men like *Littrow*, *Burg*, and still others, all indeed excellent mathematicians; add to this that I shall make but this one voyage in my life. Could one find anything but reasonableness in this wish of mine to see some of the life of the South? I could work assiduously enough while travelling. Once in Vienna and leaving there for Paris, it is almost a bee-line via Switzerland. Why shouldn't I see a little of it too? My God! I, even I, have some taste for the beauties of nature, like everybody else. This whole trip would bring me to Paris two months later, that's all. I could quickly catch up the time lost. Don't you think such a trip would do me good?

So Abel went South, leaving his masterpiece in Cauchy's care to be presented to the Institut. The prolific Cauchy was so busy laying eggs of his own and cackling about them that he had no time to examine the veritable roc's egg which the modest Abel had deposited in the nest. Hachette, a mere pot-washer of a mathematician, presented Abel's *Memoir on a general property of a very extensive class of transcendental functions* to the Paris Academy of Sciences on 10 October 1826. This is the work which Legendre later described in the words of Horace as '*monumentum aere perennius*', and the 500 years' work which Hermite said Abel had laid out for future generations of mathematicians. It is one of the crowning achievements of modern mathematics.

What happened to it? Legendre and Cauchy were appointed as referees. Legendre was seventy-four, Cauchy thirty-nine. The veteran was losing his edge, the captain was in his self-centred prime. Legendre complained (letter to Jacobi, 9 April 1829) that 'we perceived that the memoir was barely legible; it was written in ink almost white, the letters badly formed; it was agreed between us that the author should be asked for a neater copy to be read.' What an alibi! Cauchy took the memoir home, mislaid it, and forgot all about it.

To match this phenomenal feat of forgetfulness we have to imagine an Egyptologist mislaying the Rosetta Stone. Only by a sort of miracle was the memoir unearthed after Abel's death. Jacobi heard of it from Legendre, with whom Abel corresponded after returning to Norway, and in a letter dated 14 March 1829 Jacobi exclaims, 'What a discovery is this of Mr Abel's! ... Did anyone ever see the like? But how comes it that this discovery, perhaps the most important mathematical discovery that has been made in our Century, having been communicated to your Academy two years ago, has escaped the attention of your colleagues?' The enquiry reached Norway. To make a long story short, the Norwegian consul at Paris raised a diplomatic row about the missing manuscript and Cauchy dug it up in 1830. Finally it was printed, but not till 1841, in the *Mémoires présentés par divers savants à l'Académie royale des sciences de l'Institut de France*, vol. 7, pp. 176–264. To crown this epic *in parvo* of crass incompetence, the editor, or the printers, or both between them, succeeded in losing the manuscript before the proof-sheets were read.* The Academy (in 1830) made amends to Abel by awarding him the Grand Prize in Mathematics jointly with Jacobi. Abel, however, was dead.

The opening paragraphs of the memoir indicate its scope.

> The transcendental functions hitherto considered by mathematicians are very few in number. Practically the entire theory of transcendental functions is reduced to that of logarithmic functions, circular and exponential functions, functions which, at bottom, form but a single species. It is only recently that some other functions have begun to be considered. Among the latter, the elliptic transcendents, several of whose remarkable and elegant properties have been developed by Mr Legendre, hold the first place. The author [Abel] has considered, in the memoir which he has the honour to present to the Academy, a very extended class of functions, namely: all those whose derivatives are expressible by means of algebraic equations whose coefficients are rational functions of one variable, and he has proved for these functions properties analogous to those of logarithmic and elliptic functions ... and he has arrived at the following theorem:
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> If we have several functions whose derivatives can be roots of *one and the same algebraic equation*, all of whose coefficients are *rational* functions of one variable, we can always express the sum of any number of such functions by an *algebraic* and *logarithmic* function, provided that we establish a certain number of *algebraic* relations between the variables of the functions in question.
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> The number of these relations does not depend at all upon the number of functions, but only upon the nature of the particular functions considered. ...

* Libri, a *soi-disant* mathematician, who saw the work through the press, adds, 'by permission of the Academy', a smug footnote acknowledging the genius of the lamented Abel. This is the last straw; the Academy might have come out with all the facts or have held its official tongue. But at all costs the honour and dignity of a stuffed shirt must be upheld. Finally it may be recalled that valuable manuscripts and books had an unaccountable trick of vanishing when Libri was round.

The theorem which Abel thus briefly describes is to-day known as Abel's Theorem. His proof of it has been described as nothing more than 'a marvellous exercise in the integral calculus'. As in his algebra, so in his analysis, Abel attained his proof with a superb parsimony. The proof, it may be said without exaggeration, is well within the purview of any seventeen-year-old who has been through a good *first* course in the calculus. There is nothing high-falutin' about the classic simplicity of Abel's own proof. The like cannot be said for some of the nineteenth-century expansions and geometrical reworkings of the original proof. Abel's proof is like a statue by Phidias; some of the others resemble a Gothic cathedral smothered in Irish lace, Italian confetti, and French pastry.

There is ground for a possible misunderstanding in Abel's opening paragraph. Abel no doubt was merely being kindly courteous to an old man who had patronized him – in the bad sense – on first acquaintance, but who, nevertheless, had spent most of his long working life on an important problem without seeing what it was all about. It is not true that Legendre had discussed the elliptic *functions*, as Abel's words might imply; what Legendre spent most of his life over was elliptic *integrals* which are as different from elliptic *functions* as a horse is from the cart it pulls, and therein precisely is the crux and the germ of one of Abel's greatest contributions to mathematics. The matter is quite simple to anyone who has had a school course in trigonometry; to obviate tedious explanations of elementary matters this much will be assumed in what follows presently.

For those who have forgotten all about trigonometry, however, the essence, the *methodology*, of Abel's epochal advance can be analogized thus. We alluded to the cart and the horse. The frowsy proverb about putting the cart before the horse describes what Legendre did; Abel saw that if the cart was to move forward the horse should precede it. To take another instance: Francis Galton, in his statistical studies of the relation between poverty and chronic drunkenness, was led, by his impartial mind, to a reconsideration of all the self-righteous platitudes by which indignant moralists and economic crusaders with an axe to grind evaluate such social phenomena. Instead of assuming that people are depraved *because* they drink to excess, Galton *inverted* this hypothesis and assumed temporarily that people drink to excess *because* they have inherited no moral guts from their ancestors, in short, *because* they *are* depraved. Brushing aside all the vaporous moralizing of the reformers, Galton took a firm grip on a *scientific*, unemotional, *workable* hypothesis to which he could apply the impartial machinery of mathematics. His work has not yet registered socially. For the moment we need note only that Galton, like Abel, *inverted* his problem – turned it upside-down and inside-out, back-end-to and foremost-end-backward. Like Hiawatha and his fabulous mittens, Galton put the skinside inside and the inside outside.

All this is far from being obvious or a triviality. It is one of the most powerful methods of mathematical discovery (or invention) ever devised, and Abel was the first human being to use it consciously as an engine of research. 'You must always invert', as Jacobi said when asked the secret of his mathematical discoveries. He was recalling what Abel and he had done. If the solution of a problem becomes hopelessly involved, try turning the problem backwards, put the quaesita for the data and *vice versa*. Thus if we find Cardan's character incomprehensible when we think of him as *a* son of his father, shift the emphasis, *invert* it, and see what we get when we analyse Cardan's father as *the* begetter and endower of his son. Instead of studying 'inheritance' concentrate on 'endowing'. To return to those who remember some trigonometry.

Suppose mathematicians had been so blind as not to see that $\sin x$, $\cos x$ and the other *direct* trigonometric functions are simpler to use, in the addition formulae and elsewhere, than the *inverse* functions $\sin^{-1} x$, $\cos^{-1} x$. Recall the formula $\sin (x + y)$ in terms of sines and cosines of x and y, and contrast it with the formula for $\sin^{-1} (x + y)$ in terms of x and y. Is not the former incomparably simpler, more elegant, more 'natural' than the latter? Now, in the integral calculus, the *inverse* trigonometric functions present themselves naturally as definite integrals of simple algebraic irrationalities (second degree); such integrals appear when we seek to find the length of an arc of a circle by means of the integral calculus. Suppose the *inverse* trigonometric functions had *first* presented themselves this way. Would it not have been 'more natural' to consider the *inverses* of these functions, that is, the familiar trigonometric functions themselves as the *given* functions to be studied and analysed? Undoubtedly; but in shoals of more advanced problems, the simplest of which is that of finding the length of the arc of an *ellipse* by the integral calculus, the awkward *inverse* 'elliptic' (not 'circular', as for the arc of a circle) functions presented themselves *first*. It took Abel to see that *these* functions should be 'inverted' and studied, precisely as in the case of sin x, cos x instead of $\sin^{-1} x$, $\cos^{-1} x$. Simple, was it not? Yet Legendre, a great mathematician, spent more than *forty years* over his 'elliptic integrals' (the awkward 'inverse functions' of his problem) without ever once suspecting that he should *invert*.* This extremely simple, uncommonsensical way of looking at an apparently simple but profoundly recondite problem was one of the greatest mathematical advances of the nineteenth century.

All this however was but the beginning, although a sufficiently tremendous beginning – like Kipling's dawn coming up like thunder – of what Abel did in his magnificent theorem and in his work on elliptic functions. The trigonometric or circular functions have a single real period, thus $\sin (x + 2\pi) = \sin x$, etc. Abel discovered that his new functions provided by the inversion of an elliptic integral have precisely *two* periods, whose ratio is imaginary. After that, Abel's followers in this direction – Jacobi, Rosenhain, Weierstrass, Riemann, and many more – mined deeply into Abel's great theorem and by carrying on and extending his ideas discovered functions of n variables having $2n$ periods. Abel himself carried the exploitation of his discoveries far. His successors have applied all this work to geometry, mechanics, parts of mathematical physics, and other tracts of mathematics, solving important problems

* In ascribing priority to Abel, rather than 'joint discovery' to Abel and Jacobi, in this matter, I have followed Mittag-Leffler. From a thorough acquaintance with all the published evidence, I am convinced that Abel's claim is indisputable, although Jacobi's compatriots argue otherwise.

which, without this work initiated by Abel, would have been unsolvable.

While in Paris Abel consulted good physicians for what he thought was merely a persistent cold. He was told that he had tuberculosis of the lungs. He refused to believe it, wiped the mud of Paris off his boots, and returned to Berlin for a short visit. His funds were running low; about seven dollars was the extent of his fortune. An urgent letter brought a loan from Holmboë after some delay. It must not be supposed that Abel was a chronic borrower on no prospects. He had good reason for believing that he should have a paying job when he got home. Moreover, money was still owed to him. On Holmboë's loan of about sixty dollars Abel existed and researched from March till May 1827. Then, all his resources exhausted, he turned homeward and arrived in Kristiania completely destitute.

But all was soon to be rosy, he hoped. Surely the University job would be forthcoming now. His genius had begun to be recognized. There was a vacancy. Abel did not get it. Holmboë reluctantly took the vacant chair which he had intended Abel to fill only after the governing board threatened to import a foreigner if Holmboë did not take it. Holmboë was in no way to blame. It was assumed that Holmboë would be a better teacher than Abel, although Abel had amply demonstrated his ability to teach. Anyone familiar with the current American pedagogical theory, fostered by professional Schools of Education, that the less a man knows about what he is to teach the better he will teach it, will understand the situation perfectly.

Nevertheless things did brighten up. The University paid Abel the balance of what it owed on his travel money and Holmboë sent pupils his way. The professor of astronomy took a leave of absence and suggested that Abel be employed to carry part of his work. A well-to-do couple, the Schjeldrups, took him in and treated him as if he were their own son. But with all this he could not free himself of the burden of his dependents. To the last they clung to him, leaving him practically nothing for himself, and to the last he never uttered an impatient word.

By the middle of January 1829 Abel knew that he had not long to live. The evidence of a haemorrhage is not to be denied. 'I will fight for my life!' he shouted in his delirium. But in more tranquil moments, exhausted and trying to work, he drooped 'like a sick eagle looking at the sun', knowing that his weeks were numbered.

Abel spent his last days at Froland, in the home of an English family where his fiancée (Crelly Kemp) was governess. His last thoughts were for her future, and he wrote to his friend Kielhau, 'She is not beautiful; she has red hair and freckles, but she is an admirable woman.' It was Abel's wish that Crelly and Kielhau should marry after his death; and although the two had never met, they did as Abel had half-jokingly proposed. Toward the last Crelly insisted on taking care of Abel without help, 'to possess these last moments alone'. Early in the morning of 6 April 1829 he died, aged twenty-six years, eight months.

Two days after Abel's death Crelle wrote to say that his negotiations had at last proved successful and that Abel would be appointed to the professorship of mathematics in the University of Berlin.

CHAPTER EIGHTEEN

THE GREAT ALGORIST

Jacobi

*

THE name Jacobi appears frequently in the sciences, not always meaning the same man. In the 1840's one very notorious Jacobi – M. H. – had a comparatively obscure brother, C. G. J., whose reputation then was but a tithe of M. H.'s. To-day the situation is reversed: C. G. J. is immortal – or seemingly so, while M. H. is rapidly receding into the obscurity of limbo. M. H. achieved fame as the founder of the fashionable quackery of galvanoplastics; C. G. J.'s much narrower but also much higher reputation is based on mathematics. During his lifetime the mathematician was always being confused with his more famous brother, or worse, being congratulated for his involuntary kinship to the sincerely deluded quack. At last C. G. J. could stand it no longer. 'Pardon me, beautiful lady', he retorted to an enthusiastic admirer of M. H. who had complimented him on having so distinguished a brother, 'but *I* am my brother.' On other occasions C. G. J. would blurt out, 'I am not *his* brother, he is *mine*'. There is where fame has left the relationship to-day.

Carl Gustav Jacob Jacobi, born at Potsdam, Prussia, Germany, on 10 December 1804 was the second son of a prosperous banker, Simon Jacobi, and his wife (family name Lehmann). There were in all four children, three boys, Moritz, Carl, and Eduard, and a girl, Therese. Carl's first teacher was one of his maternal uncles, who taught the boy classics and mathematics, preparing him to enter the Potsdam Gymnasium in 1816 in his twelfth year. From the first Jacobi gave evidence of the 'universal mind' which the rector of the Gymnasium declared him to be on his leaving the school in 1821 to enter the University of Berlin. Like Gauss, Jacobi could easily have made a high reputation in philology had not mathematics attracted him more strongly. Having seen that the boy had mathematical genius, the teacher (Heinrich Bauer) let Jacobi work by himself – after a prolonged tussle in which Jacobi rebelled at learning mathematics by rote and by rule.

Young Jacobi's mathematical development was in some respects curiously parallel to that of his greater rival Abel. Jacobi also went to the masters; the works of Euler and Lagrange taught him algebra and the calculus, and introduced him to the theory of numbers. This earliest self-instruction was to give Jacobi's first outstanding work – in elliptic functions – its definite direction, for Euler, the master of ingenious devices, found in Jacobi his brilliant successor. For sheer manipulative ability in tangled algebra Euler and Jacobi have had no rival, unless it be the Indian mathematical genius, Srinivasa Ramanujan, in our own century. Abel also could handle formulae like a master when he wished, but his genius was more philosophical, less formal than Jacobi's. Abel is closer to Gauss in his insistence upon rigour than Jacobi was by nature – not that Jacobi's work lacked rigour, for it did not, but its inspiration appears to have been formalistic rather than rigoristic.

Abel was two years older than Jacobi. Unaware that Abel had attacked the general quintic in 1820, Jacobi in the same year attempted a solution, reducing the general quintic to the form $x^5 - 10q^2x = p$ and showing that the solution of this equation would follow from that of a certain equation of the tenth degree. Although the attempt was abortive it taught Jacobi a great deal of algebra and he ascribed considerable importance to it as a step in his mathematical education. But it does not seem to have occurred to him, as it did to Abel, that the general quintic might be unsolvable algebraically. This oversight, or lack of imagination, or whatever we wish to call it, on Jacobi's part is typical of the difference between him and Abel. Jacobi, who had a magnificently objective mind and not a particle of envy or jealousy in his generous nature, himself said of one of Abel's masterpieces, 'It is above my praises as it is above my own works.'

Jacobi's student days at Berlin lasted from April 1821 to

May 1825. During the first two years he spent his time about equally between philosophy, philology, and mathematics. In the philological seminar Jacobi attracted the favourable attention of P. A. Boeckh, a renowned classical scholar who brought out (among other works) a fine edition of Pindar. Boeckh, luckily for mathematics, failed to convert his most promising pupil to classical studies as a life interest. In mathematics not much was offered for an ambitious student and Jacobi continued his private study of the masters. The university lectures in mathematics he characterized briefly and sufficiently as twaddle. Jacobi was usually blunt and to the point, although he knew how to be as subservient as any courtier when trying to insinuate some deserving mathematical friend into a worthy position.

While Jacobi was diligently making a mathematician of himself Abel was already well started on the very road which was to lead Jacobi to fame. Abel had written to Holmboë on 4 August 1823 that he was busy with elliptic functions: 'This little work, you will recall, deals with the inverses of the elliptic transcendents, and I proved something [that seemed] impossible; I begged Degen to read it as soon as he could from one end to the other, but he could find no false conclusion, nor understand where the mistake was; God knows how I shall get myself out of it.' By a curious coincidence Jacobi at last made up his mind to put his all on mathematics almost exactly when Abel wrote this. Two years' difference in the ages of young men around twenty (Abel was twenty-one, Jacobi nineteen) count for more than two decades of maturity. Abel got a tremendous start but Jacobi, unaware that he had a competitor in the race, soon caught up. Jacobi's first great work was in Abel's field of elliptic functions. Before considering this we shall outline his busy life.

Having decided to go into mathematics for all he was worth, Jacobi wrote to his uncle Lehmann his estimate of the labour he had undertaken. 'The huge colossus which the works of Euler, Lagrange, and Laplace have raised demands the most prodigious force and exertion of thought if one is to penetrate into its inner nature and not merely rummage about on its surface. To dominate this colossus and not to fear being crushed by it demands a strain which permits neither rest nor peace till one stands on top of it and surveys the work in its entirety. Then only, when one has comprehended its spirit, is it possible to work justly and in peace at the completion of its details.'

With this declaration of willing servitude Jacobi forthwith became one of the most terrific workers in the history of mathematics. To a timid friend who complained that scientific research is exacting and likely to impair bodily health, Jacobi retorted:

'Of course! Certainly I have sometimes endangered my health by overwork, but what of it? Only cabbages have no nerves, no worries. And what do they get out of their perfect well-being?'

In August 1825 Jacobi received his Ph.D. degree for a dissertation on partial fractions and allied topics. There is no need to explain the nature of this – it is not of any great interest and is now a detail in the second course of algebra or the integral calculus. Although Jacobi handled the general case of his problem and showed considerable ingenuity in manipulating formulae, it cannot be said that the dissertation exhibited any marked originality or gave any definite hint of the author's superb talent. Concurrently with his examination for the Ph.D. degree, Jacobi rounded off his training for the teaching profession.

After his degree Jacobi lectured at the University of Berlin on the applications of the calculus to curved surfaces and twisted curves (roughly, curves determined by the intersections of surfaces). From the very first lectures it was evident that Jacobi was a born teacher. Later, when he began developing his own ideas at an amazing speed, he became the most inspiring mathematical teacher of his time.

Jacobi seems to have been the first regular mathematical instructor in a university to train students in research by lecturing on his own latest discoveries and letting the students see the creation of a new subject taking place before them. He believed in pitching young men into the icy water to learn to swim or drown by themselves. Many students put off attempting anything on their own account till they have mastered everything relating to their problem that has been done by others. The result is that but few ever acquire the knack of independent work. Jacobi combated this dilatory erudition. To drive home the point to a gifted but diffident young man who was always putting off doing anything until he had learned something more, Jacobi delivered himself of the following parable. 'Your father would never have married, and you wouldn't be here now, if he had insisted on knowing *all* the girls in the world before marrying *one*.'

Jacobi's entire life was spent in teaching and research except for one ghastly interlude, to be related, and occasional trips to attend scientific meetings in England and on the Continent, or forced vacations to recuperate after too intensive work. The chronology of his life is not very exciting – a professional scientist's seldom is, except to himself.

Jacobi's talents as a teacher secured him the position of lecturer at the University of Königsberg in 1826 after only half a year in a similar position at Berlin. A year later some results which Jacobi had published in the theory of numbers (relating to cubic reciprocity; see chapter on Gauss) excited Gauss' admiration. As Gauss was not an easy man to stir up, the Ministry of Education took prompt notice and promoted Jacobi over the heads of his colleagues to an assistant professorship – quite a step for a young man of twenty-three. Naturally the men he had stepped over resented the promotion; but two years later (1829) when Jacobi published his first masterpiece, *Fundamenta Nova Theoriae Functionum Ellipticarum* (New Foundations of the Theory of Elliptic Functions) they were the first to say that no more than justice had been done and to congratulate their brilliant young colleague.

In 1832 Jacobi's father died. Up till this he need not have worked for a living. His prosperity continued about eight years longer, when the family fortune went to smash in 1840. Jacobi was cleaned out himself at the age of thirty-six and in addition had to provide for his mother, also ruined.

Gauss all this time had been watching Jacobi's phenomenal activity with more than a mere scientific interest, as many of Jacobi's discoveries overlapped some of those of his own youth which he had never published. He had also (it is said) met the young man personally: Jacobi called on Gauss (no account of the visit has survived) in September 1839, on his return trip to Königsberg after a vacation in Marienbad to recuperate from overwork. Gauss appears to have feared that Jacobi's financial collapse would have a disastrous effect on his mathematics, but Bessel reassured him: 'Fortunately such a talent cannot be destroyed, but I should have liked him to have the sense of freedom which money assures.'

The loss of his fortune had no effect whatever on Jacobi's mathematics. He never alluded to his reverses but kept on working as assiduously as ever. In 1842 Jacobi and Bessel attended the meeting of the British Association at Manchester, where the German Jacobi and the Irish Hamilton met in the flesh. It was to be one of Jacobi's greatest glories to continue the work of Hamilton in dynamics and, in a sense, to complete what the Irishman had abandoned in favour of a will-o-the-wisp (which will be followed when we come to it).

At this point in his career Jacobi suddenly attempted to blossom out into something showier than a mere mathematician. Not to interrupt the story of his scientific life when we take it up, we shall dispose here of the illustrious mathematician's singular misadventures in politics.

The year following his return from the trip of 1842, Jacobi had a complete breakdown from overwork. The advancement of science in the 1840's in Germany was in the hands of the benevolent princes and kings of the petty states which were later to coalesce into the German Empire. Jacobi's good angel was the King of Prussia, who seems to have appreciated fully the honour which Jacobi's researches conferred on the Kingdom. Accordingly, when Jacobi fell ill, the benevolent King urged him to take as long a vacation as he liked in the mild climate of Italy. After five months at Rome and Naples with Borchardt (whom we shall meet later in the company of Weierstrass) and Dirichlet, Jacobi returned to Berlin in June 1844. He was now permitted to stay on in Berlin until his health should be completely restored but, owing to jealousies, was not given a professorship in the University, although as a member of the Academy he was permitted to lecture on anything he chose. Further, out of his own pocket, practically, the King granted Jacobi a substantial allowance.

After all this generosity on the part of the King one might think that Jacobi would have stuck to his mathematics. But on the utterly imbecile advice of his physician he began meddling in politics 'to benefit his nervous system'. If ever a more idiotic prescription was handed out by a doctor to a patient whose complaint he could not diagnose it has yet to be exhumed. Jacobi swallowed the dose. When the democratic upheaval of 1848 began to erupt Jacobi was ripe for office. On the advice of a friend – who, by the way, happened to be one of the men over whose head Jacobi had been promoted some twenty years before – the guileless mathematician stepped into the arena of politics with all the innocence of an enticingly plump missionary setting foot on a cannibal island. They got him.

The mildly liberal club to which his slick friend had introduced him ran Jacobi as their candidate for the May election of 1848. But he never saw the inside of parliament. His eloquence before the club convinced the wiser members that Jacobi was no candidate for them. Quite properly, it would seem, they pointed out that Jacobi, the King's pensioner, might possibly be the liberal he now professed to be, but that it was more probable he was a trimmer, a turncoat, and a stool pigeon for the royalists. Jacobi refuted these base insinuations in a magnificent speech packed with irrefutable logic – oblivious of the axiom that logic is the last thing on earth for which a practical politician has any use. They let him hang himself in his own noose. He was not elected. Nor was his nervous system benefited by the uproar over his candidacy which rocked the beer halls of Berlin to their cellars.

Worse was to come. Who can blame the Minister of Education for enquiring the following May whether Jacobi's health had recovered sufficiently for him to return safely to Königsberg? Or who can wonder that his allowance from the King was stopped a few days later? After all even a King may be permitted a show of petulance when the mouth he tries to feed bites him. Nevertheless Jacobi's desperate plight was enough to excite anybody's sympathy. Married and practically penniless he had seven small children to support in addition to his wife. A friend in Gotha took in the wife and children, while Jacobi retired to a dingy hotel room to continue his researches.

He was now (1849) in his forty-fifth year and, except for Gauss, the most famous mathematician in Europe. Hearing of his plight, the University of Vienna began angling for him. As an item of interest here, Littrow, Abel's Viennese friend, took a leading part in the negotiations. At last, when a definite and generous offer was tendered, Alexander von Humboldt talked the sulky King round; the allowance was restored, and Jacobi was not permitted to rob Germany of her second greatest man. He remained in Berlin, once more in favour but definitely out of politics.

The subject, elliptic functions, in which Jacobi did his first great work, has already been given what may seem like its share of space; for after all it is to-day more or less of a detail in the vaster theory of functions of a complex variable which, in its turn, is fading from the ever changing scene as a thing of living interest. As the theory of elliptic functions will be mentioned several times in succeeding chapters we shall attempt a brief justification of its apparently unmerited prominence.

No mathematician would dispute the claim of the theory of functions of a complex variable to have been one of the major fields of nineteenth-century mathematics. One of the reasons why this theory was of such importance may be repeated here. Gauss had shown that *complex* numbers are both necessary and sufficient to provide every algebraic equation with a root. Are any further, more general, kinds of 'numbers' possible? How might such 'numbers' arise?

Instead of regarding *complex* numbers as having first presented themselves in the attempt to solve certain simple equations, say $x^2 + 1 = 0$, we may also see their origin in another problem of elementary algebra, that of *factorization.* To resolve $x^2 - y^2$ into factors of the *first* degree we need nothing more mysterious than the positive and negative integers: $(x^2 - y^2) = (x + y)(x - y)$. But the same problem for $x^2 + y^2$ demands 'imaginaries': $x^2 + y^2 = (x + y\sqrt{-1})(x - y\sqrt{-1})$. Carrying this up a step in one of many possible ways open, we might seek to resolve $x^2 + y^2 + z^2$ into two factors of the *first* degree. Are the positives, negatives, and imaginaries sufficient? Or must some new kind of 'number' be invented to solve the problem? The latter is the case. It was found that for the new 'numbers' necessary the rules of common algebra break down in one important particular: it is no longer true that the *order* in which 'numbers' are *multiplied* together is indifferent; that is, for the new numbers it is not true that $a \times b$ is equal to $b \times a$. More will be said on this when we come to Hamilton. For the moment we note that the elementary algebraic problem of factorization quickly leads us into regions where complex numbers are inadequate.

How far can we go, what are the *most general numbers possible*, if we insist that for these numbers *all* the familiar laws of common algebra are to hold? It was proved in the latter part of the nineteenth century that the complex numbers $x + iy$, where x,y are real numbers and $i = \sqrt{-1}$, are the most general for which common algebra is true. The real numbers, we recall, correspond to the distances measured along a fixed straight line in either direction (positive, negative) from a fixed point, and the graph of a function $f(x)$, plotted as $y = f(x)$, in Cartesian geometry, gives us a picture of a function y of a *real* variable x. The mathematicians of the seventeenth and eighteenth centuries imagined their functions as being of this kind. But if the common algebra and its extensions into the calculus which they applied to their functions are equally applicable to complex numbers, which include the real numbers as a very degenerate case, it was but natural that many of the things the early analysts found were less than half the whole story possible. In particular the integral calculus presented many inexplicable anomalies which were cleared up only when the field of operations was enlarged to its fullest possible extent and functions of *complex* variables were introduced by Gauss and Cauchy.

The importance of elliptic functions in all this vast and fundamental development cannot be over-estimated. Gauss, Abel, and Jacobi, by their extensive and detailed elaboration of the theory of elliptic functions, in which complex numbers appear inevitably, provided a testing ground for the discovery and improvement of general theorems in the theory of functions of a complex variable. The two theories seemed to have been designed by fate to complement and supplement one another – there is a reason for this, also for the deep connexion of elliptic functions with the Gaussian theory of quadratic forms, which considerations of space force us to forego. Without the innumerable clues for a general theory provided by the special instances of more inclusive theorems occurring in elliptic functions, the theory of functions of a complex variable would have developed much more slowly than it did – Liouville's theorem, the entire subject of multiple periodicity with its impact on the theory of algebraic functions and their integrals, may be recalled to mathematical readers. If some of these great monuments of nineteenth-century mathematics are already receding into the mists of yesterday, we need only remind ourselves that Picard's theorem on exceptional values in the neighbourhood of an essential singularity, one of the most suggestive in current analysis, was first proved by devices originating in the theory of elliptic functions. With this partial summary of the reason why elliptic functions were important in the mathematics of the nineteenth century we may pass on to Jacobi's cardinal part in the development of the theory.

The history of elliptic functions is quite involved, and although of considerable interest to specialists, is not likely to appeal to the general reader. Accordingly we shall omit the evidence (letters of Gauss, Abel, Jacobi, Legendre, and others) on which the following bare summary is based.

First, it is established that Gauss anticipated both Abel and Jacobi by as much as twenty-seven years in some of their most striking work. Indeed Gauss says that 'Abel has followed exactly the same road that I did in 1798'. That this claim is just will be admitted by anyone who will study the evidence published only after Gauss' death. Second, it seems to be agreed that Abel anticipated Jacobi in certain important details, but that Jacobi made his great start in entire ignorance of his rival's work.

A capital property of the elliptic functions is their *double periodicity* (discovered in 1825 by Abel): if $E(x)$ is an elliptic function, then there are two distinct numbers, say p_1, p_2, such that

$$E(x + p_1) = E(x), \text{ and } E(x + p_2) = E(x)$$

for all values of the variable x.

Finally, on the historical side, is the somewhat tragic part played by Legendre. For forty years he had slaved over elliptic *integrals* (*not* elliptic *functions*) without noticing what both Abel and Jacobi saw almost at once, namely that by *inverting* his point of view the whole subject would become infinitely simpler. Elliptic integrals first present themselves in the problem of finding the length of an arc of an ellipse. To what was said about inversion in connexion with Abel the following statement in symbols may be added. This will bring out more clearly the point which Legendre missed.

If $R(t)$ denotes a polynomial in t, an integral of the type

$$\int_0^x \frac{1}{\sqrt{R(t)}}dt$$

is called an *elliptic integral* if $R(t)$ is of either the third or the fourth degree; if $R(t)$ is of degree higher than the fourth, the integral is called *Abelian* (after Abel, some of whose greatest work concerned such integrals). If $R(t)$ is of only the second degree, the integral can be calculated out in terms of elementary functions. In particular

$$\int_0^x \frac{1}{\sqrt{1 - t^2}}dt = \sin^{-1}x,$$

($\sin^{-1}x$ is read, 'an angle whose sine is x'). That is, if

$$y = \int_0^x \frac{1}{\sqrt{1 - t^2}}dt$$

we consider the *upper limit, x,* of the integral, as a function of the integral itself, namely of y. This *inversion* of the problem removed most of the difficulties which Legendre had grappled with for forty years. The true theory of these important integrals rushed forth almost of itself after this obstruction had been removed – like a log-jam going down the river after the king log has been snaked out.

When Legendre grasped what Abel and Jacobi had done he encouraged them most cordially, although he realized that their simpler approach (that of inversion) nullified what was to have been his own masterpiece of forty years' labour. For Abel, alas, Legendre's praise came too late, but for Jacobi it was an inspiration to surpass himself. In one of the finest correspondences in the whole of scientific literature the young man in his early twenties and the veteran in his late seventies strive to outdo one another in sincere praise and gratitude. The only jarring note is Legendre's outspoken disparagement of Gauss, whom Jacobi vigorously defends. But as Gauss never condescended to publish his researches – he had planned a major work on elliptic functions when Abel and Jacobi anticipated him in publication – Legendre can hardly be blamed for holding a totally mistaken opinion. For lack of space we must omit extracts from this beautiful correspondence (the letters are given in full in vol. 1 of Jacobi's *Werke* – in French).

The joint creation with Abel of the theory of elliptic functions was only a small if highly important part of Jacobi's huge output. Only to enumerate all the fields he enriched in his brief working life of less than a quarter of a century would take more space than can be devoted to one man in an account like the present, so we shall merely mention a few of the other great things he did.

Jacobi was the first to apply elliptic functions to the theory of numbers. This was to become a favourite diversion with some of the greatest mathematicians who followed Jacobi. It is a curiously recondite subject, where arabesques of ingenious algebra unexpectedly reveal hitherto unsuspected relations between the common whole numbers. It was by this means that Jacobi proved the famous assertion of Fermat that every integer 1,2,3, ... is a sum of four integer squares (zero being counted as an integer) and, moreover, his beautiful analysis told him *in how many ways* any given integer may be expressed as such a sum.*

For those whose tastes are more practical we may cite Jacobi's work in dynamics. In this subject, of fundamental importance in both applied science and mathematical physics, Jacobi made the first significant advance beyond Lagrange and Hamilton. Readers acquainted with quantum mechanics will recall the important part played in some presentations of that revolutionary theory by the Hamilton-Jacobi equation. His work in differential equations began a new era.

In algebra, to mention only one thing of many, Jacobi cast the theory of determinants into the simple form now familiar to every student in a second course of school algebra.

To the Newton-Laplace-Lagrange theory of attraction Jacobi made substantial contributions by his beautiful investigations on the functions which recur repeatedly in that theory and by applications of elliptic and Abelian functions to the attraction of ellipsoids.

Of a far higher order of originality is his great discovery in Abelian functions. Such functions arise in the inversion of an Abelian integral, in the same way that the elliptic functions arise from the inversion of an elliptic integral. (The technical terms were noted earlier in this chapter.) Here he had nothing to guide him, and for long he wandered lost in a maze that had no clue. The appropriate inverse functions in the simplest case are functions of *two* variables having *four* periods; in the general case the functions have n variables and $2n$ periods; the elliptic functions correspond to $n = 1$. This discovery was to nineteenth-century analysis what Columbus' discovery of America was to fifteenth-century geography.

Jacobi did not suffer an early death from overwork, as his lazier friends predicted that he would, but from smallpox (18 February 1851) in his forty-seventh year. In taking leave of this large-minded man we may quote his retort to the great French mathematical physicist Fourier, who had reproached

* If n is odd, the number of ways is 8 times the sum of all the divisors of n (1 and n included); if n is even, the number of ways is 24 times the sum of all the odd divisors of n.

both Abel and Jacobi for 'wasting' their time on elliptic functions while there were still problems in heat-conduction to be solved.

'It is true', Jacobi says, 'that M. Fourier had the opinion that the principal aim of mathematics was public utility and the explanation of natural phenomena; but a philosopher like him should have known that the sole end of science is the honour of the human mind, and that under this title a question about numbers is worth as much as a question about the system of the world.'

If Fourier could revisit the glimpses of the moon he might be disgusted at what has happened to the analysis he invented for 'public utility and the explanation of natural phenomena'. So far as mathematical physics is concerned Fourier analysis today is but a detail in the infinitely vaster theory of boundary-value problems, and it is in the purest of pure mathematics that the analysis which Fourier invented finds its interest and its justification. Whether 'the human mind' is honoured by these modern researches may be put up to the experts – provided the behaviourists have left anything of the human mind to be honoured.

CHAPTER NINETEEN

AN IRISH TRAGEDY

Hamilton

*

WILLIAM ROWAN HAMILTON is by long odds the greatest man of science that Ireland has produced. His nationality is emphasized because one of the driving impulses behind Hamilton's incessant activity was his avowed desire to put his superb genius to such uses as would bring glory to his native land. Some have claimed that he was of Scotch descent. Hamilton himself insisted that he was Irish, and it is certainly difficult for a Scot to see anything Scotch in Ireland's greatest and most eloquent mathematician.

Hamilton's father was a solicitor in Dublin, Ireland, where William, the youngest of three brothers and one sister, was born on 3 August 1805.* The father was a first-rate business man with an 'exuberant eloquence', a religious zealot, and last, but unfortunately not least, a very convivial man, all of which traits he passed on to his gifted son. Hamilton's extraordinary intellectual brilliance was probably inherited from his mother, Sarah Hutton, who came of a family well known for its brains.

However, on the father's side, the swirling clouds of eloquence, 'both of lips and pen', which made the jolly toper the life of every party he graced with his reeling presence, condensed into something less gaseous in William's uncle, the Reverend James Hamilton, curate of the village of Trim (about twenty miles from Dublin). Uncle James was in fact an inhumanly accomplished linguist – Greek, Latin, Hebrew, Sanskrit, Chaldee, Pali, and heaven knows what other heathen dialects,

* The date on his tombstone is 4 August, 1805. Actually he was born at midnight; hence the confusion in dates. Hamilton, who had a passion for accuracy in such trifles, chose 3 August until in later life he shifted to 4 August for sentimental reasons.

came to the tip of his tongue as readily as the more civilized languages of Continental Europe and Ireland. This polyglot fluency played no inconsiderable part in the early and extremely extensive miseducation of the hapless but eager William, for at the age of three, having already given signs of genius, he was relieved of his doting mother's affection and packed off by his somewhat stupid father to glut himself with languages under the expert tutelage of the supervoluble Uncle James.

Hamilton's parents had very little to do with his upbringing; his mother died when he was twelve, his father two years later. To James Hamilton belongs whatever credit there may be for having wasted young William's abilities in the acquisition of utterly useless languages and turning him out, at the age of thirteen, as one of the most shocking examples of a linguistic monstrosity in history. That Hamilton did not become an insufferable prig under his misguided parson-uncle's instruction testifies to the essential soundness of his Irish common sense. The education he suffered might well have made a permanent ass of even a humorous boy, and Hamilton had no humour.

The tale of Hamilton's infantile accomplishments reads like a bad romance, but it is true: at three he was a superior reader of English and was considerably advanced in arithmetic; at four he was a good geographer; at five he read and translated Latin, Greek, and Hebrew, and loved to recite yards of Dryden, Collins, Milton, and Homer – the last in Greek; at eight he added a mastery of Italian and French to his collection and extemporized fluently in Latin, expressing his unaffected delight at the beauty of the Irish scene in Latin hexameters when plain English prose offered too plebeian a vent for his nobly exalted sentiments; and finally, before he was ten he had laid a firm foundation for his extraordinary scholarship in oriental languages by beginning Arabic and Sanskrit.

The tally of Hamilton's languages is not yet complete. When William was three months under ten years old his uncle reports that 'His thirst for the Oriental languages is unabated. He is now master of most, indeed of all except the minor and comparatively provincial ones. The Hebrew, Persian, and Arabic are about to be confirmed by the superior and intimate acquaintance with the Sanskrit, in which he is already a proficient. The Chaldee and Syriac he is grounded in, also the Hindoostani, Malay, Mahratta, Bengali, and others. He is about to commence the Chinese, but the difficulty of procuring books is very great It cost me a large sum to supply him from London, but I hope the money was well expended.' To which we can only throw up our hands and ejaculate Good God! What was the sense of it all?

By thirteen William was able to brag that he had mastered one language for each year he had lived. At fourteen he composed a flowery welcome in Persian to the Persian Ambassador, then visiting Dublin, and had it transmitted to the astonished potentate. Wishing to follow up his advantage and slay the already slain, young Hamilton called on the Ambassador, but that wily oriental, forewarned by his faithful secretary, 'much regretted that on account of a bad headache he was unable to receive me [Hamilton] personally.' Perhaps the Ambassador had not yet recovered from the official banquet, or he may have read the letter. In translation at least it is pretty awful – just the sort of thing a boy of fourteen, taking himself with devastating seriousness and acquainted with all the stickiest and most bombastic passages of the Persian poets, might imagine a sophisticated oriental out on a wild Irish spree would relish as a pick-me-up the morning after. Had young Hamilton really wished to view the Ambassador he should have sent in a salt herring, not a Persian poem.

Except for his amazing ability, the maturity of his conversation and his poetical love of nature in all her moods, Hamilton was like any other healthy boy. He delighted in swimming and had none of the grind's interesting if somewhat repulsive pallor. His disposition was genial and his temper – rather unusually so for a sturdy Irish boy – invariably even. In later life, however, Hamilton showed his Irish by challenging a detractor – who had called him a liar – to mortal combat. But the affair was amicably arranged by Hamilton's second, and Sir William cannot be legitimately counted as one of the great mathematical duellists. In other respects young Hamilton was not a normal boy. The infliction of pain or suffering on beast or man he would not tolerate. All his life Hamilton loved animals and, what is regrettably rarer, respected them as equals.

Hamilton's redemption from senseless devotion to useless languages began when he was twelve and was completed before he was fourteen. The humble instrument selected by Providence to turn Hamilton from the path of error was the American calculating boy, Zerah Colburn (1804–39), who at the time had been attending Westminster School in London. Colburn and Hamilton were brought together in the expectation that the young Irish genius would be able to penetrate the secret of the American's methods, which Colburn himself did not fully understand (as was seen in the chapter on Fermat). Colburn was entirely frank in exposing his tricks to Hamilton, who in his turn improved upon what he had been shown. There was but little abstruse or remarkable about Colburn's methods. His feats were largely a matter of memory. Hamilton's acknowledgement of Colburn's influence occurs in a letter written when he was seventeen (August 1822) to his cousin Arthur.

By the age of seventeen Hamilton had mastered mathematics through the integral calculus and had acquired enough mathematical astronomy to be able to calculate eclipses. He read Newton and Lagrange. All this was his recreation; the classics were still his serious study, although only a second love. What is more important, he had already made 'some curious *discoveries*', as he wrote to his sister Eliza.

The discoveries to which Hamilton refers are probably the germs of his first great work, that on systems of rays in optics. Thus in his seventeenth year Hamilton had already begun his career of fundamental discovery. Before this he had brought himself to the attention of Dr Brinkley, Professor of Astronomy at Dublin, by the detection of an error in Laplace's attempted proof of the parallelogram of forces.

Hamilton never attended any school before going to the University but received all his preliminary training from his uncle and by private study. His forced devotion to the classics in preparation for the entrance examinations to Trinity College, Dublin, did not absorb all of his time, for on 31 May 1823, he writes to his cousin Arthur, 'In Optics I have made a very curious discovery – at least it seems so to me. . . .'

If, as has been supposed, this refers to the 'characteristic function', which Hamilton will presently describe for us, the discovery marks its author as the equal of any mathematician in history for genuine precocity. On 7 July 1823 young Hamilton passed, easily first out of 100 candidates, into Trinity College. His fame had preceded him, and as was only to be expected, he quickly became a celebrity: indeed his classical and mathematical prowess, while he was yet an undergraduate, excited the curiosity of academic circles in England and Scotland as well as in Ireland, and it was even declared by some that a second Newton had arrived. The tale of his undergraduate triumphs can be imagined – he carried off practically all the available prizes and obtained the highest honours in both classics and mathematics. But more important than all these triumphs, he completed the first draft of Part I of his epoch-making memoir on systems of rays. 'This young man', Dr Brinkley remarked, when Hamilton presented his memoir to the Royal Irish Academy, 'I do not say *will* be, but *is*, the first mathematician of his age.'

Even his laborious drudgeries to sustain his brilliant academic record and the hours spent more profitably on research did not absorb all of young Hamilton's superabundant energies. At nineteen he experienced the first of his three serious love affairs. Being conscious of his own 'unworthiness' – especially as concerned his material prospects – William contented himself with writing poems to the young lady, with the usual result: a solider, more prosaic man married the girl. Early in May 1825 Hamilton learned from his sweetheart's mother that his love had married his rival. Some idea of the shock he experienced can be inferred from the fact that Hamilton, a deeply religious man to whom suicide was a deadly sin, was tempted to drown himself. Fortunately for science he solaced himself with another poem. All his life Hamilton was a prolific versifier. But his true poetry, as he told his friend and ardent admirer, William Wordsworth, was his mathematics. From this no mathematician will dissent.

We may dispose here of Hamilton's lifelong friendships with some of the shining literary lights of his day – the poets Wordsworth, Southey, and Coleridge, of the so-called Lake School, Aubrey de Vere, and the didactic novelist Maria Edgeworth, a litteratrice after Hamilton's own pious heart. Wordsworth and Hamilton first met on the latter's trip of September 1827 to the English Lake District. Having 'waited on Wordsworth at tea', Hamilton oscillated back and forth with the poet all night, each desperately trying to see the other home. The following day Hamilton sent Wordsworth a poem of ninety iron lines which the poet himself might have warbled in one of his heavier flights. Naturally Wordsworth did not relish the eager young mathematician's unconscious plagiarism, and after damning it with faint praise, proceeded to tell the hopeful author – at great length – that 'the workmanship (what else could be expected from so young a writer?) is not what it ought to be.' Two years later, when Hamilton was already installed as astronomer at the Dunsink Observatory, Wordsworth returned the visit. Hamilton's sister Eliza, on being introduced to the poet, felt herself 'involuntarily parodying the first lines of his own poem *Yarrow Visited*:

> *And this is* Wordsworth! *this the man*
> *Of whom my fancy cherished*
> *So faithfully a waking dream,*
> *An image that hath perished!*

One great benefit accrued from Wordsworth's visit: Hamilton realized at last that 'his path must be the path of Science, and not that of Poetry; that he must renounce the hope of habitually cultivating both, and that, therefore, he must brace himself up to bid a painful farewell to Poetry'. In short, Hamilton grasped the obvious truth that there was not a spark of poetry in him, in the *literary* sense. Nevertheless he continued to versify all his life. Wordsworth's opinion of Hamilton's intellect was high. In fact he graciously said (in effect) that only two men he had ever known gave him a feeling of inferiority, Coleridge and Hamilton.

Hamilton did not meet Coleridge till 1832, when the poet had practically ceased to be anything but a spurious copy of a mediocre German metaphysician. Nevertheless each formed a high estimate of the other's capacity, as Hamilton had for long been a devoted student of Kant in the original. Indeed philosophical speculation always fascinated Hamilton, and at one time he declared himself a wholehearted believer – intellectually, but not intestinally – in Berkeley's devitalized idealism. Another bond between the two was their preoccupation with the theological side of philosophy (if there is such a side), and Coleridge favoured Hamilton with his half-digested ruminations on the Holy Trinity, by which the devout mathematician set considerable store.

The close of Hamilton's undergraduate career at Trinity College was even more spectacular than its beginning; in fact it was unique in university annals. Dr Brinkley resigned his professorship of astronomy to become Bishop of Cloyne. According to the usual British custom the vacancy was advertised, and several distinguished astronomers, including George Biddell Airy (1801–92), later Astronomer Royal of England, sent in their credentials. After some discussion the Governing Board passed over all the applicants and unanimously elected Hamilton, then (1827) an undergraduate of twenty-two, to the professorship. Hamilton had not applied. 'Straight was the path of gold' for him now, and Hamilton resolved not to disappoint the hopes of his enthusiastic electors. Since the age of fourteen he had had a passion for astronomy, and once as a boy he had pointed out the Observatory on its hill at Dunsink, commanding a beautiful view, as the place of all others where he would like to live were he free to choose. He now, at the age of twenty-two, had his ambition by the bit; all he had to do was to ride straight ahead.

He started brilliantly. Although Hamilton was no practical astronomer, and although his assistant observer was incompetent, these drawbacks were not serious. From its situation the Dunsink Observatory could never have cut any important figure in modern astronomy, and Hamilton did wisely in putting his major efforts on his mathematics. At the age of twenty-three he published the completion of the 'curious discoveries' he had made as a boy of seventeen, Part I of *A Theory of Systems of Rays*, the great classic which does for optics what Lagrange's *Mécanique analytique* does for mechanics and which, in Hamilton's own hands, was to be extended to dynamics, putting that fundamental science in what is perhaps its ultimate, perfect form.

The techniques which Hamilton introduced into applied mathematics in this, his first masterpiece, are to-day indispensable in mathematical physics, and it is the aim of many workers in particular branches of theoretical physics to sum up the whole of a theory in a Hamiltonian principle. This magnificent work is that which caused Jacobi, fourteen years later at the British Association meeting at Manchester in 1842, to assert that 'Hamilton is the Lagrange of your country' – (meaning of the English-speaking race). As Hamilton himself took great pains to describe the essence of his new methods in terms comprehensible to non-specialists, we shall quote from his own abstract presented to the Royal Irish Academy on 23 April 1827.

'A Ray, in Optics, is to be considered here as a straight or bent or curved line, along which light is propagated; and a *System of Rays* as a collection or aggregate of such lines, connected by some common bond, some similarity of origin or production, in short some optical unity. Thus the rays which diverge from a luminous point compose one optical system, and, after they have been reflected at a mirror, they compose another. To investigate the geometrical relations of the rays of a system of which we know (as in these simple cases) the optical origin and history, to inquire how they are disposed among themselves, how they diverge or converge, or are parallel, what surfaces or curves they touch or cut, and at what angles of section, how they can be combined in partial pencils, and how each ray in particular can be determined and distinguished from every other, is to study that System of Rays. And to generalize this study of one system so as to become able to pass, without change of plan, to the study of other systems, to assign general rules and a general method whereby these separate optical arrangements may be connected and harmonized together, is to form a *Theory of Systems of Rays*. Finally, to do this in such a manner as to make available the powers of the modern mathesis, replacing figures by functions and diagrams by formulae, is to construct an Algebraic Theory of such Systems, or an *Application of Algebra to Optics*.

'Towards constructing such an application it is natural, or rather necessary, to employ the method introduced by Descartes for the application of Algebra to Geometry. That great and philosophical mathematician conceived the possibility, and employed the plan, of representing or expressing algebraically the position of any point in space by three co-ordinate numbers which answer respectively how far the point is in three rectangular directions (such as north, east, and west), from some fixed point or origin selected or assumed for the purpose; the three dimensions of space thus receiving their three algebraical equivalents, their appropriate conceptions and symbols in the general science of progression [order]. A plane or curved surface became thus algebraically defined by assigning as *its equation* the relation connecting the three co-ordinates of any point upon it, and common to all those points: and a line, straight or curved, was expressed according to the same method, by the assigning two such relations, correspondent to two surfaces of which the line might be regarded as the intersection. In this manner it became possible to conduct general investigations respecting surfaces and curves, and to discover properties common to all, through the medium of general investigations respecting equations between three variable numbers: every geometrical problem could be at least algebraically expressed, if not at once resolved, and every improvement or discovery in Algebra became susceptible of application or interpretation in Geometry. The sciences of Space and Time (to adopt here a view of Algebra which I have elsewhere ventured to propose) became intimately intertwined and indissolubly connected with each other. Henceforth it was almost impossible to improve either science without improving the other also. The problem of drawing tangents to curves led to the discovery of Fluxions or Differentials: those of rectification and quadrature to the inversion of Fluents or Integrals: the investigation of curvatures of surfaces required the Calculus of Partial Differentials: the isoperimetrical problems resulted in the formation of the Calculus of Variations. And reciprocally, all these great steps in Algebraic Science had immediately their applications to Geometry, and led to the discovery of new relations between points or lines or surfaces. But even if the applications of the method had not been so manifold and important, there would still have been derivable a high intellectual pleasure from the contemplation of it *as* a method.

'The first important application of this algebraical method of co-ordinates to the study of optical systems was made by Malus, a French officer of engineers in Napoleon's army in Egypt, and who has acquired celebrity in the history of Physical Optics as the discoverer of polarization of light by reflection. Malus presented to the Institute of France, in 1807, a profound mathematical work which is of the kind above alluded to, and is entitled *Traité d'Optique.* The method employed in that treatise may be thus described:– The direction of a straight ray of any final optical system being considered as dependent on the position of some assigned point on the ray, according to some law which characterizes the particular system and distinguishes it from others; this law may be algebraically expressed by assigning three expressions for the three co-ordinates of some other point of the ray, as *functions* of the three co-ordinates of the point proposed. Malus accordingly introduces general symbols denoting three such functions (or at least three functions equivalent to these), and proceeds to draw several important general conclusions, by very complicated yet symmetric calculations; many of which conclusions, along with many others, were also obtained afterwards by myself, when, by a method nearly similar, without knowing what Malus had done, I began my own attempt to apply Algebra to Optics. But my researches soon conducted me to substitute, for this method of Malus, a very different, and (as I conceive that I have proved) a much more *appropriate* one, for the study of optical systems; by which, instead of employing the *three* functions above mentioned, or at least their *two* ratios, it becomes sufficient to employ *one function*, which I call *characteristic* or principal. And thus, whereas he made his deductions by setting out with the *two equations of a ray*, I on the other hand establish and employ the *one equation of a system*.

'The function which I have introduced for this purpose, and made the basis of my method of *deduction* in mathematical Optics, had, in another connexion, presented itself to former writers as expressing the result of a very high and extensive *induction* in that science. This known result is usually called the *law of least action*, but sometimes also the principle of *least time* [see chapter on Fermat], and includes all that has hitherto been discovered respecting the rules which determine the forms and positions of the lines along which light is propagated, and the changes of direction of those lines produced by reflection or refraction, ordinary or extraordinary [the latter as in a doubly refracting crystal, say Iceland spar, in which a single ray is split into two, both refracted, on entering the crystal]. A certain quantity which in one physical theory is the *action*, and in another the *time*, expended by light in going from any first to any second point, is found to be less than if the light had gone in any other than its actual path, or at least to have what is technically called its variation null, the extremities of the path being unvaried. The mathematical novelty of my method consists in considering this quantity as a *function* of the coordinates of these extremities, which varies when they vary, according to a law which I have called the *law of varying action;* and in *reducing all researches respecting optical systems of rays to the study of this single function:* a reduction which presents mathematical Optics under an entirely novel view, and one analogous (as it appears to me) to the aspect under which Descartes presented the application of Algebra to Geometry.'

Nothing need be added to this account of Hamilton's, except possibly the remark that no science, no matter how ably expounded, is understood as readily as any novel, no matter how badly written. The whole extract will repay a second reading.

In this great work on systems of rays Hamilton had builded better than even he knew. Almost exactly 100 years after the above abstract was written the methods which Hamilton introduced into optics were found to be just what was required in the wave mechanics associated with the modern quantum theory and the theory of atomic structure. It may be recalled that Newton had favoured an emission, or corpuscular, theory of light, while Huygens and his successors up to almost our own time sought to explain the phenomena of light wholly by means of a wave theory. Both points of view were united and, in a purely mathematical sense, reconciled in the modern quantum theory, which came into being in 1925–6. In 1834, when he was twenty-eight, Hamilton realized his ambition of extending the principles which he had introduced into optics to the whole of dynamics.

Hamilton's theory of rays, shortly after its publication when its author was but twenty-seven, had one of the promptest and most spectacular successes of any of the classics of mathematics. The theory purported to deal with phenomena of the actual physical universe as it is observed in everyday life and in scientific laboratories. Unless any such mathematical theory is capable of predictions which experiments later verify, it is no better than a concise dictionary of the subject it systematizes, and it is almost certain to be superseded shortly by a more imaginative picture which does not reveal its whole meaning at the first glance. Of the famous predictions which have certified the value of truly mathematical theories in physical science, we may recall three: the mathematical discovery by John Couch Adams (1819–92) and Urbain-Jean-Joseph Leverrier (1811–77) of the planet Neptune, independently and almost simultaneously in 1845, from an analysis of the perturbations of the planet Uranus according to the Newtonian theory of gravitation; the mathematical prediction of wireless waves by James Clerk Maxwell (1831–79) in 1864, as a consequence of his own electromagnetic theory of light; and finally, Einstein's prediction in 1915–16, from his theory of general relativity, of the deflection of a ray of light in a gravitational field, first confirmed by observations of the solar eclipse on the historic 29 May 1919, and his prediction, also from his theory, that the spectral lines in light issuing from a massive body would be shifted by an amount, which Einstein stated, toward the red end of the spectrum – also confirmed. The last two of these instances – Maxwell's and Einstein's – are of a different order from the first: in both, *totally unknown and unforeseen phenomena* were predicted mathematically; that is, these predictions were *qualitative*. Both Maxwell and Einstein amplified their qualitative foresight by precise *quantitative* predictions which precluded any charge of mere guessing when their prophecies were finally verified experimentally.

Hamilton's prediction of what is called conical refraction in optics was of this same qualitative plus quantitative order. From his theory of systems of rays he predicted mathematically that a wholly unexpected phenomenon would be found in connexion with the refraction of light in biaxial crystals. While polishing the Third Supplement to his memoir on rays he surprised himself by a discovery which he thus describes:

'The law of the reflection of light at ordinary mirrors appears to have been known to Euclid; that of ordinary refraction at a surface of water, glass, or other uncrystallized medium, was discovered at a much later date by Snellius; Huygens discovered, and Malus confirmed, the law of extraordinary refraction produced by uniaxal crystals, such as Iceland spar; and finally, the law of the extraordinary double refraction at the faces of biaxal crystals, such as topaz or arragonite, was found in our own time by Fresnel. But even in these cases of extraordinary or crystalline refraction, no more than *two* refracted rays had ever been observed or even suspected to exist, if we except a theory of Cauchy, that there might possibly be a *third* ray, though probably imperceptible to our senses. Professor Hamilton, however, in investigating by his general method the consequences of the law of Fresnel, was led to conclude that there ought to be in certain cases, which he assigned, not merely two, nor three, nor any finite number, but an *infinite* number, or a *cone* of refracted rays *within* a biaxal crystal, corresponding to and resulting from a *single* incident ray; and that in certain other cases, a single ray within such a crystal should give rise to an infinite number of emergent rays, arranged in a certain other cone. He was led, therefore, to anticipate from theory two new laws of light, to which he gave the names of *Internal and External Conical Refraction*.'

The prediction and its experimental verification by Humphrey Lloyd evoked unbounded admiration for young Hamilton from those who could appreciate what he had done. Airy, his former rival for the professorship of astronomy, estimated Hamilton's achievement thus: 'Perhaps the most remarkable prediction that has ever been made is that lately made by Professor Hamilton.' Hamilton himself considered this, like any similar prediction, 'a subordinate and secondary result' compared to the grand object which he had in view, 'to introduce harmony and unity into the contemplations and reasonings of optics, regarded as a branch of pure science.'

According to some this spectacular success was the high-water mark in Hamilton's career; after the great work on optics and dynamics his tide ebbed. Others, particularly members of what has been styled the High Church of Quaternions, hold that Hamilton's greatest work was still to come – the creation of what Hamilton himself considered his masterpiece and his title to immortality, his theory of quaternions. Leaving quaternions out of the indictment for the moment, we may simply state that, from his twenty-seventh year till his death at sixty, two disasters raised havoc with Hamilton's scientific career, marriage and alcohol. The second was partly, but not wholly, a consequence of the unfortunate first.

After a second unhappy love affair, which ended with a thoughtless remark that meant nothing but which the hypersensitive suitor took to heart, Hamilton married his third fancy, Helen Maria Bayley, in the spring of 1833. He was then in his twenty-eighth year. The bride was the daughter of a country parson's widow. Helen was 'of pleasing ladylike appearance, and early made a favourable impression upon him [Hamilton] by her truthful nature and by the religious principles which he knew her to possess, although to these recommendations was not added any striking beauty of face or force of intellect.' Now, any fool can tell the truth, and if truthfulness is all a fool has to recommend her, whoever commits matrimony with her will get the short end of the indiscretion. In the summer of 1832 Miss Bayley 'passed through a dangerous illness, . . . , and this event doubtless drew his [the lovelorn Hamilton's] thoughts especially toward her, in the form of anxiety for her recovery, and, coming at a time [when he had just broken with the girl he really wanted] when he felt obliged to suppress his former passion, prepared the way for tenderer and warmer feelings.' Hamilton in short was properly hooked by an ailing female who was to become a semi-invalid for the rest of her life and who, either through incompetence or ill-health, let her husband's slovenly servants run his house as they chose, which at least in some quarters – especially his study – came to resemble a pig-sty. Hamilton needed a sympathetic woman with backbone to keep him and his domestic affairs in some semblance of order; instead he got a weakling.

Ten years after his marriage Hamilton tried to pull himself up short on the slippery trail he realized with a brutal shock he was treading. As a young man, fêted and toasted at dinners, he had rather let himself go, especially as his great gifts for eloquence and conviviality were naturally enough heightened by a drink or two. After his marriage, irregular meals or no meals at all, and his habit of working twelve or fourteen hours at a stretch, were compensated for by taking nourishment from a bottle.

It is a moot question whether mathematical *inventiveness* is accelerated or retarded by moderate indulgence in alcohol, and until an exhaustive set of *controlled* experiments is carried out to settle the matter, the doubt must remain a doubt, precisely as in any other biological research. If, as some maintain, poetic and mathematical inventiveness are akin, it is by no means obvious that reasonable alcoholic indulgence (if there is such a thing) is destructive of mathematical inventiveness; in fact numerous well-attested instances would seem to indicate the contrary. In the case of poets, of course, 'wine and song' have often gone together, and in at least one instance – Swinburne – without the first the second dried up almost completely. Mathematicians have frequently remarked on the terrific strain induced by prolonged concentrations on a difficulty, and some have found the let-down occasioned by a drink a decided relief. But poor Hamilton quickly passed beyond this stage and became careless, not only in the untidy privacy of his study, but also in the glaring publicity of a banquet hall. He got drunk at a scientific dinner. Realizing what had overtaken him, he resolved never to touch alcohol again, and for two years he kept his resolution. Then, during a scientific meeting at the estate of Lord Rosse (owner of the largest and most useless telescope then in existence), his old rival, Airy, jeered at him for drinking nothing but water. Hamilton gave in, and thereafter took all he wanted – which was more than enough. Still, even this handicap could not put him out of the race, although without it he probably would have gone farther and have reached a greater height than he did. However, he got high enough, and moralizing may be left to moralists.

Before considering what Hamilton regarded as his masterpiece, we may briefly summarize the principal honours which came his way. At thirty he held an influential office in the British Association for the Advancement of Science at its Dublin meeting, and at the same time the Lord-Lieutenant bade him to 'Kneel down, Professor Hamilton', and then, having dubbed him on both shoulders with the sword of State, to 'Rise up, Sir William Rowan Hamilton'. This was one of the few occasions in his life on which Hamilton had nothing whatever to say. At thirty-two he became President of the Royal Irish Academy, and at thirty-eight was awarded a Civil List life pension of £200 a year from the British Government, Sir Robert Peel, Ireland's reluctant friend, being then Premier. Shortly before this Hamilton had made his capital invention – quaternions.

An honour which pleased him more than any he had ever received was the last, as he lay on his deathbed: he was elected the first foreign member of the National Academy of Sciences of the United States, which was founded during the Civil War. This honour was in recognition of his work in quaternions, principally, which for some unfathomable reason stirred American mathematicians of the time (there were only one or two in existence, Benjamin Peirce of Harvard being the chief) more profoundly than had any other British mathematics since Newton's *Principia*. The early popularity of quaternions in the United States is somewhat of a mystery. Possibly the turgid eloquence of the *Lectures on Quaternions* captivated the taste of a young and vigorous nation which had yet to outgrow its morbid addiction to senatorial oratory and Fourth of July verbal fireworks.

Quaternions has too long a history for the whole story to be told here. Even Gauss with his anticipation of 1817 was not the first in the field; Euler preceded him with an isolated result which is most simply interpreted in terms of quaternions. The origin of quaternions may go back even farther than this, for Augustus de Morgan once half-jokingly offered to trace their history for Hamilton from the ancient Hindus to Queen Victoria. However, we need glance here only at the lion's share in the invention and consider briefly what inspired Hamilton.

The British school of algebraists, as will be seen in the chapter on Boole, put common algebra on its own feet during the first half of the nineteenth century. Anticipating the currently accepted procedure in developing any branch of mathematics carefully and rigorously they founded algebra *postulationally.* Before this, the various kinds of 'numbers' – fractions, negatives, irrationals – which enter mathematics when it is *assumed* that all algebraic equations have roots, had been allowed to function on precisely the same footing as the common positive integers which were so staled by custom that all mathematicians believed them to be 'natural' and in some vague sense completely understood – they are not, even to-day, as will be seen when the work of Georg Cantor is discussed. This naïve faith in the self-consistency of a system founded on the blind, formal juggling of mathematical symbols may have been sublime but it was also slightly idiotic. The climax of this credulity was reached in the notorious *principle of permanence of form*, which stated in effect that a set of rules which yield consistent results for one kind of numbers – say the positive integers – will continue to yield consistency when applied to any other kind – say the imaginaries – even when no interpretation of the results is evident. It does not seem surprising that this faith in the integrity of meaningless symbols frequently led to absurdity.

The British school changed all this, although they were unable to take the final step and *prove* that their postulates for common algebra will never lead to a contradiction. That step was taken only in our own generation by the German workers in the foundations of mathematics. In this connexion it must be kept in mind that algebra deals only with *finite* processes; when *infinite* processes enter, as for example in summing an infinite series, we are thrust out of algebra into another domain. This is emphasized because the usual elementary text labelled 'Algebra' contains a great deal – infinite geometric progressions, for instance – that is *not* algebra in the modern meaning of the word.

The nature of what Hamilton did in his creation of quaternions will show up more clearly against the background of a set of postulates (taken from L. E. Dickson's *Algebras and Their Arithmetics*. Chicago, 1923) for common algebra or, as it is technically called, a *field* (English writers sometimes use *corpus* as the equivalent of the German *Korper* or French *corps*).

'A field F is a system consisting of a set S of elements $a, b, c, \ldots$ and two operations, called addition and multiplication, which may be performed upon any two (equal or distinct) elements a and b of S, taken in that order, to produce uniquely determined elements $a \oplus b$ and $a \odot b$ of S, such that postulates I–V are satisfied. For simplicity we shall write $a + b$ for $a \oplus b$, and ab for $a \odot b$, and call them the *sum* and *product*, respectively, of a and b. Moreover, elements of S will be called elements of F.

'I. If a and b are any two elements of F, $a + b$ and ab are uniquely determined elements of F, and

$$b + a = a + b, \quad ba = ab.$$

'II. If a, b, c are any three elements of F,

$$(a + b) + c = a + (b + c), \quad (ab)c = a(bc), \quad a(b + c) = ab + ac.$$

'III. There exist in F two distinct elements, denoted by 0, 1, such that if a is any element of F, $a + 0 = a$, $a1 = a$ (whence $0 + a = a$, $1a = a$, by I).

'IV. Whatever be the element a of F, there exists in F an element x such that $a + x = 0$ (whence $x + a = 0$ by I).

'V. Whatever be the element a (distinct from 0) of F, there exists in F an element y such that $ay = 1$ (whence $ya = 1$, by I).'

From these simple postulates the whole of common algebra follows. A word or two about some of the statements may be helpful to those who have not seen algebra for years. In II, the statement $(a + b) + c = a + (b + c)$, called the *associative law of addition*, says that if a and b are added, and to this sum is added c, the result is the same as if a and the sum of b and c are added. Similarly with respect to multiplication, for the second statement in II. The third statement in II is called the *distributive law*. In III a 'zero' and 'unity' are postulated; in IV, the postulated x gives the negative of a; and the first parenthetical remark in V forbids 'division by zero'. The demands in Postulate I are called the *commutative laws of addition and multiplication* respectively.

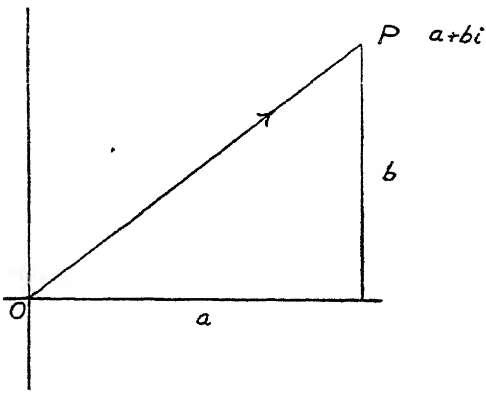
Such a set of postulates may be regarded as a distillation of experience. Centuries of working with numbers and getting useful results according to the rules of arithmetic – empirically arrived at – suggested most of the rules embodied in these precise postulates, but once the suggestions of experience are understood, the *interpretation* (here common arithmetic) furnished by experience is deliberately suppressed or forgotten, and the *system* defined by the postulates is developed *abstractly*, on its own merits, by common logic plus mathematical tact.

Notice in particular IV, which *postulates the existence* of negatives. We do not attempt to *deduce* the existence of negatives from the behaviour of positives. When negative numbers first appeared in experience, as in debits instead of credits, they, *as numbers*, were held in the same abhorrence as 'unnatural' monstrosities as were later the 'imaginary' numbers $\sqrt{-1}$, $\sqrt{-2}$, etc., arising from the *formal* solution of equations such as $x^2 + 1 = 0$, $x^2 + 2 = 0$, etc. If the reader will glance back at what Gauss did for complex numbers he will appreciate more fully the complete simplicity of the following partial statement of Hamilton's original way of stripping 'imaginaries' of their silly, purely imaginary mystery. This simple thing was one of the steps which led Hamilton to his quaternions, although strictly it has nothing to do with them. It is the *method* and the *point of view* behind this ingenious recasting of the algebra of complex numbers which are of importance for the sequel.

If as usual i denotes $\sqrt{-1}$, a 'complex number' is a number of the type $a + bi$, where a,b are 'real numbers' or, if preferred, and more generally, elements of the field F defined by the above postulates. Instead of regarding $a + bi$ as one 'number', Hamilton conceived it as an *ordered couple* of 'numbers', and he designated this couple by writing it (a,b). He then proceeded to impose definitions of *sum* and *product* on these couples, as suggested by the *formal* rules of combination sublimated from the experience of algebraists in manipulating complex numbers *as if* the laws of common algebra did in fact hold for them. One advantage of this new way of approaching complex numbers was this: the definitions for sum and product of couples were seen to be *instances* of the general, abstract definitions of sum and product as in a field. Hence, if the consistency of the system defined by the postulates for a field is proved, the like follows, without further proof, for complex numbers and the usual rules by which they are combined. It will be sufficient to state the definitions of sum and product in Hamilton's theory of complex numbers considered as couples (a,b) (c,d), etc.

The *sum* of (a,b) and (c,d) is $(a + b, c + d)$; their *product* is $(ac - bd, ad + bc)$. In the last, the minus sign is as in a field; namely, the element x postulated in IV is denoted by $-a$. To the 0, 1 of a field correspond here the couples (0,0), (1,0). With these definitions it is easily verified that Hamilton's couples satisfy all the stated postulates for a field. But they also accord with the *formal* rules for manipulating complex numbers. Thus, to (a,b), (c,d) correspond respectively $a + bi$, $c + di$, and the formal 'sum' of these two is $(a + c) + i(b + d)$, to which corresponds the couple $(a + c, b + d)$. Again, formal multiplication of $a + bi$, $c + id$ gives $(ac - bd) + i(ad + bc)$, to which corresponds the couple $(ac - bd, ad + bc)$. If this sort of thing is new to any reader, it will repay a second inspection, as it is an example of the way in which modern mathematics eliminates mystery. So long as there is a shred of mystery attached to any concept that concept is not mathematical.

Having disposed of complex numbers by *couples*, Hamilton sought to extend his device to ordered *triples* and *quadruples*. Without some idea of what is sought to be accomplished such an undertaking is of course so vague as to be meaningless. Hamilton's object was to invent an algebra which would do for rotations in space of *three* dimensions what complex numbers, or his couples, do for rotations in space of *two* dimensions, both spaces being Euclidean as in elementary geometry. Now, a complex number $a + bi$ can be thought of as representing a *vector*, that is, a line segment having both *length and direction*, as is evident from the diagram, in which the directed segment (indicated by the arrow) represents the vector OP.



But on attempting to symbolize the behaviour of vectors in three-dimensional space so as to preserve those properties of vectors which are of use in physics, particularly in the combination of rotations, Hamilton was held up for years by an unforeseen difficulty whose very nature he for long did not even suspect. We may glance in passing at one of the clues he followed. That this led him anywhere – as he insisted it did – is all the more remarkable as it is now almost universally regarded as an absurdity, or at best a metaphysical speculation without foundation in history or in mathematical experience.

Objecting to the purely abstract, postulational formulation of algebra advocated by his British contemporaries, Hamilton sought to found algebra on something 'more real', and for this strictly meaningless enterprise he drew on his knowledge of Kant's mistaken notions – exploded by the creation of non-Euclidean geometry – of space as 'a pure form of sensuous intuition'. Indeed Hamilton, who seems to have been unacquainted with non-Euclidean geometry, followed Kant in believing that 'Time and space are two sources of knowledge from which various *a priori* synthetical cognitions can be derived. Of this, pure mathematics gives a splendid example in the case of our cognition of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetic propositions *a priori* possible.' Of course any not utterly illiterate mathematician to-day knows that Kant was mistaken in this conception of mathematics, but in the 1840's, when Hamilton was on his way to quaternions, the Kantian philosophy of mathematics still made sense to those – and they were nearly all – who had never heard of Lobatchewsky. By what looks like a bad mathematical pun, Hamilton applied the Kantian doctrine to algebra and drew the remarkable conclusion that, since geometry is the science of space, and since time and space are 'pure sensuous forms of intuition', therefore the rest of mathematics must belong to time, and he wasted much of his own time in elaborating the bizarre doctrine that *algebra is the science of pure time.*

This queer crotchet has attracted many philosophers, and quite recently it has been exhumed and solemnly dissected by owlish metaphysicians seeking the philosopher's stone in the gall bladder of mathematics. Just because 'algebra as the science of pure time' is of no earthly mathematical significance, it will continue to be discussed with animation till time itself ends. The opinion of a great mathematician on the 'pure time' aspect of algebra may be of interest. 'I cannot myself recognize the connexion of algebra with the notion of time,' Cayley confessed: 'granting that the notion of continuous progression presents itself and is of importance, I do not see that it is in any wise the fundamental notion of the science.'

Hamilton's difficulties in trying to construct an algebra of vectors and rotations for three-dimensional space were rooted in his subconscious conviction that the most important laws of common algebra must persist in the algebra he was seeking. How were vectors in three-dimensional space to be multiplied together?

To sense the difficulty of the problem it is essential to bear in mind (see Chapter on Gauss) that *ordinary complex numbers* $a + bi$ ($i = \sqrt{-1}$) had been given a simple interpretation in terms of *rotations in a plane*, and further that *complex numbers obey all the rules of common algebra*, in particular the *commutative law of multiplication*: if A, B are any complex numbers, then $A \times B = B \times A$, whether A, B are interpreted *algebraically*, *or in terms of rotations in a plane*. It was but human then to anticipate that *the same commutative law* would hold for the *generalizations of complex numbers* which represent *rotations in space of three dimensions*.

Hamilton's great discovery – or invention – was an algebra, one of the 'natural' algebras of rotations in space of three dimensions, in which the commutative law of multiplication does not hold. In this Hamiltonian algebra of *quaternions* (as he called his invention), a multiplication appears in which $A \times B$ is *not* equal to $B \times A$ but to *minus* $B \times A$, that is, $A \times B = -B \times A$.

That a consistent, practically useful system of algebra could be constructed in defiance of the commutative law of multiplication was a discovery of the first order, comparable, perhaps, to the conception of non-Euclidean geometry. Hamilton himself was so impressed by the magnitude of what suddenly dawned on his mind (after fifteen years of fruitless thought) one day (16 October 1843) when he was out walking with his wife that he carved the fundamental formulae of the new algebra in the stone of the bridge on which he found himself at the moment. His great invention showed algebraists the way to other algebras until to-day, following Hamilton's lead, mathematicians manufacture algebras practically at will by negating one or more of the postulates for a field and developing the consequences. Some of these 'algebras' are extremely useful; the general theories embracing swarms of them include Hamilton's great invention as a mere detail, although a highly important one.

In line with Hamilton's quaternions the numerous brands of *vector analysis* favoured by physicists of the past two generations sprang into being. To-day all of these, including quaternions, *so far as physical applications are concerned*, are being swept aside by the incomparably simpler and more general *tensor analysis* which came into vogue with general relativity in 1915. Something will be said about this later.

In the meantime it is sufficient to remark that Hamilton's deepest tragedy was neither alcohol nor marriage but his obstinate belief that quaternions held the key to the mathematics of the physical universe. History has shown that Hamilton tragically deceived himself when he insisted '... I still must assert that this discovery appears to me to be as important for the middle of the nineteenth century as the discovery of fluxions [the calculus] was for the close of the seventeenth.' Never was a great mathematician so hopelessly wrong.

The last twenty-two years of Hamilton's life were devoted almost exclusively to the elaboration of quaternions, including their application to dynamics, astronomy, and the wave theory of light, and his voluminous correspondence. The style of the overdeveloped *Elements of Quaternions*, published the year after Hamilton's death, shows plainly the effects of the author's mode of life. After his death from gout on 2 September 1865 in the sixty-first year of his age, it was found that Hamilton had left behind a mass of papers in indescribable confusion and about sixty huge manuscript books full of mathematics. An adequate edition of his works is now in progress. The state of his papers testified to the domestic difficulties under which the last third of his life had been lived: innumerable dinner plates with the remains of desiccated, unviolated chops were found buried in the mountainous piles of papers, and dishes enough to supply a large household were dug out from the confusion. During his last period Hamilton lived as a recluse, ignoring the meals shoved at him as he worked, obsessed by the dream that the last tremendous effort of his magnificent genius would immortalize both himself and his beloved Ireland, and stand forever unshaken as the greatest mathematical contribution to science since the *Principia* of Newton.

His early work, on which his imperishable glory rests, he came to regard as a thing of but little moment in the shadow of what he believed was his masterpiece. To the end he was humble and devout, and wholly without anxiety for his scientific reputation. 'I have very long admired Ptolemy's description of his great astronomical master, Hipparchus, as *ἀνήρ φιλόπονος καὶ φιλαληθής*; a labour-loving and truth-loving man. Be such my epitaph.'

CHAPTER TWENTY

GENIUS AND STUPIDITY

Galois

ABEL was done to death by poverty, Galois by stupidity. In all the history of science there is no completer example of the triumph of crass stupidity over untamable genius than is afforded by the all too brief life of Évariste Galois. The record of his misfortunes might well stand as a sinister monument to all self-assured pedagogues, unscrupulous politicians, and conceited academicians. Galois was no 'ineffectual angel', but even his magnificent powers were shattered before the massed stupidity aligned against him, and he beat his life out fighting one unconquerable fool after another.

The first eleven years of Galois' life were happy. His parents lived in the little village of Bourg-la-Reine, just outside Paris, where Évariste was born on 25 October 1811. Nicolas-Gabriel Galois, the father of Évariste, was a relic of the eighteenth century, cultivated, intellectual, saturated with philosophy, a passionate hater of royalty and an ardent lover of liberty. During the Hundred Days after Napoleon's escape from Elba, Galois was elected mayor of the village. After Waterloo he retained his office and served faithfully under the King, backing the villagers against the priest and delighting social gatherings with the old-fashioned rhymes which he composed himself. These harmless activities were later to prove the amiable man's undoing. From his father, Évariste acquired the trick of rhyming and a hatred of tyranny and baseness.

Until the age of twelve Galois had no teacher but his mother, Adélaïde-Marie Demante. Several of the traits of Galois' character were inherited from his mother, who came from a long line of distinguished jurists. Her father appears to have been somewhat of a Tartar. He gave his daughter a thorough classical and religious education, which she in turn passed on to her eldest son, not as she had received it, but fused into a virile stoicism in her own independent mind. She had not rejected Christianity, nor had she accepted it without question; she had merely contrasted its teachings with those of Seneca and Cicero, reducing all to their basic morality. Her friends remembered her as a woman of strong character with a mind of her own, generous, with a marked vein of originality, quizzical, and, at times, inclined to be paradoxical. She died in 1872 at the age of eighty-four. To the last she retained the full vigour of her mind. She, like her husband, hated tyranny.

There is no record of mathematical talent on either side of Galois' family. His own mathematical genius came on him like an explosion, probably at early adolescence. As a child he was affectionate and rather serious, although he entered readily enough into the gaiety of the recurrent celebrations in his father's honour, even composing rhymes and dialogues to entertain the guests. All this changed under the first stings of petty persecution and stupid misunderstanding, not by his parents, but by his teachers.

In 1823, at the age of twelve, Galois entered the lycée of Louis-le-Grand in Paris. It was his first school. The place was a dismal horror. Barred and grilled, and dominated by a provisor who was more of a political jailer than a teacher, the place looked like a prison, and it was. The France of 1823 still remembered the Revolution. It was a time of plots and counterplots, of riots and rumours of revolution. All this was echoed in the school. Suspecting the provisor of scheming to bring back the Jesuits, the students struck, refusing to chant in chapel. Without even notifying their parents the provisor expelled those whom he thought most guilty. They found themselves in the street. Galois was not among them, but it would have been better for him if he had been.

Till now tyranny had been a mere word to the boy of twelve. Now he saw it in action, and the experience warped one side of his character for life. He was shocked into unappeasable rage. His studies, owing to his mother's excellent instruction in the classics, went very well and he won prizes. But he had also gained something more lasting than any prize, the stubborn conviction, right or wrong, that neither fear nor the utmost severity of discipline can extinguish the sense of justice and fair dealing in young minds experiencing their first unselfish devotion. This his fellow students had taught him by their courage. Galois never forgot their example. He was too young not to be embittered.

The following year marked another crisis in the young boy's life. Docile interest in literature and the classics gave way to boredom; his mathematical genius was already stirring. His teachers advised that he be demoted. Évariste's father objected, and the boy continued with his interminable exercises in rhetoric, Latin, and Greek. His work was reported as mediocre, his conduct 'dissipated', and the teachers had their way. Galois was demoted. He was forced to lick up the stale leavings which his genius had rejected. Bored and disgusted he gave his work perfunctory attention and passed it without effort or interest. Mathematics was taught more or less as an aside to the serious business of digesting the classics, and the pupils of various grades and assorted ages took the elementary mathematical course at the convenience of their other studies.

It was during this year of acute boredom that Galois began mathematics in the regular school course. The splendid geometry of Legendre came his way. It is said that two years was the usual time required by even the better mathematicians among the boys to master Legendre. Galois read the geometry from cover to cover as easily as other boys read a pirate yarn. The book aroused his enthusiasm; it was no textbook written by a hack, but a work of art composed by a creative mathematician. A single reading sufficed to reveal the whole structure of elementary geometry in crystal clarity to the fascinated boy. He had mastered it.

His reaction to algebra is illuminating. It disgusted him, and for a very good reason when we consider what sort of mind Galois had. Here was no master like Legendre to inspire him. The text in algebra was a school book and nothing more. Galois contemptuously tossed it aside. It lacked, he said, the creator's touch that only a creative mathematician can give.

Having made the acquaintance of one great mathematician through his work, Galois took matters into his own hands. Ignoring the meticulous pettifogging of his teacher, Galois went directly for his algebra to the greatest master of the age, Lagrange. Later he read Abel. The boy of fourteen or fifteen absorbed masterpieces of algebraical analysis addressed to mature professional mathematicians – the memoirs on the numerical solution of equations, the theory of analytical functions, and the calculus of functions. His class work in mathematics was mediocre: the traditional course was trivial to a mathematical genius and not necessary for the mastering of real mathematics.

Galois' peculiar gift of being able to carry on the most difficult mathematical investigations almost entirely in his head helped him with neither teachers nor examiners. Their insistence upon details which to him were obvious or trivial exasperated him beyond endurance, and he frequently lost his temper. Nevertheless he carried off the prize in the general examination. To the amazement of teachers and students alike Galois had taken his own kingdom by assault while their backs were turned.

With this first realization of his tremendous power, Galois' character underwent a profound change. Knowing his kinship to the great masters of algebraical analysis he felt an immense pride and longed to rush on to the front rank to match his strength with theirs. His family – even his unconventional mother – found him strange. At school he seems to have inspired a curious mixture of fear and anger in the minds of his teachers and fellow students. His teachers were good men and patient, but they were stupid, and to Galois stupidity was the unpardonable sin. At the beginning of the year they had reported him as 'very gentle, full of innocence and good qualities, *but* –' And they went on to say that 'there is something strange about him.' No doubt there was. The boy had unusual brains. A little later they admit that he is not 'wicked', but merely 'original and queer', 'argumentative', and they complain that he delights to tease his comrades. All very reprehensible, no doubt, but they might have used their eyes. The boy had discovered mathematics and he was already being driven by his daemon. By the end of the year of awakening we learn that 'his queerness has alienated him from all his companions', and his teachers observe 'something secret in his character'. Worse, they accuse him of '*affecting* ambition and originality'. But it is admitted by some that Galois is good in mathematics. His rhetoric teachers indulge in a little classical sarcasm: 'His cleverness is now a legend that we cannot credit.' They rail that there is only slovenliness and eccentricity in his assigned tasks – when he deigns to pay any attention to them – and that he goes out of his way to weary his teachers by incessant 'dissipation'. The last does not refer to vice, because Galois had no viciousness in him. It is merely a strong word to describe the heinous inability of a mathematical genius of the first rank to squander his intellect on the futilities of rhetoric as expounded by pedants.

One man, to the everlasting credit of his pedagogical insight, declared that Galois was as able in literary studies as he was in mathematics. Galois appears to have been touched by this man's kindness. He promised to give rhetoric a chance. But his mathematical devil was now fully aroused and raging to get out, and poor Galois fell from grace. In a short time the dissenting teacher joined the majority and made the vote unanimous. Galois, he sadly admitted, was beyond salvation, 'conceited with an insufferable affectation of originality'. But the pedagogue redeemed himself by one excellent, exasperated suggestion. Had it been followed, Galois might have lived to eighty. 'The mathematical madness dominates this boy. I think his parents had better let him take only mathematics. He is wasting his time here, and all he does is to torment his teachers and get into trouble.'

At the age of sixteen Galois made a curious mistake. Unaware that Abel at the beginning of his career had convinced himself that he had done the impossible and had solved the general equation of the fifth degree, Galois repeated the error. For a time – a very short time, however – he believed that he had done what cannot be done. This is merely one of several extraordinary similarities in the careers of Abel and Galois.

While Galois at the age of sixteen was already well started on his career of fundamental discovery, his mathematical teacher – Vernier – kept fussing over him like a hen that has hatched an eaglet and does not know how to keep the unruly creature's feet on the good dirt of the barnyard. Vernier implored Galois to work systematically. The advice was ignored and Galois, without preparation, took the competitive examinations for entrance to the École Polytechnique. This great school, the mother of French mathematicians, founded during the French Revolution (some say by Monge), to give civil and military engineers the best scientific and mathematical education available anywhere in the world, made a double appeal to the ambitious Galois. At the Polytechnique his mathematical talent would be recognized and encouraged to the utmost. And his craving for liberty and freedom of utterance would be gratified; for were not the virile, audacious young Polytechnicians, among them the future leaders of the army, always a thorn in the side of reactionary schemers who would undo the glorious work of the Revolution and bring back the corrupt priesthood and the divine right of kings? The fearless Polytechnicians, at least in Galois' boyish eyes, were no race of puling rhetoricians like the browbeaten nonentities at Louis-le-Grand, but a consecrated band of young patriots. Events were presently to prove him at least partly right in his estimate.

Galois failed in the examinations. He was not alone in believing his failure the result of a stupid injustice. The comrades he had teased unmercifully were stunned. They believed that Galois had mathematical genius of the highest order and they suspected his examiners of incompetence in their office. Nearly a quarter of a century later Terquem, editor of the *Nouvelles Annales de Mathématiques*, the mathematical journal devoted to the interests of candidates for the Polytechnique and Normale schools, reminded his readers that the controversy was not yet dead. Commenting on the failure of Galois and on the inscrutable decrees of the examiners in another instance, Terquem remarks, 'A candidate of superior intelligence is lost with an examiner of inferior intelligence. *Hic ego barbarus sum quia non intelligor illis* [Because *they* don't understand *me*, *I* am a barbarian.] ... Examinations are mysteries before which I bow. Like the mysteries of theology, the reason must admit them with humility, without seeking to understand them.' As for Galois, the failure was almost the finishing touch. It drove him in upon himself and embittered him for life.

In 1828 Galois was seventeen. It was his great year. For the first time he met a man who had the capacity to understand his genius, Louis-Paul-Émile Richard (1795–1849), teacher of advanced mathematics (*mathématiques spéciales*) at Louis-le-Grand. Richard was no conventional pedagogue, but a man of talent who followed the advanced lectures on geometry at the Sorbonne in his spare time and kept himself abreast of the progress of living mathematicians to pass it on to his pupils. Timid and unambitious on his own account, he threw all his talent on the side of his pupils. The man who would not go a step out of his way to advance his own interests counted no sacrifice too great where the future of one of his students was at stake. In his zeal to advance mathematics through the work of abler men he forgot himself completely, although his scientific friends urged him to write, and to his inspired teaching more than one outstanding French mathematician of the nineteenth century has paid grateful tribute: Leverrier, co-discoverer with Adams by pure mathematical analysis of the planet Neptune; Serret, a geometer of repute and author of a classic on higher algebra in which he gave the first systematic exposition of Galois' theory of equations; Hermite, master algebraist and arithmetician of the first rank; and last, Galois.

Richard recognized instantly what had fallen into his hands – 'the Abel of France'. The original solutions to difficult problems which Galois handed in were proudly explained to the class, with just praise for the young author, and Richard shouted from the housetops that this extraordinary pupil should be admitted to the Polytechnique without examination. He gave Galois the first prize and wrote in his term report, 'This pupil has a marked superiority above all his fellow students; he works only at the most advanced parts of mathematics.' All of which was the literal truth. Galois at seventeen was making discoveries of epochal significance in the theory of equations, discoveries whose consequences are not yet exhausted after more than a century. On 1 March 1829, Galois published his first paper, on continued fractions. This contains no hint of the great things he had done, but it served to announce him to his fellow students as no mere scholar but an inventive mathematician.

The leading French mathematician of the time was Cauchy. In fertility of invention Cauchy has been equalled by but few; and as we have seen, the mass of his collected works is exceeded in bulk only by the outputs of Euler and Cayley,* the most prolific mathematicians of history. Whenever the Academy of Sciences wished an authoritative opinion on the merits of a mathematical work submitted for its consideration it called upon Cauchy. As a rule he was a prompt and just referee. But occasionally he lapsed. Unfortunately the occasions of his lapses were the most important of all. To Cauchy's carelessness mathematics is indebted for two of the major disasters in its history: the neglect of Galois and the shabby treatment of Abel. For the latter Cauchy was only partly to blame, but for the inexcusable laxity in Galois' case Cauchy alone is responsible.

Galois had saved the fundamental discoveries he had made up to the age of seventeen for a memoir to be submitted to the Academy. Cauchy promised to present this, but he forgot. To put the finishing touch to his ineptitude he lost the author's abstract. That was the last Galois ever heard of Cauchy's generous promise. This was only the first of a series of similar disasters which fanned the thwarted boy's sullen contempt of academies and academicians into a fierce hate against the whole of the stupid society in which he was condemned to live.

In spite of his demonstrated genius the harassed boy was not even now left to himself at school. The authorities gave him no peace to harvest the rich field of his discoveries, but pestered him to distraction with petty tasks and goaded him to open revolt by their everlasting preachings and punishments. Still

* That is, so far as actually published work is concerned up to 1936. Euler undoubtedly will surpass Cayley in bulk when the full edition of his works is finally printed.

they could find nothing in him but conceit and an iron determination to be a mathematician. He already was one, but they did not know it.

Two further disasters in his eighteenth year put the last touches to Galois' character. He presented himself a second time for the entrance examinations at the Polytechnique. Men who were not worthy to sharpen his pencils sat in judgement on him. The result was what might have been anticipated. Galois failed. It was his last chance; the doors of the Polytechnique were closed forever against him.

That examination has become a legend. Galois' habit of working almost entirely in his head put him at a serious disadvantage before a blackboard. Chalk and erasers embarrassed him – till he found a proper use for one of them. During the oral part of the examination one of the inquisitors ventured to argue a mathematical difficulty with Galois. The man was both wrong and obstinate. Seeing all his hopes and his whole life as a mathematician and polytechnic champion of democratic liberty slipping away from him, Galois lost all patience. He knew that he had officially failed. In a fit of rage and despair he hurled the eraser at his tormentor's face. It was a hit.

The final touch was the tragic death of Galois' father. As the mayor of Bourg-la-Reine the elder Galois was a target for the clerical intrigues of the times, especially as he had always championed the villagers against the priest. After the stormy elections of 1827 a resourceful young priest organized a scurrilous campaign against the mayor. Capitalizing the mayor's well-known gift for versifying, the ingenious priest composed a set of filthy and stupid verses against a member of the mayor's family, signed them with Mayor Galois' name, and circulated them freely among the citizens. The thoroughly decent mayor developed a persecution mania. During his wife's absence one day he slipped off to Paris and, in an apartment but a stone's throw from the school where his son sat at his studies, committed suicide. At the funeral serious disorder broke out. Stones were hurled by the enraged citizens; a priest was gashed on the forehead. Galois saw his father's coffin lowered into the grave in the midst of an unseemly riot. Thereafter, suspecting everywhere the injustice which he hated, he could see no good in anything.

After his second failure at the Polytechnique, Galois returned to school to prepare for a teaching career. The school now had a new director, a time-serving, somewhat cowardly stool-pigeon for the royalists and clerics. This man's shilly-shally temporizing in the political upheaval which was presently to shake France to its foundations had a tragic influence on Galois' last years.

Still persecuted and maliciously misunderstood by his preceptors, Galois prepared himself for the final examinations. The comments of his examiners are interesting. In mathematics and physics he got 'very good'. The final oral examination drew the following comments: 'This pupil is sometimes obscure in expressing his ideas, but he is intelligent and shows a remarkable spirit of research. He has communicated to me some new results in applied analysis.' In literature: 'This is the only student who has answered me poorly; he knows absolutely nothing. I was told that this student has an extraordinary capacity for mathematics. This astonishes me greatly; for, after his examination, I believed him to have but little intelligence. He succeeded in hiding such as he had from me. If this pupil is really what he has seemed to me to be, I seriously doubt whether he will ever make a good teacher.' To which Galois, remembering some of his own good teachers, might have replied, 'God forbid.'

In February 1830, at the age of nineteen, Galois was definitely admitted to university standing. Again his sure knowledge of his own transcendent ability was reflected in a withering contempt for his plodding teachers and he continued to work in solitude on his own ideas. During this year he composed three papers in which he broke new ground. These papers contain some of his great work on the theory of algebraic equations. It was far in advance of anything that had been done, and Galois had hopefully submitted it all (with further results) in a memoir to the Academy of Sciences, in competition for the Grand Prize in Mathematics. This prize was still the blue ribbon in mathematical research; only the foremost mathematicians of the day could sensibly compete. Experts agree that Galois' memoir was more than worthy of the prize. It was work of the highest originality. As Galois said with perfect justice, 'I have carried out researches which will halt many savants in theirs.'

The manuscript reached the Secretary safely. The Secretary took it home with him for examination, but died before he had time to look at it. When his papers were searched after his death no trace of the manuscript was found, and that was the last Galois ever heard of it. He can scarcely be blamed for ascribing his misfortunes to something less uncertain than blind chance. After Cauchy's lapse a repetition of the same sort of thing looked too providential to be a mere accident. 'Genius', he said, 'is condemned by a malicious social organization to an eternal denial of justice in favour of fawning mediocrity.' His hatred grew, and he flung himself into politics on the side of republicanism, then a forbidden radicalism.

The first shots of the revolution of 1830 filled Galois with joy. He tried to lead his fellow students into the fray, but they hung back, and the temporizing director put them on their honour not to quit the school. Galois refused to pledge his word, and the director begged him to stay in till the following day. In his speech the director displayed a singular lack of tact and a total absence of common sense. Enraged, Galois tried to escape during the night, but the wall was too high for him. Thereafter, all through 'the glorious three days' while the heroic young Polytechnicians were out in the streets making history, the director prudently kept his charges under lock and key. Whichever way the cat should jump the director was prepared to jump with it. The revolt successfully accomplished, the astute director very generously placed his pupils at the disposal of the temporary government. This put the finishing touch to Galois' political creed. During the vacation he shocked his family and boyhood friends with his fierce championship of the rights of the masses.

The last months of 1830 were as turbulent as is usual after a thorough political stir-up. The dregs sank to the bottom, the scum rose to the top, and suspended between the two the moderate element of the population hung in indecision. Galois, back at college, contrasted the time-serving vacillations of the director and the wishy-washy loyalty of the students with their exact opposites at the Polytechnique. Unable to endure the humiliation of inaction longer he wrote a blistering letter to the *Gazette des Écoles* in which he let both students and director have what he thought was their due. The students could have saved him. But they lacked backbone, and Galois was expelled. Incensed, Galois wrote a second letter to the *Gazette*, addressed to the students. 'I ask nothing of you for myself', he wrote: 'but speak out for your honour and according to your conscience.' The letter was unanswered, for the apparent reason that those to whom Galois appealed had neither honour nor conscience.

Foot-loose now, Galois announced a private class in higher algebra, to meet once a week. Here he was at nineteen, a creative mathematician of the very first rank, peddling lessons to no takers. The course was to have included 'a new theory of imaginaries [what is now known as the theory of "Galois Imaginaries", of great importance in algebra and the theory of numbers]; the theory of the solution of equations by radicals, and the theory of numbers and elliptic functions treated by pure algebra' – all his own work.

Finding no students, Galois temporarily abandoned mathematics and joined the artillery of the National Guard, two of whose four battalions were composed almost wholly of the liberal group calling themselves 'Friends of the People'. He had not yet given up mathematics entirely. In one last desperate effort to gain recognition, encouraged by Poisson, he had sent a memoir on the general solution of equations – now called the 'Galois theory' – to the Academy of Sciences. Poisson, whose name is remembered wherever the mathematical theories of gravitation, electricity, and magnetism are studied, was the referee. He submitted a perfunctory report. The memoir, he said was 'incomprehensible', but he did not state how long it had taken him to reach his remarkable conclusion. This was the last straw. Galois devoted all his energies to revolutionary politics. 'If a carcase is needed to stir up the people', he wrote, 'I will donate mine.'

The ninth of May 1831 marked the beginning of the end. About 200 young republicans held a banquet to protest against the royal order disbanding the artillery which Galois had joined. Toasts were drunk to the Revolutions of 1789 and 1793, to Robespierre, and to the Revolution of 1830. The whole atmosphere of the gathering was revolutionary and defiant. Galois rose to propose a toast, his glass in one hand, his open pocket knife in the other: 'To Louis Philippe' – the King. His companions misunderstood the purpose of the toast and whistled him down. Then they saw the open knife. Interpreting this as a threat against the life of the King, they howled their approval. A friend of Galois, seeing the great Alexandre Dumas and other notables passing by the open windows, implored Galois to sit down, but the uproar continued. Galois was the hero of the moment, and the artillerists adjourned to the street to celebrate their exuberance by dancing all night. The following day Galois was arrested at his mother's house and thrown into the prison of Sainte-Pélagie.

A clever lawyer, with the help of Galois' loyal friends, devised an ingenious defence, to the effect that Galois had really said: 'To Louis Philippe, *if he turns traitor*.' The open knife was easily explained; Galois had been using it to cut his chicken. This was the fact. The saving clause in his toast, according to his friends who swore they had heard it, was drowned by the whistling, and only those close to the speaker caught what was said. Galois would not claim the saving clause.

During the trial Galois' demeanour was one of haughty contempt for the court and his accusers. Caring nothing for the outcome, he launched into an impassioned tirade against all the forces of political injustice. The judge was a human being with children of his own. He warned the accused that he was not helping his own case and sharply silenced him. The prosecution quibbled over the point whether the restaurant where the incident occurred was or was not a 'public place' when used for a semi-private banquet. On this nice point of law hung the liberty of Galois. But it was evident that both court and jury were moved by the youth of the accused. After only ten minutes' deliberation the jury returned a verdict of not guilty. Galois picked up his knife from the evidence table, closed it, slipped it in his pocket, and left the court-room without a word.

He did not keep his freedom long. In less than a month, on 14 July 1831, he was arrested again, this time as a precautionary measure. The republicans were about to hold a celebration, and Galois, being a 'dangerous radical' in the eyes of the authorities, was locked up *on no charge whatever*. The government papers of all France played up this brilliant coup of the police. They now had 'the dangerous *republican*, Évariste Galois', where he could not possibly start a revolution. But they were hard put to it to find a legal accusation under which he could be brought to trial. True, he had been armed to the teeth when arrested, but he had not resisted arrest. Galois was no fool. Should they accuse him of plotting against the Government? Too strong; it wouldn't go; no jury would convict. Ah! After two months of incessant thought they succeeded in trumping up a charge. When arrested Galois had been wearing his artillery uniform. But the artillery had been disbanded. Therefore Galois was guilty of illegally wearing a uniform. This time they convicted him. A friend, arrested with him, got three months; Galois got six. He was to be incarcerated in Sainte-Pélagie till 29 April 1832. His sister said he looked about fifty years old at the prospect of the sunless days ahead of him. Why not? 'Let justice prevail though the heavens fall.'

Discipline in the jail for political prisoners was light, and they were treated with reasonable humanity. The majority spent their waking hours promenading in the courtyard reserved for their use, or boozing in the canteen – the private graft of the governor of the prison. Soon Galois, with his sombre visage, abstemious habits, and perpetual air of intense concentration, became the butt of the jovial swillers. He was concentrating on his mathematics, but he could not help hearing the taunts hurled at him.

'What! You drink only water? Quit the Republican Party and go back to your mathematics.' – 'Without wine and women you'll never be a man.' Goaded beyond endurance Galois seized a bottle of brandy, not knowing or caring what it was, and drank it down. A decent fellow prisoner took care of him till he recovered. His humiliation when he realized what he had done devastated him.

At last he escaped from what one French writer of the time calls the foulest sewer in Paris. The cholera epidemic of 1832 caused the solicitous authorities to transfer Galois to a hospital on 16 March. The 'important political prisoner' who had threatened the life of Louis Philippe was too precious to be exposed to the epidemic.

Galois was put on parole, so he had only too many occasions to see outsiders. Thus it happened that he experienced his one and only love affair. In this, as in everything else, he was unfortunate. Some worthless girl ('*quelque coquette de bas étage*') initiated him. Galois took it violently and was disgusted with love, with himself, and with his girl. To his devoted friend Auguste Chevalier he wrote, 'Your letter, full of apostolic unction, has brought me a little peace. But how obliterate the mark of emotions as violent as those which I have experienced? ... On re-reading your letter, I note a phrase in which you accuse me of being inebriated by the putrefied slime of a rotten world which has defiled my heart, my head, and my hands. .. Inebriation! I am disillusioned of everything, even love and fame. How can a world which I detest defile me?' This is dated 25 May 1832. Four days later he was at liberty. He had planned to go into the country to rest and meditate.

What happened on 29 May is not definitely known. Extracts from two letters suggest what is usually accepted as the truth: Galois had run foul of political enemies immediately after his release. These 'patriots' were always spoiling for a fight, and it fell to the unfortunate Galois' lot to accommodate them in an affair of 'honour'. In a 'Letter to All Republicans,' dated 29 May 1832, Galois writes:

> I beg patriots and my friends not to reproach me for dying otherwise than for my country. 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, die for something so despicable! ... Pardon for those who have killed me, they are of good faith.

In another letter to two unnamed friends :

> I have been challenged by two patriots – it was impossible for me to refuse. I beg your pardon for having advised neither of you. But my opponents had put me on my honour not to warn any patriot. Your task is very simple: prove that I fought in spite of myself, that is to say after having exhausted every means of accommodation. ... Preserve my memory since fate has not given me life enough for my country to know my name. I die your friend
>
> E. GALOIS.

These were the last words he wrote. All night, before writing these letters, he had spent the fleeting hours feverishly dashing off his scientific last will and testament, writing against time to glean a few of the great things in his teeming mind before the death which he foresaw could overtake him. Time after time he broke off to scribble in the margin 'I have not time; I have not time,' and passed on to the next frantically scrawled outline. What he wrote in those desperate last hours before the dawn will keep generations of mathematicians busy for hundreds of years. He had found, once and for all, the true solution of a riddle which had tormented mathematicians for centuries: under what conditions can an equation be solved? But this was only one thing of many. In this great work, Galois used the theory of groups (see chapter on Cauchy) with brilliant success. Galois was indeed one of the great pioneers in this abstract theory, to-day of fundamental importance in all mathematics.

In addition to this distracted letter Galois entrusted his scientific executor with some of the manuscripts which had been intended for the Academy of Sciences. Fourteen years later, in 1846, Joseph Liouville edited some of the manuscripts for the *Journal de Mathématiques pures et appliquées*. Liouville, himself a distinguished and original mathematician, and editor of the great *Journal*, writes as follows in his introduction:

'The principal work of Évariste Galois has as its object the conditions of solvability of equations by radicals. The author lays the foundations of a general theory which he applies in detail to equations whose degree is a prime number. At the age of sixteen, and while a student at the college of Louis-le-Grand ... Galois occupied himself with this difficult subject.' Liouville then states that the referees at the Academy had rejected Galois' memoirs on account of their obscurity. He continues: 'An exaggerated desire for conciseness was the cause of this defect which one should strive above all else to avoid when treating the abstract and mysterious matters of pure Algebra. Clarity is, indeed, all the more necessary when one essays to lead the reader farther from the beaten path and into wilder territory. As Descartes said, "When transcendental questions are under discussion be transcendentally clear." Too often Galois neglected this precept; and we can understand how illustrious mathematicians may have judged it proper to try, by the harshness of their sage advice, to turn a beginner, full of genius but inexperienced, back on the right road. The author they censured was before them, ardent, active; he could profit by their advice.

'But now everything is changed. Galois is no more! Let us not indulge in useless criticisms; let us leave the defects there and look at the merits.' Continuing, Liouville tells how he studied the manuscripts, and singles out one perfect gem for special mention.

'My zeal was well rewarded, and I experienced an intense pleasure at the moment when, having filled in some slight gaps, I saw the complete correctness of the method by which Galois proves, in particular, this beautiful theorem: *In order that an irreducible equation of prime degree be solvable by radicals it is necessary and sufficient that all its roots be rational functions of any two of them.*' *

Galois addressed his will to his faithful friend Auguste Chevalier, to whom the world owes its preservation. 'My dear friend', he began, 'I have made some new discoveries in analysis.' He then proceeds to outline such as he has time for. They were epoch-making. He concludes: '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

* The significance of this theorem will be clear if the reader will glance through the extracts from Abel in Chapter 17.

people who will find it to their advantage to decipher all this mess. *Je t'embrasse avec effusion.* E. Galois.'

Confiding Galois! Jacobi was generous; what would Gauss have said? What did he say of Abel? What did he omit to say of Cauchy, or of Lobatchewsky? For all his bitter experience Galois was still a hopeful boy.

At a very early hour on 13 May 1832, Galois confronted his adversary on the 'field of honour'. The duel was with pistols at twenty-five paces. Galois fell, shot through the intestines. No surgeon was present. He was left lying where he had fallen. At nine o'clock a passing peasant took him to the Cochin Hospital. Galois knew he was about to die. Before the inevitable peritonitis set in, and while still in the full possession of his faculties, he refused the offices of a priest. Perhaps he remembered his father. His young brother, the only one of his family who had been warned, arrived in tears. Galois tried to comfort him with a show of stoicism. 'Don't cry', he said, 'I need all my courage to die at twenty.'

Early in the morning of 31 May 1832 Galois died, being then in the twenty-first year of his age. He was buried in the common ditch of the South Cemetery, so that to-day there remains no trace of the grave of Évariste Galois. His enduring monument is his collected works. They fill sixty pages.

CHAPTER TWENTY-ONE

INVARIANT TWINS

Cayley ; Sylvester

*

'It is difficult to give an idea of the vast extent of modern mathematics. The word "extent" is not the right one: I mean extent crowded with beautiful detail – not an extent of mere uniformity such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through and studied in every detail of hillside and valley, stream, rock, wood, and flower. But, as for everything else, so for a mathematical theory – beauty can be perceived but not explained.'

These words from Cayley's presidential address in 1883 to the British Association for the Advancement of Science might well be applied to his own colossal output. For prolific inventiveness Euler, Cauchy, and Cayley are in a class by themselves, with Poincaré (who died younger than any of the others) a far second. This applies only to the bulk of these men's work; its quality is another matter, to be judged partly by the frequency with which the ideas originated by these giants recur in mathematical research, partly by mere personal opinion, and partly by national prejudice.

Cayley's remarks about the vast extent of modern mathematics suggest that we confine our attention to some of those features of his own work which introduced distinctly new and far-reaching ideas. The work on which his greatest fame rests is in the theory of invariants and what grew naturally out of that vast theory of which he, brilliantly sustained by his friend Sylvester, was the originator and unsurpassed developer. The concept of invariance is of great importance for modern physics, particularly in the theory of relativity, but this is not its chief claim to attention. Physical theories are notoriously subject to revision and rejection; the theory of invariance as a permanent addition to pure mathematical thought appears to rest on firmer ground.

Another of the ideas originated by Cayley, that of the geometry of 'higher space' (space of *n* dimensions) is likewise of present scientific significance but of incomparably greater importance as pure mathematics. Similarly for the theory of matrices, again an invention of Cayley's. In non-Euclidean geometry Cayley prepared the way for Klein's splendid discovery that the geometry of Euclid and the non-Euclidean geometries of Lobatchewsky and Riemann are, all three, merely different aspects of a more general kind of geometry which includes them as special cases. The nature of these contributions of Cayley's will be briefly indicated after we have sketched his life and that of his friend Sylvester.

The lives of Cayley and Sylvester should be written simultaneously, if that were possible. Each is a perfect foil to the other, and the life of each, in large measure, supplies what is lacking in that of the other. Cayley's life was serene; Sylvester, as he himself bitterly remarks, spent much of his spirit and energy 'fighting the world'. Sylvester's thought was at times as turbulent as a millrace; Cayley's was always strong, steady, and unruffled. Only rarely did Cayley permit himself the printed expression of anything less severe than a precise mathematical statement – the simile quoted at the beginning of this chapter is one of the rare exceptions; Sylvester could hardly talk about mathematics without at once becoming almost orientally poetic, and his unquenchable enthusiasm frequently caused him to go off half-cocked. Yet these two became close friends and inspired one another to some of the best work that either of them did, for example in the theories of invariants and matrices (described later).

With two such temperaments it is not surprising that the course of friendship did not always run smoothly. Sylvester was frequently on the point of exploding; Cayley sat serenely on the safety valve, confident that his excitable friend would presently cool down, when he would calmly resume whatever they had been discussing as if Sylvester had never blown off, while

Sylvester for his part ignored his hot-headed indiscretion – till he got himself all steamed up for another. In many ways this strangely congenial pair were like a honeymoon couple, except that one party to the friendship never lost his temper. Although Sylvester was Cayley's senior by seven years, we shall begin with Cayley. Sylvester's life breaks naturally into the calm stream of Cayley's like a jagged rock in the middle of a deep river.

Arthur Cayley was born on 16 August 1821 at Richmond, Surrey, the second son of his parents, then residing temporarily in England. On his father's side Cayley traced his descent back to the days of the Norman Conquest (1066) and even before, to a baronial estate in Normandy. The family was a talented one which, like the Darwin family, should provide much suggestive material for students of heredity. His mother was Maria Antonia Doughty, by some said to have been of Russian origin. Cayley's father was an English merchant engaged in the Russian trade; Arthur was born during one of the periodical visits of his parents to England.

In 1829, when Arthur was eight, the merchant retired, to live thenceforth in England. Arthur was sent to a private school at Blackheath and later, at the age of fourteen, to King's College School in London. His mathematical genius showed itself very early. The first manifestations of superior talent were like those of Gauss; young Cayley developed an amazing skill in long numerical calculations which he undertook for amusement. On beginning the formal study of mathematics he quickly outstripped the rest of the school. Presently he was in a class by himself, as he was later when he went up to the University, and his teachers agreed that the boy was a born mathematician who should make mathematics his career. In grateful contrast to Galois' teachers, Cayley's recognized his ability from the beginning and gave him every encouragement. At first the retired merchant objected strongly to his son's becoming a mathematician but finally, won over by the Principal of the school, gave his consent, his blessing, and his money. He decided to send his son to Cambridge.

Cayley began his university career at the age of seventeen at

Trinity College, Cambridge. Among his fellow students he passed as 'a mere mathematician' with a queer passion for novel-reading. Cayley was indeed a lifelong devotee of the somewhat stilted fiction, now considered classical, which charmed readers of the 1840's and '50's. Scott appears to have been his favourite, with Jane Austen a close second. Later he read Thackeray and disliked him; Dickens he could never bring himself to read. Byron's tales in verse excited his admiration, although his somewhat puritanical Victorian taste rebelled at the best of the lot and he never made the acquaintance of that diverting scapegrace Don Juan. Shakespeare's plays, especially the comedies, were a perpetual delight to him. On the more solid – or stodgier – side he read and re-read Grote's interminable *History of Greece* and Macaulay's rhetorical *History of England.* Classical Greek, acquired at school, remained a reading-language for him all his life; French he read and wrote as easily as English, and his knowledge of German and Italian gave him plenty to read after he had exhausted the Victorian classics (or they had exhausted him). The enjoyment of solid fiction was only one of his diversions; others will be noted as we go.

By the end of his third year at Cambridge Cayley was so far in front of the rest in mathematics that the head examiner drew a line under his name, putting the young man in a class by himself 'above the first'. In 1842, at the age of twenty-one, Cayley was senior wrangler in the mathematical tripos, and in the same year he was placed first in the yet more difficult test for Smith's prize.

Under an excellent plan Cayley was now in line for a fellowship which would enable him to do as he pleased for a few years. He was elected Fellow of Trinity and assistant tutor for a period of three years. His appointment might have been renewed had he cared to take holy orders, but although Cayley was an orthodox Church of England Christian he could not quite stomach the thought of becoming a parson to hang on to his job or to obtain a better one – as many did, without disturbing either their faith or their conscience.

His duties were light almost to the point of non-existence.

He took a few pupils, but not enough to hurt either himself or his work. Making the best possible use of his liberty he continued the mathematical researches which he had begun as an undergraduate. Like Abel, Galois, and many others who have risen high in mathematics, Cayley went to the masters for his inspiration. His first work, published in 1841 when he was an undergraduate of twenty, grew out of his study of Lagrange and Laplace.

With nothing to do but what he wanted to do after taking his degree Cayley published eight papers the first year, four the second, and thirteen the third. These early papers by the young man who was not yet twenty-five when the last of them appeared map out much of the work that is to occupy him for the next fifty years. Already he has begun the study of geometry of n dimensions (which he originated), the theory of invariants, the enumerative geometry of plane curves, and his distinctive contributions to the theory of elliptic functions.

During this extremely fruitful period he was no mere grind. In 1843, when he was twenty-two, and occasionally thereafter till he left Cambridge at the age of twenty-five, he escaped to the Continent for delightful vacations of tramping, mountaineering, and water-colour sketching. Although he was slight and frail in appearance he was tough and wiry, and often after a long night spent in tramping over hilly country, would turn up as fresh as the dew for breakfast and ready to put in a few hours at his mathematics. During his first trip he visited Switzerland and did a lot of mountaineering. Thus began another lifelong passion. His description of the 'extent of modern mathematics' is no mere academic exercise by a professor who had never climbed a mountain or rambled lovingly over a tract of beautiful country, but the accurate simile of a man who had known nature intimately at first hand.

During the last four months of his first vacation abroad he became acquainted with northern Italy. There began two further interests which were to solace him for the rest of his life: an understanding appreciation of architecture and a love of good painting. He himself delighted in water-colours, in which he showed marked talent. With his love of good literature, travel, painting, and architecture, and with his deep understanding of natural beauty, he had plenty to keep him from degenerating into the 'mere mathematician' of conventional literature – written for the most part by people who may indeed have known some pedantic college professor of mathematics, but who never in their lives saw a real mathematician in the flesh.

In 1846, when he was twenty-five, Cayley left Cambridge. No position as a mathematician was open to him unless possibly he could square his conscience to the formality of 'holy orders'. As a mathematician Cayley felt no doubt that it would be easier to square the circle. Anyhow, he left. The law, which with the India Civil Service has absorbed much of England's most promising intellectual capital at one time or another, now attracted Cayley. It is somewhat astonishing to see how many of England's leading barristers and judges in the nineteenth century were high wranglers in the Cambridge tripos, but it does not follow, as some have claimed, that a mathematical training is a good preparation for the law. What seems less doubtful is that it may be a social imbecility to put a young man of Cayley's demonstrated mathematical genius to drawing up wills, transfers, and leases.

Following the usual custom of those looking toward an English legal career of the more gentlemanly grade (that is, above the trade of solicitor), Cayley entered Lincoln's Inn to prepare himself for the Bar. After three years as a pupil of a Mr Christie, Cayley was called to the Bar in 1849. He was then twenty-eight. On being admitted to the Bar, Cayley made a wise resolve not to let the law run off with his brains. Determined not to rot, he rejected more business than he accepted. For fourteen mortal years he stuck it, making an ample living and deliberately turning away the opportunity to smother himself in money and the somewhat blathery sort of renown that comes to prominent barristers, in order that he might earn enough, but no more than enough, to enable him to get on with his work.

His patience under the deadening routine of dreary legal business was exemplary, almost saintly, and his reputation in his branch of the profession (conveyancing) rose steadily. It is even recorded that his name has passed into one of the law books in connexion with an exemplary piece of legal work he did. But it is extremely gratifying to record also that Cayley was no milk-and-water saint but a normal human being who could, when the occasion called for it, lose his temper. Once he and his friend Sylvester were animatedly discussing some point in the theory of invariants in Cayley's office when the boy entered and handed Cayley a large batch of legal papers for his perusal. A glance at what was in his hands brought him down to earth with a jolt. The prospect of spending days straightening out some petty muddle to save a few pounds to some comfortable client's already plethoric income was too much for the man with real brains in his head. With an exclamation of disgust and a contemptuous reference to the 'wretched rubbish' in his hands, he hurled the stuff to the floor and went on talking mathematics. This, apparently, is the only instance on record when Cayley lost his temper. Cayley got out of the law at the first opportunity – after fourteen years of it. But during his period of servitude he had published between 200 and 300 mathematical papers, many of which are now classic.

As Sylvester entered Cayley's life during the legal phase we shall introduce him here.

James Joseph – to give him first the name with which he was born – was the youngest of several brothers and sisters, and was born of Jewish parents on 3 September 1814 in London. Very little is known of his childhood, as Sylvester appears to have been reticent about his early years. His eldest brother emigrated to the United States, where he took the name of Sylvester, an example followed by the rest of the family. But why an orthodox Jew should have decorated himself with a name favoured by Christian popes hostile to Jews is a mystery. Possibly that eldest brother had a sense of humour; anyhow, plain James Joseph, son of Abraham Joseph, became henceforth and for evermore James Joseph Sylvester.

Like Cayley's, Sylvester's mathematical genius showed itself early. Between the ages of six and fourteen he attended private schools. The last five months of his fourteenth year were spent at the University of London, where he studied under De Morgan. In a paper written in 1840 with the somewhat mystical title *On the Derivation of Coexistence*, Sylvester says 'I am indebted for this term [recurrents] to Professor De Morgan, whose pupil I may boast to have been'.

In 1829, at the age of fifteen, Sylvester entered the Royal Institution at Liverpool, where he stayed less than two years. At the end of his first year he won the prize in mathematics. By this time he was so far ahead of his fellow students in mathematics that he was placed in a special class by himself. While at the Royal Institution he also won another prize. This is of particular interest as it establishes the first contact of Sylvester with the United States of America where some of the happiest – also some of the most wretched – days of his life were to be spent. The American brother, by profession an actuary, had suggested to the Directors of the Lotteries Contractors of the United States that they submit a difficult problem in arrangements to young Sylvester. The budding mathematician's solution was complete and practically most satisfying to the Directors, who gave Sylvester a prize of 500 dollars for his efforts.

The years at Liverpool were far from happy. Always courageous and open, Sylvester made no bones about his Jewish faith, but proudly proclaimed it in the face of more than petty persecution at the hands of the sturdy young barbarians at the Institution who humorously called themselves Christians. But there is a limit to what one lone peacock can stand from a pack of dull jays, and Sylvester finally fled to Dublin with only a few shillings in his pocket. Luckily he was recognized in the street by a distant relative who took him in, straightened him out, and paid his way back to Liverpool.

Here we note another curious coincidence: Dublin, or at least one of its citizens, accorded the religious refugee from Liverpool decent human treatment on his first visit; on his second, some eleven years later, Trinity College, Dublin, granted him the academic degrees (B.A. and M.A.) which his own alma mater, Cambridge University, had refused him because he could not, being a Jew, subscribe to that remarkable compost of nonsensical statements known as the Thirty-Nine Articles prescribed by the Church of England as the minimum of religious belief permissible to a rational mind. It may be added here, however, that when English higher education finally unclutched itself from the stranglehold of the dead hand of the Church in 1871 Sylvester was promptly given his degrees *honoris causa*. And it should be remarked that in this as in other difficulties Sylvester was no meek, long-suffering martyr. He was full of strength and courage, both physical and moral, and he knew how to put up a devil of a fight to get justice for himself – and frequently did. He was in fact a born fighter with the untamed courage of a lion.

In 1831, when he was just over seventeen, Sylvester entered St John's College, Cambridge. Owing to severe illnesses his university career was interrupted, and he did not take the mathematical tripos till 1837. He was placed second. The man who beat him was never heard of again as a mathematician. Not being a Christian, Sylvester was ineligible to compete for Smith's prizes.

In the breadth of his intellectual interests Sylvester resembles Cayley. Physically the two men were nothing alike. Cayley, though wiry and full of physical endurance as we have seen, was frail in appearance and shy and retiring in manner. Sylvester, short and stocky, with a magnificent head set firmly above broad shoulders, gave the impression of tremendous strength and vitality, and indeed he had both. One of his students said he might have posed for the portrait of Hereward the Wake in Charles Kingsley's novel of the same name. As to interests outside of mathematics, Sylvester was much less restricted and far more liberal than Cayley. His knowledge of the Greek and Latin classics in the originals was broad and exact, and he retained his love of them right up to his last illness. Many of his papers are enlivened by quotations from these classics. The quotations are always singularly apt and really do illuminate the matter in hand.

The same may be said for his allusions from other literatures. It might amuse some literary scholar to go through the four volumes of the collected *Mathematical Papers* and reconstruct

Sylvester's wide range of reading from the credited quotations and the curious hints thrown out without explicit reference. In addition to the English and classical literatures he was well acquainted with the French, German, and Italian in the originals. His interest in language and literary form was keen and penetrating. To him is due most of the graphic terminology of the theory of invariants. Commenting on his extensive coinage of new mathematical terms from the mint of Greek and Latin, Sylvester referred to himself as the 'mathematical Adam'.

On the literary side it is quite possible that had he not been a very great mathematician he might have been something a little better than a merely passable poet. Verse, and the 'laws' of its construction, fascinated him all his life. On his own account he left much verse (some of which has been published), a sheaf of it in the form of sonnets. The subject-matter of his verse is sometimes rather apt to raise a smile, but he frequently showed that he understood what poetry is. Another interest on the artistic side was music, in which he was an accomplished amateur. It is said that he once took singing lessons from Gounod and that he used to entertain working-men's gatherings with his songs. He was prouder of his 'high C' than he was of his invariants.

One of the many marked differences between Cayley and Sylvester may be noted here: Cayley was an omnivorous reader of other mathematicians' work; Sylvester found it intolerably irksome to attempt to master what others had done. Once, in later life, he engaged a young man to teach him something about elliptic functions as he wished to apply them to the theory of numbers (in particular to the theory of partitions, which deals with the number of ways a given number can be made up by adding together numbers of a given kind, say all odd, or some odd and some even). After about the third lesson Sylvester had abandoned his attempt to learn and was lecturing to the young man on his own latest discoveries in algebra. But Cayley seemed to know everything, even about subjects in which he seldom worked, and his advice as a referee was sought by authors and editors from all over Europe. Cayley never forgot anything he had seen; Sylvester had difficulty in remembering his own inventions and once even disputed that a certain theorem of his own could possibly be true. Even comparatively trivial things that every working mathematician knows were sources of perpetual wonder and delight to Sylvester. As a consequence almost any field of mathematics offered an enchanting world for discovery to Sylvester, while Cayley glanced serenely over it all, saw what he wanted, took it, and went on to something fresh.

In 1838, at the age of twenty-four, Sylvester got his first regular job, that of Professor of Natural Philosophy (science in general, physics in particular) at University College, London, where his old teacher De Morgan was one of his colleagues. Although he had studied chemistry at Cambridge, and retained a lifelong interest in it, Sylvester found the teaching of science thoroughly uncongenial and, after about two years, abandoned it. In the meantime he had been elected a Fellow of the Royal Society at the unusually early age of twenty-five. Sylvester's mathematical merits were so conspicuous that they could not escape recognition, but they did not help him into a suitable position.

At this point in his career Sylvester set out on one of the most singular misadventures of his life. Depending upon how we look at it, this mishap is silly, ludicrous, or tragic. Sanguine and filled with his usual enthusiasm, he crossed the Atlantic to become Professor of Mathematics at the University of Virginia in 1841 – the year in which Boole published his discovery of invariants.

Sylvester endured the University only about three months. The refusal of the University authorities to discipline a young gentleman who had insulted him caused the professor to resign. For over a year after this disastrous experience Sylvester tried vainly to secure a suitable position, soliciting – unsuccessfully – both Harvard and Columbia Universities. Failing, he returned to England.

Sylvester's experiences in America gave him his fill of teaching for the next ten years. On returning to London he became an energetic actuary for a life insurance company. Such work for a creative mathematician is poisonous drudgery, and

Sylvester almost ceased to be a mathematician. However, he kept alive by taking a few private pupils, one of whom was to leave a name that is known and revered in every country of the world to-day. This was in the early 1850's, the 'potatoes, prunes, and prisms' era of female propriety when young women were not supposed to think of much beyond dabbling in paints and piety. So it is rather surprising to find that Sylvester's most distinguished pupil was a young woman, Florence Nightingale, the first human being to get some decency and cleanliness into military hospitals – over the outraged protests of bull-headed military officialdom. Sylvester at the time was in his late thirties, Miss Nightingale six years younger than her teacher. Sylvester escaped from his makeshift ways of earning a living in the same year (1854) that Miss Nightingale went out to the Crimean War.

Before this, however, he had taken another false step that landed him nowhere. In 1846, at the age of thirty-two, he entered the Inner Temple (where he coyly refers to himself as 'a dove nestling among hawks') to prepare for a legal career, and in 1850 was called to the Bar. Thus he and Cayley came together at last.

Cayley was twenty-nine, Sylvester thirty-six at the time; both were out of the real jobs to which nature had called them. Lecturing at Oxford thirty-five years later Sylvester paid grateful tribute to 'Cayley, who, though younger than myself is my spiritual progenitor – who first opened my eyes and purged them of dross so that they could see and accept the higher mysteries of our common Mathematical faith.' In 1852, shortly after their acquaintance began, Sylvester refers to 'Mr Cayley, who habitually discourses pearls and rubies'. Mr Cayley for his part frequently mentions Mr Sylvester, but always in cold blood, as it were. Sylvester's earliest outburst of gratitude in print occurs in a paper of 1851 where he says, 'The theorem above enunciated [it is his relation between the minor determinants of linearly equivalent quadratic forms] was in part suggested in the course of a conversation with Mr Cayley (to whom I am indebted for my restoration to the enjoyment of mathematical life). . . .'

Perhaps Sylvester overstated the case, but there was a lot in what he said. If he did not exactly rise from the dead he at least got a new pair of lungs: from the hour of his meeting with Cayley he breathed and lived mathematics to the end of his days. The two friends used to tramp round the Courts of Lincoln's Inn discussing the theory of invariants which both of them were creating and later, when Sylvester moved away, they continued their mathematical rambles, meeting about halfway between their respective lodgings. Both were bachelors at the time.

The theory of algebraic invariants from which the various extensions of the concept of invariance have grown naturally originated in an extremely simple observation. As will be noted in the chapter on Boole, the earliest instance of the idea appears in Lagrange, from whom it passed into the arithmetical works of Gauss. But neither of these men noticed that the simple but remarkable algebraical phenomenon before them was the germ of a vast theory. Nor does Boole seem to have fully realized what he had found when he carried on and greatly extended the work of Lagrange. Except for one slight tiff, Sylvester was always just and generous to Boole in the matter of priority, and Cayley, of course, was always fair.

The simple observation mentioned above can be understood by anyone who has ever seen a quadratic equation solved, and is merely this. A necessary and sufficient condition that the equation $ax^2 + 2bx + c = 0$ shall have two equal roots is that $b^2 - ac$ shall be zero. Let us replace the variable x by its value in terms of y obtained by the transformation $y = (px + q)/(rx + s)$. Thus x is to be replaced by the result of solving this for x, namely $x = (q - sy)/(ry - p)$. This transforms the given equation into another in y; say the new equation is $Ay^2 + 2By + C = 0$. Carrying out the algebra we find that the new coefficients A, B, C are expressed in terms of the old a, b, c as follows,

$$\begin{aligned} A &= as^2 - 2bsr + cr^2, \\ B &= -aqs + b(qr + sp) - cpr, \\ C &= aq^2 - 2bpq + cp^2. \end{aligned}$$

From these it is easy to show (by brute-force reductions, if necessary, although there is a simpler way of reasoning the result out, without actually calculating A, B, C) that

$$B^2 - AC = (ps - qr)^2 (b^2 - ac).$$

Now $b^2 - ac$ is called the discriminant of the quadratic equation in x; hence the discriminant of the quadratic in y is $B^2 - AC$, and it has been shown that *the discriminant of the transformed equation is equal to the discriminant of the original equation, times the factor* $(ps - qr)^2$ *which depends only upon the coefficients* p, q, r, s *in the transformation* $y = (px + q)/(rx + s)$ *by means of which* x *was expressed in terms of* y.

Boole was the first (in 1841) to observe something worth looking at in this particular trifle. Every algebraic equation has a discriminant, that is, a certain expression (such as $b^2 - ac$ for the quadratic) which is equal to zero if, and only if, two or more roots of the equation are equal. Boole first asked, does the discriminant of every equation when its x is replaced by the related y (as was done for the quadratic) come back unchanged except for a factor depending only on the coefficients of the transformation? He found that this was true. Next he asked whether there might not be expressions other than discriminants constructed from the coefficients having this same property of *invariance* under *transformation*. He found two such for the general equation of the fourth degree. Then another man, the brilliant young German mathematician, F. M. G. Eisenstein (1823–52) following up a result of Boole's, in 1844, discovered that certain expressions involving *both the coefficients and the* x of the original equations exhibit the same sort of invariance: the original coefficients and the original x pass into the transformed coefficients and y (as for the quadratic), and the expressions in question constructed from the originals differ from those constructed from the transforms only by a factor which depends solely on the coefficients of the transformation.

Neither Boole nor Eisenstein had any *general* method for finding such *invariant* expressions. At this point Cayley entered the field in 1845 with his pathbreaking memoir, *On the Theory of Linear Transformations*. At the time he was twenty-four. He set himself the problem of finding uniform methods which would give him *all* the invariant expressions of the kind described. To avoid lengthy explanations the problem has been stated in terms of equations; actually it was attacked otherwise, but this is of no importance here.

As this question of invariance is fundamental in modern scientific thought we shall give three further illustrations of what it means, none of which involves any symbols or algebra. Imagine any figure consisting of intersecting straight lines and curves drawn on a sheet of paper. Crumple the paper in any way you please without tearing it, and try to think what is the most obvious property of the figure that is the same before and after crumpling. Do the same for any figure drawn on a sheet of rubber, stretching but not tearing the rubber in any complicated manner dictated by whim. In this case it is obvious that sizes of areas and angles, and lengths of lines, have *not* remained 'invariant'. By suitably stretching the rubber the straight lines may be distorted into curves of almost any tortuosity you like, and at the same time the original curves – or at least some of them – may be transformed into straight lines. Yet *something* about the whole figure has remained unchanged; its very simplicity and obviousness might well cause it to be overlooked. This is the order of the points on any one of the lines of the figure which mark the places where other lines intersect the given one. Thus, if moving the pencil along a given line from A to C, we had to pass over the point B on the line before the figure was distorted, we shall have to pass over B in going from A to C after distortion. The *order* (as described) is an *invariant* under the particular *transformations* which crumpled the sheet of paper into a crinkly ball, say, or which stretched the sheet of rubber.

This illustration may seem trivial, but anyone who has read a non-mathematical description of the intersections of 'world-lines' in general relativity, and who recalls that an intersection of two such lines marks a physical '*point-event*', will see that what we have been discussing is of the same stuff as one of our pictures of the physical universe. The mathematical machinery powerful enough to handle such complicated 'transformations' and actually to produce the invariants was the creation of many workers, including Riemann, Christoffel, Ricci, Levi-Civita, Lie, and Einstein – all names well known to readers of popular accounts of relativity; the whole vast programme was originated by the early workers in the theory of algebraic invariants, of which Cayley and Sylvester were the true founders.

As a second example, imagine a knot to be looped in a string whose ends are then tied together. Pulling at the knot, and running it along the string, we distort it into any number of 'shapes'. What remains 'invariant', what is 'conserved', under all these distortions which, in this case, are our transformations? Obviously neither the shape nor the size of the knot is invariant. But the 'style' of the knot itself is invariant; in a sense that need not be elaborated, it is the *same sort* of a knot whatever we do to the string provided we do not untie its ends. Again, in the older physics, energy was 'conserved'; the total amount of energy in the universe was assumed to be an invariant, the same under all transformations from one form, such as electrical energy, into others, such as heat and light.

Our third illustration of invariance need be little more than an allusion to physical science. An observer fixes his 'position' in space and time with reference to three mutually perpendicular axes and a standard timepiece. Another observer, moving relatively to the first, wishes to describe the same physical event that the first describes. He also has his space-time reference system; his movement relatively to the first observer can be expressed as a transformation of his own co-ordinates (or of the other observer's). The descriptions given by the two may or may not differ in mathematical form, according to the particular kind of transformation concerned. If their descriptions do differ, the difference is not, obviously, inherent in the physical event they are both observing, but in their reference systems and the transformation. The problem then arises to formulate only those mathematical expressions of natural phenomena which shall be independent, mathematically, of any *particular* reference system and therefore be expressed by all observers in the same form. This is equivalent to finding the invariants of the transformation which expresses the most general shift in 'space-time' of one reference system with respect to any other. Thus the problem of finding the mathematical expressions for the intrinsic laws of nature is replaced by an attackable one in the theory of invariants. More will be said on this when we come to Riemann.

In 1863 Cambridge University established a new professorship of mathematics (the Sadlerian) and offered the post to Cayley, who promptly accepted. The same year, at the age of forty-two, he married Susan Moline. Although he made less money as a professor of mathematics than he had at the law, Cayley did not regret the change. Some years later the affairs of the University were reorganized and Cayley's salary was raised. His duties also were increased from one course of lectures during one term to two. His life was now devoted almost entirely to mathematical research and university administration. In the latter his sound business training, even temper, impersonal judgement, and legal experience proved invaluable. He never had a great deal to say, but what he said was usually accepted as final, for he never gave an opinion without having reasoned the matter through. His marriage and home life were happy; he had two children, a son and a daughter. As he gradually aged his mind remained as vigorous as ever and his nature became, if anything, gentler. No harsh judgement uttered in his presence was allowed to pass without a quiet protest. To younger men and beginners in mathematical careers he was always generous with his help, encouragement, and sound advice.

During his professorship the higher education of women was a hotly contested issue. Cayley threw all his quiet, persuasive influence on the side of civilization and largely through his efforts women were at last admitted as students (in their own nunneries of course) to the monkish seclusion of medieval Cambridge.

While Cayley was serenely mathematicizing at Cambridge his friend Sylvester was still fighting the world. Sylvester never married. In 1854, at the age of forty, he applied for the professorship of mathematics at the Royal Military Academy, Woolwich. He did not get it. Nor did he get another position for which he applied at Gresham College, London. His trial lecture was too good for the governing board. However, the successful Woolwich candidate died the following year and Sylvester was appointed. Among his not too generous emoluments was the right of pasturage on the common. As Sylvester kept neither horse, cow, nor goat, and did not eat grass himself, it is difficult to see what particular benefit he got out of this inestimable boon.

Sylvester held the position at Woolwich for sixteen years, till he was forcibly retired as 'superannuated' in 1870 at the age of fifty-six. He was still full of vigour but could do nothing against the hidebound officialdom against him. Much of his great work was still in the future, but his superiors took it for granted that a man of his age must be through.

Another aspect of his forced retirement roused all his fighting instincts. To put the matter plainly, the authorities attempted to swindle Sylvester out of part of the pension which was legitimately his. Sylvester did not take it lying down. To their chagrin the would-be gyppers learned that they were not browbeating some meek old professor but a man who could give them a little better than he took. They came through with the full pension.

While all these disagreeable things were happening in his material affairs Sylvester had no cause to complain on the scientific side. Honours frequently came his way, among them one of those most highly prized by scientific men, foreign correspondent of the French Academy of Sciences. Sylvester was elected in 1863 to the vacancy in the section of geometry caused by the death of Steiner.

After his retirement from Woolwich Sylvester lived in London, versifying, reading the classics, playing chess, and enjoying himself generally, but not doing much mathematics. In 1870 he published his pamphlet, *The Laws of Verse*, by which he set great store. Then, in 1876, he suddenly came to mathematical life again at the age of sixty-two. The 'old 'man was simply inextinguishable.

The Johns Hopkins University had been founded at Baltimore in 1875 under the brilliant leadership of President Gilman.

Gilman had been advised to start off with an outstanding classicist and the best mathematician he could afford as the nucleus of his faculty. All the rest would follow, he was told, and it did. Sylvester at last got a job where he might do practically as he pleased and in which he could do himself justice. In 1876 he again crossed the Atlantic and took up his professorship at Johns Hopkins. His salary was generous for those days, five thousand dollars a year. In accepting the call Sylvester made one curious stipulation; his salary was 'to be paid in gold'. Perhaps he was thinking of Woolwich, which gave him the equivalent of $2750.00 (plus pasturage), and wished to be sure that this time he really got what was coming to him, pension or no pension.

The years from 1876 to 1883 spent at Johns Hopkins were probably the happiest and most tranquil Sylvester had thus far known. Although he did not have to 'fight the world' any longer he did not recline on his honours and go to sleep. Forty years seemed to fall from his shoulders and he became a vigorous young man again, blazing with enthusiasm and scintillating with new ideas. He was deeply grateful for the opportunity Johns Hopkins gave him to begin his second mathematical career at the age of sixty-three, and he was not backward in expressing his gratitude publicly, in his address at the Commemoration Day Exercises of 1877.

In this Address he outlined what he hoped to do (he did it) in his lectures and researches.

'There are things called Algebraical Forms. Professor Cayley calls them Quantics. [Examples: $ax^2 + 2bxy + cy^2$, $ax^3 + 3bx^2y + 3cxy^2 + dy^3$; the numerical coefficients 1,2,1 in the first, 1,3,3,1 in the second, are binomial coefficients, as in the third and fourth lines of Pascal's triangle (Chapter 5); the next in order would be $x^4 + 4x^3y + 6x^2y^2 + 4xy^3 + y^4$]. They are not, properly speaking, Geometrical Forms, although capable, to some extent, of being embodied in them, but rather schemes of process, or of operations for forming, for calling into existence, as it were, Algebraic quantities.

'To every such Quantic is associated an infinite variety of other forms that may be regarded as engendered from and floating, like an atmosphere, around it – but infinite as were these derived existences, these emanations from the parent form, it is found that they admit of being obtained by composition, by mixture, so to say, of a certain limited number of fundamental forms, standard rays, as they might be termed in the Algebraic Spectrum of the Quantic to which they belong. And, as it is a leading pursuit of the Physicists of the present day [1877, and even to-day] to ascertain the fixed lines in the spectrum of every chemical substance, so it is the aim and object of a great school of mathematicians to make out the fundamental derived forms, the *Covariants* [that kind of 'invariant' expression, already described, which involves *both* the variables *and* the coefficients of the form or quantic] and *Invariants*, as they are called, of these Quantics.'

To mathematical readers it will be evident that Sylvester is here giving a very beautiful analogy for the fundamental system and the syzygies for a given form; the non-mathematical reader may be recommended to re-read the passage to catch the spirit of the algebra Sylvester is talking about, as the analogy is really a close one and as fine an example of 'popularized' mathematics as one is likely to find in a year's marching.

In a footnote Sylvester presently remarks 'I have at present a class of from eight to ten students attending my lectures on the Modern Higher Algebra. One of them, a young engineer, engaged from eight in the morning to six at night in the duties of his office, with an interval of an hour and a half for his dinner or lectures, has furnished me with the best proof, and the best expressed, I have ever seen of what I call [a certain theorem]. . . .' Sylvester's enthusiasm – he was past sixty – was that of a prophet inspiring others to see the promised land which he had discovered or was about to discover. Here was teaching at its best, at the only level, in fact, which justifies advanced teaching at all.

He had complimentary things to say (in footnotes) about the country of his adoption: '. . . I believe there is no nation in the world where ability with character counts for so much, and the mere possession of wealth (in spite of all that we hear about the Almighty dollar), for so little as in America. . . .'

He also tells how his dormant mathematical instincts were again aroused to full creative power. 'But for the persistence of a student of this University [Johns Hopkins] in urging upon me his desire to study with me the modern Algebra, I should never have been led into this investigation. ... He stuck with perfect respectfulness, but with invincible pertinacity, to his point. He would have the New Algebra (Heaven knows where he had heard about it, for it is almost unknown on this continent), that or nothing. I was obliged to yield, and what was the consequence? In trying to throw light on an obscure explanation in our text-book, my brain took fire. I plunged with requickened zeal into a subject which I had for years abandoned, and found food for thoughts which have engaged my attention for a considerable time past, and will probably occupy all my powers of contemplation advantageously for several months to come.'

Almost any public speech or longer paper of Sylvester's contains much that is quotable *about* mathematics in addition to technicalities. A refreshing anthology for beginners and even for seasoned mathematicians could be gathered from the pages of his collected works. Probably no other mathematician has so transparently revealed his personality through his writings as has Sylvester. He liked meeting people and infecting them with his own contagious enthusiasm for mathematics. Thus he says, truly in his own case, 'So long as a man remains a gregarious and sociable being, he cannot cut himself off from the gratification of the instinct of imparting what he is learning, of propagating through others the ideas and impressions seething in his own brain, without stunting and atrophying his moral nature and drying up the surest sources of his future intellectual replenishment.'

As a pendant to Cayley's description of the extent of modern mathematics, we may hang Sylvester's beside it. 'I should be sorry to suppose that I was to be left for long in sole possession of so vast a field as is occupied by modern mathematics. 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 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, the life, which seems to slumber in each monad, in every atom of matter, in each leaf and bud and cell, and is forever ready to burst forth into new forms of vegetable and animal existence.'

In 1878 the *American Journal of Mathematics* was founded by Sylvester and placed under his editorship by Johns Hopkins University. The *Journal* gave mathematics in the United States a tremendous urge in the right direction – research. To-day it is still flourishing mathematically but hard pressed financially.

Two years later occurred one of the classic incidents in Sylvester's career. We tell it in the words of Dr Fabian Franklin, Sylvester's successor in the chair of mathematics at Johns Hopkins for a few years and later editor of the Baltimore *American*, who was an eye (and ear) witness.

'He [Sylvester] made some excellent translations from Horace and from German poets, besides writing a number of pieces of original verse. The *tours de force* in the way of rhyming, which he performed while in Baltimore, were designed to illustrate the theories of versification of which he gives illustrations in his little book called *The Laws of Verse*. The reading of the Rosalind poem at the Peabody Institute was the occasion of an amusing exhibition of absence of mind. The poem consisted of no less than four hundred lines, all rhyming with the name Rosalind (the long and short sound of the *i* both being allowed). The audience quite filled the hall, and expected to find much interest or amusement in listening to this unique experiment in verse. But Professor Sylvester had found it necessary to write a large number of explanatory footnotes, and he announced that in order not to interrupt the poem he would read the footnotes in a body first. Nearly every footnote suggested some additional extempore remark, and the reader was so interested in each one that he was not in the least aware of the flight of time, or of the amusement of the audience. When he had dispatched the last of the notes, he looked up at the clock, and was horrified to find that he had kept the audience an hour and a half before beginning to read the poem they had come to hear. The astonishment on his face was answered by a burst of good-humoured laughter from the audience; and then, after begging all his hearers to feel at perfect liberty to leave if they had engagements, he read the Rosalind poem.'

Doctor Franklin's estimate of his teacher sums the man up admirably: 'Sylvester was quick-tempered and impatient, but generous, charitable and tender-hearted. He was always extremely appreciative of the work of others and gave the warmest recognition to any talent or ability displayed by his pupils. He was capable of flying into a passion on slight provocation, but he did not harbour resentment, and was always glad to forget the cause of quarrel at the earliest opportunity.'

Before taking up the thread of Cayley's life where it crossed Sylvester's again, we shall let the author of *Rosalind* describe how he made one of his most beautiful discoveries, that of what are called 'canonical forms'. [This means merely the reduction of a given 'quantic' to a 'standard' form. For example $ax^2 + 2bxy + cy^2$ can be expressed as the sum of two squares, say $X^2 + Y^2$; $ax^5 + 5bx^4y + 10cx^3y^2 + 10dx^2y^3 + 5exy^4 + fy^5$ can be expressed as a sum of three fifth powers, $X^5 + Y^5 + Z^5$.]

'I discovered and developed the whole theory of canonical binary forms for odd degrees, and, so far as yet made out, for even degrees* too, at one sitting, with a decanter of port wine to sustain nature's flagging energies, in a back office in Lincoln's Inn Fields. The work was done, and well done, but at the usual cost of racking thought – a brain on fire, and feet feeling, or feelingless, as if plunged in an ice-pail. *That night we slept no more.*' Experts agree that the symptoms are unmistakable. But

* This part of the theory was developed many years later by E. K. Wakeford (1894–1916), who lost his life in the First World War. 'Now, God be thanked who has matched us with his hour' (Rupert Brooke).

it must have been ripe port, to judge by what Sylvester got out of the decanter.

Cayley and Sylvester came together again professionally when Cayley accepted an invitation to lecture at Johns Hopkins for half a year in 1881–82. He chose Abelian functions, in which he was researching at the time, as his topic, and the sixty-seven-year-old Sylvester faithfully attended every lecture of his famous friend. Sylvester had still several prolific years ahead of him, Cayley not quite so many.

We shall now briefly describe three of Cayley's outstanding contributions to mathematics in addition to his work on the theory of algebraic invariants. It has already been mentioned that he invented the theory of matrices, the geometry of space of n dimensions, and that one of his ideas in geometry threw a new light (in Klein's hands) on non-Euclidean geometry. We shall begin with the last because it is the hardest.

Desargues, Pascal, Poncelet, and others had created *projective* geometry (see chapters 5, 13) in which the object is to discover those properties of figures which are invariant under projection. Measurements – sizes of angles, lengths of lines – and theorems which depend upon measurement, as for example the Pythagorean proposition that the square on the longest side of a right angle is equal to the sum of the squares on the other two sides, are not projective but *metrical*, and are not handled by *ordinary* projective geometry. It was one of Cayley's greatest achievements in geometry to transcend the barrier which, before he leapt it, had separated projective from metrical properties of figures. From his higher point of view metrical geometry also became projective, and the great power and flexibility of projective methods were shown to be applicable, by the introduction of 'imaginary' elements (for instance points whose co-ordinates involve $\sqrt{-1}$) to metrical properties. Anyone who has done any analytic geometry will recall that two circles intersect in four points, two of which are always 'imaginary'. (There are cases of apparent exception, for example concentric circles, but this is close enough for our purpose.) The fundamental notions in metrical geometry are the distance between two points and the angle between two lines.

Replacing the concept of distance by another, also involving 'imaginary' elements, Cayley provided the means for unifying Euclidean geometry and the common non-Euclidean geometries into one comprehensive theory. Without the use of some algebra it is not feasible to give an intelligible account of how this may be done; it is sufficient for our purpose to have noted Cayley's main advance of uniting projective and metrical geometry with its cognate unification of the other geometries just mentioned.

The matter of n-dimensional geometry when Cayley first put it out was much more mysterious than it seems to us to-day, accustomed as we are to the special case of four dimensions (space-time) in relativity. It is still sometimes said that a four-dimensional geometry is inconceivable to human beings. This is a superstition which was exploded long ago by Plücker; it is easy to put four-dimensional figures on a flat sheet of paper, and so far as *geometry* is concerned the *whole* of a four-dimensional 'space' can be easily imagined. Consider first a rather unconventional three-dimensional space: *all* the *circles* that may be drawn in a *plane*. This 'all' is a three-dimensional 'space' for the simple reason that it takes *precisely three numbers*, or *three co-ordinates*, to individualize any one of the swarm of circles, namely *two* to fix the position of the centre with reference to any arbitrarily given pair of axes, and *one* to give the length of the radius.

If the reader now wishes to visualize a four-dimensional space he may think of *straight lines*, instead of *points*, as the *element* out of which our common 'solid' space is built. Instead of our familiar solid space looking like an agglomeration of infinitely fine birdshot it now resembles a cosmic haystack of infinitely thin, infinitely long straight straws. That it is indeed four-dimensional in *straight lines* can be seen easily if we convince ourselves (as we may do) that *precisely four numbers* are necessary and sufficient to individualize a particular straw in our haystack. The 'dimensionality' of a 'space' can be anything we choose to make it, provided we suitably select the elements (points, circles, lines, etc.) out of which we build it. Of course if we take *points* as the elements out of which our space is to be constructed, nobody outside of a lunatic asylum has yet succeeded in visualizing a space of more than three dimensions.

Modern physics is fast teaching some to shed their belief in a mysterious 'absolute space' over and above the mathematical 'spaces' – like Euclid's, for example – that were *constructed* by geometers to correlate their physical experiences. Geometry to-day is largely a matter of analysis, but the old terminology of 'points', 'lines', 'distances', and so on, is helpful in suggesting interesting things to do with our sets of co-ordinates. But it does not follow that these particular things are the most useful that might be done in analysis; it may turn out some day that all of them are comparative trivialities by more significant things which we, hidebound in outworn traditions, continue to do merely because we lack imagination.

If there is any mysterious virtue in talking about situations which arise in analysis as if we were back with Archimedes drawing diagrams in the dust, it has yet to be revealed. Pictures after all may be suitable only for very young children; Lagrange dispensed entirely with such infantile aids when he composed his analytical mechanics. Our propensity to 'geometrize' our analysis may only be evidence that we have not yet grown up. Newton himself, it is known, first got his marvellous results analytically and reclothed them in the demonstrations of an Apollonius partly because he knew that the multitude – mathematicians less gifted than himself – would believe a theorem true only if it were accompanied by a pretty picture and a skilled Euclidean demonstration, partly because he himself still lingered by preference in the pre-Cartesian twilight of geometry.

The last of Cayley's great inventions which we have selected for mention is that of matrices and their algebra in its broad outline. The subject originated in a memoir of 1858 and grew directly out of simple observations on the way in which the transformations (linear) of the theory of algebraic invariants are combined. Glancing back at what was said on discriminants and their invariance we note the transformation (the arrow is here read 'is replaced by') $y \rightarrow \frac{px+q}{rx+s}$. Suppose we have two such transformations,

$$y \rightarrow \frac{px + q}{rx + s}, \quad x \rightarrow \frac{Pz + Q}{Rz + S},$$

the second of which is to be applied to the x in the first. We get

$$y \rightarrow \frac{(pP + qR)z + (pQ + qS)}{(rP + sR)z + (rQ + sS)}.$$

Attending only to the coefficients in the three transformations we write them in square arrays, thus

$$\begin{Vmatrix} p & q \\ r & s \end{Vmatrix}, \quad \begin{Vmatrix} P & Q \\ R & S \end{Vmatrix}, \quad \begin{Vmatrix} pP + qR & pQ + qS \\ rP + sR & rQ + sS \end{Vmatrix},$$

and see that the result of performing the first two transformations successively could have been written down by the following rule of 'multiplication',

$$\begin{Vmatrix} p & q \\ r & s \end{Vmatrix} \times \begin{Vmatrix} P & Q \\ R & S \end{Vmatrix} = \begin{Vmatrix} pP + qR & pQ + qS \\ rP + sR & rQ + sS \end{Vmatrix},$$

where the *rows* of the array on the right are obtained, in an obvious way, by applying the *rows* of the first array on the left onto the columns of the second. Such arrays (of any number of rows and columns) are called *matrices*. Their algebra follows from a few simple postulates, of which we need cite only the following. The matrices $\begin{Vmatrix} a & b \\ c & d \end{Vmatrix}$ and $\begin{Vmatrix} A & B \\ C & D \end{Vmatrix}$ are *equal* (by definition) when, and only when, $a = A$, $b = B$, $c = C$, $d = D$. The *sum* of the two matrices just written is the matrix $\begin{Vmatrix} a + A & b + B \\ c + C & d + D \end{Vmatrix}$. The result of multiplying $\begin{Vmatrix} a & b \\ c & d \end{Vmatrix}$ by m (any *number*) is the matrix $\begin{Vmatrix} ma & mb \\ mc & md \end{Vmatrix}$. The rule for 'multiplying', $\times$, (or 'compounding') matrices is as exemplified for $\begin{Vmatrix} p & q \\ r & s \end{Vmatrix}$, $\begin{Vmatrix} P & Q \\ R & S \end{Vmatrix}$ above.

A distinctive feature of these rules is that multiplication is *not commutative*, except for *special* kinds of matrices. For example, by the rule we get

$$\begin{Vmatrix} P & Q \\ R & S \end{Vmatrix} \times \begin{Vmatrix} p & q \\ r & s \end{Vmatrix} = \begin{Vmatrix} Pp + Qr & Pq + Qs \\ Rp + Sr & Rq + Ss \end{Vmatrix},$$

and the matrix on the right is not equal to that which arises from the multiplication

$$\begin{Vmatrix} p & q \\ r & s \end{Vmatrix} \times \begin{Vmatrix} P & Q \\ R & S \end{Vmatrix}.$$

All this detail, particularly the last, has been given to illustrate a phenomenon of frequent occurrence in the history of mathematics: the necessary mathematical tools for scientific applications have often been invented decades before the science to which the mathematics is the key was imagined. The bizarre rule of 'multiplication' for matrices, by which we get different results according to the order in which we do the multiplication (unlike common algebra where $x = y$ is always equal to $y \times x$), seems about as far from anything of scientific or practical use as anything could possibly be. Yet sixty-seven years after Cayley invented it, Heisenberg in 1925 recognized in the algebra of matrices exactly the tool which he needed for his revolutionary work in quantum mechanics.

Cayley continued in creative activity up to the week of his death, which occurred after a long and painful illness, borne with resignation and unflinching courage, on 26 January 1895. To quote the closing sentences of Forsyth's biography: 'But he was more than a mathematician. With a singleness of aim, which Wordsworth would have chosen for his "Happy Warrior", he persevered to the last in his nobly lived ideal. His life had a significant influence on those who knew him [Forsyth was a pupil of Cayley and became his successor at Cambridge]: they admired his character as much as they respected his genius: and they felt that, at his death, a great man had passed from the world.'

Much of what Cayley did has passed into the main current of mathematics, and it is probable that much more in his massive *Collected Mathematical Papers* (thirteen large quarto volumes of about 600 pages each, comprising 966 papers) will suggest profitable forays to adventurous mathematicians for generations to come. At present the fashion is away from the fields of Cayley's greatest interest, and the same may be said for Sylvester; but mathematics has a habit of returning to its old problems to sweep them up into more inclusive syntheses.

In 1883 Henry John Stephen Smith, the brilliant Irish specialist in the theory of numbers and Savilian Professor of Geometry in Oxford University, died in his scientific prime at the age of fifty-seven. Oxford invited the aged Sylvester, then in his seventieth year, to take the vacant chair. Sylvester accepted, much to the regret of his innumerable friends in America. But he felt homesick for his native land which had treated him none too generously; possibly also it gave him a certain satisfaction to feel that 'the stone which the builders rejected, the same is become the head of the corner'.

The amazing old man arrived in Oxford to take up his duties with a brand-new mathematical theory ('Reciprocants' - differential invariants) to spring on his advanced students. Any praise or just recognition always seemed to inspire Sylvester to outdo himself. Although he had been partly anticipated in his latest work by the French mathematician Georges Halphen, he stamped it with his peculiar genius and enlivened it with his ineffaceable individuality.

The inaugural lecture, delivered on 12 December 1885 at Oxford when Sylvester was seventy-one, has all the fire and enthusiasm of his early years, perhaps more, because he now felt secure and knew that he was recognized at last by that snobbish world which had fought him. Two extracts will give some idea of the style of the whole.

'The theory I am about to expound, or whose birth I am about to announce, stands to this ["the great theory of Invariants"] in the relation not of a younger sister, but of a brother, who, though of later birth, on the principle that the masculine is more worthy than the feminine, or at all events, according to the regulations of the Salic law, is entitled to take precedence over his elder sister, and exercise supreme sway over their united realms.'

Commenting on the unaccountable absence of a term in a certain algebraic expression he waxes lyric.

'Still, in the case before us, this unexpected absence of a member of the family, whose appearance might have been looked for, made an impression on my mind, and even went to the extent of acting on my emotions. I began to think of it as a sort of lost Pleïad in an Algebraical Constellation, and in the end, brooding over the subject, my feelings found vent, or sought relief, in a thymed effusion, a *jeu de sottise*, which, not without some apprehension of appearing singular or extravagant, I will venture to rehearse. It will at least serve as an interlude, and give some relief to the strain upon your attention before I proceed to make my final remarks on the general theory.

TO A MISSING MEMBER

OF A FAMILY OF TERMS IN AN ALGEBRAICAL FORMULA

Lone and discarded one! divorced by fate,
From thy wished-for fellows – whither art flown?
Where lingerest thou in thy bereaved estate,
Like some lost star or buried meteor stone?
Thou mindst me much of that presumptuous one
Who loth, aught less than greatest, to be great,
From Heaven's immensity fell headlong down
To live forlorn, self-centred, desolate:
Or who, new Heraklid, hard exile bore,
Now buoyed by hope, now stretched on rack of fear,
Till throned Astraea, wafting to his ear
Words of dim portent through the Atlantic roar,
Bade him "the sanctuary of the Muse revere
And strew with flame the dust of Isis' shore."

Having refreshed ourselves and bathed the tips of our fingers in the Pierian spring, let us turn back for a few brief moments to a light banquet of the reason, and entertain ourselves as a sort of after-course with some general reflections arising naturally out of the previous matter of my discourse.'

If the Pierian spring was the old boy's finger bowl at this astonishing feast of reason, it is a safe bet that the faithful decanter of port was never very far from his elbow.

Sylvester's sense of the kinship of mathematics to the finer arts found frequent expression in his writings. Thus, in a paper on Newton's rule for the discovery of imaginary roots of algebraic equations, he asks in a footnote 'May not Music be described as the Mathematic of sense, Mathematic as Music of the reason? Thus the musician *feels* Mathematic, the mathematician *thinks* Music – Music the dream, Mathematic the working life – each to receive its consummation from the other when the human intelligence, elevated to its perfect type, shall shine forth glorified in some future Mozart-Dirichlet or Beethoven-Gauss – a union already not indistinctly foreshadowed in the genius and labours of a Helmholtz!'

Sylvester loved life, even when he was forced to fight it, and if ever a man got the best that is in life out of it, he did. He gloried in the fact that the great mathematicians, except for what may be classed as avoidable or accidental deaths, have been long-lived and vigorous of mind to their dying days. In his presidential address to the British Association in 1869 he called the honour roll of some of the greatest mathematicians of the past and gave their ages at death to bear out his thesis that '... there is no study in the world which brings into more harmonious action all the faculties of the mind than [mathematics], ... or, like this, seems to raise them, by successive steps of initiation, to higher and higher states of conscious intellectual being. ... The mathematician lives long and lives young; the wings of the soul do not early drop off, nor do its pores become clogged with the earthy particles blown from the dusty highways of vulgar life.'

Sylvester was a living example of his own philosophy. But even he at last began to bow to time. In 1893 – he was then seventy-nine – his eyesight began to fail, and he became sad and discouraged because he could no longer lecture with his old enthusiasm. The following year he asked to be relieved of the more onerous duties of his professorship, and retired to live, lonely and dejected, in London or at Tunbridge Wells. All his brothers and sisters had long since died, and he had outlived most of his dearest friends.

But even now he was not through. His mind was still vigorous, although he himself felt that the keen edge of his inventiveness was dulled for ever. Late in 1896, in the eighty-second year of his age, he found a new enthusiasm in a field which had always fascinated him, and he blazed up again over the theory of compound partitions and Goldbach's conjecture that every even number is the sum of two primes.

He had not much longer. While working at his mathematics in his London rooms early in March 1897 he suffered a paralytic stroke which destroyed his power of speech. He died on 15 March 1897, at the age of eighty-three. His life can be summed up in his own words, 'I really love my subject'.

CHAPTER TWENTY-TWO

MASTER AND PUPIL

Weierstrass; Sonja Kowalewski

*

YOUNG doctors in mathematics, anxiously seeking positions in which their training and talents may have some play, often ask whether it is possible for a man to do elementary teaching for long and keep alive mathematically. It is. The life of Boole is a partial answer; the career of Weierstrass, the prince of analysts, 'the father of modern analysis', is conclusive.

Before considering Weierstrass in some detail, we place him chronologically with respect to those of his German contemporaries, each of whom, like him, gave at least one vast empire of mathematics a new outlook during the second half of the nineteenth century and the first three decades of the twentieth. The year 1855, which marks the death of Gauss and the breaking of the last link with the outstanding mathematicians of the preceding century, may be taken as a convenient point of reference. In 1855 Weierstrass (1815–97) was forty; Kronecker (1823–91), thirty-two; Riemann (1826–66), twenty-nine; Dedekind (1831–1916), twenty-four; while Cantor (1845–1918) was a small boy of ten. Thus German mathematics did not lack recruits to carry on the great tradition of Gauss. Weierstrass was just gaining recognition; Kronecker was well started; some of Riemann's greatest work was already behind him, and Dedekind was entering the field (the theory of numbers) in which he was to gain his greatest fame. Cantor, of course, had not yet been heard from.

We have juxtaposed these names and dates because four of the men mentioned, dissimilar and totally unrelated as much of their finest work was, came together on one of the central problems of all mathematics, that of irrational numbers: Weierstrass and Dedekind resumed the discussion of irrationals and continuity practically where Eudoxus had left it in the fourth century B.C.; Kronecker, a modern echo of Zeno, made Weierstrass' last years miserable by sceptical criticism of the latter's revision of Eudoxus; while Cantor, striking out on a new road of his own, sought to compass the actual infinite itself which is implicit – according to some – in the very concept of continuity. Out of the work of Weierstrass and Dedekind developed the modern epoch of analysis, that of critical logical precision in analysis (the calculus, the theory of functions of a complex variable, and the theory of functions of real variables) in distinction to the looser intuitive methods of some of the older writers – invaluable as heuristic guides to discovery but quite worthless from the standpoint of the Pythagorean ideal of mathematical proof. As has already been noted, Gauss, Abel, and Cauchy inaugurated the first period of rigour; the movement started by Weierstrass and Dedekind was on a higher plane, suitable to the more exacting demands of analysis in the second half of the century, for which the earlier precautions were inadequate.

One discovery by Weierstrass in particular shocked the intuitive school of analysts into a decent regard for caution: he produced a continuous curve which has no tangent at any point. Gauss once called mathematics 'the science of the eye'; it takes more than a good pair of eyes to 'see' the curve which Weierstrass presented to the advocates of sensual intuition.

Since to every action there is an equal and opposite reaction it was but natural that all this revamped rigour should engender its own opposition. Kronecker attacked it vigorously, even viciously, and quite exasperatingly. He denied that it meant anything. Although he succeeded in hurting the venerable and kindly Weierstrass, he made but little impression on his conservative contemporaries and practically none on mathematical analysis. Kronecker was a generation ahead of his time. Not till the second decade of the twentieth century did his strictures on the currently accepted doctrines of continuity and irrational numbers receive serious consideration. To-day it is true that not all mathematicians regard Kronecker's attack as merely the release of his pent-up envy of the more famous Weierstrass which some of his contemporaries imagined it to be, and it is admitted that there may be something – not much, perhaps – in his disturbing objections. Whether there is or not, Kronecker's attack was partly responsible for the *third* period of rigour in modern mathematical reasoning, that which we ourselves are attempting to enjoy. Weierstrass was not the only fellow-mathematician whom Kronecker harried; Cantor also suffered deeply under what he considered his influential colleague's malicious persecution. All these men will speak for themselves in the proper place; here we are only attempting to indicate that their lives and work were closely interwoven in at least one corner of the gorgeous pattern.

To complete the picture we must indicate other points of contact between Weierstrass, Kronecker, and Riemann on one side and Kronecker and Dedekind on the other. Abel, we recall, died in 1829, Galois in 1832, and Jacobi in 1851. In the epoch under discussion one of the outstanding problems in mathematical analysis was the completion of the work of Abel and Jacobi on multiple periodic functions – elliptic functions, Abelian functions (see chapters 17, 18). From totally different points of view Weierstrass and Riemann accomplished what was to be done – Weierstrass indeed considered himself in some degree a successor of Abel; Kronecker opened up new vistas in elliptic functions but he did not compete with the other two in the field of Abelian functions. Kronecker was primarily an arithmetician and an algebraist; some of his best work went into the elaboration and extension of the work of Galois in the theory of equations. Thus Galois found a worthy successor not too long after his death.

Apart from his forays into the domain of continuity and irrational numbers, Dedekind's most original work was in the higher arithmetic, which he revolutionized and renovated. In this Kronecker was his able and sagacious rival, but again their whole approaches were entirely different and characteristic of the two men: Dedekind overcame his difficulties in the theory of algebraic numbers by taking refuge in the infinite (in his theory of 'ideals', as will be indicated in the proper place); Kronecker sought to solve his problems in the finite.

Karl Wilhelm Theodor Weierstrass, the eldest son of Wilhelm Weierstrass (1790–1869) and his wife Theodora Forst, was born on 31 October 1815, at Ostenfelde in the district of Münster, Germany. The father was then a customs officer in the pay of the French. It may be recalled that 1815 was the year of Waterloo; the French were still dominating Europe. That year also saw the birth of Bismarck, and it is interesting to observe that whereas the more famous statesman's life work was shot to pieces in World War I, if not earlier, the contributions of his comparatively obscure contemporary to science and the advancement of civilization in general are even more highly esteemed to-day than they were during his lifetime.

The Weierstrass family were devout liberal Catholics all their lives; the father had been converted from Protestantism, probably at the time of his marriage. Karl had a brother, Peter (died in 1904), and two sisters, Klara (1823–96), and Elise (1826–98) who looked after his comfort all their lives. The mother died in 1826, shortly after Elise's birth, and the father married again the following year. Little is known of Karl's mother, except that she appears to have regarded her husband with a restrained aversion and to have looked on her marriage with moderated disgust. The stepmother was a typical German housewife; her influence on the intellectual development of her stepchildren was probably nil. The father, on the other hand, was a practical idealist, and a man of culture who at one time had been a teacher. The last ten years of his life were spent in peaceful old age in the house of his famous son in Berlin, where the two daughters also lived. None of the children ever married, although poor Peter once showed an inclination toward matrimony which was promptly squelched by his father and sisters.

One possible discord in the natural sociability of the children was the father's uncompromising righteousness, domineering authority, and Prussian pigheadedness. He nearly wrecked Peter's life with his everlasting lecturing and came perilously close to doing the same by Karl, whom he attempted to force into an uncongenial career without ascertaining where his brilliant young son's abilities lay. Old Weierstrass had the audacity to preach at his younger son and meddle in his affairs till the 'boy' was nearly forty. Luckily Karl was made of more resistant stuff. As we shall see his fight against his father – although he himself was probably quite unaware that he was fighting the tyrant – took the not unusual form of making a mess of the life his father had chosen for him. It was as neat a defence as he could possibly have devised, and the best of it was that neither he nor his father ever dreamed what was happening, although a letter of Karl's when he was sixty shows that he had at last realized the cause of his early difficulties. Karl at last got his way, but it was a long, roundabout way, beset with trials and errors. Only a shaggy man like himself, huge and rugged of body and mind, could have won through to the end.

Shortly after Karl's birth the family moved to Westernkotten, Westphalia, where the father became a customs officer at the salt works. Westernkotten, like other dismal holes in which Weierstrass spent the best years of his life, is known in Germany to-day only because Weierstrass once was condemned to rot there – only he did not rust; his first published work is dated as having been written in 1841 (he was then twenty-six) at Westernkotten. There being no school in the village, Karl was sent to the adjacent town of Münster whence, at fourteen, he entered the Catholic Gymnasium at Paderborn. Like Descartes under somewhat similar conditions, Weierstrass thoroughly enjoyed his school and made friends of his expert, civilized instructors. He traversed the set course in considerably less than the standard time, making a uniformly brilliant record in all his studies. He left in 1834 at the age of nineteen. Prizes fell his way with unfailing regularity; one year he carried off seven; he was usually first in German and in two of the three, Latin, Greek, and mathematics. By a beautiful freak of irony he never won a prize for calligraphy, although he was destined to teach penmanship to little boys but recently emancipated from their mother's apron strings.

As mathematicians often have a liking for music it is of interest to note here that Weierstrass, broad as he was, could not tolerate music in any form. It meant nothing to him and he did not pretend that it did. When he had become a success his solicitous sisters tried to get him to take music lessons to make him more conventional socially, but after a half-hearted lesson or two he abandoned the distasteful project. Concerts bored him and grand opera put him to sleep – when his sisters could drag him out to either.

Like his good father, Karl was not only an idealist but was also extremely practical – for a time. In addition to capturing most of the prizes in purely impractical studies he secured a paying job, at the age of fifteen, as accountant for a prosperous female merchant in the ham and butter business.

All of these successes had a disastrous effect on Karl's future. Old Weierstrass, like many parents, drew the wrong conclusion from his son's triumphs. He 'reasoned' as follows. Because the boy has won a cartload of prizes, therefore he must have a good mind – this much may be admitted; and because he has kept himself in pocket money by posting the honoured female butter and ham merchant's books efficiently, therefore he will be a brilliant bookkeeper. Now what is the acme of all bookkeeping? Obviously a government nest – in the higher branches of course – in the Prussian civil service. But to prepare for this exalted position, a knowledge of the law is desirable in order to pluck effectively and to avoid being plucked.

As the grand conclusion of all this logic, paterfamilias Weierstrass shoved his gifted son, at the age of nineteen, head first into the University of Bonn to master the chicaneries of commerce and the quibblings of the law.

Karl had more sense than to attempt either. He devoted his great bodily strength, his lightning dexterity, and his keen mind almost exclusively to fencing and the mellow sociability that is induced by nightly and liberal indulgence in honest German beer. What a shocking example for ant-eyed Ph.D.'s who shrink from a spell of school-teaching lest their dim lights be dimmed for ever! But to do what Weierstrass did, and get away with it, one must have at least a tenth of his constitution and not less than one tenth of 1 per cent of his brains.

Bonn found Weierstrass unbeatable. His quick eye, his long reach, his devilish accuracy, and his lightning speed in fencing made him an opponent to admire but not to touch. As a matter of historical fact he never was touched: no jagger scar adorned his cheeks, and in all his bouts he never lost a drop of blood. Whether or not he was ever put under the table in the subsequent celebrations of his numerous victories is not known. His discreet biographers are somewhat reticent on this important point, but to anyone who has ever contemplated one of Weierstrass' mathematical masterpieces it is inconceivable that so strong a head as his could ever have nodded over a half-gallon stein. His four mis-spent years in the university were perhaps after all well spent.

His experiences at Bonn did three things of the greatest moment for Weierstrass: they cured him of his father fixation without in any way damaging his affection for his deluded parent; they made him a human being capable of entering fully into the pathetic hopes and aspirations of human beings less gifted than himself – his pupils – and thus contributed directly to his success as probably the greatest mathematical teacher of all time; and last, the humorous geniality of his boyhood became a fixed life-habit. So the 'student years' were not the loss his disappointed father and his fluttering sisters – to say nothing of the panicky Peter – thought they were when Karl returned, after four 'empty' years at Bonn, without a degree, to the bosom of his wailing family.

There was a terrific row. They lectured him – 'sick of body and soul' as he was, possibly the result of not enough law, too little mathematics, and too much beer; they sat around and glowered at him and, worst of all, they began to discuss him as if he were dead: what was to be done with the corpse? Touching the law, Weierstrass had only one brief encounter with it at Bonn, but it sufficed: he astonished the Dean and his friends by his acute 'opposition' of a candidate for the doctor degree in law. As for the mathematics at Bonn – it was inconsiderable. The one gifted man, Julius Plücker, who might have done Weierstrass some good was so busy with his manifold duties that he had no time to spare on individuals and Weierstrass got nothing out of him.

But like Abel and so many other mathematicians of the first rank, Weierstrass had gone to the masters in the interludes between his fencing and drinking: he had been absorbing the

Celestial Mechanics of Laplace, thereby laying the foundations for his lifelong interest in dynamics and systems of simultaneous differential equations. Of course he could get none of this through the head of his cultured, petty-official father, and his obedient brother and his dismayed sisters knew not what the devil he was talking about. The fact alone was sufficient: brother Karl, the genius of the timorous little family, on whom such high hopes of bourgeois respectability had been placed, had come home, after four years of rigid economy on father's part, without a degree.

At last – after weeks – a sensible friend of the family who had sympathized with Karl as a boy, and who had an intelligent amateur's interest in mathematics, suggested a way out: let Karl prepare himself at the neighbouring Academy of Münster for the state teachers' examination. Young Weierstrass would not get a Ph.D. out of it, but his job as a teacher would provide a certain amount of evening leisure in which he could keep alive mathematically provided he had the right stuff in him. Freely confessing his 'sins' to the authorities, Weierstrass begged the opportunity of making a fresh start. His plea was granted, and Weierstrass matriculated on 22 May 1839 at Münster to prepare himself for a secondary school teaching career. This was a most important stepping stone to his later mathematical eminence, although at the time it looked like a total rout.

What made all the difference to Weierstrass was the presence at Münster of Christof Gudermann (1798–1852) as Professor of Mathematics. Gudermann at the time (1839) was an enthusiast for elliptic functions. We recall that Jacobi had published his *Fundamenta nova* in 1829. Although few are now familiar with Gudermann's elaborate investigations (published at the instigation of Crelle in a series of articles in his *Journal*), he is not to be dismissed as contemptuously as it is sometimes fashionable to do merely because he is outmoded. For his time Gudermann had what appears to have been an original idea. The theory of elliptic functions can be developed in many different ways – too many for comfort. At one time some particular way seems the best; at another, a slightly different approach is highly advertised for a season and is generally regarded as being more chic.

Gudermann's idea was to base everything on the *power series* expansion of the functions. (This statement will have to do for the moment; its meaning will become clear when we describe one of the leading motivations of the work of Weierstrass.) This really was a good new idea, and Gudermann slaved over it with overwhelming German thoroughness for years without, perhaps, realizing what lay behind his inspiration, and himself never carried it through. The important thing to note here is that Weierstrass made the theory of power series – Gudermann's inspiration – the nerve of all his work in analysis. He got the idea from Gudermann, whose lectures he attended. In later life, contemplating the scope of the methods he had developed in analysis, Weierstrass was wont to exclaim, 'There is nothing but power series!'

At the opening lecture of Gudermann's course on elliptic functions (he called them by a different name, but that is of no importance) there were thirteen auditors. Being in love with his subject the lecturer quickly left the earth and was presently soaring practically alone in the aether of pure thought. At the second lecture only one auditor appeared and Gudermann was happy. The solitary student was Weierstrass. Thereafter no incautious third party ventured to profane the holy communion between the lecturer and his unique disciple. Gudermann and Weierstrass were fellow Catholics; they got along splendidly together.

Weierstrass was duly grateful for the pains Gudermann lavished on him, and after he had become famous he seized every opportunity – the more public the better – to proclaim his gratitude for what Gudermann had done for him. The debt was not inconsiderable: it is not every professor who can drop a hint like the one – power series representation of functions as a point of attack – which inspired Weierstrass. In addition to the lectures on elliptic functions, Gudermann also gave Weierstrass private lessons on 'analytical spherics' – whatever that may have been.

In 1841, at the age of twenty-six, Weierstrass took his examinations for his teacher's certificate. The examination was in two sections, written and oral. For the first he was allowed six months in which to write out essays on three topics acceptable to the examiners. The third question inspired a fine dissertation on the Socratic method in secondary teaching, a method which Weierstrass followed with brilliant success when he became the foremost mathematical teacher of advanced students in the world.

A teacher – at least in higher mathematics – is judged by his students. If his students are enthusiastic about his 'beautifully clear lectures', of which they take copious notes, but never do any original mathematics themselves after getting their advanced degrees, the teacher is a flat failure as a university instructor and his proper sphere – if anywhere – is in a secondary school or a small college where the aim is to produce tame gentlemen but not independent thinkers. Weierstrass' lectures were models of perfection. But if they had been nothing more than finished expositions they would have been pedagogically worthless. To perfection of form Weierstrass added that intangible something which is called inspiration. He did not rant about the sublimity of mathematics and he never orated; but somehow or another he made creative mathematicians out of a disproportionately large fraction of his students.

The examination which admitted Weierstrass after a year of probationary teaching to the profession of secondary school work is one of the most extraordinary of its kind on record. One of the essays which he submitted must be the most abstruse production ever accepted in a teacher's examination. At the candidate's request Gudermann had set Weierstrass a real mathematical problem: to find the power series developments of the elliptic functions. There was more than this, but the part mentioned was probably the most interesting.

Gudermann's report on the work might have changed the course of Weierstrass' life had it been listened to, but it made no practical impression where it might have done good. In a postscript to the official report Gudermann states that 'This problem, which in general would be far too difficult for a young analyst, was set at the candidate's express request with the consent of the commission.' After the acceptance of his written work and the successful conclusion of his oral examination, Weierstrass got a special certificate on his original contribution to mathematics. Having stated what the candidate had done, and having pointed out the originality of the attack and the novelty of some of the results attained, Gudermann declares that the work evinces a fine mathematical talent 'which, provided it is not frittered away, will inevitably contribute to the advancement of science. For the author's sake and that of science it is to be desired that he shall not become a secondary teacher, but that favourable conditions will make it possible for him to function in academic instruction. . . . The candidate hereby enters by birthright into the ranks of the famous discoverers.'

These remarks, in part underlined by Gudermann, were very properly stricken from the official report. Weierstrass got his certificate and that was all. At the age of twenty-six he entered his trade of secondary teaching which was to absorb nearly fifteen years of his life, including the decade from thirty to forty which is usually rated as the most fertile in a scientific man's career.

His work was excessive. Only a man with iron determination and a rugged physique could have done what Weierstrass did. The nights were his own and he lived a double life. Not that he became a dull drudge; far from it. Nor did he pose as the village scholar absorbed in mysterious meditations beyond the comprehension of ordinary mortals. With quiet satisfaction in his later years he loved to dwell on the way he had fooled them all; the gay government officials and the young officers found the amiable school teacher a thoroughly good fellow and a lively tavern companion.

But in addition to these boon companions of an occasional night out, Weierstrass had another, unknown to his happy-go-lucky fellows – Abel, with whom he kept many a long vigil. He himself said that Abel's works were never very far from his elbow. When he became the leading analyst in the world and the greatest mathematical teacher in Europe his first and last advice to his numerous students was 'Read Abel!' For the great Norwegian he had an unbounded admiration undimmed by any shadow of envy. 'Abel, the lucky fellow!' he would exclaim: 'He has done something everlasting! His ideas will always exercise a fertilizing influence on our science.'

The same might be said for Weierstrass, and the creative ideas with which he fertilized mathematics were for the most part thought out while he was an obscure schoolteacher in dismal villages where advanced books were unobtainable, and at a time of economic stress when the postage on a letter absorbed a prohibitive part of the teacher's meagre weekly wage. Being unable to afford postage, Weierstrass was barred from scientific correspondence. Perhaps it is as well that he was: his originality developed unhampered by the fashionable ideas of the time. The independence of outlook thus acquired characterized his work in later years. In his lectures he aimed to develop everything from the ground up in his own way and made almost no reference to the work of others. This occasionally mystified his auditors as to what was the master's and what another's.

It will be of interest to mathematical readers to note one or two stages in Weierstrass' scientific career. After his probationary year as a teacher at the Gymnasium at Münster, Weierstrass wrote a memoir on analytic functions in which, among other things, he arrived independently at Cauchy's integral theorem – the so-called fundamental theorem of analysis. In 1842 he heard of Cauchy's work but claimed no priority (as a matter of fact Gauss had anticipated them both away back in 1811, but as usual had laid his work aside to ripen). In 1842, at the age of twenty-seven, Weierstrass applied the methods he had developed to systems of differential equations – such as those occurring in the Newtonian problem of three bodies, for example; the treatment was mature and rigorous. These works were undertaken without thought of publication merely to prepare the ground on which Weierstrass' lifework (on Abelian functions) was to be built.

In 1842 Weierstrass was assistant teacher of mathematics and physics at the Pro-Gymnasium in Deutsch-Krone, West Prussia. Presently he was promoted to the dignity of ordinary teacher. In addition to the subjects mentioned the leading analyst in Europe also taught German, geography, and writing to the little boys under his charge; gymnastics was added in 1845.

In 1848, at the age of thirty-three, Weierstrass was transferred as ordinary teacher to the Gymnasium at Braunsberg. This was something of a promotion, but not much. The head of the school was an excellent man who did what he could to make things agreeable for Weierstrass although he had only a remote conception of the intellectual eminence of his colleague. The school boasted a very small library of carefully selected books on mathematics and science.

It was in this year that Weierstrass turned aside for a few weeks from his absorbing mathematics to indulge in a little delicious mischief. The times were somewhat troubled politically; the virus of liberty had infected the patient German people and at least a few of the bolder souls were out on the warpath for democracy. The royalist party in power clamped a strict censorship on all spoken or printed sentiments not sufficiently laudatory to their régime. Fugitive hymns to liberty began appearing in the papers. The authorities of course could tolerate nothing so subversive of law and order as this, and when Braunsberg suddenly blossomed out with a lush crop of democratic poets all singing the praises of liberty in the local paper, as yet uncensored, the flustered government hastily appointed a local civil servant as censor and went to sleep, believing that all would be well.

Unfortunately the newly appointed censor had a violent aversion to all forms of literature, poetry especially. He simply could not bring himself to read the stuff. Confining his supervision to blue-pencilling the dull political prose, he turned over all the literary effusions to schoolteacher Weierstrass for censoring. Weierstrass was delighted. Knowing that the official censor would never glance at any poem, Weierstrass saw to it that the most inflammatory ones were printed in full right under the censor's nose. This went merrily on to the great delight of the populace till a higher official stepped in and put an end to the farce. As the censor was the officially responsible offender, Weierstrass escaped scot-free.

The obscure hamlet of Deutsch-Krone has the honour of being the place where Weierstrass (in 1842–43) first broke into print. German schools publish occasional 'programmes' containing papers by members of the staff. Weierstrass contributed *Remarks on Analytical Factorials*. It is not necessary to explain what these are; the point of interest here is that the subject of factorials was one which had caused the elder analysts many a profitless headache. Until Weierstrass attacked the problems connected with factorials the nub of the matter had been missed.

Crelle, we recall, wrote extensively on factorials, and we have seen how interested he was when Abel somewhat rashly informed him that his work contained serious oversights. Crelle now enters once more, and again in the same fine spirit he showed Abel.

Weierstrass' work was not published till 1856, fourteen years after it had been written, when Crelle printed it in his *Journal*. Weierstrass was then famous. Admitting that the rigorous treatment by Weierstrass clearly exposes the errors of his own work, Crelle continues as follows: 'I have never taken the personal point of view in my work, nor have I striven for fame and praise, but only for the advancement of truth to the best of my ability; and it is all one to me whoever it may be that comes nearer to the truth – whether it is I or someone else, provided only a closer approximation to the truth is attained.' There was nothing neurotic about Crelle. Nor was there about Weierstrass.

Whether or not the tiny village of Deutsch-Krone is conspicuous on the map of politics and commerce it stands out like the capital of an empire in the history of mathematics, for it was there that Weierstrass, without even an apology for a library and with no scientific connexions whatever, laid the foundations of his life work – 'to complete the life work of Abel and Jacobi growing out of Abel's Theorem and Jacobi's discovery of multiple periodic functions of several variables.'

Abel, he observes, cut down in the flower of his youth, had no opportunity to follow out the consequences of his tremendous discovery, and Jacobi had failed to see clearly that the true meaning of his own work was to be sought in Abel's Theorem.

'The consolidation and extension of these gains – the task of actually exhibiting the functions and working out their properties – is one of the major problems of mathematics.' Weierstrass thus declares his intention of devoting his energies to this problem as soon as he shall have understood it deeply and have developed the necessary tools. Later he tells how slowly he progressed: 'The fabrication of methods and other difficult problems occupied my time. Thus years slipped away before I could get at the main problem itself, hampered as I was by an unfavourable environment.'

The whole of Weierstrass' work in analysis can be regarded as a grand attack on his main problem. Isolated results, special developments, and even extensive theories – for example that of irrational numbers as developed by him – all originated in some phase or another of the central problem. He early became convinced that for a clear understanding of what he was attempting to do a radical revision of the fundamental concepts of mathematical analysis was necessary, and from this conviction he passed to another, of more significance to-day perhaps than the central problem itself: analysis must be founded on the common whole numbers 1,2,3, ... The irrationals which give us the concepts of limits and continuity, from which analysis springs, must be referred back by irrefrangible reasoning to the integers; shoddy proofs must be discarded or reworked, gaps must be filled up, and obscure 'axioms' must be dragged out into the light of critical inquiry till all are understood and all are stated in comprehensible language in terms of the integers. This in a sense is the Pythagorean dream of basing all mathematics on the integers, but Weierstrass gave the programme constructive definiteness and made it work.

Thus originated the nineteenth-century movement known as *the arithmetization of analysis* – something quite different from Kronecker's arithmetical programme, at which we shall glance in a later chapter; indeed the two approaches were mutually antagonistic.

In passing it may be pointed out that Weierstrass' plan for his life work and his magnificent accomplishment of most of what he set himself as a young man to do, is a good illustration of the value of the advice Felix Klein once gave a perplexed student who had asked him the secret of mathematical discovery. 'You must have a problem', Klein replied. 'Choose one definite objective and drive ahead toward it. You may never reach your goal, but you will find something of interest on the way.'

From Deutsch-Krone Weierstrass moved to Braunsberg, where he taught in the Royal Catholic Gymnasium for six years, beginning in 1848. The school 'programme' for 1848–9 contains a paper by Weierstrass which must have astonished the native: *Contributions to the Theory of Abelian Integrals.* If this work had chanced to fall under the eyes of any of the professional mathematicians of Germany, Weierstrass would have been made. But, as his Swedish biographer, Mittag-Leffler, dryly remarks, one does not look for epochal papers on pure mathematics in secondary-school programmes. Weierstrass might as well have used his paper to light his pipe.

His next effort fared better. The summer vacation of 1853 (Weierstrass was then thirty-eight) was passed in his father's house at Westernkotten. Weierstrass spent the vacation writing up a memoir on Abelian functions. When it was completed he sent it to Crelle's great *Journal.* It was accepted and appeared in volume 47 (1854).

This may have been the paper whose composition was responsible for an amusing incident in Weierstrass' career as a schoolteacher at Braunsberg. Early one morning the director of the school was startled by a terrific uproar proceeding from the classroom where Weierstrass was supposed to be holding forth. On investigation he discovered that Weierstrass had not shown up. He hurried over to Weierstrass' dwelling, and on knocking was bidden to enter. There sat Weierstrass pondering by the glimmering light of a lamp, the curtains of the room still drawn. He had worked the whole night through and had not noticed the approach of dawn. The director called his attention to the fact that it was broad daylight and told him of the uproar in his classroom. Weierstrass replied that he was on the trail of an important discovery which would rouse great interest in the scientific world and he could not possibly interrupt his work.

The memoir on Abelian functions published in Crelle's *Journal* in 1854 created a sensation. Here was a masterpiece from the pen of an unknown schoolmaster in an obscure village nobody in Berlin had ever heard of. This in itself was sufficiently astonishing. But what surprised those who could appreciate the magnitude of the work even more was the almost unprecedented fact that the solitary worker had published no preliminary bulletins announcing his progress from time to time, but with admirable restraint had held back everything till the work was completed.

Writing to a friend some ten years later, Weierstrass gives his modest version of his scientific reticence: '... the infinite emptiness and boredom of those years [as a schoolteacher] would have been unendurable without the hard work that made me a recluse – even if I was rated rather a good fellow by the circle of my friends among the junkers, lawyers, and young officers of the community. ... The present offered nothing worth mentioning, and it was not my custom to speak of the future.'

Recognition was immediate. At the University of Königsberg, where Jacobi had made his great discoveries in the field which Weierstrass had now entered with a masterpiece of surpassing excellence, Richelot, himself a worthy successor of Jacobi in the theory of multiple periodic functions, was Professor of Mathematics. His expert eyes saw at once what Weierstrass had done. He forthwith persuaded his university to confer the degree of doctor, *honoris causa*, on Weierstrass and himself journeyed to Braunsberg to present the diploma.

At the dinner organized by the director of the Gymnasium in Weierstrass' honour Richelot asserted that 'we have all found our master in Mr Weierstrass'. The Ministry of Education immediately promoted him and granted him a year's leave to prosecute his scientific work. Borchardt, the editor of Crelle's *Journal* at the time, hurried to Braunsberg to congratulate the greatest analyst in the world, thus starting a warm friendship which lasted till Borchardt's death a quarter of a century later.

None of this went to Weierstrass' head. Although he was deeply moved and profoundly grateful for all the generous recognition so promptly accorded him, he could not refrain from casting a backward glance over his career. Years later, thinking of the happiness of the occasion and of what that occasion had opened up for him when he was forty years of age, he remarked sadly that 'everything in life comes too late.'

Weierstrass did not return to Braunsberg. No really suitable position being open at the time, the leading German mathematicians did what they could to tide over the emergency and got Weierstrass appointed Professor of Mathematics at the Royal Polytechnic School in Berlin. This appointment dated from 1 July 1856; in the autumn of the same year he was made Assistant Professor (in addition to the other post) at the University of Berlin and was elected to the Berlin Academy.

The excitement of novel working conditions and the strain of too much lecturing presently brought on a nervous breakdown. Weierstrass had also been overworking at his researches. In the summer of 1859 he was forced to abandon his course and take a rest cure. Returning in the autumn he continued his work, apparently refreshed, but in the following March was suddenly attacked by spells of vertigo, and he collapsed in the middle of a lecture.

All the rest of his life he was bothered with the same trouble off and on, and after resuming his work – as full professor, with a considerably lightened load – never trusted himself to write his own formulae on the board. His custom was to sit where he could see the class and the blackboard, and dictate to some student delegated from the class what was to be written. One of these 'mouthpieces' of the master developed a rash propensity to try to improve on what he had been told to write. Weierstrass would reach up and rub out the amateur's efforts and make him write what he had been told. Occasionally the battle between the professor and the obstinate student would go to several rounds, but in the end Weierstrass always won. He had seen little boys misbehaving before.

As the fame of his work spread over Europe (and later to America), Weierstrass' classes began to grow rather unwieldy and he would sometimes regret that the quality of his auditors lagged far behind their rapidly mounting quantity. Nevertheless he gathered about him an extremely able band of young mathematicians who were absolutely devoted to him and who did much to propagate his ideas, for Weierstrass was always slow about publication, and without the broadcasting of his lectures which his disciples took upon themselves his influence on the mathematical thought of the nineteenth century would have been considerably retarded.

Weierstrass was always accessible to his students and sincerely interested in their problems, whether mathematical or human. There was nothing of the 'great man' complex about him, and he would as gladly walk home with any of the students – and there were many – who cared to join him as with the most famous of his colleagues, perhaps more gladly when the colleague happened to be Kronecker. He was happiest when, sitting at a table over a glass of wine with a few of his devoted disciples, he became a jolly student again himself and insisted on paying the bill for the crowd.

An anecdote (about Mittag-Leffler) may suggest that the Europe of the present century has partly lost something it had in the 1870's. The Franco-Prussian war (1870–71) had left France pretty sore at Germany. But it had not befogged the minds of mathematicians regarding one another's merits irrespective of their nationalities. The like holds for the Napoleonic wars and the mutual esteem of the French and British mathematicians. In 1873 Mittag-Leffler arrived in Paris from Stockholm all set and full of enthusiasm to study analysis under Hermite. 'You have made a mistake, sir', Hermite told him: 'you should follow Weierstrass' course at Berlin. He is the master of all of us.'

Mittag-Leffler took the sound advice of the magnanimous Frenchman and not so long afterwards made a capital discovery of his own which is to be found to-day in all books on the theory of functions. 'Hermite was a Frenchman and a patriot', Mittag-Leffler remarks; 'I learned at the same time in what degree he was also a mathematician.'

The years (1864–97) of Weierstrass' career at Berlin as Professor of Mathematics were full of scientific and human interests for the man who was acknowledged as the leading analyst in the world. One phase of these interests demands more than the passing reference that might suffice in a purely scientific biography of Weierstrass: his friendship with his favourite pupil, Sonja (or Sophie) Kowalewski.

Madame Kowalewski's maiden name was Sonja Corvin-Kroukowsky; she was born at Moscow, Russia, on 15 January 1850, and died at Stockholm, Sweden, on 10 February 1891, six years before the death of Weierstrass.

At fifteen Sonja began the study of mathematics. By eighteen she had made such rapid progress that she was ready for advanced work and was enamoured of the subject. As she came of an aristocratic and prosperous family, she was enabled to gratify her ambition for foreign study and matriculated at the University of Heidelberg.

This highly gifted girl became not only the leading woman mathematician of modern times, but also made a reputation as a leader in the movement for the emancipation of women, particularly as regarded their age-old disabilities in the field of higher education.

In addition to all this she was a brilliant writer. As a young girl she hesitated long between mathematics and literature as a career. After the composition of her most important mathematical work (the prize memoir noted later), she turned to literature as a relaxation and wrote the reminiscences of her childhood in Russia in the form of a novel (published first in Swedish and Danish). Of this work it is reported that 'the literary critics of Russia and Scandinavia were unanimous in declaring that Sonja Kowalewski had equalled the best writers of Russian literature in style and thought.' Unfortunately this promising start was blocked by her premature death, and only fragments of other literary works survive. Her one novel was translated into many languages.

Although Weierstrass never married he was no panicky bachelor who took to his heels every time he saw a pretty woman coming. Sonja, according to competent judges who knew her, was extremely good-looking. We must first tell how she and Weierstrass met.

Weierstrass used to enjoy his summer vacations in a thoroughly human manner. The Franco-Prussian war caused him to forego his usual summer trip in 1870, and he stayed in Berlin, lecturing on elliptic functions. Owing to the war his class had dwindled to only twenty instead of the fifty who heard the lectures two years before. Since the autumn of 1869 Sonja Kowalewski, then a dazzling young woman of nineteen, had been studying elliptic functions under Leo Königsberger (born 1837) at the University of Heidelberg, where she had also followed the lectures on physics by Kirchhoff and Helmholtz and had met Bunsen the famous chemist under rather amusing circumstances – to be related presently. Königsberger, one of Weierstrass' first pupils, was a first-rate publicity agent for his master. Sonja caught her teacher's enthusiasm and resolved to go direct to the master himself for inspiration and enlightenment.

The status of unmarried women students in the 1870's was somewhat anomalous. To forestall gossip, Sonja at the age of eighteen contracted what was to have been a nominal marriage, left her husband in Russia, and set out for Germany. Her one indiscretion in her dealings with Weierstrass was her neglect to inform him at the beginning that she was married.

Having decided to learn from the master himself, Sonja took her courage in her hands and called on Weierstrass in Berlin. She was twenty, very earnest, very eager, and very determined; he was fifty-five, vividly grateful for the lift Gudermann had given him toward becoming a mathematician by taking him on as a pupil, and sympathetically understanding of the ambitions of young people. To hide her trepidation Sonja wore a large and floppy hat, 'so that Weierstrass saw nothing of those marvellous eyes whose eloquence, when she wished it, none could resist.'

Some two or three years later, on a visit to Heidelberg, Weierstrass learned from Bunsen – a crabbed bachelor – that Sonja was 'a dangerous woman'. Weierstrass enjoyed his friend's terror hugely, as Bunsen at the time was unaware that Sonja had been receiving frequent private lessons from Weierstrass for over two years.

Poor Bunsen based his estimate of Sonja on bitter personal experience. He had proclaimed for years that no woman, and especially no Russian woman, would ever be permitted to profane the masculine sanctity of his laboratory. One of Sonja's Russian girl friends, desiring ardently to study chemistry in Bunsen's laboratory, and having been thrown out herself, prevailed upon Sonja to try her powers of persuasion on the crusty chemist. Leaving her hat at home, Sonja interviewed Bunsen. He was only too charmed to accept Sonja's friend as a student in his laboratory. After she left he woke up to what she had done to him. 'And now *that woman* has made me eat my own words,' he lamented to Weierstrass.

Sonja's evident earnestness on her first visit impressed Weierstrass favourably and he wrote to Königsberger inquiring about her mathematical aptitudes. He asked also whether 'the lady's personality offers the necessary guarantees.' On receiving an enthusiastic reply, Weierstrass tried to get the university senate to admit Sonja to his mathematical lectures. Being brusquely refused he took care of her himself in his own time. Every Sunday afternoon was devoted to teaching Sonja at his house, and once a week Weierstrass returned her visit. After the first few lessons Sonja lost her hat. The lessons began in the autumn of 1870 and continued with slight interruptions due to vacations or illnesses till the autumn of 1874. When for any reason the friends were unable to meet they corresponded. After Sonja's death in 1891 Weierstrass burnt all her letters to him, together with much of his other correspondence and probably more than one mathematical paper.

The correspondence between Weierstrass and his charming young friend is warmly human, even when most of a letter is given over to mathematics. Much of the correspondence was undoubtedly of considerable scientific importance, but unfortunately Sonja was a very untidy woman when it came to papers, and most of what she left behind was fragmentary or in hopeless confusion.

Weierstrass himself was no paragon in this respect. Without keeping records he loaned his unpublished manuscripts right and left to students who did not always return what they borrowed. Some even brazenly rehashed parts of their teacher's work, spoiled it, and published the results as their own. Although Weierstrass complains about this outrageous practice in letters to Sonja his chagrin is not over the petty pilfering of his ideas but of their bungling in incompetent hands and the consequent damage to mathematics. Sonja of course never descended to anything of this sort, but in another respect she was not entirely blameless. Weierstrass sent her one of his unpublished works by which he set great store, and that was the last he ever saw of it. Apparently she lost it, for she discreetly avoids the topic – to judge from his letters – whenever he brings it up.

To compensate for this lapse Sonja tried her best to get Weierstrass to exercise a little reasonable caution in regard to the rest of his unpublished work. It was his custom to carry about with him on his frequent travels a large white wooden box in which he kept all his working notes and the various versions of papers which he had not yet perfected. His habit was to rework a theory many times until he found the best, the 'natural' way in which it should be developed. Consequently he published slowly and put out a work under his own name only when he had exhausted the topic from some coherent point of view. Several of his rough-hewn projects are said to have been confided to the mysterious box. In 1880, while Weierstrass was on a vacation trip, the box was lost in the baggage. It has never been heard of since.

After taking her degree *in absentia* from Göttingen in 1874, Sonja returned to Russia for a rest as she was worn out by excitement and overwork. Her fame had preceded her and she 'rested' by plunging into the hectic futilities of a crowded social season in St Petersburg while Weierstrass, back in Berlin, pulled wires all over Europe trying to get his favourite pupil a position worthy of her talents. His fruitless efforts disgusted him with the narrowness of the orthodox academic mind.

In October 1875 Weierstrass received from Sonja the news that her father had died. She apparently never replied to his tender condolences, and for nearly three years she dropped completely out of his life. In August 1878 he writes to ask whether she ever received a letter he had written her so long before that he has forgotten its date. 'Didn't you get my letter?

Or what can be preventing you from confiding freely in me, your best friend as you so often called me, as you used to do? This is a riddle whose solution only you can give me. . . .'

In the same letter Weierstrass rather pathetically begs her to contradict the rumour that she has abandoned mathematics: Tchebycheff, a Russian mathematician, had called on Weierstrass when he was out, but had told Borchardt that Sonja had 'gone social', as indeed she had. 'Send your letter to Berlin at the old address', he concludes; 'it will certainly be forwarded to me.'

Man's ingratitude to man is a familiar enough theme; Sonja now demonstrated what a woman can do in that line when she puts her mind to it. She did not answer her old friend's letter for two years although she knew he had been unhappy and in poor health.

The answer when it did come was rather a let-down. Sonja's sex had got the better of her ambitions and she had been living happily with her husband. Her misfortune at the time was to be the focus for the flattery and unintelligent, sideshow wonder of a superficially brilliant mob of artists, journalists, and dilettante litterateurs who gabbled incessantly about her unsurpassable genius. The shallow praise warmed and excited her. Had she frequented the society of her intellectual peers she might still have lived a normal life and have kept her enthusiasm. And she would not have been tempted to treat the man who had formed her mind as shabbily as she did.

In October 1878 Sonja's daughter 'Foufie' was born.

The forced quiet after Foufie's arrival roused the mother's dormant mathematical interests once more, and she wrote to Weierstrass for technical advice. He replied that he must look up the relevant literature before venturing an opinion. Although she had neglected him, he was still ready with his ungrudging encouragement. His only regret (in a letter of October 1880) is that her long silence has deprived him of the opportunity of helping her. 'But I don't like to dwell so much on the past – so let us keep the future before our eyes.'

Material tribulations aroused Sonja to the truth. She was a born mathematician and could no more keep away from mathematics than a duck can from water. So in October 1880 (she was then thirty), she wrote begging Weierstrass to advise her again. Not waiting for his reply she packed up and left Moscow for Berlin. His reply, had she received it, might have caused her to stay where she was. Nevertheless when the distracted Sonja arrived unexpectedly he devoted a whole day to going over her difficulties with her. He must have given her some pretty straight talk, for when she returned to Moscow three months later she went after her mathematics with such fury that her gay friends and silly parasites no longer recognized her. At Weierstrass' suggestion she attacked the problem of the propagation of light in a crystalline medium.

In 1882 the correspondence takes two new turns, one of which is of mathematical interest. The other is Weierstrass' outspoken opinion that Sonja and her husband are unsuited to one another, especially as the latter has no true appreciation of her intellectual merits. The mathematical point refers to Poincaré, then at the beginning of his career. With his sure instinct for recognizing young talent, Weierstrass hails Poincaré as a coming man and hopes that he will outgrow his propensity to publish too rapidly and let his researches ripen without scattering them over too wide a field. 'To publish an article of real merit every week – that is impossible', he remarks, referring to Poincaré's deluge of papers.

Sonja's domestic difficulties presently resolved themselves through the sudden death of her husband in March 1883. She was in Paris at the time, he in Moscow. The shock prostrated her. For four days she shut herself up alone, refused food, lost consciousness the fifth day, and on the sixth recovered, asked for paper and pencil, and covered the paper with mathematical formulae. By autumn she was herself again, attending a scientific congress at Odessa.

Thanks to Mittag-Leffler, Madame Kowalewski at last obtained a position where she could do herself justice; in the autumn of 1884 she was lecturing at the University of Stockholm, where she was to be appointed (in 1889) as professor for life. A little later she suffered a rather embarrassing setback when the Italian mathematician Vito Volterra pointed out a serious mistake in her work on the refraction of light in crystalline media. This oversight had escaped Weierstrass, who at the time was so overwhelmed with official duties that outside of them he had 'time only for eating, drinking, and sleeping. . . . In short', he says, 'I am what the doctors call brain-weary.' He was now nearly seventy. But as his bodily ills increased his intellect remained as powerful as ever.

The master's seventieth birthday was made the occasion for public honours and a gathering of his disciples and former pupils from all over Europe. Thereafter he lectured publicly less and less often, and for ten years received a few of his students at his own house. When they saw that he was tired out they avoided mathematics and talked of other things, or listened eagerly while the companionable old man reminisced of his student pranks and the dreary years of his isolation from all scientific friends. His eightieth birthday was celebrated by an even more impressive jubilee than his seventieth and he became in some degree a national hero of the German people.

One of the greatest joys Weierstrass experienced in his declining years was the recognition won at last by his favourite pupil. On Christmas Eve, 1888, Sonja received in person the Bordin Prize of the French Academy of Sciences for her memoir *On the rotation of a solid body about a fixed point.*

As is the rule in competition for such prizes, the memoir had been submitted anonymously (the author's name being in a sealed envelope bearing on the outside the same motto as that inscribed on the memoir, the envelope to be opened only if the competing work won the prize), so there was no opportunity for jealous rivals to hint at undue influence. In the opinion of the judges the memoir was of such exceptional merit that they raised the value of the prize from the previously announced 3,000 francs to 5,000. The monetary value, however, was the least part of the prize.

Weierstrass was overjoyed. 'I do not need to tell you', he writes, 'how much your success has gladdened the hearts of myself and my sisters, also of your friends here. I particularly experienced a true satisfaction; competent judges have now delivered their verdict that my "faithful pupil", my "weakness" is indeed not a "frivolous humbug".'

We may leave the friends in their moment of triumph. Two years later (10 February 1891) Sonja died in Stockholm at the age of forty-one after a brief attack of influenza which at the time was epidemic. Weierstrass outlived her six years, dying peacefully in his eighty-second year on 19 February 1897 at his home in Berlin after a long illness followed by influenza. His last wish was that the priest say nothing in his praise at the funeral but restrict the services to the customary prayers.

Sonja is buried in Stockholm, Weierstrass with his two sisters in a Catholic cemetery in Berlin. Sonja also was of the Catholic faith, belonging to the Eastern Church.

We shall now give some intimation of two of the basic ideas on which Weierstrass founded his work in analysis. Details or an exact description are out of the question here, but may be found in the earlier chapters of any competently written book on the theory of functions.

A *power series* is an expression of the form

$$a_0 + a_1z + a_2z^2 + \dots + a_nz^n + \dots,$$

in which the coefficients $a_0, a_1, a_2 \dots, a_n, \dots$ are constant numbers and z is a variable number; the numbers concerned may be real or complex.

The sums of 1,2,3, ... terms of the series, namely a_0, $a_0 + a_1z$, $a_0 + a_1z + a_2z^2$, ... are called the *partial sums*. If for some particular value of z these partial sums give a sequence of numbers which converge to a definite limit, the power series is said to converge to the same limit for that value of z.

All the values of z for which the power series converges to a limit constitute the *domain of convergence* of the series; for any value of the variable z in this domain the series *converges*; for other values of z it *diverges*.

If the series converges for some value of z, its value can be calculated to any desired degree of approximation, for that value, by taking a sufficiently large number of terms.

Now, in the majority of mathematical problems which have applications to science, the 'answer' is indicated as the solution in series of a differential equation (or system of such equations), and this solution is only rarely obtainable as a finite expression in terms of mathematical functions which have been tabulated (for instance logarithms, trigonometric functions, elliptic functions, etc.). In such problems it then becomes necessary to do two things: prove that the series converges, if it does; calculate its numerical values to the required accuracy.

If the series does not converge it is usually a sign that the problem has been either incorrectly stated or wrongly solved. The multitude of functions which present themselves in pure mathematics are treated in the same way, whether they are ever likely to have scientific applications or not, and finally a general theory of convergence has been elaborated to cover vast tracts of all this, so that the individual examination of a particular series is often referred to more inclusive investigations already carried out.

Finally, all this (both pure and applied) is extended to power series in 2, 3, 4, ... variables instead of the single variable z above; for example, in two variables,

$$a + b_0 z + b_1 w + c_0 z^2 + c_1 zw + c_2 w^2 + \ldots.$$

It may be said that without the theory of power series most of mathematical physics (including much of astronomy and astro-physics) as we know it would not exist.

Difficulties arising with the concepts of limits, continuity, and convergence drove Weierstrass to the creation of his theory of irrational numbers.

Suppose we extract the square root of 2 as we did in school, carrying the computation to a large number of decimal places. We get as successive approximations to the required square root the *sequence* of numbers 1, 1.4, 1.41, 1.412, ... With sufficient labour, proceeding by well-defined steps according to the usual rule, we could if necessary exhibit the first thousand, or the first million, of the *rational* numbers 1, 1.4, ... constituting this sequence of approximations. Examining this sequence we see that when we have gone far enough we have determined a perfectly definite rational number containing as many decimal places as we please (say 1,000), and that *this* rational number differs from any of the *succeeding* rational numbers in the sequence by a number (decimal), such as ·000 ... ·000 ..., in which a correspondingly large number of zeros occur *before* another digit (1, 2, ... or 9) appears.

This illustrates what is meant by a *convergent sequence* of numbers: the *rationals* 1, 1·4, ... constituting the sequence give us ever closer approximations to the 'irrational number' which we *call* the square root of 2, and which we conceive of as having been *defined* by the *convergent sequence of rationals*, this definition being in the sense that a method has been indicated (the usual school one) of calculating *any particular member of the sequence in a finite number of steps*.

Although it is impossible actually to exhibit the whole sequence, as it does not stop at any finite number of terms, nevertheless we regard the *process* for constructing *any* member of the sequence as a sufficiently clear conception of the whole sequence as a single definite object which we can reason about. Doing so, we have a workable method for *using* the square root of 2 and similarly for any irrational number, in mathematical analysis.

As has been indicated it is impossible to make this precise in an account like the present, but even a careful statement might disclose some of the logical objections glaringly apparent in the above description – objections which inspired Kronecker and others to attack Weierstrass' 'sequential' definition of irrationals.

Nevertheless, right or wrong, Weierstrass and his school made the theory *work*. The most useful results they obtained have not yet been questioned, at least on the ground of their great utility in mathematical analysis and its applications, by any competent judge in his right mind. This does not mean that objections cannot be well taken: it merely calls attention to the fact that in mathematics, as in everything else, this earth is not yet to be confused with the Kingdom of Heaven, that perfection is a chimaera, and that, in the words of Crelle, we can only hope for closer and closer approximations to mathematical truth – whatever that may be, if anything – precisely as in the Weierstrassian theory of convergent sequences of rationals defining irrationals.

After all, why should mathematicians, who are human beings like the rest of us, always be so pedantically exact and so inhumanly perfect? As Weierstrass said, 'It is true that a mathematician who is not also something of a poet will never be a perfect mathematician'. That is the answer: a perfect mathematician, by the very fact of his poetic perfection, would be a mathematical impossibility.

CHAPTER TWENTY-THREE

COMPLETE INDEPENDENCE

Boole

*

'OH, we never read anything the English mathematicians do.' This characteristically Continental remark was the reply of a distinguished European mathematician when he was asked whether he had seen some recent work of one of the leading English mathematicians. The 'we' of his frank superiority included Continental mathematicians in general.

This is not the sort of story that mathematicians like to tell on themselves, but as it illustrates admirably that characteristic of British mathematicians – insular originality – which has been the chief claim to distinction of the British school, it is an ideal introduction to the life and work of one of the most insularly original mathematicians England has produced, George Boole. The fact is that British mathematicians have often serenely gone their own way, doing the things that interested them personally as if they were playing cricket for their own amusement only, with a self-satisfied disregard for what others, shouting at the top of their scientific lungs, have assured the world is of supreme importance. Sometimes, as in the prolonged idolatry of Newton's methods, indifference to the leading fashions of the moment has cost the British school dearly, but in the long run the take-it-or-leave-it attitude of this school has added more new fields to mathematics than a slavish imitation of the Continental masters could ever have done. The theory of invariance is a case in point; Maxwell's electrodynamic field theory is another.

Although the British school has had its share of powerful developers of work started elsewhere, its greater contribution to the progress of mathematics has been in the direction of originality. Boole's work is a striking illustration of this. When first put out it was ignored *as mathematics*, except by a few, chiefly Boole's own more unorthodox countrymen, who recognized that here was the germ of something of supreme interest for all mathematics. To-day the natural development of what Boole started is rapidly becoming one of the major divisions of pure mathematics, with scores of workers in practically all countries extending it to all fields of mathematics where attempts are being made to consolidate our gains on firmer foundations. As Bertrand Russell remarked some years ago, pure mathematics was *discovered* by George Boole in his work *The Laws of Thought* published in 1854. This may be an exaggeration, but it gives a measure of the importance in which mathematical logic and its ramifications are held to-day. Others before Boole, notably Leibniz and De Morgan, had dreamed of adding logic itself to the domain of algebra; Boole did it.

George Boole was not, like some of the other originators in mathematics, born into the lowest economic stratum of society. His fate was much harder. He was born on 2 November 1815 at Lincoln, England, and was the son of a petty shopkeeper. If we can credit the picture drawn by English writers themselves of those hearty old days – 1815 was the year of Waterloo – to be the son of a small tradesman at that time was to be damned by foreordination.

The whole class to which Boole's father belonged was treated with a contempt a trifle more contemptuous than that reserved for enslaved scullery maids and despised second footmen. The 'lower classes', into whose ranks Boole had been born, simply did not exist in the eyes of the 'upper classes' – including the more prosperous wine merchants and moneylenders. It was taken for granted that a child in Boole's station should dutifully and gratefully master the shorter catechism and so live as never to transgress the strict limits of obedience imposed by that remarkable testimonial to human conceit and class-conscious snobbery.

To say that Boole's early struggles to educate himself into a station above that to which 'it had pleased God to call him' were a fair imitation of purgatory is putting it mildly. By an act of divine providence Boole's great spirit had been assigned to the meanest class; let it stay there then and stew in its own ambitious juice. Americans may like to recall that Abraham Lincoln, only six years older than Boole, had his struggle about the same time. Lincoln was not sneered at but encouraged.

The schools where young gentlemen were taught to knock one another about in training for their future parts as leaders in the sweatshop and coal mine systems then coming into vogue were not for the likes of George Boole. No; his 'National School' was designed chiefly with the end in view of keeping the poor in their proper, unwashable place.

A wretched smattering of Latin, with perhaps a slight exposure to Greek, was one of the mystical stigmata of a gentleman in those incomprehensible days of the sooty industrial revolution. Although few of the boys ever mastered Latin enough to enable them to read it without a crib, an assumed knowledge of its grammar was one of the hallmarks of gentility, and its syntax, memorized by rote, was, oddly enough, esteemed as mental discipline of the highest usefulness in preparation for the ownership and conservation of property.

Of course no Latin was taught in the school that Boole was permitted to attend. Making a pathetically mistaken diagnosis of the abilities which enabled the propertied class to govern those beneath them in the scale of wealth, Boole decided that he must learn Latin and Greek if he was ever to get his feet out of the mire. This was Boole's mistake. Latin and Greek had nothing to do with the cause of his difficulties. He did teach himself Latin with his poor struggling father's sympathetic encouragement. Although the poverty-stricken tradesman knew that he himself would never escape he did what he could to open the door for his son. He knew no Latin. The struggling boy appealed to another tradesman, a small bookseller and friend of his father. This good man could only give the boy a start in the elementary grammar. Thereafter Boole had to go it alone. Anyone who has watched even a good teacher trying to get a normal child of eight through Caesar will realize what the untutored Boole was up against. By the age of twelve he had mastered enough Latin to translate an ode of Horace into English verse. His father, hopefully proud but understanding nothing of the technical merits of the translation, had it printed in the local paper. This precipitated a scholarly row, partly flattering to Boole, partly humiliating.

A classical master denied that a boy of twelve could have produced such a translation. Little boys of twelve often know more about some things than their forgetful elders give them credit for. On the technical side grave defects showed up. Boole was humiliated and resolved to supply the deficiencies of his self-instruction. He had also taught himself Greek. Determined now to do a good job or none he spent the next two years slaving over Latin and Greek, again without help. The effect of all this drudgery is plainly apparent in the dignity and marked Latinity of much of Boole's prose.

Boole got his early mathematical instruction from his father, who had gone considerably beyond his own meagre schooling by private study. The father had also tried to interest his son in another hobby, that of making optical instruments, but Boole, bent on his own ambition, stuck to it that the classics were the key to dominant living. After finishing his common schooling he took a commercial course. This time his diagnosis was better, but it did not help him greatly. By the age of sixteen he saw that he must contribute at once to the support of his wretched parents. School teaching offered the most immediate opportunity of earning steady wages – in Boole's day 'ushers', as assistant teachers were called, were not paid salaries but wages. There is more than a monetary difference between the two. It may have been about this time that the immortal Squeers, in Dickens' *Nicholas Nickleby*, was making his great but unappreciated contribution to modern pedagogy at Dotheboys Hall with his brilliant anticipation of the 'project' method. Young Boole may even have been one of Squeers' ushers; he taught at two schools.

Boole spent four more or less happy years teaching in these elementary schools. The chilly nights, at least, long after the pupils were safely and mercifully asleep, were his own. He still was on the wrong track. A third diagnosis of his social unworthiness was similar to his second but a considerable advance over both his first and second. Lacking anything in the way of capital – practically every penny the young man earned went to the support of his parents and the barest necessities of his own meagre existence – Boole now cast an appraising eye over the gentlemanly professions. The Army at that time was out of his reach as he could not afford to purchase a commission. The Bar made obvious financial and educational demands which he had no prospect of satisfying. Teaching, of the grade in which he was then engaged, was not even a reputable trade, let alone a profession. What remained? Only the Church. Boole resolved to become a clergyman.

In spite of all that has been said for and against God, it must be admitted even by his severest critics that he has a sense of humour. Seeing the ridiculousness of George Boole's ever becoming a clergyman, he skilfully turned the young man's eager ambition into less preposterous channels. An unforeseen affliction of greater poverty than any they had yet enjoyed compelled Boole's parents to urge their son to forego all thoughts of ecclesiastical eminence. But his four years of private preparation (and rigid privation) for the career he had planned were not wholly wasted; he had acquired a mastery of French, German, and Italian, all destined to be of indispensable service to him on his true road.

At last he found himself. His father's early instruction now bore fruit. In his twentieth year Boole opened up a civilized school of his own. To prepare his pupils properly he had to teach them some mathematics as it should be taught. His interest was aroused. Soon the ordinary and execrable textbooks of the day awoke his wonder, then his contempt. Was this stuff mathematics? Incredible. What did the great masters of mathematics say? Like Abel and Galois, Boole went directly to great headquarters for his marching orders. It must be remembered that he had had no mathematical training beyond the rudiments. To get some idea of his mental capacity we can imagine the lonely student of twenty mastering, by his own unaided efforts, the *Mécanique céleste* of Laplace, one of the toughest masterpieces ever written for a conscientious student to assimilate, for the mathematical reasoning in it is full of gaps and enigmatical declarations that 'it is easy to see', and then we must think of him making a thorough, understanding study of the excessively abstract *Mécanique analytique* of Lagrange, in which there is not a single diagram to illuminate the analysis from beginning to end. Yet Boole, self-taught, found his way and saw what he was doing. He even got his first contribution to mathematics out of his unguided efforts. This was a paper on the calculus of variations.

Another gain that Boole got out of all this lonely study deserves a separate paragraph to itself. He discovered invariants. The significance of this great discovery which Cayley and Sylvester were to develop in grand fashion has been sufficiently explained; here we repeat that without the mathematical theory of invariance (which grew out of the early algebraic work) the theory of relativity would have been impossible. Thus at the very threshold of his scientific career Boole noticed something lying at his feet which Lagrange himself might easily have seen, picked it up, and found that he had a gem of the first water. That Boole saw what others had overlooked was due no doubt to his strong feeling for the symmetry and beauty of algebraic relations – when of course they happen to be both symmetrical and beautiful; they are not always. Others might have thought his find merely pretty. Boole recognized that it belonged to a higher order.

Opportunities for mathematical publication in Boole's day were inadequate unless an author happened to be a member of some learned society with a journal or transactions of its own. Luckily for Boole, *The Cambridge Mathematical Journal*, under the able editorship of the Scotch mathematician, D. F. Gregory, was founded in 1837. Boole submitted some of his work. Its originality and style impressed Gregory favourably, and a cordial mathematical correspondence began a friendship which lasted out Boole's life.

It would take us too far afield to discuss here the great contribution which the British school was making at the time to the understanding of algebra as *algebra*, that is, as the abstract development of the consequences of a set of postulates without necessarily any interpretation or application to 'numbers' or anything else, but it may be mentioned that the modern conception of algebra began with the British 'reformers', Peacock, Herschel, De Morgan, Babbage, Gregory, and Boole. What was a somewhat heretical novelty when Peacock published his *Treatise on Algebra* in 1830 is to-day a commonplace in any competently written school book. Once and for all Peacock broke away from the superstitition that the $x, y, z, \ldots$ in such relations as $x + y = y + x$, $xy = yx$, $x(y + z) = xy + xz$, and so on, as we find them in elementary algebra, necessarily 'represent numbers'; they do not, and that is one of the most important things about algebra and the source of its power in applications. The $x, y, z, \ldots$ are merely arbitrary marks, combined according to certain operations, one of which is symbolized as $+$, another by $\times$ (or simply as xy instead of $x \times y$), in accordance with postulates laid down at the beginning, like the specimens $x + y = y + x$, etc., above.

Without this realization that algebra is of itself nothing more than an abstract system, algebra might still have been stuck fast in the arithmetical mud of the eighteenth century, unable to move forward to its modern and extremely useful variants under the direction of Hamilton. We need only note here that this renovation of algebra gave Boole his first opportunity to do fine work appreciated by his contemporaries. Striking out on his own initiative he separated the *symbols* of mathematical *operations* from the things upon which they operate and proceeded to investigate these operations on their own account. How did they combine? Were they too subject to some sort of symbolic algebra? He found that they were. His work in this direction is extremely interesting, but it is overshadowed by the contribution which is peculiarly his own, the creation of a simple, workable system of symbolic or mathematical logic.

To introduce Boole's splendid invention properly we must digress slightly and recall a famous row of the first half of the nineteenth century, which raised a devil of a din in its own day but which is now almost forgotten except by historians of pathological philosophy. We mentioned Hamilton a moment ago. There were two Hamiltons of public fame at this time, one the Irish mathematician Sir William Rowan Hamilton (1805–65), the other the Scotch philosopher Sir William Hamilton

(1788–1856). Mathematicians usually refer to the philosopher as the *other* Hamilton. After a somewhat unsuccessful career as a Scotch barrister and candidate for official university positions the eloquent philosopher finally became Professor of Logic and Metaphysics in the University of Edinburgh. The mathematical Hamilton, as we have seen, was one of the outstanding original mathematicians of the nineteenth century. This is perhaps unfortunate for the *other* Hamilton, as the latter had no earthly use for mathematics, and hasty readers sometimes confuse the two famous Sir Williams. This causes the other one to turn and shiver in his grave.

Now, if there is anything more obtuse mathematically than a thick-headed Scotch metaphysician it is probably a mathematically thicker-headed German metaphysician. To surpass the ludicrous absurdity of some of the things the Scotch Hamilton said about mathematics we have to turn to what Hegel said about astronomy or Lotze about non-Euclidean geometry. Any depraved reader who wishes to fuddle himself can easily run down all he needs. It was the metaphysician Hamilton's misfortune to have been too dense or too lazy to get more than the most trivial smattering of elementary mathematics at school, but 'omniscience was his foible', and when he began lecturing and writing on philosophy, he felt constrained to tell the world exactly how worthless mathematics is.

Hamilton's attack on mathematics is probably the most famous of all the many savage assaults mathematics has survived, undented. Less than ten years ago lengthy extracts from Hamilton's diatribe were vigorously applauded when a pedagogical enthusiast retailed them at a largely attended meeting of America's National Educational Association. Instead of applauding, the auditors might have got more out of the exhibition if they had paused to swallow some of Hamilton's philosophy as a sort of compulsory sauce for the proper enjoyment of his mathematical herring. To be fair to him we shall pass on a few of his hottest shots and let the reader make what use of them he pleases.

'Mathematics [Hamilton always used 'mathematics' as a plural, not a singular, as customary to-day] freeze and parch the mind'; 'an excessive study of mathematics absolutely incapacitates the mind for those intellectual energies which philosophy and life require'; 'mathematics can not conduce to logical habits at all'; 'in mathematics dullness is thus elevated into talent, and talent degraded into incapacity'; 'mathematics may distort, but can never rectify, the mind'.

This is only a handful of the birdshot; we have not room for the cannon balls. The whole attack is most impressive – for a man who knew far less mathematics than any intelligent child of ten knows. One last shot deserves special mention, as it introduces the figure of mathematical importance in the whole wordy war, De Morgan (1806–71), one of the most expert controversialists who ever lived, a mathematician of vigorous independence, a great logician who prepared the way for Boole, the remorselessly good-humoured enemy of all cranks, charlatans, and humbugs, and finally father of the famous novelist (*Alice for Short*, etc.). Hamilton remarks, 'This [a perfectly nonsensical reason that need not be repeated] is why Mr De Morgan among other mathematicians so often argues right. Still, had Mr De Morgan been less of a Mathematician, he might have been more of a Philosopher; and be it remembered, that mathematics and dram-drinking tell especially, in the long run.' Although the esoteric punctuation is obscure the meaning is clear enough. But it was not De Morgan who was given to tippling.

De Morgan, having gained some fame from his pioneering studies in logic, allowed himself in an absent-minded moment to be trapped into a controversy with Hamilton over the latter's famous principle of 'the quantification of the predicate.' There is no need to explain what this mystery is (or was); it is as dead as a coffin nail. De Morgan had made a real contribution to the syllogism; Hamilton thought he detected De Morgan's diamond in his own blue mud; the irate Scottish lawyer-philosopher publicly accused De Morgan of plagiarism – an insanely unphilosophical thing to do – and the fight was on. On De Morgan's side, at least, the row was a hilarious frolic. De Morgan never lost his temper; Hamilton had never learned to keep his.

If this were merely one of the innumerable squabbles over priority which disfigure scientific history it would not be worth a passing mention. Its historical importance is that Boole by now (1848) was a firm friend and warm admirer of De Morgan. Boole was still teaching school, but he knew many of the leading British mathematicians personally or by correspondence. He now came to the aid of his friend – not that the witty De Morgan needed any mortal's aid, but because he knew that De Morgan was right and Hamilton wrong. So, in 1848, Boole published a slim volume, *The Mathematical Analysis of Logic*, his first public contribution to the vast subject which his work inaugurated and in which he was to win enduring fame for the boldness and perspicacity of his vision. The pamphlet – it was hardly more than that – excited De Morgan's warm admiration. Here was the master, and De Morgan hastened to recognize him. The booklet was only the promise of greater things to come six years later, but Boole had definitely broken new, stubborn ground.

In the meantime, reluctantly turning down his mathematical friends' advice that he proceed to Cambridge and take the orthodox mathematical training there, Boole went on with the drudgery of elementary teaching, without a complaint, because his parents were now wholly dependent upon his support. At last he got an opportunity where his conspicuous abilities as an investigator and a lecturer could have some play. He was appointed Professor of Mathematics at the recently opened Queen's College at what was then called the city of Cork, Ireland. This was in 1849.

Needless to say, the brilliant man who had known only poverty and hard work all his life made excellent use of his comparative freedom from financial worry and everlasting grind. His duties would now be considered onerous; Boole found them light by contrast with the dreary round of elementary teaching to which he had been accustomed. He produced much notable miscellaneous mathematical work, but his main effort went on licking his masterpiece into shape. In 1854 he published it: *An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and Probabilities.*

Boole was thirty-nine when this appeared. It is somewhat unusual for a mathematician as old as that to produce work of such profound originality, but the phenomenon is accounted for when we remember the long, devious path Boole was compelled to follow before he could set his face fairly toward his goal. (Compare the careers of Boole and Weierstrass.)

A few extracts will give some idea of Boole's style and the scope of his work.

'The design of the following treatise is to investigate the fundamental laws of those operations of the mind by which reasoning is performed; to give expression to them in the language of a Calculus, and upon this foundation to establish the science of Logic and construct its method; to make that method itself the basis of a general method for the application of the mathematical doctrine of probabilities; and, finally, to collect from the various elements of truth brought to view in the course of these inquiries some probable intimations concerning the nature and constitution of the human mind. . . .'

'Shall we then err in regarding that as the true science of Logic which, laying down certain elementary laws, confirmed by the very testimony of the mind, permits us thence to deduce, by uniform processes, the entire chain of its secondary consequences, and furnishes, for its practical applications, methods of perfect generality? . . .'

'There exist, indeed, certain general principles founded in the very nature of language, by which the use of symbols, which are but the elements of scientific language, is determined. To a certain extent these elements are arbitrary. Their interpretation is purely conventional: we are permitted to employ them in whatever sense we please. But this permission is limited by two indispensable conditions, – first, that from the sense once conventionally established we never, in the same process of reasoning, depart; secondly, that the laws by which the process is conducted be founded exclusively upon the above fixed sense or meaning of the symbols employed. In accordance with these principles, any agreement which may be established between the laws of the symbols of Logic and those of Algebra can but issue in an agreement of processes. The two provinces of interpretation remain apart and independent, each subject to its own laws and conditions.

'Now the actual investigations of the following pages exhibit Logic, in its practical aspect, as a system of processes carried on by the aid of symbols having a definite interpretation, and subject to laws founded upon that interpretation alone. But at the same time they exhibit those laws as identical in form with the laws of the general symbols of Algebra, with this single addition, viz., that the symbols of Logic are further subject to a special law [$x^2 = x$ in the algebra of logic, which can be interpreted, among other ways, as "the class of all those things common to a class x and itself is merely the class x"], to which the symbols of quantity, as such, are not subject.' (That is, in common algebra, it is not true that *every* x is equal to its square, whereas in the Boolean algebra of logic, this *is* true.)

This programme is carried out in detail in the book. Boole reduced logic to an extremely easy and simple type of algebra. 'Reasoning' upon appropriate material becomes in this algebra a matter of elementary manipulations of formulae far simpler than most of those handled in a second year of school algebra. Thus logic itself was brought under the sway of mathematics.

Since Boole's pioneering work his great invention has been modified, improved, generalized, and extended in many directions. To-day symbolic or mathematical logic is indispensable in any serious attempt to understand the nature of mathematics and the state of its foundations on which the whole colossal superstructure rests. The intricacy and delicacy of the difficulties explored by the *symbolic* reasoning would, it is safe to say, defy human reason if only the old, pre-Boole methods of *verbal* logical arguments were at our disposal. The daring originality of Boole's whole project needs no signpost. It is a landmark in itself.

Since 1899, when Hilbert published his classic on the foundations of geometry, much attention has been given to the postulational formulation of the several branches of mathematics. This movement goes back as far as Euclid, but for some strange reason – possibly because the techniques invented by Descartes, Newton, Leibniz, Euler, Gauss, and others gave mathematicians plenty to do in developing their subject freely and somewhat uncritically – the Euclidean method was for long neglected in everything but geometry. We have already seen that the British school applied the method to algebra in the first half of the nineteenth century. Their successes seem to have made no very great impression on the work of their contemporaries and immediate successors, and it was only with the work of Hilbert that the postulational method came to be recognized as the clearest and most rigorous approach to any mathematical discipline.

To-day this tendency to abstraction, in which the symbols and rules of operation in a particular subject are emptied of all meaning and discussed from a purely formal point of view, is all the rage, rather to the neglect of applications (practical or mathematical) which some say are the ultimate human justification for any scientific activity. Nevertheless the abstract method does give insights which looser attacks do not, and in particular the true simplicity of Boole's algebra of logic is most easily seen thus.

Accordingly we shall state the postulates for Boolean algebra (the algebra of logic) and, having done so, see that they can indeed be given an interpretation consistent with classical logic. The following set of postulates is taken from a paper by E. V. Huntington, in the *Transactions of the American Mathematical Society* (vol. 35, 1933, pp. 274–304). The whole paper is easily understandable by anyone who has had a week of algebra, and may be found in most large public libraries. As Huntington points out, this first set of his which we transcribe is not as elegant as some of his others. But as its interpretation in terms of class inclusion as in formal logic is more immediate than the like for the others, it is to be preferred here.

The set of postulates is expressed in terms of K, $+$, $\times$, where K is a class of undefined (wholly arbitrary, without any assigned meaning or properties beyond those given in the postulates) elements $a, b, c, \ldots$, and $a + b$ and $a \times b$ (written also simply as ab) are the results of two undefined binary operations, $+$, $\times$ ('binary', because each of $+$, $\times$ operates on *two* elements of K). There are ten postulates, I a–VI:

'I a. *If a and b are in the class K, then $a + b$ is in the class K.*

'I b. *If a and b are in the class K, then ab is in the class K.*

'II a. *There is an element Z such that $a + Z = a$ for every element a.*

'II b. *There is an element U such that $aU = a$ for every element a.*

'III a. $a + b = b + a$.

'III b. $ab = ba$.

'IV a. $a + bc = (a + b)(a + c)$.

'IV b. $a(b + c) = ab + ac$.

'V. *For every element a there is an element a' such that $a + a' = U$ and $aa' = Z$.*

'VI. *There are at least two distinct elements in the class K.*'

It will be readily seen that these postulates are satisfied by the following interpretation: $a, b, c, \ldots$ are *classes*; $a + b$ is the class of all those things that are in *at least one* of the classes, a, b; ab is the class of all those things that are in *both* of the classes a, b; Z is the 'null class' – the class that has no members; U is the 'universal class' – the class that contains *all* the things in *all* the classes under discussion. Postulate V then states that given any class a, there is a class a' consisting of all those things which are not in a. Note that VI implies that U, Z are not the same class.

From such a simple and obvious set of statements it seems rather remarkable that the whole of classical logic can be built up symbolically by means of the easy algebra generated by the postulates. From these postulates a theory of what may be called 'logical equations' is developed: problems in logic are translated into such equations, which are then 'solved' by the devices of the algebra; the solution is then reinterpreted in terms of the logical data, giving the solution of the original problem. We shall close this description with the symbolic equivalent of 'inclusion' – also interpretable, when *propositions* rather than *classes* are the elements of K, as 'implication'.

'*The relation $a < b$* [read, *a is included in b*] *is defined by any one of the following equations*

$$a + b = b, ab = a, a' + b = U, ab' = Z.\text{'}$$

To see that these are reasonable, consider for example the second, $ab = a$. This states that if a is included in b, then everything that is in *both* a and b is the whole of a.

From the stated postulates the following theorems on inclusion (with thousands of more complicated ones, if desired) can be *proved*. The specimens selected all agree with our intuitive conception of what 'inclusion' means.

(1) $a < a$.

(2) *If* $a < b$ *and* $b < c$, *then* $a < c$.

(3) *If* $a < b$ *and* $b < a$, *then* $a = b$.

(4) $Z < a$ (*where* Z *is the element in* II a – it is proved to be the *only* element satisfying II a).

(5) $a < U$ (*where* U *is the element in* II b – likewise unique).

(6) $a < a + b$; *and if* $a < y$ *and* $b < y$, *then* $a + b < y$.

(7) $ab < a$; *and if* $x < a$ *and* $x < b$, *then* $x < ab$.

(8) *If* $x < a$ *and* $x < a'$, *then* $x = Z$; *and if* $a < y$ *and* $a' < y$, *then* $y = U$.

(9) *If* $a < b'$ *is false, then there is at least one element* x, *distinct from* Z, *such that* $x < a$ *and* $x < b$.

It may be of interest to observe that '$<$' in arithmetic and analysis is the symbol for 'less than'. Note that if $a, b, c, \ldots$ are real numbers, and Z denotes zero, then (2) is satisfied for this interpretation of '$<$', and similarly for (4), provided a is positive; but that (1) is not satisfied, nor is the second part of (6) – as we see from $5 < 10, 7 < 10$, but $5 + 7 < 10$ is false.

The tremendous power and fluent ease of the method can be readily appreciated by seeing what it does in any work on symbolic logic. But, as already emphasized, the importance of this 'symbolic reasoning' is in its applicability to subtle questions regarding the foundations of all mathematics which, were it not for this precise method of fixing meanings of 'words' or other 'symbols' once for all, would probably be unapproachable by ordinary mortals.

Like nearly all novelties, symbolic logic was neglected for many years after its invention. As late as 1910 we find eminent mathematicians scorning it as a 'philosophical' curiosity without mathematical significance. The work of Whitehead and Russell in *Principia Mathematica* (1910–13) was the first to convince any considerable body of professional mathematicians that symbolic logic might be worth their serious attention. One staunch hater of symbolic logic may be mentioned – Cantor, whose work on the infinite will be noticed in the concluding chapter. By one of those little ironies which make mathematical history such amusing reading for the open-minded, symbolic logic was to play an important part in the drastic criticism of Cantor's work that caused its author to lose faith in himself and his theory.

Boole did not long survive the production of his masterpiece. The year after its publication, still subconsciously striving for the social respectability that he once thought a knowledge of Greek could confer, he married Mary Everest, niece of the Professor of Greek in Queen's College. His wife became his devoted disciple. After her husband's death, Mary Boole applied some of the ideas which she had acquired from him to rationalizing and humanizing the education of young children. In her pamphlet, *Boole's Psychology*, Mary Boole records an interesting speculation of Boole's which readers of *The Laws of Thought* will recognize as in keeping with the unexpressed but implied personal philosophy in certain sections. Boole told his wife that in 1832, when he was about seventeen, it 'flashed upon' him as he was walking across a field that besides the knowledge gained from direct observation, man derives knowledge from some source undefinable and invisible – which Mary Boole calls 'the unconscious'. It will be interesting (in a later chapter) to hear Poincaré expressing a similar opinion regarding the genesis of mathematical 'inspirations' in the 'subconscious mind'. Anyhow, Boole was inspired, if ever a mortal was, when he wrote *The Laws of Thought.*

Boole died, honoured and with a fast-growing fame, on 8 December 1864, in the fiftieth year of his age. His premature death was due to pneumonia contracted after faithfully keeping a lecture engagement when he was soaked to the skin. He fully realized that he had done great work.

CHAPTER TWENTY-FOUR

THE MAN, NOT THE METHOD

Hermite

OUTSTANDING unsolved problems demand new methods for their solution, while powerful new methods beget new problems to be solved. But, as Poincaré observed, it is the man, not the method, that solves a problem.

Of old problems responsible for new methods in mathematics that of motion and all it implies for mechanics, terrestrial and celestial, may be recalled as one of the principal instigators of the calculus and present attempts to put reasoning about the infinite on a firm basis. An example of new problems suggested by powerful new methods is the swarm which the tensor calculus, popularized to geometers by its successes in relativity, let loose in geometry. And finally, as an illustration of Poincaré's remark, it was Einstein, and not the method of tensors, that solved the problem of giving a coherent mathematical account of gravitation. All three theses are sustained in the life of Charles Hermite, the leading French mathematician of the second half of the nineteenth century – if we except Hermite's pupil Poincaré, who belonged partly to our own century.

Charles Hermite, born at Dieuze, Lorraine, France, on 24 December 1822 could hardly have chosen a more propitious era for his birth than the third decade of the nineteenth century. His was just the rare combination of creative genius and the ability to master the best in the work of other men which was demanded in the middle of the century to co-ordinate the arithmetical creations of Gauss with the discoveries of Abel and Jacobi in elliptic functions, the striking advances of Jacobi in Abelian functions, and the vast theory of algebraic invariants in process of rapid development by the English mathematicians Boole, Cayley, and Sylvester.

Hermite almost lost his life in the French Revolution – although the last head had fallen nearly a quarter of a century before he was born. His paternal grandfather was ruined by the Commune and died in prison; his grandfather's brother went to the guillotine. Hermite's father escaped owing to his youth.

If Hermite's mathematical ability was inherited, it probably came from the side of the father, who had studied engineering. Finding engineering uncongenial, Hermite senior gave it up, and after an equally distasteful start in the salt industry, finally settled down in business as a cloth merchant. This resting place was no doubt chosen by the rolling stone because he had married his employer's daughter, Madeleine Lallemand, a domineering woman who wore the breeches in her family and ran everything from the business to her husband. She succeeded in building both up to a state of solid bourgeois prosperity. Charles was the sixth of seven children – five sons and two daughters. He was born with a deformity of the right leg which rendered him lame for life – possibly a disguised blessing, as it effectively barred him from any career even remotely connected with the army – and he had to get about with a cane. His deformity never affected the uniform sweetness of his disposition.

Hermite's earliest education was received from his parents. As the business continued to prosper, the family moved from Dieuze to Nancy when Hermite was six. Presently the growing demands of the business absorbed all the time of the parents and Hermite was sent as a boarder to the *lycée* at Nancy. This school proving unsatisfactory the prosperous parents decided to give Charles the best and packed him off to Paris. There he studied for a short time at the Lycée Henri IV, moving on at the age of eighteen (1840) to the more famous (or infamous) Louis-le-Grand – the 'Alma' Mater of the wretched Galois – to prepare for the Polytechnique.

For a while it looked as if Hermite was to repeat the disaster of his untamable predecessor at Louis-le-Grand. He had the same dislike for rhetoric and the same indifference to the elementary mathematics of the classroom. But the competent lectures on physics fascinated him and won his cordial co-operation in the bilateral process of acquiring an education. Later on, unpestered by pedants, Hermite became a good classicist and the master of a beautiful clear prose.

Those who hate examinations will love Hermite. There is something in the careers of these two most famous alumni of Louis-le-Grand, Galois and Hermite, which might well cause the advocates of examinations as a reliable yardstick for arranging human beings in order of intellectual merit to ask themselves whether they have used their heads or their feet in arriving at their conclusions. It was only by the grace of God and the diplomatic persistence of the devoted and intelligent Professor Richard, who had done his unavailing best fifteen years before to save Galois for science, that Hermite was not tossed out by stupid examiners to rot on the rubbish heap of failure. While still a student at the *lycée*, Hermite, following in the steps of Galois, supplemented and neglected his elementary lessons by private reading at the library of Sainte-Geneviève, where he found and mastered the memoir of Lagrange on the solution of numerical equations. Saving up his pennies, he bought the French translation of the *Disquisitiones Arithmeticae* of Gauss and, what is more, mastered it as few before or since have mastered it. By the time he had followed what Gauss had done Hermite was ready to *go on*. 'It was in these two books', he loved to say in later life, 'that I learned Algebra.' Euler and Laplace also instructed him through their works. And yet Hermite's performance in examinations was, to say the most flattering thing possible of it, mediocre. Mathematical nonentities beat him out of sight.

Mindful of the tragic end of Galois, Richard tried his best to steer Hermite away from original investigation to the less exciting though muddier waters of the competitive examinations for entrance to the École Polytechnique – the filthy ditch in which Galois had drowned himself. Nevertheless the good Richard could not refrain from telling Hermite's father that Charles was 'a young Lagrange'.

The *Nouvelles Annales de Mathématiques*, a journal devoted to the interests of students in the higher schools, was founded in 1842. The first volume contains two papers composed by Hermite while he was still a student at Louis-le-Grand. The first is a simple exercise in the analytic geometry of conic sections and betrays no originality. The second, which fills only six and a half pages in Hermite's collected works, is a horse of quite a different colour. Its unassuming title is *Considerations on the algebraic solution of the equation of the fifth degree* (translation).

'It is known', the modest mathematician of twenty begins, 'that Lagrange made the algebraic solution of the general equation of the fifth degree depend on the determination of a root of a *particular* equation of the sixth degree, which he calls a *reduced equation* [to-day, a "resolvent"] .... So that, if this resolvent were decomposable into rational factors of the second or third degrees, we should have the solution of the equation of the fifth degree. I shall try to show that such a decomposition is impossible.' Hermite not only succeeded in his attempt – by a beautifully simple argument – but showed also in doing so that he was an algebraist. With but a few slight changes this short paper will do all that is required.

It may seem strange that a young man capable of genuine mathematical reasoning of the calibre shown by Hermite in his paper on the general quintic should find elementary mathematics difficult. But it is not necessary to understand – or even to have heard of – much of classical mathematics as it has evolved in the course of its long history in order to be able to follow or work creatively in the mathematics that has been developed since 1800 and is still of living interest to mathematicians. The geometrical treatment (synthetic) of conic sections of the Greeks, for instance, need not be mastered to-day by anyone who wishes to follow modern geometry; nor need any geometry at all be learned by one whose tastes are algebraic or arithmetical. To a lesser degree the same is true for analysis, where such geometrical language as is used is of the simplest and is neither necessary nor desirable if up-to-date proofs are the object. As a last example, descriptive geometry, of great use to designing engineers, is of practically no use whatever to a working mathematician. Some quite difficult subjects that are still mathematically alive require only a school education in algebra and a clear head for their comprehension.

Such are the theory of finite groups, the mathematical theory of the infinite, and parts of the theory of probabilities and the higher arithmetic. So it is not astonishing that large tracts of what a candidate is required to know for entrance to a technical or scientific school, or even for graduation from the same, are less than worthless for a mathematical career. This accounts for Hermite's spectacular success as a budding mathematician and his narrow escape from complete disaster as an examinee.

Late in 1842, at the age of twenty, Hermite sat for the entrance examinations to the École Polytechnique. He passed, but only as sixty-eighth in order of merit. Already he was a vastly better mathematician than some of the men who examined him were, or were ever to become. The humiliating outcome of this test made an impression on the young master which all the triumphs of his manhood never effaced.

Hermite stayed only one year at the Polytechnique. It was not his head that disqualified him but his lame foot which, according to a ruling of the authorities, unfitted him for any of the positions open to successful students of the school. Perhaps it is as well that Hermite was thrown out; he was an ardent patriot and might easily have been embroiled in one or other of the political or military rows so precious to the effervescent French temperament. However, the year was by no means wasted. Instead of slaving over descriptive geometry, which he hated, Hermite spent his time on Abelian functions, then (1842) perhaps the topic of outstanding interest and importance to the great mathematicians of Europe. He had also made the acquaintance of Joseph Liouville (1809–82), a first-class mathematician and editor of the *Journal des Mathématiques.*

Liouville recognized genius when he saw it. In passing it may be amusing to recall that Liouville inspired William Thomson, Lord Kelvin, the famous Scotch physicist, to one of the most satisfying definitions of a mathematician that has ever been given. 'Do you know what a mathematician is?' Kelvin once asked a class. He stepped to the board and wrote

$$\int_{-\infty}^{+\infty} e^{-x^2}dx = \sqrt{\pi}.$$

Putting his finger on what he had written, he turned to the class. 'A mathematician is one to whom *that* is as obvious as that twice two makes four is to you. Liouville was a mathematician.' Young Hermite's pioneering work in Abelian functions, well begun before he was twenty-one, was as far beyond Kelvin's example in unobviousness as the example is beyond 'twice two makes four.' Remembering the cordial welcome the aged Legendre had accorded the revolutionary work of the young and unknown Jacobi, Liouville guessed that Jacobi would show a similar generosity to the beginning Hermite. He was not mistaken.

The first of Hermite's astonishing letters to Jacobi is dated from Paris, January 1843. 'The study of your [Jacobi's] memoir on quadruple periodic functions arising in the theory of Abelian functions has led me to a theorem, for the division of the arguments [variables] of these functions, analogous to that which you gave . . . to obtain the simplest expression for the roots of the equations treated by Abel. M. Liouville induced me to write to you, to submit this work to you; dare I hope, Sir, that you will be pleased to welcome it with all the indulgence it needs?' With that he plunges at once into the mathematics.

To recall briefly the bare nature of the problem in question: the trigonometric functions are functions of *one* variable with *one* period, thus $\sin (x + 2\pi) = \sin x$, where x is the variable and 2π is the period; Abel and Jacobi, by 'inverting' the elliptic integrals, had discovered functions of *one* variable and *two* periods, say $f(x + p + q) = f(x)$, where p, q are the periods (see Chapters 12, 18); Jacobi had discovered functions of *two* variables and *four* periods, say

$$F(x + a + b, y + c + d) = F(x,y),$$

where a,b,c,d are the periods. A problem early encountered in trigonometry is to express $\sin \left(\frac{x}{2}\right)$, or $\sin \left(\frac{x}{3}\right)$, or generally $\sin \left(\frac{x}{n}\right)$, where n is any given integer, in terms of $\sin x$ (and possibly other trigonometric functions of x). The corresponding problem for the functions of two variables and four periods was that which Hermite attacked. In the trigonometric problem we are finally led to quite simple equations; in Hermite's incomparably more difficult problem the upshot is again an equation (of degree n^4), and the unexpected thing about this equation is that it can be solved algebraically, that is, by radicals.

Barred from the Polytechnique by his lameness, Hermite now cast longing eyes on the teaching profession as a haven where he might earn his living while advancing his beloved mathematics. The career should have been flung wide open to him, degree or no degree, but the inexorable rules and regulations made no exceptions. Red tape always hangs the wrong man, and it nearly strangled Hermite.

Unable to break himself of his 'pernicious originality', Hermite continued his researches to the last possible moment when, at the age of twenty-four, he abandoned the fundamental discoveries he was making to master the trivialities required for his first degrees (bachelor of letters and science). Two harder ordeals would normally have followed the first before the young mathematical genius could be certified as fit to teach, but fortunately Hermite escaped the last and worst when influential friends got him appointed to a position where he could mock the examiners. He passed his examinations (in 1847–8) very badly. But for the friendliness of two of the inquisitors – Sturm and Bertrand, both fine mathematicians who recognized a fellow craftsman when they saw one – Hermite would probably not have passed at all. (Hermite married Bertrand's sister Louise in 1848.)

By an ironic twist of fate Hermite's first academic success was his appointment in 1848 as an examiner for admissions to the very Polytechnique which had almost failed to admit him. A few months later he was appointed quiz master (*répétiteur*) at the same institution. He was now securely established in a niche where no examiner could get at him. But to reach this 'bad eminence' he had sacrificed nearly five years of what almost certainly was his most inventive period to propitiate the stupidities of the official system.

Having finally satisfied or evaded his rapacious examiners, Hermite settled down to become a great mathematician. His life was peaceful and uneventful. In 1848 to 1850 he substituted for Libri at the Collège de France. Six years later, at the early age of thirty-four, he was elected to the Institut (as a member of the Academy of Sciences). In spite of his world-wide reputation as a creative mathematician Hermite was forty-seven before he obtained a suitable position: he was appointed professor in 1869 at the École Normale and finally, in 1870, he became professor at the Sorbonne, a position which he held till his retirement twenty-seven years later. During his tenure of this influential position he trained a whole generation of distinguished French mathematicians, among whom Émile Picard, Gaston Darboux, Paul Appell, Émile Borel, Paul Painlevé and Henri Poincaré may be mentioned. But his influence extended far beyond France, and his classic works helped to educate his contemporaries in all lands.

A distinguishing feature of Hermite's beautiful work is closely allied to his repugnance to take advantage of his authoritative position to re-create all his pupils in his own image: this is the unstinted generosity which he invariably displays to his fellow mathematicians. Probably no other mathematician of modern times has carried on such a voluminous scientific correspondence with workers all over Europe as Hermite, and the tone of his letters is always kindly, encouraging, and appreciative. Many a mathematician of the second half of the nineteenth century owed his recognition to the publicity which Hermite gave his first efforts. In this, as in other respects, there is no finer character than Hermite in the whole history of mathematics. Jacobi was as generous – with the one exception of his early treatment of Eisenstein – but he had a tendency to sarcasm (often highly amusing, except possibly to the unhappy victim) which was wholly absent from Hermite's general wit. Such a man deserved the generous reply of Jacobi when the unknown young mathematician ventured to approach him with his first great work on Abelian functions. 'Do not be put out, Sir', Jacobi wrote, 'if some of your discoveries coincide with old work of my own. As you must begin where I end, there is necessarily a small sphere of contact. In future, if you honour me with your communications, I shall have only to learn.'

Encouraged by Jacobi, Hermite shared with him not only the discoveries in Abelian functions, but also sent him four tremendous letters on the theory of numbers, the first early in 1847. These letters, the first of which was composed when Hermite was only twenty-four, break new ground (in what respect we shall indicate presently) and are sufficient alone to establish Hermite as a creative mathematician of the first rank. The generality of the problems he attacked and the bold originality of the methods he devised for their solution assure Hermite's remembrance as one of the born arithmeticians of history.

The first letter opens with an apology. 'Nearly two years have elapsed without my answering the letter full of goodwill which you did me the honour to write to me. To-day I shall beg you to pardon my long negligence and express to you all the joy I felt in seeing myself given a place in the repertory of your works. [Jacobi has published parts of Hermite's letter, with all due acknowledgement, in some work of his own.] Having been for long away from the work, I was greatly touched by such an attestation of your kindness; allow me, Sir, to believe that it will not desert me.' Hermite then says that another research of Jacobi's has inspired him to his present efforts.

If the reader will glance at what was said about *uniform* functions of a single variable in the chapter on Gauss (a uniform function takes *only one* value for each value of the variable), the following statement of what Jacobi had proved should be intelligible: a *uniform* function of only *one* variable with *three* distinct periods is impossible. That uniform functions of *one* variable exist having either *one* period or *two* periods is proved by exhibiting the trigonometric functions and the elliptic functions. This theorem of Jacobi's, Hermite declares, gave him his own idea for the novel methods which he introduced into the higher arithmetic. Although these methods are too technical for description here, the spirit of one of them can be briefly indicated.

Arithmetic in the sense of Gauss deals with properties of the rational integers 1,2,3, ... ; irrationals (like the square root of 2) are excluded. In particular Gauss investigated the integer solutions of large classes of indeterminate equations in two or three unknowns, for example as in $ax^2 + 2bxy + cy^2 = m$, where a,b,c,m are any given integers and it is required to discuss all integer solutions x, y of the equation. The point to be noted here is that the problem is stated and is to be solved entirely in the domain of the rational integers, that is, in the realm of *discrete* number. To fit *analysis,* which is adapted to the investigation of *continuous* number, to such a *discrete* problem would seem to be an impossibility, yet this is what Hermite did. Starting with a *discrete* formulation, he applied *analysis* to the problem, and in the end came out with results in the discrete domain from which he had started. As analysis is far more highly developed than any of the discrete techniques invented for algebra and arithmetic, Hermite's advance was comparable to the introduction of modern machinery into a medieval handicraft.

Hermite had at his disposal much more powerful machinery, both algebraic and analytic, than any available to Gauss when he wrote the *Disquisitiones Arithmeticae.* With Hermite's own great invention these more modern tools enabled him to attack problems which would have baffled Gauss in 1800. At one stride Hermite caught up with *general* problems of the type which Gauss and Eisenstein had discussed, and he at least began the arithmetical study of quadratic forms in any number of unknowns. The general nature of the arithmetical 'theory of forms' can be seen from the statement of a special problem. Instead of the Gaussian equation $ax^2 + 2bxy + cy^2 = m$ of degree *two* in *two* unknowns (x, y), it is required to discuss the integer solutions of similar equations of degree n in s unknowns, where n, s are *any* integers, and the degree of each term on the left of the equation is n (not 2 as in Gauss' equation). After stating how he had seen after much thought that Jacobi's researches on the periodicity of uniform functions depend upon deeper questions in the theory of quadratic forms, Hermite outlines his programmes.

'But, having once arrived at this point of view, the problems – vast enough – which I had thought to propose to myself, seemed inconsiderable beside the great questions of the general theory of forms. In this boundless expanse of researches which

Monsieur Gauss [Gauss was still living when Hermite wrote this, hence the polite 'Monsieur'] has opened up to us, Algebra and the Theory of Numbers seem necessarily to be merged in the same order of analytical concepts, of which our present knowledge does not yet permit us to form an accurate idea.'

He then makes a remark which, although not very clear, can be interpreted as meaning that the key to the subtle connexions between algebra, the higher arithmetic, and certain parts of the theory of functions will be found in a thorough understanding of *what sort* of 'numbers' are both necessary and sufficient for the explicit solution of all types of algebraic equations. Thus, for $x^3 - 1 = 0$, it is necessary and sufficient to understand $\sqrt[3]{1}$; for $x^5 + ax + b = 0$, where a,b are any given numbers, what sort of a 'number' x must be invented in order that x may be expressed *explicitly* in terms of a,b? Gauss of course gave one kind of answer: any root x is a complex number. But this is only a beginning. Abel proved that if only a *finite* number of rational operations and extractions of roots are permitted, then there is *no* explicit formula giving x in terms of a,b. We shall return to this question later; Hermite even at this early date (1848; he was then twenty-six) seems to have had one of his greatest discoveries somewhere at the back of his head.

In his attitude toward numbers Hermite was somewhat of a mystic in the tradition of Pythagoras and Descartes – the latter's mathematical creed, as will appear in a moment, was essentially Pythagorean. In other matters, too, the gentle Hermite exhibited a marked leaning toward mysticism. Up to the age of forty-three he was a tolerant agnostic, like so many French men of science of his time. Then, in 1856, he fell suddenly and dangerously ill. In this debilitated condition he was no match for even the least persistent evangelist, and the ardent Cauchy, who had always deplored his brilliant young friend's open-mindedness on religious matters, pounced on the prostrate Hermite and converted him to Roman Catholicism. Thenceforth Hermite was a devout Catholic, and the practice of his religion gave him much satisfaction.

Hermite's number-mysticism is harmless enough and it is one of those personal things on which argument is futile. Briefly, Hermite believed that numbers have an existence of their own above all control by human beings. Mathematicians, he thought, are permitted now and then to catch glimpses of the superhuman harmonies regulating this ethereal realm of numerical existence, just as the great geniuses of ethics and morals have sometimes claimed to have visioned the celestial perfections of the Kingdom of Heaven.

It is probably right to say that no reputable mathematician to-day who has paid any attention to what has been done in the past fifty years (especially the last twenty-five) in attempting to understand the nature of mathematics and the processes of mathematical reasoning would agree with the mystical Hermite. Whether this modern scepticism regarding the otherworldliness of mathematics is a gain or a loss over Hermite's creed must be left to the taste of the reader. What is now almost universally held by competent judges to be the wrong view of 'mathematical existence' was so admirably expressed by Descartes in his theory of the eternal triangle that it may be quoted here as an epitome of Hermite's mystical beliefs.

'I imagine a triangle, although perhaps such a figure does not exist and never has existed anywhere in the world outside my thought. Nevertheless this figure has a certain nature, or form, or determinate essence which is immutable or eternal, which I have not invented and which in no way depends on my mind. This is evident from the fact that I can demonstrate various properties of this triangle, for example that the sum of its three interior angles is equal to two right angles, that the greatest angle is opposite the greatest side, and so forth. Whether I desire to or not, I recognize very clearly and convincingly that these properties are in the triangle although I have never thought about them before, and even if this is the first time I have imagined a triangle. Nevertheless no one can say that I have invented or imagined them.' Transposed to such simple 'eternal verities' as $1 + 2 = 3$, $2 + 2 = 4$, Descartes' everlasting geometry becomes Hermite's superhuman arithmetic.

One arithmetical investigation of Hermite's, although rather technical, may be mentioned here as an example of the prophetic aspect of pure mathematics. Gauss, we recall, introduced *complex integers* (numbers of the form $a + bi$, where a, b are rational integers and i denotes $\sqrt{-1}$) into the higher arithmetic in order to give the law of biquadratic reciprocity its simplest expression. Dirichlet and other followers of Gauss then discussed quadratic forms in which the rational integers appearing as variables and coefficients are replaced by Gaussian complex integers. Hermite passed to the general case of this situation and investigated the representation of integers in what are to-day called *Hermitian forms*. An example of such a form (for the special case of two complex variables x_1, x_2 and their 'conjugates' $\bar{x}_1$, $\bar{x}_2$ instead of n variables) is

$$a_{11}x_1\bar{x}_1 + a_{12}x_1\bar{x}_2 + a_{21}x_2\bar{x}_1 + a_{22}x_2\bar{x}_2,$$

in which the bar over a letter denoting a complex number indicates the *conjugate* of that number; namely, if $x + iy$ is the complex number, its 'conjugate' is $x - iy$; and the coefficients a_{11}, a_{12}, a_{21}, a_{22} are such that $a_{ij} = \bar{a}_{ji}$, for $(i,j) = (1,1), (1,2), (2,1), (2,2)$, so that a_{12} and a_{21} are conjugates, and each of a_{11}, a_{22} is its own conjugate (so that a_{11}, a_{22} are real numbers). It is easily seen that the entire form is real (free of i) if all products are multiplied out, but it is most 'naturally' discussed in the shape given.

When Hermite invented such forms he was interested in finding what numbers are represented by the forms. Over seventy years later it was found that the algebra of Hermitian forms is indispensable in mathematical physics, particularly in the modern quantum theory. Hermite had no idea that his pure mathematics would prove valuable in science long after his death – indeed, like Archimedes, he never seemed to care much for the scientific applications of mathematics. But the fact that Hermite's work has given physics a useful tool is perhaps another argument favouring the side that believes mathematicians best justify their abstract existence when left to their own inscrutable devices.

Leaving aside Hermite's splendid discoveries in the theory of algebraic invariants as too technical for discussion here, we shall pass on in a moment to two of his most spectacular achievements in other fields. The high esteem in which Hermite's work in invariants was held by his contemporaries may, however, be indicated by Sylvester's characteristic remark that 'Cayley, Hermite, and I constitute an Invariantive Trinity.' Who was who in this astounding trinity Sylvester omitted to state; but perhaps this oversight is immaterial, as each member of such a trefoil would be capable of transforming himself into himself or into either of his coinvariantive beings.

The two fields in which Hermite found what are perhaps the most striking individual results in all his beautiful work are those of the general equation of the fifth degree and transcendental numbers. The nature of what he found in the first is clearly indicated in the introduction to his short note *Sur la résolution de l'équation du cinquième degré* (On the Solution of the [general] Equation of the Fifth Degree; published in the *Comptes rendus de l'Académie des Sciences* for 1858, when Hermite was thirty-six).

'It is known that the general equation of the fifth degree can be reduced, by a substitution [on the unknown x] whose coefficients are determined without using any irrationalities other than square roots or cube roots, to the form

$$x^5 - x - a = 0.$$

[That is, *if* we can solve *this* equation for x, *then* we can solve the general equation of the fifth degree.]

'This remarkable result, due to the English mathematician Jerrard, is the most important step that has been taken in the algebraic theory of equations of the fifth degree since Abel proved that a solution by radicals is impossible. This impossibility shows in fact the necessity for introducing some new analytic element [some new kind of function] in seeking the solution, and, on this account, it seems natural to take as an auxiliary the roots of the very simple equation we have just mentioned. Nevertheless, in order to legitimize its use rigorously as an essential element in the solution of the general equation, it remains to see if this simplicity of form actually permits us to arrive at some idea of the nature of its roots, to grasp what is peculiar and essential in the mode of existence of these quantities, of which nothing is known beyond the fact that they are not expressible by radicals.

'Now it is very remarkable that Jerrard's equation lends itself with the greatest ease to this research, and is, in the sense which we shall explain, susceptible of an actual analytic solution. For we may indeed conceive the question of the algebraic solution of equations from a point of view different from that which for long has been indicated by the solution of equations of the first four degrees, and to which we are especially committed.

'Instead of expressing the closely interconnected system of roots, considered as functions of the coefficients, by a formula involving many-valued radicals,* we may seek to obtain the roots expressed separately by as many distinct uniform [one-valued] functions of auxiliary variables, as in the case of the third degree. In this case, where the equation

$$x^3 - 3x + 2a = 0$$

is under discussion, it suffices, as we know, to represent the coefficient a by the sine of an angle, say A, in order that the roots be isolated as the following well-determined functions

$$2 \sin \frac{A}{3},\ 2 \sin \frac{a + 2\pi}{3},\ 2 \sin \frac{A + 4\pi}{3}.$$

[Hermite is here recalling the familiar 'trigonometric solution' of the cubic usually discussed in the second course of school algebra. The 'auxiliary variable' is A; the 'uniform functions' are here sines.]

'Now it is an entirely similar fact which we have to exhibit concerning the equation

$$x^5 - x - a = 0.$$

Only, instead of sines or cosines, it is the elliptic functions which it is necessary to introduce. . . .'

* For example, as in the simple quadratic $x^2 - a = 0$: the roots are $x = +\sqrt{a}$, and $x = -\sqrt{a}$; the 'many-valuedness' of the radical involved, here a square root, or irrationality of the *second* degree, appears in the double sign, $\pm$, when we say briefly that the *two* roots are $\sqrt{a}$. The formula giving the *three* roots of cubic equations involves the three-valued irrationality $\sqrt[3]{1}$, which has the *three* values 1, $\frac{1}{2}(-1 + \sqrt{-3})$, $\frac{1}{2}(-1 - \sqrt{-3})$.

In short order Hermite then proceeds to solve *the general equation of the fifth degree*, using for the purpose elliptic functions (strictly, elliptic modular functions, but the distinction is of no importance here). It is almost impossible to convey to a non-mathematician the spectacular brilliance of such a feat; to give a very inadequate simile, Hermite found the famous 'lost chord' when no mortal had the slightest suspicion that such an elusive thing existed anywhere in time and space. Needless to say his totally unforeseen success created a sensation in the mathematical world. Better, it inaugurated a new department of algebra and analysis in which the grand problem is to discover and investigate those functions in terms of which the general equation of the nth degree can be solved explicitly in finite form. The best result so far obtained is that of Hermite's pupil, Poincaré (in the 1880's), who created the functions giving the required solution. These turned out to be a 'natural' generalization of the elliptic functions. The characteristic of those functions that was generalized was periodicity. Further details would take us too far afield here, but if there is space we shall recur to this point when we reach Poincaré.

Hermite's other sensational isolated result was that which established the *transcendence* (explained in a moment) of the number denoted in mathematical analysis by the letter e, namely

$$1 + \frac{1}{1!} + \frac{1}{2!} + \frac{1}{3!} + \frac{1}{4!} + \dots,$$

where $1!$ means 1, $2! = 1 \times 2$, $3! = 1 \times 2 \times 3$, $4! = 1 \times 2 \times 3 \times 4$, and so on; this number is the 'base' of the so-called 'natural' system of logarithms, and is approximately 2·718281828.... It has been said that it is impossible to conceive of a universe in which e and π (the ratio of the circumference of a circle to its diameter) are lacking. However that may be (as a matter of fact it is false), it is a fact that e turns up everywhere in current mathematics, pure and applied. Why this should be so, at least so far as applied mathematics is concerned, may be inferred from the following fact: e^x, considered as a function of x, is the *only* function of x whose rate of change with respect to x is equal to the function itself – that is, e^x is the only function which is equal to its derivative.*

The concept of 'transcendence' is extremely simple, also extremely important. Any root of an algebraic equation whose coefficients are rational integers $(0, \pm 1, \pm 2, \ldots)$ is called an *algebraic number*. Thus $\sqrt{-1}$, 2·78 are algebraic numbers, because they are roots of the respective algebraic equations $x^2 + 1 = 0$, $50x - 139 = 0$, in which the coefficients (1, 1 for the first; 50, – 139 for the second) are rational integers. A 'number' which is *not* algebraic is called transcendental. Otherwise expressed, a transcendental number is one which satisfies *no* algebraic equation with rational integer coefficients.

Now, given any 'number' constructed according to some definite law, it is a meaningful question to ask whether it is algebraic or transcendental. Consider, for example, the following simply defined number,

$$\frac{1}{10} + \frac{1}{10^2} + \frac{1}{10^6} + \frac{1}{10^{24}} + \frac{1}{10^{120}} + \ldots,$$

in which the exponents 2, 6, 24, 120, ... are the successive 'factorials', namely $2 = 1 \times 2$, $6 = 1 \times 2 \times 3$, $24 = 1 \times 2 \times 3 \times 4$, $120 = 1 \times 2 \times 3 \times 4 \times 5$, ... , and the indicated series continues 'to infinity' according to the same law as that for the terms given. The next term is $\frac{1}{10^{720}}$; the sum of the first three terms is ·1 + ·01 + ·000001, or ·110001, and it can be proved that the series does actually define some definite number which is less than ·12. Is this number a root of *any* algebraic equation with rational integer coefficients? The answer is no, although to prove this without having been shown how to go about it is a severe test of high mathematical ability. On the other hand, the number defined by the infinite series

$$\frac{1}{10^5} + \frac{1}{10^8} + \frac{1}{10^{11}} + \frac{1}{10^{14}} + \ldots$$

* Strictly, ae^x, where a does not depend upon x, is the most general, but the 'multiplicative constant' a is trivial here.

is algebraic; it is the root of $99900x - 1 = 0$ (as may be verified by the reader who remembers how to sum an infinite convergent geometrical progression).

The first to prove that certain numbers are transcendental was Joseph Liouville (the same man who encouraged Hermite to write to Jacobi) who, in 1844, discovered a very extensive class of transcendental numbers, of which all those of the form

$$\frac{1}{n} + \frac{1}{n^2} + \frac{1}{n^6} + \frac{1}{n^{24}} + \frac{1}{n^{120}} + \ldots,$$

where n is a real number greater than 1 (the example given above corresponds to $n = 10$), are among the simplest. But it is probably a much more difficult problem to prove that a *particular* suspect, like e or π, is or is not transcendental than it is to invent a whole infinite class of transcendentals: the inventive mathematician dictates – to a certain extent – the working conditions, while the suspected number is entire master of the situation, and it is the mathematician in this case, not the suspect, who takes orders which he only dimly understands. So when Hermite proved in 1873 that e (defined a short way back) is transcendental, the mathematical world was not only delighted but astonished at the marvellous ingenuity of the proof.

Since Hermite's time many numbers (and classes of numbers) have been proved transcendental. What is likely to remain a high-water mark on the shores of this dark sea for some time may be noted in passing. In 1934 the young Russian mathematician Alexis Gelfond proved that *all* numbers of the type a^b, where a is neither 0 nor 1 and b is *any irrational algebraic number*, are transcendental. This disposes of the seventh of David Hilbert's list of twenty-three outstanding mathematical problems which he called to the attention of mathematicians at the Paris International Congress in 1900. Note that 'irrational' is *necessary* in the statement of Gelfond's theorem (if $b = n/m$, where n, m are rational integers, then a^b, where a is any algebraic number, is a root of $x^m - a^n = 0$, and it can be shown that this equation is equivalent to one in which all the coefficients are rational integers.

Hermite's unexpected victory over the obstinate e inspired mathematicians to hope that π would presently be subdued in a similar manner. For himself, however, Hermite had had enough of a good thing. 'I shall risk nothing', he wrote to Borchardt, 'on an attempt to prove the transcendence of the number π. If others undertake this enterprise, no one will be happier than I at their success, but believe me, my dear friend, it will not fail to cost them some efforts.' Nine years later (in 1882) Ferdinand Lindemann of the University of Munich, using methods very similar to those which had sufficed Hermite to dispose of e, proved that π is transcendental, thus settling for ever the problem of 'squaring the circle'. From what Lindemann proved it follows that it is impossible with straight-edge and compass alone to construct a square whose area is equal to that of any given circle – a problem which had tormented generations of mathematicians since before the time of Euclid.

As cranks are still tormented by the problem, it may be in order to state concisely how Lindemann's proof settles the matter. He proved that π is *not* an *algebraic* number. But any *geometrical* problem that *is* solvable by the aid of straight-edge and compass alone, when *restated* in its equivalent *algebraic* form, leads to one or more algebraic equations with rational integer coefficients which can be solved by successive extractions of *square roots*. As π satisfies no such equation, the circle cannot be 'squared' with the implements named. If other mechanical apparatus is permitted, it is easy to square the circle. To all but mild lunatics the problem has been completely dead for over half a century. Nor is there any merit at the present time in computing π to a large number of decimal places – more accuracy in this respect is already available than is ever likely to be of use to the human race if it survives for a billion to the billionth power years. Instead of trying to do the impossible, mystics may like to contemplate the following useful relation between e, π, -1 and $\sqrt{-1}$ till it becomes as plain to them as Buddha's navel is to a blind Hindu swami,

$$e^{\pi\sqrt{-1}} = -1.$$

Anyone who can perceive this mystery intuitively will not need to square the circle.

Since Lindemann settled π the one outstanding unsolved problem that attracts amateurs is Fermat's 'Last Theorem'. Here an amateur with real genius undoubtedly has a chance. Lest this be taken as an invitation to all and sundry to swamp the editors of mathematical journals with attempted proofs, recall what happened to Lindemann when he boldly tackled the famous theorem. If this does not suggest that more than ordinary talent will be required to settle Fermat, nothing can. In 1901 Lindemann published a memoir of seventeen pages purporting to contain the long-sought proof. The vitiating error being pointed out, Lindemann, undaunted, spent the best part of the next seven years in attempting to patch the unpatchable, and in 1907 published sixty-three pages of alleged proof which were rendered nonsensical by a slip in reasoning near the very beginning.

Great as were Hermite's contributions to the technical side of mathematics, his steadfast adherence to the ideal that science is beyond nations and above the power of creeds to dominate or to stultify was perhaps an even more significant gift to civilization in the long view of things as they now appear to a harassed humanity. We can only look back on his serene beauty of spirit with a poignant regret that its like is nowhere to be found in the world of science to-day. Even when the arrogant Prussians were humiliating Paris in the Franco-Prussian war, Hermite, patriot though he was, kept his head, and he saw clearly that the mathematics of 'the enemy' was mathematics and nothing else. To-day, even when a man of science does take the civilized point of view, he is not impersonal about his supposed broadmindedness, but aggressive, as befits a man on the defensive. To Hermite it was so obvious that knowledge and wisdom are not the prerogatives of any sect, any creed, or any nation that he never bothered to put his instinctive sanity into words. In respect of what Hermite knew by instinct our generation is two centuries behind him. He died, loved the world over, on 14 January 1901.

CHAPTER TWENTY-FIVE

THE DOUBTER

Kronecker

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PROFESSIONAL mathematicians who could properly be called business men are extremely rare. The one who most closely approximates to this ideal is Kronecker (1823–91), who did so well for himself by the time he was thirty that thereafter he was enabled to devote his superb talents to mathematics in considerably greater comfort than most mathematicians can afford.

The obverse of Kronecker's career is to be found – according to a tradition familiar to American mathematicians – in the exploits of John Pierpont Morgan, founder of the banking house of Morgan and Company. If there is anything in this tradition, Morgan as a student in Germany showed such extraordinary mathematical ability that his professors tried to induce him to follow mathematics as his life work and even offered him a university position in Germany which would have sent him off to a flying start. Morgan declined and dedicated his gifts to finance, with results familiar to all. Speculators (in academic studies, not Wall Street) may amuse themselves by reconstructing world history on the hypothesis that Morgan had stuck to mathematics.

What might have happened to Germany had Kronecker not abandoned finance for mathematics also offers a wide field for speculation. His business abilities were of a high order; he was an ardent patriot with an uncanny insight into European diplomacy and a shrewd cynicism – his admirers called it realism – regarding the unexpressed sentiments cherished by the great Powers for one another.

At first a liberal like so many intellectual young Jews, Kronecker quickly became a rock-ribbed conservative when he saw which side his own abundant bread was buttered on – after his financial exploits, and proclaimed himself a loyal supporter of that callous old truth-doctor Bismarck. The famous episode of the Ems telegram which, according to some, was the electric spark that touched off the Franco-Prussian war in 1870, had Kronecker's warm approval, and his grasp of the situation was so firm that *before* the battle of Weissenburg, when even the military geniuses of Germany were doubtful as to the outcome of their bold challenging of France, Kronecker confidently predicted the success of the entire campaign and was proved right in detail. At the time, and indeed all his life, he was on cordial terms with the leading French mathematicians, and he was clear-headed enough not to let his political opinions cloud his just perception of his scientific rivals' merits. It is perhaps as well that so realistic a man as Kronecker cast his lot with mathematics.

Leopold Kronecker's life was easy from the day of his birth. The son of prosperous Jewish parents, he was born on 7 December 1823, at Liegnitz, Prussia. By an unaccountable oversight Kronecker's official biographers (Heinrich Weber and Adolf Kneser) omit all mention of Leopold's mother, although he probably had one, and concentrate on the father, who owned a flourishing mercantile business. The father was a well-educated man with an unquenchable thirst for philosophy which he passed on to Leopold. There was another son, Hugo, seventeen years younger than Leopold, who became a distinguished physiologist and professor at Berne. Leopold's early education under a private tutor was supervised by the father; Hugo's upbringing later became the loving duty of Leopold.

In the second stage of his education at the preparatory school for the Gymnasium Leopold was strongly influenced by the co-rector Werner, a man with philosophical and theological leanings, who later taught Kronecker when he entered the Gymnasium. Among other things Kronecker imbibed from Werner was a liberal draught of Christian theology, for which he acquired a lifelong enthusiasm. With what looks like his usual caution, Kronecker did not embrace the Christian faith till practically on his deathbed when, having seen that it did his six children no noticeable mischief, he permitted himself to be converted from Judaism to evangelical Christianity in his sixty-eighth year.

Another of Kronecker's teachers at the Gymnasium also influenced him profoundly and became his lifelong friend, Ernst Eduard Kummer (1810–93), subsequently professor at the University of Berlin and one of the most original mathematicians Germany has produced, of whom more will be said in connexion with Dedekind. These three, Kronecker senior, Werner, and Kummer, capitalized Leopold's immense native abilities, formed his mind, and charted the future course of his life so cunningly that he could not have departed from it if he had wished.

Already in this early stage of his education we note an outstanding feature of Kronecker's genial character, his ability to get along with people and his instinct for forming lasting friendships with men who had risen in the world or were to rise, and who would be useful to him either in business or mathematics. This genius for friendships of the right sort, which is one of the successful business man's distinguishing traits, was one of Kronecker's more valuable assets and he never mislaid it. He was not consciously mercenary, nor was he a snob; he was merely one of those lucky mortals who is more at ease with the successful than with the unsuccessful.

Kronecker's performance at school was uniformly brilliant and many-sided. In addition to the Greek and Latin classics which he mastered with ease and for which he retained a lifelong liking, he shone in Hebrew, philosophy, and mathematics. His mathematical talent appeared early under the expert guidance of Kummer, from whom he received special instruction. Young Kronecker however did not concentrate to any great extent on mathematics, although it was obvious that his greatest talent lay in that field, but set himself to acquiring a broad liberal education commensurate with his manifold abilities. In addition to his formal studies he took music lessons and became an accomplished pianist and vocalist. Music, he declared when he was an old man, is the finest of all the fine arts, with the possible exception of mathematics, which he likened to poetry. These many interests he retained throughout his life. In none of them was he a mere dabbler: his love of the classics of antiquity bore tangible fruit in his affiliation with Graeca, a society dedicated to the translation and popularization of the Greek classics; his keen appreciation of art made him an acute critic of painting and sculpture, and his beautiful house in Berlin became a rendezvous for musicians, among them Felix Mendelssohn.

Entering the University of Berlin in the spring of 1841, Kronecker continued his broad education but began to concentrate on mathematics. Berlin at that time boasted Dirichlet (1805–59), Jacobi (1804–51) and Steiner (1796–1863) on its mathematical faculty; Eisenstein (1823–52), the same age as Kronecker, also was about, and the two became friends.

The influence of Dirichlet on Kronecker's mathematical tastes (particularly in the application of analysis to the theory of numbers) is clear all through his mature writings. Steiner seems to have made no impression on him; Kronecker had no feeling for geometry. Jacobi gave him a taste for elliptic functions which he was to cultivate with striking originality and brilliant success, chiefly in novel applications of magical beauty to the theory of numbers.

Kronecker's university career was a repetition on a larger scale of his years at school: he attended lectures on the classics and the sciences and indulged his bent for philosophy by profounder studies than any he had as yet undertaken, particularly in the system of Hegel. The last is emphasized because some curious and competent reader may be moved to seek the origin of Kronecker's mathematical heresies in the abstrusities of Hegel's dialectic – a quest wholly beyond the powers of the present writer. Nevertheless there is a strange similarity between some of the weird unorthodoxies of recent doubts concerning the self-consistency of mathematics – doubts for which Kronecker's 'revolution' was partly responsible – and the subtleties of Hegel's system. The ideal candidate for such an undertaking would be a Marxian communist with a sound training in Polish many-valued logic, though in what incense tree this rare bird is to be sought God only knows.

Following the usual custom of German students, Kronecker did not spend all his time at Berlin but moved about. Part of his course was pursued at the University of Bonn, where his old teacher and friend Kummer had taken the chair of mathematics. During Kronecker's residence at Bonn the University authorities were in the midst of a futile war to suppress the student societies whose chief object was the fostering of drinking, duelling, and brawling in general. With his customary astuteness, Kronecker allied himself secretly with the students and thereby made many friends who were later to prove useful.

Kronecker's dissertation, accepted by Berlin for his Ph.D. in 1845, was inspired by Kummer's work in the theory of numbers and dealt with the units in certain algebraic number fields. Although the problem is one of extreme difficulty when it comes to actually exhibiting the units, its nature can be understood from the following rough description of the *general* problem of units (for *any* algebraic number field, not merely for the *special* fields which interested Kummer and Kronecker). This sketch may also serve to make more intelligible some of the allusions in the present and subsequent chapters to the work of Kummer, Kronecker, and Dedekind in the higher arithmetic. The matter is quite simple but requires several preliminary definitions.

The common whole numbers 1,2,3, ... are called the (positive) rational integers. If m is any rational integer, it is the root of an algebraic equation of the *first* degree, whose coefficients are *rational integers*, namely $x - m = 0$. This, among other properties of the rational integers, suggested the *generalization* of the concept of integers to the 'numbers' defined as roots of algebraic equations. Thus if r is a root of the equation

$$x^n + a_1x^{n-1} + \dots + a_{n-1}x + a_n = 0,$$

where the a's are rational integers (positive or negative), and if further r satisfies no equation of degree less than n, all of whose coefficients are rational integers and whose leading coefficient is 1 (as it is in the above equation, namely the coefficient of the highest power, x^n, of x in the equation is 1), then r is called an *algebraic integer* of *degree n*. For example, $1 + \sqrt{-5}$ is an algebraic integer of degree 2, because it is a root of $x^2 - 2x + 6 = 0$, and is not a root of any equation of degree less than 2 with coefficients of the prescribed kind; in fact $1 + \sqrt{-5}$ is the root of $x - (1 + \sqrt{-5}) = 0$, and the last coefficient, $-(1 + \sqrt{-5})$, is not a rational integer.

If in the above definition of an algebraic *integer* of degree n we suppress the requirement that the leading coefficient be 1, and say that it can be any rational integer (other than zero, which is considered an integer), a root of the equation is then called an *algebraic number* of *degree n*. Thus $\frac{1}{2}(1 + \sqrt{-5})$ is an algebraic *number* of degree 2, but is not an algebraic *integer*; it is a root of $2x^2 - 2x + 3 = 0$.

Another concept, that of an *algebraic number field* of *degree n*, is now introduced: if r is an algebraic number of degree n, the totality of all expressions that can be constructed from r by repeated additions, subtractions, multiplications, and divisions (division by zero is not defined and hence is not attempted or permitted), is called *the algebraic number field generated by r*, and may be denoted by $F[r]$. For example, from r we get $r + r$, or $2r$; from this and r we get $2r/r$ or 2, $2r - r$ or r, $2r \times r$ or $2r^2$, etc. The *degree* of this $F[r]$ is n.

It can be proved that every member of $F[r]$ is of the form $c_0r^{n-1} + c_1r^{n-2} + \ldots + c_{n-1}$, where the c's are rational numbers, and further every member of $F[r]$ is an algebraic number of degree not greater than n (in fact the degree is some divisor of n). *Some*, but not all, algebraic numbers in $F[r]$ will be algebraic *integers*.

The central problem of the theory of algebraic numbers is to investigate the laws of arithmetical divisibility of algebraic integers in an algebraic number field of degree n. To make this problem definite it is necessary to lay down exactly what is meant by 'arithmetical divisibility', and for this we must understand the like for the *rational* integers.

We say that one rational integer, m, is divisible by another, d, if we can find a rational integer, q, such that $m = q \times d$; d (also q) is called a *divisor* of m. For example 6 is a divisor of 12, because $12 = 2 \times 6$; 5 is not a divisor of 12 because there does not exist a rational integer q such that $12 = q \times 5$.

A (positive) rational *prime* is a rational integer greater than 1 whose only positive divisors are 1 and the integer itself. When we try to extend this definition to algebraic integers we soon see that we have not found the root of the matter, and we must seek some property of rational primes which can be carried over to algebraic integers. This property is the following: if a rational prime p divides the product $a \times b$ of two rational integers, then (it can be proved that) p divides at least one of the factors a, b of the product.

Considering the unit, 1, of rational arithmetic, we notice that 1 has the peculiar property that it divides *every* rational integer; -1 also has the same property, and $1, -1$ are the *only* rational integers having this property.

These and other clues suggest something simple that will work, and we lay down the following definitions as the basis for a theory of arithmetical divisibility for algebraic integers. We shall suppose that all the integers considered lie in an algebraic number field of degree n.

If r,s,t are algebraic integers such that $r = s \times t$, each of s, t is called a *divisor of* r.

If j is an algebraic integer which divides *every* algebraic integer in the field, j is called a *unit* (in that field). A given field may contain an infinity of units, in distinction to the pair $1, -1$ for the rational field, and this is one of the things that breeds difficulties.

The next introduces a radical and disturbing distinction between rational integers and algebraic integers of degree greater than 1.

An algebraic integer other than a unit whose only divisors are units and the integer itself, is called *irreducible*. An irreducible algebraic integer which has the property that *if* it divides the product of two algebraic integers, *then* it divides at least one of the factors, is called a *prime* algebraic integer. All primes are irreducibles, but not all irreducibles are primes in *some* algebraic number fields, for example in $F[\sqrt{-5}]$, as will be seen in a moment. In the common arithmetic of 1,2,3 ... the irreducibles and the primes are the same.

In the chapter on Fermat the fundamental theorem of (rational) arithmetic was mentioned: a rational integer is the product of (rational) primes *in only one way*. From this theorem springs all the intricate theory of divisibility for rational integers. Unfortunately the fundamental theorem does *not* hold in *all* algebraic number fields of degree greater than one, and the result is chaos.

To give an instance (it is the stock example usually exhibited in text-books on the subject), in the field $F[\sqrt{-5}]$ we have

$$6 = 2 \times 3 = (1 + \sqrt{-5}) \times (1 - \sqrt{-5});$$

each of $2, 3, 1 + \sqrt{-5}, 1 - \sqrt{-5}$ is a prime in this field (as may be verified with some ingenuity), so that 6, in this field, is *not uniquely* decomposable into a product of primes.

It may be stated here that Kronecker overcame this difficulty by a beautiful method which is too detailed to be explained untechnically, and that Dedekind did likewise by a totally different method which is much easier to grasp, and which will be noted when we consider his life. Dedekind's method is the one in widest use to-day, but this does not imply that Kronecker's is less powerful, nor that it will not come into favour when more arithmeticians become familiar with it.

In his dissertation of 1845 Kronecker attacked the theory of the units in certain special fields – those defined by the equations arising from the algebraic formulation of Gauss' problem to divide the circumference of a circle into n equal parts or, what is the same, to construct a regular polygon of n sides.

We can now close up one part of the account opened by Fermat. In struggling to prove Fermat's 'Last Theorem' that $x^n + y^n = z^n$ is impossible in rational integers x, y, z (none zero) if n is an integer greater than 2, arithmeticians took what looks like a natural step and resolved the left-hand side, $x^n + y^n$, into its n factors of the first degree (as is done in the usual second course of school algebra). This led to the exhaustive investigation of the algebraic number field mentioned above in connexion with Gauss' problem – after serious but readily understandable mistakes had been made.

The problem at first was studded with pitfalls, into which many a competent mathematician and at least one great one – Cauchy – tumbled headlong. Cauchy assumed as a matter of course that in the algebraic number field concerned the fundamental theorem of arithmetic must hold. After several exciting but premature communications to the French Academy of Sciences, he admitted his error. Being restlessly interested in a large number of other problems at the time, Cauchy turned aside and failed to make the great discovery which was well within the capabilities of his prolific genius and left the field to Kummer. The central difficulty was serious: here was a species of 'integers' – those of the field concerned – which defied the fundamental theorem of arithmetic; how reduce them to law and order?

The solution of this problem by the invention of a totally new kind of 'number' appropriate to the situation, which (in terms of these 'numbers') automatically restored the fundamental theorem of arithmetic, ranks with the creation of non-Euclidean geometry as one of the outstanding scientific achievements of the nineteenth century, and it is well up in the high mathematical achievements of all history. The creation of the new 'numbers' – so-called 'ideal numbers' – was the invention of Kummer in 1845. These new 'numbers' were not constructed for all algebraic number fields but only for those fields arising from the division of the circle.

Kummer too had fallen foul of the net which snared Cauchy, and for a time he believed that he had proved Fermat's 'Last Theorem'. Then Dirichlet, to whom the supposed proof was submitted for criticism, pointed out by means of an example that the fundamental theorem of arithmetic, contrary to Kummer's tacit assumption, does *not* hold in the field concerned. This failure of Kummer's was one of the most fortunate things that ever happened in mathematics. Like Abel's initial mistake in the matter of the general quintic, Kummer's turned him into the right track, and he invented his 'ideal numbers'.

Kummer, Kronecker, and Dedekind, in their invention of the modern theory of algebraic numbers, by enlarging the scope of arithmetic *ad infinitum* and bringing algebraic equations within the purview of number, did for the higher arithmetic and the theory of algebraic equations what Gauss, Lobatchewsky, Johann Bolyai, and Riemann did for geometry in emancipating it from slavery in Euclid's too narrow economy. And just as the inventors of non-Euclidean geometry revealed vast and hitherto unsuspected horizons to geometry and physical science, so the creators of the theory of algebraic numbers uncovered an entirely new light, illuminating the whole of arithmetic and throwing the theories of equations, of systems of algebraic curves and surfaces, and the very nature of number itself, into sharp relief against a firm background of shiningly simple postulates.

The creation of 'ideals' – Dedekind's inspiration from Kummer's vision of 'ideal numbers' – renovated not only arithmetic but the whole of the algebra which springs from the theory of algebraic equations and systems of such equations, and it proved also a reliable clue to the inner significance of the 'enumerative geometry' * of Plücker, Cayley and others, which absorbed so large a fraction of the energies of the geometers of the nineteenth century who busied themselves with the intersections of nets of curves and surfaces. And last, if Kronecker's heresy against Weierstrassian analysis (noted later) is some day to become a stale orthodoxy, as all not utterly insane heresies sooner or later do, these renovations of our familiar integers, 1,2,3, . . . , on which all analysis strives to base itself, may ultimately indicate extensions of analysis, and the Pythagorean speculation may envisage generative properties of 'number' that Pythagoras never dreamed of in all his wild philosophy.

Kronecker entered this beautifully difficult field of algebraic numbers in 1845 at the age of twenty-two with his famous dissertation *De Unitatibus Complexis* (*On Complex Units*). The particular units he discussed were those in algebraic number fields arising from the Gaussian problem of the division of the circumference of a circle into n equal arcs. For this work he got his Ph.D.

The German universities used to have – and may still have –

* One problem in this subject: an algebraic curve may have loops on it, or places where the curve crosses its tangents; given the degree of the curve, how many such points are there? Or if we cannot answer that, what equations connecting the number of these and other exceptional points must hold? Similarly for surfaces.

a laudable custom in connexion with the taking of a Ph.D.: the successful candidate was in honour bound to fling a party – usually a prolonged beer bust with all the trimmings – for his examiners. At such festivities a mock examination consisting of ridiculous questions and more ridiculous answers was sometimes part of the fun. Kronecker invited practically the whole faculty, including the Dean, and the memory of that undignified feast in celebration of his degree was, he declared in later years, the happiest of his life.

In at least one respect Kronecker and his scientific enemy Weierstrass were much alike: they were both very great gentlemen, as even those who did not particularly care for either admitted. But in nearly everything else they were almost comically different. The climax of Kronecker's career was his prolonged mathematical war against Weierstrass, in which quarter was neither given nor asked. One was a born algebraist, the other almost made a religion of analysis. Weierstrass was large and rambling, Kronecker a compact, diminutive man, not over five feet tall, but perfectly proportioned and sturdy. After his student days Weierstrass gave up his fencing; Kronecker was always an expert gymnast and swimmer and in later life a good mountaineer.

Eye-witnesses of the battles between this curiously mismatched pair tell how the big fellow, annoyed by the persistence of the little fellow, would stand shaking himself like a good-natured St Bernard dog trying to rid himself of a determined fly, only to excite his persecutor to more ingenious attacks, till Weierstrass, giving up in despair, would amble off, Kronecker at his heels still talking maddeningly. But for all their scientific differences the two were good friends, and both were great mathematicians without a particle of the 'great man' complex that too often inflates the shirts of the would-be mighty.

Kronecker was blessed with a rich uncle in the banking business. The uncle also controlled extensive farming enterprises. All this fell into young Kronecker's hands for administration on the death of the uncle, shortly after the budding mathematician had taken his degree at the age of twenty-two. The eight years from 1845 to 1853 were spent in managing the estate and running the business, which Kronecker did with great thoroughness and financial success. To manage the landed property efficiently he even mastered the principles of agriculture.

In 1848, at the age of twenty-five, the energetic young business man very prudently fell in love with his cousin, Fanny Prausnitzer, daughter of the defunct wealthy uncle, married her, and settled down to raise a family. They had six children, four of whom survived their parents. Kronecker's married life was ideally happy, and he and his wife – a gifted, pleasant woman – brought up their children with the greatest devotion. The death of Kronecker's wife a few months before his own last illness was the blow which broke him.

During his eight years in business Kronecker produced no mathematics. But that he did not stagnate mathematically is shown by his publication in 1853 of a fundamental memoir on the algebraic solution of equations. All through his activity as a man of affairs Kronecker had maintained a lively scientific correspondence with his former master, Kummer, and on escaping from business in 1853 he visited Paris, where he made the acquaintance of Hermite and other leading French mathematicians. Thus he did not sever communications with the scientific world when circumstances forced him into business, but kept his soul alive by making mathematics rather than whist, pinochle, or draughts his hobby.

In 1853, when Kronecker's memoir on the algebraic solvability of equations (the nature of the problem was discussed in the chapters on Abel and Galois) was published, the Galois theory of equations was understood by very few. Kronecker's attack was characteristic of much of his finest work. Kronecker had mastered the Galois theory, indeed he was probably the only mathematician of the time (the late 1840's) who had penetrated deeply into Galois' ideas; Liouville had contented himself with a sufficient insight into the theory to enable him to edit some of Galois' remains intelligently.

A distinguishing feature of Kronecker's attack was its comprehensive thoroughness. In this, as in other investigations in algebra and the theory of numbers, Kronecker took the refined gold of his predecessors, toiled over it like an inspired jeweller, added gems of his own, and made from the precious raw material a flawless work of ark with the unmistakable impress of his artistic individuality upon it. He delighted in perfect things; a few of his pages will often exhibit a complete development of one isolated result with all its implications immanent but not loading the unique theme with expressed detail. Consequently even the shortest of his papers has suggested important developments to his successors, and his longer works are inexhaustible mines of beautiful things.

Kronecker was what is called an 'algorist' in most of his works. He aimed to make concise, expressive formulae tell the story and automatically reveal the action from one step to the next so that, when the climax was reached, it was possible to glance back over the whole development and see the apparent inevitability of the conclusion from the premises. Details and accessory aids were ruthlessly pruned away until only the main trunk of the argument stood forth in naked strength and simplicity. In short, Kronecker was an artist who used mathematical formulae as his medium.

After Kronecker's works on the Galois theory the subject passed from the private ownership of a few into the common property of all algebraists, and Kronecker had wrought so artistically that the next phase of the theory of equations – the current postulational formulation of the theory and its extensions – can be traced back to him. His aim in algebra, like that of Weierstrass in analysis, was to find the 'natural' way – a matter of intuition and taste rather than scientific definition – to the heart of his problems.

The same artistry and tendency to unification appeared in another of his most celebrated papers, which occupied only a couple of pages in his collected works, *On the Solution of the General Equation of the Fifth Degree,* first published in 1858. Hermite, we recall, had given the first solution, by means of elliptic (modular) functions in the same year. Kronecker attains Hermite's solution – or what is practically the same – by applying the ideas of Galois to the problem, thereby making the miracle appear more 'natural'. In another paper, also short, over which he has spent most of his time for five years, he returns to the subject in 1861, and seeks the reason *why* the general equation of the fifth degree is solvable in the manner in which it is, thus taking a step beyond Abel who settled the question of solvability 'by radicals'.

Much of Kronecker's work has a distinct arithmetical tinge, either of rational arithmetic or of the broader arithmetic of algebraic numbers. Indeed, if his mathematical activity had any guiding clue, it may be said to have been his desire, perhaps subconscious, to *arithmetize* all mathematics, from algebra to analysis. 'God made the integers', he said, 'all the rest is the work of man.' Kronecker's demand that analysis be replaced by finite arithmetic was the root of his disagreement with Weierstrass. Universal arithmetization may be too narrow an ideal for the luxuriance of modern mathematics, but at least it has the merit of greater clarity than is to be found in some others.

Geometry never seriously attracted Kronecker. The period of specialization was already well advanced when Kronecker did most of his work, and it would probably have been impossible for any man to have done the profoundly perfect sort of work that Kronecker did as an algebraist and in his own peculiar type of analysis and at the same time have accomplished anything of significance in other fields. Specialization is frequently damned, but it has its virtues.

A distinguishing feature of many of Kronecker's technical discoveries was the intimate way in which he wove together the three strands of his greatest interests – the theory of numbers, the theory of equations, and elliptic functions – into one beautiful pattern in which unforeseen symmetries were revealed as the design developed and many details were unexpectedly imaged in others far away. Each of the tools with which he worked seemed to have been designed by fate for the more efficient functioning of the others. Not content to accept this mysterious unity as a mere mystery, Kronecker sought and found its underlying structure in Gauss' theory of binary quadratic forms, in which the main problem is to investigate the solutions in integers of indeterminate equations of the second degree in two unknowns.

Kronecker's great work in the theory of algebraic numbers was not part of this pattern. In another direction he also departed occasionally from his principal interests when, according to the fashion of his times, he occupied himself with the purely mathematical aspects of certain problems (in the theory of attraction as in Newton's gravitation) of mathematical physics. His contributions in this field were of mathematical rather than physical interest.

Up till the last decade of his life Kronecker was a free man with obligations to no employer. Nevertheless he voluntarily assumed scientific duties, for which he received no remuneration, when he availed himself of his privilege as a member of the Berlin Academy to lecture at the University of Berlin. From 1861 to 1883 he conducted regular courses at the university, principally on his personal researches, after the necessary introductions. In 1883 Kummer, then at Berlin, retired, and Kronecker succeeded his old master as ordinary professor. At this period of his life he travelled extensively and was a frequent and welcome participant in scientific meetings in Great Britain, France, and Scandinavia.

Throughout his career as a mathematical lecturer Kronecker competed with Weierstrass and other celebrities whose subjects were more popular than his own. Algebra and the theory of numbers have never appealed to so wide an audience as have geometry and analysis, possibly because the connexions of the latter with physical science are more apparent.

Kronecker took his aristocratic isolation good-naturedly and even with a certain satisfaction. His beautifully clear introductions deluded his auditors into a belief that the subsequent course of lectures would be easy to follow. This belief evaporated rapidly as the course progressed, until after three sessions all but a faithful and obstinate few had silently stolen away – many of them to listen to Weierstrass. Kronecker rejoiced. A curtain could now be drawn across the room behind the first few rows of chairs, he joked, to bring lecturer and auditors into cosier intimacy. The few disciples he retained followed him devotedly, walking home with him to continue the discussions of the lecture room and frequently affording the crowded sidewalks of Berlin the diverting spectacle of an excited little man talking with his whole body – especially his hands – to a spell-bound group of students blocking the traffic. His house was always open to his pupils, for Kronecker really liked people, and his generous hospitality was one of the greatest satisfactions of his life. Several of his students became eminent mathematicians, but his 'school' was the whole world and he made no effort to acquire an artificially large following.

The last is characteristic of Kronecker's own most startlingly independent work. In an atmosphere of confident belief in the soundness of analysis Kronecker assumed the unpopular role of the philosophical doubter. Not many of the great mathematicians have taken philosophy seriously; in fact the majority seem to have regarded philosophical speculations with repugnance, and any epistemological doubt affecting the soundness of their work has usually been ignored or impatiently brushed aside.

With Kronecker it was different. The most original part of his work, in which he was a true pioneer, was a natural outgrowth of his philosophical inclinations. His father, Werner, Kummer, and his own wide reading in philosophical literature had influenced him in the direction of a critical outlook on all human knowledge, and when he contemplated mathematics from this questioning point of view he did not spare it because it happened to be the field of his own particular interest, but infused it with an acid, beneficial scepticism. Although but little of this found its way into print it annoyed some of his contemporaries intensely and it has survived. The doubter did not address himself to the living but, as he said, 'to those who shall come after me'. To-day these followers have arrived, and owing to their united efforts – although they often succeed only in contradicting one another – we are beginning to get a clearer insight into the nature and meaning of mathematics.

Weierstrass (Chapter 22) would have constructed mathematical analysis on his conception of irrationals as defined by infinite sequences of rationals. Kronecker not only disputes Weierstrass; he would nullify Eudoxus. For him as for Pythagoras only the God-given integers 1,2,3, ... , 'exist'; all the rest is a futile attempt of mankind to improve on the Creator.

Weierstrass on the other hand believed that he had at last made the square root of 2 as comprehensible and as safe to handle as 2 itself; Kronecker denied that the square root of 2 'exists', and he asserted that it is impossible to reason consistently with or about the Weierstrassian construction for this root or for any other irrational. Neither his older colleagues nor the young to whom Kronecker addressed himself gave his revolutionary idea a very enthusiastic welcome.

Weierstrass himself seems to have felt uneasy; certainly he was hurt. His strong emotion is released mostly in one tremendous German sentence* like a fugue, which it is almost impossible to preserve in English. 'But the worst of it is', he complains, 'that Kronecker uses his authority to proclaim that *all* those who up to now have laboured to establish the theory of functions are sinners before the Lord. When a whimsical eccentric like Christoffel [the man whose somewhat neglected work was to become, years after his death, an important tool in differential geometry as it is cultivated to-day in the mathematics of relativity] says that in twenty or thirty years the present theory of functions will be buried and that the whole of analysis will be referred to the theory of forms, we reply with a shrug. But when Kronecker delivers himself of the following verdict which I repeat *word for word*: "If time and strength are granted me, I myself will show the mathematical world that not only geometry, but also arithmetic can point the way to analysis, and certainly a more rigorous way. If I cannot do it myself those who come after me will . . . and they will recognize the incorrectness of *all* those conclusions with which *so-called* analysis works at present" – such a verdict from a man whose eminent talent and distinguished performance in mathematical research I admire as sincerely and with as much pleasure as all his colleagues, is not only humiliating for those whom he adjures to acknowledge as an error and to forswear the substance of what has constituted the object of their thought and unremitting labour, but it is a direct appeal to the younger generation to desert their present leaders and rally around him as the disciple of a new system which *must* be founded. Truly it is sad,

* In a letter to Sonja Kowalewski, 1885.

and it fills me with a bitter grief, to see a man, whose glory is without flaw, let himself be driven by the well justified feeling of his own worth to utterances whose injurious effect upon others he seems not to perceive.

'But enough of these things, on which I have touched only to explain to you the reason why I can no longer take the same joy that I used to take in my teaching, even if my health were to permit me to continue it a few years longer. But you must not speak of it; I should not like others, who do not know me as well as you, to see in what I say the expression of a sentiment which is in fact foreign to me.'

Weierstrass was seventy and in poor health when he wrote this. Could he have lived till to-day he would have seen his own great system still flourishing like the proverbial green bay tree. Kronecker's doubts have done much to instigate a critical re-examination of the foundations of all mathematics, but they have not yet destroyed analysis. They go deeper, and if anything of far-reaching significance is to be replaced by something firmer but as yet unknown, it seems likely that a good part of Kronecker's own work will go too, for the critical attack which he foresaw has uncovered weaknesses where he suspected nothing. Time makes fools of us all. Our only comfort is that greater shall come after us.

Kronecker's 'revolution', as his contemporaries called his subversive assault on analysis, would banish all but the positive integers from mathematics. Geometry since Descartes has been largely an affair of analysis applied to ordered pairs, triples, ... of real numbers (the 'numbers' which correspond to the distances measured on a given straight line from a fixed point on the line); hence it too would come under the sway of Kronecker's programme. So familiar a concept as that of a negative integer, -2 for instance, would not appear in the mathematics Kronecker prophesied, nor would common fractions.

Irrationals, as Weierstrass points out, roused Kronecker's special displeasure. To speak of $x^2 - 2 = 0$ having a root would be meaningless. All of these dislikes and objections are of course themselves meaningless unless they can be backed by a definite programme to replace what is rejected.

Kronecker actually did this, at least in outline, and indicated how the whole of algebra and the theory of numbers, including algebraic numbers, can be reconstructed in accordance with his demand. To get rid of $\sqrt{-1}$, for example, we need only put a letter for it temporarily, say i, and consider polynomials containing i and other letters, say $x,y,z, \ldots$ Then we manipulate these polynomials as in elementary algebra, treating i like any of the other letters, till the last step, when every polynomial containing i is divided by $i^2 + 1$ and everything but the remainder obtained from this division is discarded. Anyone who remembers a little elementary algebra may readily convince himself that this leads to all the familiar properties of the mysteriously misnamed 'imaginary' numbers of the text-books. In a similar manner negatives and fractions and *all* algebraic numbers (other than the positive rational integers) are eliminated from mathematics – if desired – and only the blessed positive integers remain. The inspiration about discarding $\sqrt{-1}$ goes back to Cauchy in 1847. This was the germ of Kronecker's programme.

Those who dislike Kronecker's 'revolution' call it a *Putsch*, which is more like a drunken brawl than an orderly revolution. Nevertheless it has led in recent years to two constructively critical movements in the whole of mathematics: the demand that a construction in a finite number of steps be given or proved to be possible for any 'number' or other mathematical 'entity' whose 'existence' is indicated, and the banishment from mathematics of all definitions that cannot be stated explicitly in a finite number of words. Insistence upon these demands has already done much to clarify our conception of the nature of mathematics, but a vast amount remains to be done. As this work is still in progress we shall defer further consideration of it until we come to Cantor, when it will be possible to exhibit examples.

Kronecker's disagreement with Weierstrass should not leave an unpleasant impression, as it may do if we ignore the rest of Kronecker's generous life. Kronecker had no intention of wounding his kindly old senior; he merely let his tongue run away with him in the heat of a purely mathematical argument, and Weierstrass, when he was in good spirits, laughed the whole attack off, as he should have done, knowing well that just as he had improved on Eudoxus, so his successors would probably improve upon him. Possibly if Kronecker had been six or seven inches taller than he was he would not have felt constrained to over-emphasize his objections to analysis so vociferously. Much of the whole wordy dispute sounds suspiciously like the over-correction of an unjustified inferiority complex.

The reaction of many mathematicians to Kronecker's 'revolution' was summed up by Poincaré when he said that Kronecker had been enabled to do so much fine mathematics because he frequently forgot his own mathematical philosophy. Like not a few epigrams this one is just untrue enough to be witty.

Kronecker died of a bronchial illness in Berlin on 29 December 1891 in his sixty-ninth year.

CHAPTER TWENTY-SIX

ANIMA CANDIDA

Riemann

IT has been said of Coleridge that he wrote but little poetry of the highest order of excellence, but that that little should be bound in gold. The like has been said of Bernhard Riemann, the mathematical fruits of whose all too brief summer fill only one octavo volume. It may also be truly said of Riemann that he touched nothing that he did not in some measure revolutionize. One of the most original mathematicians of modern times, Riemann unfortunately inherited a poor constitution, and he died before he had reaped a tithe of the golden harvests in his fertile mind. Had he been born a century later than he was, medical science could probably have leased him twenty or thirty more years of life, and mathematics would not now be waiting for his successor.

Georg Friedrich Bernhard Riemann, the son of a Lutheran pastor, and the second of six children (two boys, four girls), was born in the little village of Breselenz, in Hanover, Germany, on 17 September 1826. His father had fought in the Napoleonic wars, and on settling down to a less barbarous mode of living had married Charlotte Ebell, daughter of a court councillor. Hanover in 1826 was not exactly prosperous, and the circumstances of an obscure country parson with a wife and six children to feed and clothe were far from affluent. It is claimed by some biographers, apparently with justice, that the frail health and early deaths of most of the Reimann children were the result of under-nourishment in their youth and were not due to poor stamina. The mother also died before her children were grown.

In spite of poverty the home life was happy, and Riemann always retained the warmest affection – and homesickness, when he was absent – for all his lovable family. From his earliest years he was a timid, diffident soul with a horror of speaking in public or attracting attention to himself. In later life this chronic shyness proved a very serious handicap and occasioned him much agonized misery till he overcame it by diligent preparation for every public utterance he was likely to make. The engaging bashfulness of Riemann's boyhood and early manhood, which endeared him to all who met him, was in strange contrast to the ruthless boldness of his matured scientific thought. Supreme in the world of his own creation, he realized his transcendent powers and shrank from nobody, real or imaginary.

While Riemann was still an infant his father was transferred to the pastorate of Quickborn. There young Riemann received his first instruction, from his father, who appears to have been an excellent teacher. From the very first lessons Bernhard showed an unquenchable thirst for learning. His earliest interests were historical, particularly in the romantic and tragic history of Poland. As a boy of five Bernhard gave his father no peace about unhappy Poland, but demanded to be told over and over again the legend of that heroic country's gallant (and at times slightly fatuous) struggles for liberty and, in the late Woodrow Wilson's rich, fruity phrase, 'self-determination'.

Arithmetic, begun at about six, offered something less harrowing for the sensitive young boy to dwell on. His inborn mathematical genius now asserted itself. Bernhard not only solved all the problems shoved at him, but invented more difficult teasers to exasperate his brother and sisters. Already the creative impulse in mathematics dominated the boy's mind. At the age of ten he received instruction in more advanced arithmetic and geometry from a professional teacher, one Schulz, a fairly good pedagogue. Schulz soon found himself following his pupil, who often had better solutions than he.

At fourteen Riemann went to stay with his grandmother at Hanover, where he entered his first Gymnasium, in the upper third class. Here he endured his first overwhelming loneliness. His shyness made him the butt of his schoolfellows and drove him in upon his own resources. After a temporary setback his schoolwork was uniformly excellent, but it gave him no comfort, and his only solace was the joy of buying such inconsiderable presents as his pocket money would permit, to send home to his parents and brother and sisters on their birthdays. One present for his parents he invented and made himself, an original perpetual calendar, much to the astonishment of his incredulous schoolfellows. On the death of his grandmother two years later, Riemann was transferred to the Gymnasium at Lüneburg, where he studied till he was prepared, at the age of nineteen, to enter the University of Göttingen. At Lüneburg Riemann was within walking distance of home. He took full advantage of his opportunities to escape to the warmth of his own fireside. These years of his secondary education, while his health was still fair, were the happiest of his life. The tramps back and forth between the Gymnasium and Quickborn taxed his strength, but in spite of his mother's anxiety that he might wear himself out, Riemann continued to over-exert himself in order that he might be with his family as often as possible.

While still at the Gymnasium Riemann suffered from the itch for finality and perfection which was later to slow up his scientific publication. This defect – if such it was – caused him great difficulty in his written language exercises and at first made it doubtful whether he would 'pass'. But this same trait was responsible later for the finished form of two of his masterpieces, one of which even Gauss declared to be perfect. Things improved when Seyffer, the teacher of Hebrew, took young Riemann into his own house as a boarder and ironed him out.

The two studied Hebrew together, Riemann frequently giving more than he took, as the future mathematician at that time was all set to gratify his father's wishes and become a great preacher – as if Riemann, with his tongue-tied bashfulness, could ever have thumped hell and damnation or redemption and paradise out of any pulpit. Riemann himself was enamoured of the pious prospect, and although he never got as far as a probationary sermon, he did employ his mathematical talents in an attempted demonstration, in the manner of Spinoza, of the truth of Genesis. Undaunted by his failure, young Riemann persevered in his faith and remained a sincere

Christian all his life. As his biographer (Dedekind) states, 'He reverently avoided disturbing the faith of others; for him the main thing in religion was daily self-examination'. By the end of his Gymnasium course it was plain even to Riemann that Great Headquarters could have but little use for him as a router of the devil, but might be able to employ him profitably in the conquest of nature. Thus once again, as in the cases of Boole and Kummer, a brand was plucked from the burning, *ad majorem Dei gloriam*.

The director of the Gymnasium, Schmalfuss, having observed Riemann's talent for mathematics, had given the boy the run of his private library and had excused him from attending mathematical classes. In this way Riemann discovered his inborn aptitude for mathematics, but his failure to realize immediately the extent of his ability is so characteristic of his almost pathological modesty as to be ludicrous.

Schmalfuss had suggested that Riemann borrow some mathematical book for private study. Riemann said that would be nice, provided the book was not too easy, and at the suggestion of Schmalfuss carried off Legendre's *Théorie des Nombres* (Theory of Numbers). This is a mere trifle of 859 large quarto pages, many of them crabbed with very close reasoning indeed. Six days later Riemann returned the book. 'How far did you read?' Schmalfuss asked. Without replying directly, Riemann expressed his appreciation of Legendre's classic. 'That is certainly a wonderful book. I have mastered it.' And in fact he had. Some time later when he was examined he answered perfectly, although he had not seen the book for months.

No doubt this is the origin of Riemann's interest in the riddle of prime numbers. Legendre has an empirical formula estimating the approximate number of primes less than any preassigned number; one of Riemann's profoundest and most suggestive works (only eight pages long) was to be in the same general field. In fact 'Riemann's hypothesis', originating in his attempt to improve on Legendre, is to-day one of the outstanding challenges, if not *the* outstanding challenge, to pure mathematicians.

To anticipate slightly, we may state here what this hypothesis is. It occurs in the famous memoir *Ueber die Anzahl der Primzahlen unter einer gegebenen Grösse* (On the number of prime numbers under a given magnitude), printed in the monthly notices of the Berlin Academy for November 1859, when Riemann was thirty-three. The problem concerned is to give a formula which will state how many primes there are less than any given number n. In attempting to solve this Riemann was driven to an investigation of the infinite series

$$1 + \frac{1}{2^s} + \frac{1}{3^s} + \frac{1}{4^s} + \frac{1}{5^s} + \dots,$$

in which s is a complex number, say $s = u + iv$ ($i = \sqrt{-1}$), where u and v are real numbers, so chosen that the series converges. With this proviso the infinite series is a definite function of s, say $\zeta(s)$ (the Greek zeta, ζ, is always used to denote this function, which is called 'Riemann's zeta function'); and as s varies, $\zeta(s)$ continuously takes on different values. *For what values of s will $\zeta(s)$ be zero?* Riemann conjectured that *all* such values of s for which u lies between 0 and 1 are of the form $\frac{1}{2} + iv$, namely, *all have their real part equal to* $\frac{1}{2}$.

This is the famous hypothesis. Whoever proves or disproves it will cover himself with glory and incidentally dispose of many extremely difficult questions in the theory of prime numbers, other parts of the higher arithmetic, and in some fields of analysis. Expert opinion favours the truth of the hypothesis. In 1914 the English mathematician G. H. Hardy proved that *an infinity* of values of s satisfy the hypothesis, but an infinity is not necessarily all. A decision one way or the other disposing of Riemann's conjecture would probably be of greater interest to mathematicians than a proof or disproof of Fermat's Last Theorem. Riemann's hypothesis is not the sort of problem that can be attacked by elementary methods. It has already given rise to an extensive and thorny literature.

Legendre was not the only great mathematician whose works Riemann absorbed by himself – always with amazing speed – at the Gymnasium; he became familiar with the calculus and its ramifications through the study of Euler. It is rather surprising that from such an antiquated start in analysis

(Euler's approach was out of date by the middle 1840's owing to the work of Gauss, Abel, and Cauchy), Riemann later became the acute analyst that he did. But from Euler he may have picked up something which also has its place in creative mathematical work, an appreciation of symmetrical formulae and manipulative ingenuity. Although Riemann depended chiefly on what may be called deep philosophical ideas – those which get at the heart of a theory – for his greater inspirations, his work nevertheless is not wholly lacking in the 'mere ingenuity' of which Euler was the peerless master and which it is now quite the fashion to despise. The pursuit of pretty formulae and neat theorems can no doubt quickly degenerate into a silly vice, but so also can the quest for austere generalities which are so very general indeed that they are incapable of application to any particular. Riemann's instinctive mathematical tact preserved him from the bad taste of either extreme.

In 1846, at the age of nineteen, Riemann matriculated as a student of philology and theology at the University of Göttingen. His desire to please his father and possibly help financially by securing a paying position as quickly as possible dictated the choice of theology. But he could not keep away from the mathematical lectures of Stern on the theory of equations and on definite integrals, those of Gauss on the method of least squares, and Goldschmidt's on terrestrial magnetism. Confessing all to his indulgent father, Riemann prayed for permission to alter his course. His father's ungrudging consent that Bernhard follow mathematics as a career made the young man supremely happy – also profoundly grateful.

After a year at Göttingen, where the instruction was decidedly antiquated, Riemann migrated to Berlin to receive from Jacobi, Dirichlet, Steiner, and Eisenstein his initiation into new and vital mathematics. From all of these masters he learned much – advanced mechanics and higher algebra from Jacobi, the theory of numbers and analysis from Dirichlet, modern geometry from Steiner, while from Eisenstein, three years older than himself, he learned not only elliptic functions but self-confidence, for he and the young master had a radical and most energizing difference of opinion as to how the theory should be developed. Eisenstein insisted on beautiful formulae, somewhat in the manner of a modernized Euler; Riemann wanted to introduce the complex variable and derive the entire theory, with a minimum of calculation, from a few simple, general principles. Thus, no doubt, originated at least the germs of one of Riemann's greatest contributions to pure mathematics. As the origin of Riemann's work in the theory of functions of a complex variable is of considerable importance in his own history and in that of modern mathematics, we shall glance at what is known about it.

Briefly, nothing definite. The definition of an analytic function of a complex variable, discussed in connexion with Gauss' anticipation of Cauchy's fundamental theorem, was essentially that of Riemann. When expressed analytically instead of geometrically that definition leads to the pair of partial differential equations* which Riemann took as his point of departure for a theory of functions of a complex variable. According to Dedekind, 'Riemann recognized in these partial differential equations the essential definition of an [analytic] function of a complex variable. Probably these ideas, of the highest importance for his future career, were worked out by him in the fall vacation of 1847 [Riemann was then twenty-one] for the first time.'

Another version of the origin of Riemann's inspiration is due to Sylvester, who tells the following story, which is interesting even if possibly untrue. In 1896, the year before his death, Sylvester recalls staying at 'a hotel on the river at Nuremberg, where I conversed outside with a Berlin bookseller, bound, like myself, for Prague. ... He told me he was formerly a fellow pupil of Riemann, at the University, and that, one day, after receipt of some numbers of the *Comptes rendus* from Paris, the latter shut himself up for some weeks, and when he returned to

* If $z = x + iy$, and $w = u + iv$, is an analytic function of z, Riemann's equations are

$$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y}, \quad \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x}.$$

These equations had been given much earlier by Cauchy, and even Cauchy was not the first, as D'Alembert had stated the equations in the eighteenth century.

the society of his friends said (referring to the newly published papers of Cauchy), "This is a new mathematic".'

Riemann spent two years at the University of Berlin. During the political upheaval of 1848 he served with the loyal student corps and had one weary spell of sixteen hours' guard duty protecting the jittery if sacred person of the king in the royal palace. In 1849 he returned to Göttingen to complete his mathematical training for the doctorate. His interests were unusually broad for the pure mathematician he is commonly rated to be, and in fact he devoted as much of his time to physical science as he did to mathematics.

From this distance it seems as though Riemann's real interest was in mathematical physics, and it is quite possible that had he been granted twenty or thirty more years of life he would have become the Newton or Einstein of the nineteenth century. His physical ideas were bold in the extreme for his time. Not till Einstein realized Riemann's dream of a geometrized (macroscopic) physics did the physics which Riemann foreshadowed – somewhat obscurely, it may be – appear reasonable to physicists. In this direction his only understanding follower till our own century was the English mathematician William Kingdon Clifford (1845–79), who also died long before his time.

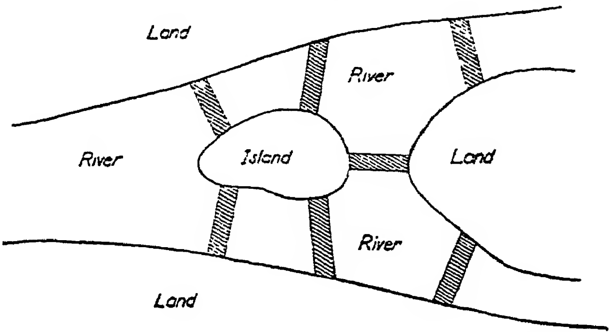
During his last three semesters at Göttingen Riemann attended lectures on philosophy and followed the course of Wilhelm Weber in experimental physics with the greatest interest. The philosophical and psychological fragments left by Riemann at his death show that as a philosophical thinker he was as original as he was in mathematics and science. Weber recognized Riemann's scientific genius and became his warm friend and helpful counsellor. To a far higher degree than the majority of great mathematicians who have written on physical science, Riemann had a feeling for what is important – or likely to be so – in physics, and this feeling is no doubt due to his work in the laboratory and his contact with men who were primarily physicists and not mathematicians. The contributions of even great pure mathematicians to physical science have usually been characterized by a singular irrelevance so far as the universe observed by scientists is concerned. Riemann, as a physical mathematician, was in the same class as Newton, Gauss, and Einstein in his instinct for what is likely to be of scientific use in mathematics.

As a sequel to his philosophical studies with Johann Friedrich Herbart (1776–1841), Riemann came to the conclusion in 1850 (he was then twenty-four) that 'a complete, well-rounded mathematical theory can be established, which progresses from the elementary laws for individual points to the processes given to us in the plenum ("continuously filled space") of reality, without distinction between gravitation, electricity, magnetism, or thermostatics'. This is probably to be interpreted as Riemann's rejection of all 'action at a distance' theories in physical science in favour of field theories. In the latter the physical properties of the 'space' surrounding a 'charged particle', say, are the object of mathematical investigation. Riemann at this stage of his career seems to have believed in a space-filling 'ether', a conception now abandoned. But as will appear from his epochal work on the foundations of geometry, he later sought the description and correlation of physical phenomena in the *geometry* of the 'space' of human experience. This is in the current fashion, which rejects an existent, unobservable ether as a cumbersome superfluity.

Fascinated by his work in physics, Riemann let his pure mathematics slide for a while and in the autumn of 1850 joined the seminar in mathematical physics which had just been founded by Weber, Ulrich, Stern, and Listing. Physical experiments in this seminar consumed the time that scholarly prudence would have reserved for the doctoral dissertation in mathematics, which Riemann did not submit till he was twenty-five.

One of the leaders in the seminar, Johann Benedict Listing (1808–82), may be noted in passing, as he probably influenced Riemann's thought in what was to be (1857) one of his greatest achievements, the introduction of topological methods into the theory of functions of a complex variable.

It will be recalled that Gauss had prophesied that analysis situs would become one of the most important fields of mathematics, and Riemann, by his inventions in the theory of functions, was to give a partial fulfilment of this prophecy. Although topology (now called analysis situs) as first developed bore but little resemblance to the elaborate theory which to-day absorbs all the energies of a prolific school, it may be of interest to state the trivial puzzle which apparently started the whole vast and intricate theory. In Euler's time seven bridges crossed the river Pregel in Königsberg, as in the diagram, the shaded bars repre-



senting the bridges. Euler proposed the problem of crossing all seven bridges without passing twice over any one. The problem is impossible.

The nature of Riemann's use of topological methods in the theory of functions may be disposed of here, although an adequate description is out of the question in untechnical language. For the meaning of 'uniformity' with respect to a function of a complex variable we must refer to what was said in the chapter on Gauss. Now, in the theory of Abelian functions, *multiform* functions present themselves inevitably; an n-valued function of z is a function which, except for certain values of z, takes precisely n distinct values for each value assigned to z. Illustrating *multiformity*, or *many-valuedness*, for functions of a real variable, we note that y, considered as a function of x, defined by the equation $y^2 = x$, is two-valued. Thus, if $x = 4$, we get $y^2 = 4$, and hence $y = 2$ or -2; if x is any real number except zero or 'infinity', y has the two distinct values of $\sqrt{x}$ and $-\sqrt{x}$. In this simplest possible example y and x are connected by an algebraic equation, namely $y^2 - x = 0$. Passing at once to the general situation of which this is a very special case, we might discuss the n-valued function y which is defined, as a function of x, by the equation

$$P_0(x)y^n + P_1(x)y^{n-1} + \ldots + P_{n-1}(x)y + P_n(x) = 0,$$

in which the P's are polynomials in x. This equation defines y as an n-valued function of x. As in the case of $y^2 - x = 0$, there will be certain values of x for which two or more of these n values of y are equal. These values of x are the so-called *branch points* of the n-valued function defined by the equation.

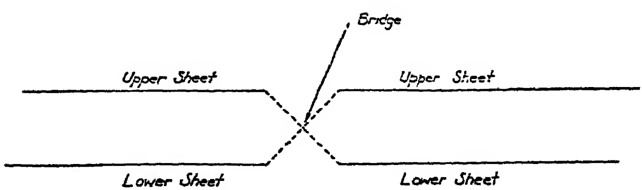
All this is now extended to functions of complex variables, and the function w (also its integral) as defined by

$$P_0(z)w^n + P_1(z)w^{n-1} + \ldots + P_{n-1}(z)w + P_n(z) = 0,$$

in which z denotes the complex variable $s + it$, where s, t are real variables and $i = \sqrt{-1}$. The n values of w are called the *branches of* the function w. Here we must refer (chapter on Gauss) to what was said about the representation of *uniform* functions of z. Let the variable z ($= s + it$) trace out any path in its plane, and let the *uniform* function $f(z)$ be expressed in the form $U + iV$, where U, V are functions of s, t. Then, to every value of z will correspond one, and only one, value for each of U, V, and, as z traces out its path in the s, t-plane, $f(z)$ will trace out a corresponding path in the U, V-plane: the path of $f(z)$ will be *uniquely* determined by that of z. But if w is a *multiform* (many-valued) function of z, such that precisely n distinct values of w are determined by each value of z (except at branch points, where several values of w may be equal), then it is obvious that *one* w-plane no longer suffices (if n is greater than 1) to represent the path, the 'march' of the function w. In the case of a *two*-valued function w, such as that determined by $w^2 = z$, *two* w-planes would be required and, quite generally, for an n-valued function (n finite or infinite), precisely n such w-planes would be required.

The advantages of considering *uniform* (*one*-valued) functions instead of n-valued functions (n greater than 1) should be obvious even to a non-mathematician. What Riemann did was this: instead of the n distinct w-planes, he introduced an n-sheeted surface, of the sort roughly described in what follows, on which the *multiform* function is *uniform*, that is, on which to each 'place' on the surface corresponds one, and only one, value of the function represented.

Riemann *united*, as it were, all the n planes into a *single* plane, and he did this by what may at first look like an inversion of the representation of the n branches of the n-valued function on n distinct planes; but a moment's consideration will show that, in effect, he *restored uniformity*. For he superimposed n z-planes on one another; each of these planes, or *sheets*, is associated with a particular branch of the function so that, as long as z moves in a particular sheet, the corresponding branch of the function is traversed by w (the n-valued function of z under discussion), and as z passes from one sheet to another, the branches are changed, one into another, until, on the variable z having traversed all the sheets and having returned to its initial position, the original branch is restored. The passage of the variable z from one sheet to another is effected by means of *cuts* (which may be thought of as straight-line bridges) joining branch points; along a given cut providing passage from one sheet to another, one 'lip' of the upper sheet is imagined as pasted or joined to the opposite lip of the under sheet, and similarly for the other lip of the upper sheet. Diagrammatically, in cross-section,



The sheets are not joined along cuts (which may be drawn in many ways for given branch points) at random, but are so joined that, as z traverses its n-sheeted surface, passing from one sheet to another as a bridge or cut is reached, the *analytical* behaviour of the function of z is pictured consistently, particularly as concerns the interchange of branches consequent on the variable z, if represented on a plane, having gone completely round a branch point. To this circuiting of a branch point on the *single* z-plane corresponds, on the n-sheeted Riemann surface, the passage from one sheet to another and the resultant interchange of the branches of the function.

There are many ways in which the variable may wander about the n-sheeted *Riemann surface*, passing from one sheet to another. To each of these corresponds a particular interchange of the branches of the function, which may be symbolized by writing, one after another, letters denoting the several branches interchanged. In this way we get the symbols of certain *substitutions* (as in chapter 15) on n letters; all of these substitutions generate a group which, in some respects, pictures the nature of the function considered.

Riemann surfaces are not easy to represent pictorially, and those who use them content themselves with diagrammatical representations of the connexion of the sheets, in much the same way that an organic chemist writes a 'graphical' formula for a complicated carbon compound which recalls in a schematic manner the chemical behaviour of the compound but which does not, and is not meant to, depict the actual spatial arrangement of the atoms in the compound. Riemann made wonderful advances, particularly in the theory of Abelian functions, by means of his surfaces and their topology – how shall the cuts be made so as to render the n-sheeted surface equivalent to a plane, being one question in this direction. But mathematicians are like other mortals in their ability to visualize complicated spatial relationships, namely, a high degree of spatial 'intuition' is excessively rare.

Early in November, 1851, Riemann submitted his doctoral dissertation, *Grundlagen für eine allgemeine Theorie der Funktionen einer veränderlichen complexen Grösse* (Foundations for a general theory of functions of a complex variable), for Gauss' consideration. This work by the young master of twenty-five was one of the few modern contributions to mathematics that roused the enthusiasm of Gauss, then an almost legendary figure within four years of his death. When Riemann called on Gauss, after the latter had read the dissertation, Gauss told him that he himself had planned for years to write a treatise on the same topic. Gauss' official report to the Philosophical Faculty of the University of Göttingen is noteworthy as one of the rare formal pronouncements in which Gauss let himself go.

'The dissertation submitted by Herr Riemann offers convincing evidence of the author's thorough and penetrating investigations in those parts of the subject treated in the dissertation, of a creative, active, truly mathematical mind, and of a gloriously fertile originality. The presentation is perspicuous and concise and, in places, beautiful. The majority of readers would have preferred a greater clarity of arrangement. The whole is a substantial, valuable work, which not only satisfies the standards demanded for doctoral dissertations, but far exceeds them.'

A month later Riemann passed his final examination, including the formality of a public 'defence' of his dissertation. All went off successfully, and Riemann began to hope for a position in keeping with his talents. 'I believe I have improved my prospects with my dissertation', he wrote to his father; 'I hope also to learn to write more quickly and more fluently in time, especially if I mingle in society and if I get a chance to give lectures; therefore am I of good courage.' He also apologizes to his father for not having gone after a vacant assistantship at the Göttingen Observatory more energetically, but as he hopes to be 'habilitated' as a *Privatdozent* the outlook is not as dark as it might be.

For his *Habilitationsschrift* (probationary essay) Riemann had planned to submit a memoir on trigonometric series (Fourier series). But two and a half years were to pass before he might hang out his sign as an unpaid university instructor picking up what he could in the way of fees from students not bound to attend his lectures. During the autumn of 1852 Riemann profited by Dirichlet's presence in Göttingen on a vacation and sought his advice on the embryonic memoir. Riemann's friends saw to it that the young man met the famous mathematician from Berlin – second only to Gauss – socially.

Dirichlet was captivated by Riemann's modesty and genius. 'Next morning [after a dinner party] Dirichlet was with me for two hours,' Riemann wrote his father. 'He gave me the notes

I needed for my probationary essay; otherwise I should have had to spend many hours in the library in laborious research. He also read over my dissertation with me and was very friendly – which I could hardly have expected, considering the great distance in rank between us. I hope he will remember me later on.' During this visit of Dirichlet's there were excursions with Weber and others, and Riemann reported to his father that these human escapes from mathematics did him more good scientifically than if he had sat all day over his books.

From 1853 (Riemann was then twenty-seven) onward he thought intensively about mathematical physics. By the end of the year he had completed the probationary essay, after many delays due to his growing passion for physical science.

There was still a trial lecture ahead of him before he could be appointed to the coveted – but unpaid – lectureship. For this ordeal he had submitted three titles for the faculty to choose from, hoping and expecting that one of the first two, on which he had prepared himself, would be selected. But he had incautiously included as his third offering a topic on which Gauss had pondered for sixty years or more – the foundations of geometry – and this he had not prepared. Gauss no doubt was curious to see what a Riemann's 'gloriously fertile originality' would make of such a profound subject. To Riemann's consternation Gauss designated the third topic as the one on which Riemann should prove his mettle as a lecturer before the critical faculty. 'So I am again in a quandary,' the rash young man confided to his father, 'since I have to work out this one. I have resumed my investigation of the connexion between electricity, magnetism, light, and gravitation, and I have progressed so far that I can publish it without a qualm. I have become more and more convinced that Gauss has worked on this subject for years, and has talked to some friends (Weber among others) about it. I tell you this in confidence, lest I be thought arrogant – I hope it is not yet too late for me and that I shall gain recognition as an independent investigator.'

The strain of carrying on two extremely difficult investigations simultaneously, while acting as Weber's assistant in the seminar in mathematical physics, combined with the usual handicaps of poverty, brought on a temporary breakdown. 'I became so absorbed in my investigation of the unity of all physical laws that when the subject of the trial lecture was given me, I could not tear myself away from my research. Then, partly as a result of brooding on it, partly from staying indoors too much in this vile weather, I fell ill; my old trouble recurred with great pertinacity and I could not get on with my work. Only several weeks later, when the weather improved and I got more social stimulation, I began feeling better. For the summer I have rented a house in a garden, and since doing so my health has not bothered me. Having finished two weeks after Easter a piece of work I could not get out of, I began at once working on my trial lecture and finished it around Pentecost [that is, in about seven weeks]. I had some difficulty in getting a date for my lecture right away and almost had to return to Quickborn without having reached my goal. For Gauss is seriously ill and the physicians fear that his death is imminent. Being too weak to examine me, he asked me to wait till August, hoping that he might improve, especially as I would not lecture anyhow till fall. Then he decided anyway on the Friday after Pentecost to set the lecture for the next day at eleven-thirty. On Saturday I was happily through with everything.'

This is Riemann's own account of the historic lecture which was to revolutionize differential geometry and prepare the way for the geometrized physics of our own generation. In the same letter he tells how the work he had been doing around Easter turned out. Weber and some of his collaborators 'had made very exact measurements of a phenomenon which up till then had never been investigated, the residual charge in a Leyden jar [after discharge it is found that the jar is not *completely* discharged] ... I sent him [one of Weber's collaborators, Kohlrausch] my theory of this phenomenon, having worked it out specially for his purposes. I had found the explanation of the phenomenon through my general investigations of the connexion between electricity, light, and magnetism. ... This matter was important to me, because it was the first time I could apply my work to a phenomenon still unknown, and I hope that the publication [of it] will contribute to a favourable reception of my larger work.'

The reception of Riemann's probationary lecture (10 June 1854) was as cordial as even he could have wished in the scared secrecy of his modest heart. The lecture had made him sweat blood to prepare because he had determined to make it intelligible even to those members of the faculty who had but little knowledge of mathematics. In addition to being one of the great masterpieces of all mathematics, Riemann's essay *Über die Hypothesen, welche der Geometrie zu Grunde liegen* (On the hypotheses which lie at the foundations of geometry), is also a classic of presentation. Gauss was enthusiastic. 'Against all tradition he had selected the third of the three topics submitted by the candidate, wishing to see how such a difficult subject would be handled by so young a man. He was surprised beyond all his expectations, and on returning from the faculty meeting expressed to Wilhelm Weber his highest appreciation of the ideas presented by Riemann, speaking with an enthusiasm that, for Gauss, was rare.' What little can be said here about this masterpiece will be reserved for the conclusion of the present chapter.

After a rest at home with his family in Quickborn, Riemann returned in September to Göttingen, where he delivered a hastily prepared lecture (sitting up most of the night to get it ready on short notice) to a convention of scientists. His topic was the propagation of electricity in non-conductors. During the year he continued his researches in the mathematical theory of electricity and prepared a paper on Nobili's colour rings because, as he wrote his sister Ida: 'This subject is important, for very exact measurements can be made in connexion with it, and the laws according to which electricity moves can be tested.'

In the same letter (9 October 1854) he expresses his unbounded joy at the success of his first academic lecture and his great satisfaction at the unexpectedly large number of auditors. Eight students had come to hear him! He had anticipated at the most two or three. Encouraged by this unhoped-for popularity, Riemann tells his father, 'I have been able to hold my classes regularly. My first diffidence and constraint have subsided more and more, and I get accustomed to think more of the auditors than of myself, and to read in their expressions whether I should go on or explain the matter further.'

When Dirichlet succeeded Gauss in 1855, Riemann's friends urged the authorities to appoint Riemann to the security of an assistant professorship, but the finances of the University could not be stretched so far. Nevertheless he was granted the equivalent of 200 dollars a year, which was better than the uncertainty of half-a-dozen voluntary students' fees. His future worried him, and when presently he lost both his father and his sister Clara, making it impossible for him to escape for vacations to Quickborn, Riemann felt poor and miserable indeed. His three remaining sisters went to live with the other brother, a postal clerk in Bremen whose salary was princely beside that of the 'economically valueless' mathematician.

The following year (1856; Riemann was then thirty) the outlook brightened a little. It was impossible for a creative genius like Riemann to be downed by despondency so long as he had the wherewithal to keep body and soul together in order that he might work. To this period belong part of his characteristically original work on Abelian functions, his classic on the hypergeometric series (see chapter on Gauss) and the differential equations – of great importance in mathematical physics – suggested by this series. In both of these works Riemann struck out on new directions of his own. The generality, the *intuitiveness*, of his approach was peculiarly his own. His work absorbed all his energies and made him happy in spite of material worries; possibly, too, the fatal optimism of the consumptive was already at work in him.

Riemann's development of the theory of Abelian functions is as unlike that of Weierstrass as moonlight is unlike sunlight. Weierstrass' attack was methodical, exact in all its details, like the advance of a perfectly disciplined army under a generalship that foresees everything and provides for all contingencies. Riemann, for his part, looked over the whole field, seeing everything but the details, which he left to take care of themselves, and was content to have grasped the key positions of the general topography in his imagination. The method of Weierstrass was arithmetical, that of Riemann geometrical and intuitive. To say that one is 'better' than the other is meaningless; both cannot be seen from a common point of view.

Overwork and lack of reasonable comforts brought on a nervous breakdown early in his thirty-first year, and Riemann was forced to spend a few weeks with a friend in the Harz mountain country, where he was joined by Dedekind. The three took long tramps together into the mountains and Riemann soon recovered. Relieved of the strain of having to keep up academic appearances, Riemann indulged his sense of humour and kept his companions amused with his spontaneous wit. They also talked shop together – most mathematicians do when they get together, just as lawyers or doctors or business men do, provided they do not have to talk drivel to maintain the social conventions. One evening after a strenuous hike Riemann dipped into Brewster's life of Newton and discovered the letter to Bentley in which Newton himself asserts the impossibility of action at a distance without intervening media. This delighted Riemann and inspired him to an impromptu lecture. To-day the 'medium' which Riemann extolled is not the luminiferous ether, but his own 'curved space', or its reflexion in the space-time of relativity.

At last, in 1857, at the age of thirty-one, Riemann got his assistant professorship. His salary was the equivalent of about 300 dollars a year, but as he had had little all his life he missed less. However, a real disaster presently descended on him: his brother died and the care of three sisters fell to his lot. It figured out at exactly seventy-five dollars a year for each of them. Love on nothing a year in a cottage may be paradise; existence on next to nothing in a university community is just plain hell. It was but little different in Riemann's day. No wonder he contracted consumption. However, the Lord, who had so generously given, shortly relieved Riemann of his youngest sister, Marie, so the individual budgets skyrocketed to 100 dollars a year. If rations had to be watched, affection was free, and Riemann was more than repaid for his sacrifices by the self-confidence inspired in him by his sisters' devotion and encouragement. The Lord may have known that if ever a struggling mortal needed encouragement, poor Riemann did; still, it seems rather an odd way of providing what was required.

In 1858 Riemann produced his paper on electrodynamics, of which he told his sister Ida, 'My discovery concerning the close connexion between electricity and light I have dedicated to the Royal Society [of Göttingen]. From what I have heard, Gauss had devised another theory regarding this close connexion, different from mine, and communicated it to his intimate friends. However, I am fully convinced that my theory is the correct one, and that in a few years it will be recognized as such. As is known, Gauss soon withdrew his memoir and did not publish it; probably he himself was not satisfied with it.' Riemann would seem here to have been over-optimistic; Clerk Maxwell's electromagnetic theory is the one which to-day holds the field – in macroscopic phenomena. The present status of theories of light and the electromagnetic field is too complicated to be described here; it is sufficient to note that Riemann's theory has not survived.

Dirichlet died on 5 May 1859. He had always appreciated Riemann and had done his best to help the struggling young man along. This interest of Dirichlet's and Riemann's rapidly mounting reputation caused the government to promote Riemann to succeed Dirichlet. At thirty-three Riemann thus became the second successor of Gauss. To ease his domestic difficulties the authorities let him reside at the Observatory, as Gauss had done. Recognition of the sincerest kind – praise from mathematicians who, although older than himself, were in some degree his rivals – now came in abundance. On a visit to Berlin he was feted by Borchardt, Kummer, Kronecker, and Weierstrass. Learned societies, including the Royal Society of London and the French Academy of Sciences, honoured him with membership, and in short he got the usual highest distinctions that can come to a man of science. A visit to Paris in 1860 acquainted him with the leading French mathematicians, particularly Hermite, whose admiration for Riemann was unbounded. This year, 1860, is memorable in the history of mathematical physics as that in which Riemann began intengeometry. Clifford was no servile copyist but a man with a brilliantly original mind of his own, of whom it may be said, as Newton said of Cotes, 'If he had lived we might have known something.' The reader who is acquainted with any of the better available popular accounts of relativistic physics and the wave theory of electrons will recognize several curious adumbrations of current theories in Clifford's brief prophecy.

'Riemann has shown that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs. In particular, the axioms of plane geometry are true within the limits of experiment on the surface of a sheet of paper, and yet we know that the sheet is really covered with a number of small ridges and furrows, upon which (the total curvature being not zero) these axioms are not true. Similarly, he says, although the axioms of solid geometry are true within the limits of experiment for finite portions of our space, yet we have no reason to conclude that they are true for very small portions; and if any help can be got thereby for the explanation of physical phenomena, we may have reason to conclude that they are not true for very small portions of space.

'I wish here to indicate a manner in which these speculations may be applied to the investigation of physical phenomena. I hold in fact

(1) That small portions of space *are* in fact of a nature analogous to little hills on a surface which is on the average flat; namely, that the ordinary laws of geometry are not valid in them.

(2) That this property of being curved or distorted is continually being passed on from one portion of space to another after the manner of a wave.

(3) That this variation of the curvature of space is what really happens in that phenomenon which we call the *motion of matter*, whether ponderable or ethereal.

(4) That in the physical world nothing else takes place but this variation, subject (possibly) to the law of continuity.

'I am endeavouring in a general way to explain the laws of double refraction on this hypothesis, but have not yet arrived at any results sufficiently decisive to be communicated.'

Riemann also believed that his new geometry would prove of scientific importance, as is shown by the conclusion of his memoir (Clifford's translation):

'Either therefore the reality which underlies space must form a discrete manifold, or we must seek the ground of its metric relations outside it, in binding forces which act upon it.

'The answer to these questions can only be got by starting from the conception of phenomena which has hitherto been justified by experience, and which Newton assumed as a foundation, and by making in this conception the successive changes required by facts which it cannot explain.' And he goes on to say that researches like his own, starting from general notions, 'can be useful in preventing this work from becoming hampered by too narrow views, and progress of knowledge of the interdependence of things from being checked by traditional prejudices.

'This leads us into the domain of another science, that of physics, into which the object of this work does not allow us to go to-day.'

Riemann's work of 1854 put geometry in a new light. The geometry he visions is non-Euclidean, not in the sense of Lobatchewsky and Johann Bolyai, nor in that of Riemann's own elaboration of the hypothesis of the obtuse angle (as explained in chapter 16), but in a more comprehensive sense depending on the conception of *measurement*. To isolate *measure-relations* as the nerve of Riemann's theory is to do it an injustice; the theory contains much more than a workable philosophy of metrics, but this is one of its main features. No paraphrase of Riemann's concise memoir can bring out all that is in it; nevertheless, we shall attempt to describe some of his basic ideas, and we shall select three: the concept of a *manifold*, the definition of *distance*, and the notion of *curvature* of a manifold.

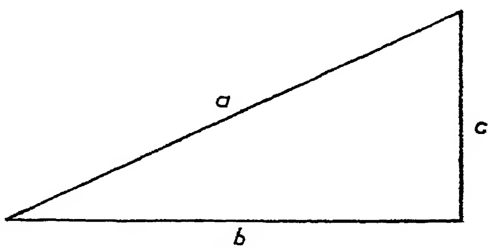
A manifold is a *class* of objects (at least in common mathematics) which is such that any member of the class can be completely specified by assigning to it certain numbers, in a definite order, corresponding to 'numberable' properties of the elements, the assignment in the given order corresponding to a preassigned ordering of the 'numberable' properties. Granted that this may be even less comprehensible than Riemann's definition, it is nevertheless a working basis from which to start, and all that it amounts to in plain mathematics is this: a manifold is a set of ordered 'n-tuples' of numbers $(x_1,x_2 \ldots, x_n)$, where the parentheses, (), indicate that the numbers $x_1,x_2, \ldots, x_n$ are to be written in the order given. Two such n-tuples, $(x_1,x_2, \ldots, x_n)$ and $(y_1y_2, \ldots, y_n)$ are *equal* when, and only when, corresponding numbers in them are respectively equal, namely, when, and only when, $x_1 = y_1$, $x_2 = y_2, \ldots, x_n = y_n$.

If precisely n numbers occur in each of these ordered n-tuples in the manifold, the manifold is said to be of *n dimensions*. Thus we are back again talking co-ordinates with Descartes. If each of the numbers in $(x_1,x_2, \ldots, x_n)$ is a positive, zero, or negative integer, or if it is an element of any countable set (a set whose elements may be counted off 1,2,3, ...), and if the like holds for every n-tuple in the set, the manifold is said to be *discrete*. If the numbers $x_1,x_2, \ldots, x_n$, may take on values *continuously* (as in the motion of a point along a line), the manifold is *continuous*.

This working definition has ignored – deliberately – the question of whether the set of ordered n-tuples is 'the manifold' or whether something 'represented by' these is 'the manifold'. Thus, when we say (x,y) are the co-ordinates of a point in a plane, we do not ask what 'a point in a plane' is, but proceed to work with these *ordered couples of numbers* (x,y) where x,y run through all real numbers independently. On the other hand it may sometimes be advantageous to fix our attention on what such a symbol as (x,y) *represents*. Thus if x is the age in seconds of a man and y his height in centimetres, we may be interested in the *man* (or the class of all men) rather than in his *co-ordinates with which alone the mathematics of our enquiry is concerned.* In this same order of ideas, geometry is no longer concerned with what 'space' 'is' – whether 'is' means anything or not in relation to 'space'. Space, for a modern mathematician, is merely a number-manifold of the kind described above, and this conception of space grew out of Riemann's 'manifolds'.

Passing on to measurement, Riemann states that 'Measurement consists in a superposition of the magnitudes to be compared. If this is lacking, magnitudes can be compared only when one is part of another, and then only the more or less, but not the how much, can be decided.' It may be said in passing that a consistent and useful theory of measurement is at present an urgent desideratum in theoretical physics, particularly in all questions where quanta and relativity are of importance.

Descending once more from philosophical generalities to less mystical mathematics, Riemann proceeded to lay down a definition of *distance*, extracted from his concept of measurement, which has proved to be extremely fruitful in both physics and mathematics. The Pythagorean proposition



that $a = b^2 + c^2$ or $a = \sqrt{b^2 + c^2}$, where a is the length of the longest side of a right-angled triangle and b,c are the lengths of the other two sides, is the fundamental formula for the measurement of distances in a *plane*. How shall this be extended to a *curved surface*? To straight lines on the plane correspond geodesics (see chapter 14) on the surface; but on a sphere, for example, the Pythagorean proposition is not true for a right-angled triangle formed by geodesics. Riemann generalized the Pythagorean formula to any manifold as follows:

Let $(x_1, x_2, \ldots, x_n)$, $(x_1 + x_1', x_2 + x_2', \ldots, x_n + x_n')$ be the co-ordinates of two 'points' in the manifold which are 'infinitesimally near' one another. For our present purpose the meaning of 'infinitesimally near' is that powers higher than the second of $x_1', x_2', \ldots, x_n'$, which measure the 'separation' of the two points in the manifold, can be neglected. For simplicity we shall state the definition when $n = 4$ – giving the distance between two neighbouring points in a space of four dimensions: the distance is the square root of

$$\begin{aligned} g_{11}x_1'^2 + g_{22}x_2'^2 + g_{33}x_3'^2 + g_{44}x_4'^2 \\ + g_{12}x_1'x_2' + g_{13}x_1'x_3' + g_{14}x_1'x_4' \\ + g_{23}x_2'x_3' + g_{24}x_2'x'_4 \\ + g_{34}x_3'x_4', \end{aligned}$$

in which the ten coefficients $g_{11}, \ldots, g_{34}$ are functions of x_1,x_2,x_3,x_4. For a particular choice of the g's, one 'space' is defined. Thus we might have $g_{11} = 1, g_{22} = 1, g_{33} = 1, g_{44} = -1$, and all the other g's zero; or we might consider a space in which all the g's except g_{44} and g_{34} were zero, and so on. A space considered in relativity is of this general kind in which all the g's except $g_{11},g_{22},g_{33},g_{44}$ are zero, and these are certain simple expressions involving x_1,x_2,x_3,x_4.

In the case of an n-dimensional space the distance between *neighbouring* points is defined in a similar manner; the general expression contains $\frac{1}{2}n(n + 1)$ terms. The generalized Pythagorean formula for the distance between neighbouring points being given, it is a solvable problem in the integral calculus to find the distance between *any* two points of the space. A space whose *metric* (system of measurement) is defined by a formula of the type described is called *Riemannian*.

Curvature, as conceived by Riemann (and before him by Gauss; see chapter on the latter) is another generalization from common experience. A straight line has zero curvature; the 'measure' of the amount by which a curved line departs from straightness may be the same for every point of the curve (as it is for a circle), or it may vary from point to point of the curve, when it becomes necessary again to express the 'amount of curvature' through the use of infinitesimals. For curved surfaces, the curvature is measured similarly by the amount of departure from a plane, which has zero curvature. This may be generalized and made a little more precise as follows. For simplicity we state first the situation for a two-dimensional space, namely for a surface as we ordinarily imagine surfaces. It is possible from the formula

$$g_{11}x_1'^2 + g_{12}x_1'^2x_2' + g_{22}x_2'^2,$$

expressing (as before) the square of the distance between neighbouring points on a given surface (determined when the functions g_{11}, g_{12}, g_{22} are given), to calculate the measure of curvature of any point of the surface *wholly in terms of the given functions* g_{11}, g_{12}, g_{22}. Now, in ordinary language, to speak of the 'curvature' of a space of more than *two* dimensions is to make a meaningless noise. Nevertheless Riemann, generalizing Gauss, proceeded in the same *mathematical* way to build up an expression involving *all* the g's in the general case of an n-dimensional space, which is of *the same kind mathematically* as the Gaussian expression for the curvature of a *surface*, and this generalized expression is what he called the *measure of curvature* of the space. It is possible to exhibit visual representations of a curved space of more than two dimensions, but such aids to perception are about as useful as a pair of broken crutches to a man with no feet, for they add nothing to the understanding and they are mathematically useless.

Why did Riemann do all this and what has come out of it? Not attempting to answer the first, except to suggest that Riemann did what he did because his daemon drove him, we may briefly enumerate some of the gains that have accrued from Riemann's revolution in geometrical thought. First, it put the creation of 'spaces' and 'geometries' in unlimited number for specific purposes – use in dynamics, or in pure geometry, or in physical science – within the capabilities of professional geometers, and it baled together huge masses of important geometrical theorems into compact bundles that could be handled easily as wholes. Second, it clarified our conception of space, at least so far as mathematicians deal in 'space', and stripped that mystic nonentity Space of its last shred of mystery. Riemann's achievement has taught mathematicians to disbelieve in *any* geometry, or in *any* space, as a *necessary* mode of human perception. It was the last nail in the coffin of absolute space, and the first in that of the 'absolutes' of nineteenth-century physics.

Finally, the curvature which Riemann defined, the processes which he devised for the investigation of quadratic differential forms (those giving the formula for the square of the distance between neighbouring points in a space of any number of dimensions), and his recognition of the fact that the curvature is an invariant (in the technical sense explained in previous chapters), all found their physical interpretations in the theory of relativity. Whether the latter is in its final form or not is beside the point; since relativity our outlook on physical science is not what it was before. Without the work of Riemann this revolution in scientific thought would have been impossible - unless some later man had created the concepts and the mathematical methods that Riemann created.

CHAPTER TWENTY-SEVEN

ARITHMETIC THE SECOND

Kummer; Dedekind

It is a curious fact that although arithmetic – the theory of numbers – has been the fertile mother of more profound problems and powerful methods than any other discipline of mathematics, it is usually regarded as standing rather to one side of the main progress as a more or less cold-blooded spectator of the flashier achievements of geometry and analysis, particularly in their services to physical science, and comparatively few of the great mathematicians of the past 2,000 years have expended their more serious efforts on the advancement of the science of 'pure number'.

Many causes have determined this strange neglect of what, after all, is mathematics par excellence. Among these we need note only the following: arithmetic at present is on a higher plane of intrinsic difficulty than the other great fields of mathematics; the immediate applications of the theory of numbers to science are few and not readily perceptible to the ordinary run of creative mathematicians, although some of the greatest have felt that the proper mathematics of nature will be found ultimately in the behaviour of the common whole numbers; and, finally, it is only human for mathematicians – at least for some, even the great – to court reputation and popularity in their own generation by reaping the easier harvests of a spectacular success in analysis, geometry, or applied mathematics. Even Gauss succumbed, to his keen regret in middle life.

Modern arithmetic – after Gauss – began with Kummer. The origin of Kummer's theory in his attempt to prove Fermat's Last Theorem has already been noted (Chapter 25). Something of the man's long life may be told before we pass to Dedekind. Kummer was a typical German of the old school with all the blunt simplicity, good nature, and racy humour, which characterized that fast-vanishing species at its best. Museum specimens, aged in the wood, could be found behind the bar in any San Francisco German beer garden a generation ago.

Although Ernst Eduard Kummer (29 January 1810–14 May 1893) was born only five years before the deflation of Napoleon, the glorious Emperor of the French played an important if unwitting part in Kummer's life. The son of a physician of Sorau (then in the principality of Brandenburg), Germany, Kummer at the age of three lost his father: the lousy remnant of Napoleon's Grand Army, filtering back through Germany to France, brought with it the characteristically Russian gift of typhus, which it shared freely with the well-washed Germans. The overworked physician caught the disease, died of it, and left Ernst and an elder brother to the care of his widow. Young Kummer grew up in cramping poverty, but his struggling mother contrived somehow or another to see her sons through the local Gymnasium. The arrogance and exactions of the Napoleonic French, no less than the memory of his father, which the mother kept alive, made young Kummer an extremely practical patriot, and it was with real gusto that he devoted much of his superb scientific talent in later life to training German army officers in ballistics at the war college of Berlin. Many of his students gave good accounts of themselves in the Franco-Prussian War.

At the age of eighteen (in 1828) Kummer was sent by his mother to the University of Halle to study theology and otherwise fit himself for a career in the church. Owing to his poverty Kummer did not reside at the University, but tramped back and forth every day from Sorau to Halle with his food and books in a knapsack on his back. Regarding his theological studies Kummer makes the interesting observation that it is more or less a matter of accident or environment whether a mind with a gift for abstract speculation turns to philosophy or to mathematics. The accident in his own case was the presence at Halle of Heinrich Ferdinand Scherk (1798–1885) as professor of mathematics. Scherk was rather old-fashioned, but he had an enthusiasm for algebra and the theory of numbers which he imparted to young Kummer. Under Scherk's guidance Kummer soon abandoned his moral and theological studies in favour of mathematics. Echoing Descartes, Kummer said he preferred mathematics to philosophy because 'mere errors and false views cannot enter mathematics.' Had Kummer lived till to-day he might have modified his statement, for he was a broadminded man, and the present philosophical tendencies in mathematics are sometimes curiously reminiscent of medieval theology. In his third year at the University Kummer solved a prize problem in mathematics and was awarded his Ph.D. degree (10 September 1831) at the age of twenty-one. No university position being open at the time, Kummer began his career as a teacher in his old Gymnasium.

In 1832 he moved to Liegnitz, where he taught for ten years in the Gymnasium. It was there that he started Kronecker off on his revolutionary career. Fortunately Kummer was not so hard up as Weierstrass under similar circumstances and was able to afford postage for scientific correspondence. The eminent mathematicians (including Jacobi) with whom Kummer shared his mathematical discoveries saw to it that the young genius of a school teacher was lifted into a more suitable position at the earliest opportunity, and in 1842 Kummer was appointed Professor of Mathematics at the University of Breslau. He taught there till 1855, when the death of Gauss caused extensive revisions in the mathematical map of Europe.

It had been assumed that Dirichlet was contented at Berlin, then the mathematical capital of the world. But when Gauss died, Dirichlet could not resist the temptation of succeeding the Prince of Mathematicians and his own former master as professor at Göttingen. Even to-day the glory of being a 'successor of Gauss' has an almost irresistible attraction for mathematicians who might easily earn more money in other positions, and until quite recently Göttingen could choose whom it would. The high esteem in which Kummer was held by his fellow mathematicians can be judged by the fact that he was the unanimous choice to succeed Dirichlet at Berlin. Since the age of twenty-nine he had been a corresponding member of the Royal Berlin Academy. He now (1855) succeeded Dirichlet in both the University and the Academy, and was also appointed professor at the Berlin War College.

Kummer was one of those rarest of all scientific geniuses who are first class in the most abstract mathematics, the applications of mathematics to practical affairs, including war, which is the most unblushingly practical of all human idiocies, and finally in the ability to do experimental physics of a high degree of excellence. His finest work was in the theory of numbers where his profound originality led him to inventions of the very first order of importance, but in other fields – analysis, geometry, and applied physics – he also did outstanding work. Although Kummer's advance in the higher arithmetic was of the pioneering sort that justifies comparing him with the creators of non-Euclidean geometry, we somehow get the impression, on reviewing his life of eighty-three years, that splendid as his achievement was, he did not accomplish all that he must have had in him. Possibly his lack of personal ambition (an instance is given presently), his easy-going geniality, and his broad sense of humour prevented him from winding himself in an attempt to beat the record.

The nature of what Kummer did in the theory of numbers has been described in the chapter on Kronecker: he *restored the fundamental theorem of arithmetic to those algebraic number fields which arise in the attempt to prove Fermat's Last Theorem and in the Gaussian theory of cyclotomy, and he effected this restoration by the creation of an entirely new species of numbers, his so-called 'ideal numbers'*. He also carried on the work of Gauss on the law of biquadratic reciprocity and sought the laws of reciprocity for degrees higher than the fourth.

As has already been mentioned in preceding chapters, Kummer's 'ideal numbers' are now largely displaced by Dedekind's 'ideals', which will be described when we come to them, so it is not necessary to attempt here the almost impossible feat of explaining in untechnical language what Kummer's 'numbers' are. But what he accomplished by means of them can be stated with sufficient accuracy for an account like the present: Kummer *proved* that $x^p + y^p = z^p$, where p is a prime, is impossible in integers x,y,z, all different from zero, for a whole very extensive class of primes p. He did not succeed in proving Fermat's theorem for *all* primes; certain slippery 'exceptional primes' eluded Kummer's net – and still do. Nevertheless the step ahead which he took so far surpassed everything that all his predecessors had done that Kummer became famous almost in spite of himself. He was awarded a prize for which he had not competed.

The report in full of the French Academy of Sciences on the competitition for its 'Grand Prize' in 1857 ran as follows. 'Report on the competition for the grand prize in mathematical sciences. Already set in the competition for 1853 and prorogued to 1856. The committee, having found no work which seemed to it worthy of the prize among those submitted to it in competition, proposed to the Academy to award it to M. Kummer, for his beautiful researches on complex numbers composed of roots of unity* and integers. The Academy adopted this proposal.'

Kummer's earliest work on Fermat's Last Theorem is dated October 1835. This was followed by further papers in 1844–47, the last of which was entitled *Proof of Fermat's Theorem on the Impossibility of* $x^p + y^p = z^p$ *for an Infinite*† *Number of Primes* p. He continued to add improvements to his theory, including its application to the laws of higher reciprocity, till 1874, when he was sixty-four years old.

Although these highly abstract researches were the field of his greatest interest, and although he said of himself, 'To

* If $x^p + y^p = z^p$, then $x^p = z^p - y^p$, and resolving $z^p - y^p$ into its p factors of the first degree, we get

$$x^p = (z-y)(z-ry)(z-r^2y)\dots(z-r^{p-1}y),$$

in which r is a 'pth root of unity' (other than 1), namely $r^p - 1 = 0$, with r not equal to 1. The algebraic integers in the field of degree p generated by r are those which Kummer introduced into the study of Fermat's equation, and which led him to the invention of his 'ideal numbers' to restore unique factorization in the field – an integer in such a field is not uniquely the product of primes in the field for *all* primes p.

† The 'infinite' in Kummer's title is still (1936) unjustified; 'many' should be put for 'infinite'.

describe my personal scientific attitude more exactly, I may conveniently designate it as *theoretical* . . . ; I have particularly striven for that mathematical knowledge which finds its proper sphere in mathematics without reference to applications.' Kummer was no narrow specialist. Somewhat like Gauss, he appeared to take equal pleasure in both pure and applied science. Gauss indeed, through his works, was Kummer's real teacher, and the apt pupil proved his mettle by extending his master's work on the hypergeometric series, adding to what Gauss had done substantial developments which to-day are of great use in the theory of those differential equations which recur most frequently in mathematical physics.

Again, the magnificent work of Hamilton on systems of rays (in optics) inspired Kummer to one of his own most beautiful inventions, that of the surface of the fourth degree which is known by his name and which plays a fundamental part in the geometry of Euclidean space when that space is four-dimensional (instead of three-dimensional, as we ordinarily imagine it), as happens when straight lines instead of points are taken as the irreducible elements out of which the space is constructed. This surface (and its generalizations to higher spaces) occupied the centre of the stage in a whole department of nineteenth-century geometry; it was found (by Cayley) to be representable (parametrically – see the chapter on Gauss) by means of the quadruple periodic functions to which Jacobi and Hermite devoted some of their best efforts.

Quite recently (since 1934) it has been observed by Sir Arthur Eddington that Kummer's surface is a sort of cousin to Dirac's wave equation in quantum mechanics (both have the same finite group; Kummer's surface is the wave surface in space of four dimensions).

To complete the circle, Kummer was led back by his study of systems of rays to physics, and he made important contributions to the theory of atmospheric refraction. In his work at the War College he astonished the scientific world by proving himself a first-rate experimenter in his work on ballistics. With characteristic humour Kummer excused himself for this bad fall from mathematical grace: 'When *I* attack a problem experimentally,' he told a young friend, 'it is a proof that the problem is mathematically impregnable.'

Remembering his own struggles to get an education and his mother's sacrifices, Kummer was not only a father to his students but something of a brother to their parents. Thousands of grateful young men who had been helped on their way by Kummer at the University of Berlin or the War College remembered him all their lives as a great teacher and a great friend. Once a needy young mathematician about to come up for his doctor's examination was stricken with smallpox and had to return to his home in Posen near the Russian border. No word came from him, but it was known that he was desperately poor. When Kummer heard that the young man was probably unable to afford proper care, he sought out a friend of the student, gave him the requisite money and sent him off to Posen to see that what was necessary was done. In his teaching Kummer was famous for his homely similes and philosophical asides. Thus, to drive home the importance of a particular factor in a certain expression, he observed that 'If you neglect this factor you will be like a man who in eating a plum swallows the stone and spits out the pulp.'

The last nine years of Kummer's life were spent in complete retirement. 'Nothing will be found in my posthumous papers,' he said, thinking of the mass of work which Gauss left to be edited after his death. Surrounded by his family (nine children survived him), Kummer gave up mathematics for good when he retired, and except for occasional trips to the scenes of his boyhood lived in the strictest seclusion. He died after a short attack of influenza on 14 May 1893, aged eighty-three.

Kummer's successor in arithmetic was Julius Wilhelm Richard Dedekind (he dropped the first two names when he grew up), one of the greatest mathematicians and one of the most original Germany – or any other country – has produced. Like Kummer, Dedekind had a long life (6 October 1831–12 February 1916), and he remained mathematically active to within a short time of his death. When he died in 1916 Dedekind had been a mathematical classic for well over a generation. As Edmund Landau (himself a friend and follower of Dedekind in some of his work) said in his commemorative address to the Royal Society of Göttingen in 1917: 'Richard Dedekind was not only a great mathematician, but one of the wholly great in the history of mathematics, now and in the past, the last hero of a great epoch, the last pupil of Gauss, for four decades himself a classic, from whose works not only we, but our teachers and the teachers of our teachers, have drawn.'

Richard Dedekind, the youngest of the four children of Julius Levin Ulrich Dedekind, a professor of law, was born in Brunswick, the natal place of Gauss.* From the age of seven to sixteen Richard studied at the Gymnasium in his home town. He gave no early evidence of unmistakable mathematical genius; in fact his first loves were physics and chemistry, and he looked upon mathematics as the handmaiden – or scullery slut – of the sciences. But he did not wander long in darkness. By the age of seventeen he had smelt numerous rats in the alleged reasoning of physics and had turned to mathematics for less objectionable logic. In 1848 he entered the Caroline College – the same institution that gave the youthful Gauss an opportunity for self-instruction in mathematics. At the college Dedekind mastered the elements of analytic geometry, 'advanced' algebra, the calculus, and 'higher' mechanics. Thus he was well prepared to begin serious work when he entered the University of Göttingen in 1850 at the age of nineteen. His principal instructors were Moritz Abraham Stern (1807–94), who wrote extensively on the theory of numbers, Gauss, and Wilhelm Weber the physicist.

* No adequate biography of Dedekind has yet appeared. A life was to have been included in the third volume of his collected works (1932), but was not, owing to the death of the editor in chief (Robert Fricke). The account here is based on Landau's commemorative address. Note that, following the good old Teutonic custom of some German biographers, Landau omits all mention of Dedekind's mother. This no doubt is in accordance with the theory of the 'three K's' propounded by the late Kaiser of Germany and heartily endorsed by Adolf Hitler: 'A woman's whole duty is comprised in the three big K's – Kissing, Kooking [kooking is spelt with a K in German], and Kids.' Still, one would like to know at least the maiden name of a great man's mother.

From these three men Dedekind got a thorough grounding in the calculus, the elements of the higher arithmetic, least squares, higher geodesy, and experimental physics.

In later life Dedekind regretted that the mathematical instruction available during his student years at Göttingen, while adequate for the rather low requirements for a state teacher's certificate, was inconsiderable as a preparation for a mathematical career. Subjects of living interest were not touched upon, and Dedekind had to spend two years of hard labour after taking his degree to get up by himself elliptic functions, modern geometry, higher algebra, and mathematical physics – all of which at the time were being brilliantly expounded at Berlin by Jacobi, Steiner, and Dirichlet. In 1852 Dedekind got his doctor's degree (at the age of twenty-one) from Gauss for a short dissertation on Eulerian integrals. There is no need to explain what this was: the dissertation was a useful, independent piece of work, but it betrayed no such genius as is evident on every page of Dedekind's later works. Gauss' verdict on the dissertation will be of interest: 'The memoir prepared by Herr Dedekind is concerned with a research in the integral calculus, which is by no means commonplace. The author evinces not only a very good knowledge of the relevant field, but also such an independence as augurs favourably for his future achievement. As a test essay for admission to the examination I find the memoir completely satisfying.' Gauss evidently saw more in the dissertation than some later critics have detected; possibly his close contact with the young author enabled him to read between the lines. However, the report, even as it stands, is more or less the usual perfunctory politeness customary in accepting a passable dissertation, and we do not know whether Gauss really foresaw Dedekind's penetrating originality.

In 1854 Dedekind was appointed lecturer (*Privatdozent*) at Göttingen, a position which he held for four years. On the death of Gauss in 1855 Dirichlet moved from Berlin to Göttingen. For the remaining three years of his stay at Göttingen, Dedekind attended Dirichlet's most important lectures. Later he was to edit Dirichlet's famous treatise on the theory of numbers and add to it the epoch-making 'Eleventh Supplement' containing an outline of his own theory of algebraic numbers. He also became a friend of the great Riemann, then beginning his career. Dedekind's university lectures were for the most part elementary, but in 1857–8 he gave a course (to two students, Selling and Auwers) on the Galois theory of equations. This was probably the first time that the Galois theory had appeared formally in a university course. Dedekind was one of the first to appreciate the fundamental importance of the concept of a group in algebra and arithmetic. In this early work Dedekind already exhibited two of the leading characteristics of his later thought, abstractness and generality. Instead of regarding a finite group from the standpoint offered by its representation in terms of substitutions (see chapters on Galois and Cauchy), Dedekind defined groups by means of their postulates (substantially as described in Chapter 15) and sought to derive their properties from this distillation of their essence. This is in the modern manner: abstractness and therefore generality. The second characteristic, generality, is, as just implied, a consequence of the first.

At the age of twenty-six Dedekind was appointed (in 1857) ordinary professor at the Zurich polytechnic, where he stayed five years, returning in 1862 to Brunswick as professor at the technical high school. There he stuck for half a century. The most important task for Dedekind's official biographer – provided one is unearthed – will be to explain (not explain away) the singular fact that Dedekind occupied a relatively obscure position for fifty years while men who were not fit to lace his shoes filled important and influential university chairs. To say that Dedekind preferred obscurity is one explanation. Those who believe it should leave the stock market severely alone, for as surely as God made little lambs they will be fleeced.

Till his death (1916) in his eighty-fifth year Dedekind remained fresh of mind and robust of body. He never married, but lived with his sister Julie, remembered as a novelist, till her death in 1914. His other sister, Mathilde, died in 1860; his brother became a distinguished jurist.

Such are the bare facts of any importance in Dedekind's material career. He lived so long that although some of his work (his theory of irrational numbers, described presently) had been familiar to all students of analysis for a generation before his death, he himself had become almost a legend and many classed him with the shadowy dead. Twelve years before his death, Teubner's *Calendar for Mathematicians* listed Dedekind as having died on 4 September 1899, much to Dedekind's amusement. The day, 4 September, might possibly prove to be correct, he wrote to the editor, but the year certainly was wrong. 'According to my own memorandum I passed this day in perfect health and enjoyed a very stimulating conversation on "system and theory" with my luncheon guest and honoured friend Georg Cantor of Halle.'

Dedekind's mathematical activity impinged almost wholly on the domain of number in its widest sense. We have space for only two of his greatest achievements and we shall describe first his fundamental contribution, that of the 'Dedekind cut', to the theory of irrational numbers and hence to the foundations of analysis. This being of the very first importance we may recall briefly the nature of the matter. If a, b are common whole numbers, the fraction a/b is called a rational number; if no whole numbers m, n exist such that a certain 'number' N is expressible as m/n, then N is called an irrational number. Thus $\sqrt{2}$, $\sqrt{3}$, $\sqrt{6}$ are irrational numbers. If an irrational number be expressed in the decimal notation the digits following the decimal point exhibit no regularities – there is no 'period' which repeats, as in the decimal representations of a rational number, say $13/11, = 1{\cdot}181818 \ldots$, where the '18' repeats indefinitely. How then, if the representation is entirely lawless, are decimals equivalent to irrationals to be defined, let alone manipulated? Have we even any clear conception of what an irrational number is? Eudoxus thought he had, and Dedekind's definition of equality between numbers, rational or irrational, is identical with that of Eudoxus (see Chapter 2).

If two rational numbers are equal, it is no doubt obvious that their square roots are equal. Thus 2×3 and 6 are equal; so also then are $\sqrt{2 \times 3}$ and $\sqrt{6}$. But it is *not* obvious that $\sqrt{2} \times \sqrt{3} = \sqrt{2 \times 3}$, and hence that $\sqrt{2} \times \sqrt{3} = \sqrt{6}$. The unobviousness of this simple assumed equality, $\sqrt{2} \times \sqrt{3} = \sqrt{6}$, taken for granted in school arithmetic, is evident if we visualize what the equality implies: the 'lawless' square roots of 2, 3, 6 are to be extracted, the first two of these are then to be multiplied together, and the result is to come out equal to the third. As not one of these three roots can be extracted exactly, no matter to how many decimal places the computation is carried, it is clear that the verification by multiplication as just described will never be complete. The whole human race toiling incessantly through all its existence could never *prove* in this way that $\sqrt{2} \times \sqrt{3} = \sqrt{6}$. Closer and closer approximations to equality would be attained as time went on, but finality would continue to recede. To make these concepts of 'approximation' and 'equality' precise, or to replace our first crude conceptions of irrationals by sharper descriptions which will obviate the difficulties indicated, was the task Dedekind set himself in the early 1870's – his work on *Continuity and Irrational Numbers* was published in 1872.

The heart of Dedekind's theory of *irrational* numbers is his concept of the 'cut' or 'section' (*Schnitt*): a cut separates *all* rational numbers into *two* classes, so that each number in the *first* class is *less than* each number in the *second* class; every such cut which does not 'correspond' to a rational number 'defines' an irrational number. This bald statement needs elaboration, particularly as even an accurate exposition conceals certain subtle difficulties rooted in the theory of the mathematical infinite, which will reappear when we consider the life of Dedekind's friend Cantor.

Assume that some rule has been prescribed which separates *all* rational numbers into *two* classes, say an 'upper' class and a 'lower' class, such that each number in the *lower* class is *less than* every number in the *upper* class. (Such an assumption would not pass unchallenged to-day by all schools of mathematical philosophy. However, for the moment, it may be regarded as unobjectionable.) On this assumption one of three mutually exclusive situations is possible.

(A) There may be a number in the *lower* class which is *greater* than every other number in that class.

(B) There may be a number in the *upper* class which is *less* than every other number in that class.

(C) *Neither* of the numbers (*greatest* in [A], *least* in [B]) described in (A), (B) may exist.

The possibility which leads to irrational numbers is (C). For if (C) holds, the assumed rule 'defines' a definite break or 'cut' in the set of all rational numbers. The upper and lower classes strive, as it were, to meet. But in order for the classes to meet the cut must be filled with some 'number', and, by (C), no such filling is possible.

Here we appeal to intuition. All the distances measured from any fixed point along a given straight line 'correspond' to 'numbers' which 'measure' the distances. If the cut is to be left unfilled, we must picture the straight line, which we may conceive of as having been traced out by the *continuous* motion of a point, as now having an unbridgeable gap in it. This violates our intuitive notions, so we say, by definition, that each cut *does define* a number. The number thus defined is not rational, namely it is irrational. To provide a manageable scheme for operating with the *irrationals* thus *defined by cuts* (of the kind [C]) we now consider the *lower class of rationals* in (C) as being equivalent to the irrational which the cut defines.

One example will suffice. The *irrational* square root of 2 is defined by the cut whose upper class contains *all* the positive rational numbers whose squares are greater than 2, and whose lower class contains *all* other *rational* numbers.

If the somewhat elusive concept of cuts is distasteful two remedies may be suggested: devise a definition of irrationals which is less mystical than Dedekind's and fully as usable; follow Kronecker and, denying that irrational numbers exist, reconstruct mathematics without them. In the present state of mathematics some theory of irrationals is convenient. But, from the very nature of an irrational number, it would seem to be necessary to understand the mathematical infinite thoroughly before an adequate theory of irrationals is possible. The appeal to infinite classes is obvious in Dedekind's definition of a cut. Such classes lead to serious logical difficulties.

It depends upon the individual mathematician's level of sophistication whether he regards these difficulties as relevant or of no consequence for the consistent development of mathematics. The courageous analyst goes boldly ahead, piling one Babel on top of another and trusting that no outraged god of reason will confound him and all his works, while the critical logician, peering cynically at the foundations of his brother's imposing skyscraper, makes a rapid mental calculation predicting the date of collapse. In the meantime all are busy and all seem to be enjoying themselves. But one conclusion appears to be inescapable: without a consistent theory of the mathematical infinite there is no theory of irrationals; without a theory of irrationals there is no mathematical analysis in any form even remotely resembling what we now have; and finally, without analysis the major part of mathematics – including geometry and most of applied mathematics – as it now exists would cease to exist.

The most important task confronting mathematicians would therefore seem to be the construction of a satisfactory theory of the infinite. Cantor attempted this, with what success will be seen later. As for the Dedekind theory of irrationals, its author seems to have had some qualms, for he hesitated over two years before venturing to publish it. If the reader will glance back at Eudoxus' definition of 'same ratio' (Chapter 2) he will see that 'infinite difficulties' occur there too, specifically in the phrase 'any whatever equimultiples'. Nevertheless some progress has been made since Eudoxus wrote; we are at least beginning to understand the nature of our difficulties.

The other outstanding contribution which Dedekind made to the concept of 'number' was in the direction of algebraic numbers. For the nature of the fundamental problem concerned we must refer to what was said in the chapter on Kronecker concerning algebraic number fields and the resolution of algebraic *integers* into their *prime* factors. The crux of the matter is that in *some* such fields resolution into prime factors is *not unique* as it is in common arithmetic; Dedekind restored this highly desirable uniqueness by the invention of what he called *ideals*. An ideal is not a number, but an infinite class of numbers, so again Dedekind overcame his difficulties by taking refuge in the infinite.

The concept of an ideal is not hard to grasp, although there is one twist – *the more inclusive class divides the less inclusive*, as will be explained in a moment – which shocks common sense. However, common sense was made to be shocked; had we nothing less dentable than shock-proof common sense we should be a race of mongoloid imbeciles. An ideal must do at least two things: it must leave common (rational) arithmetic substantially as it is, and it must force the recalcitrant algebraic integers to obey that fundamental law of arithmetic – *unique* decomposition into primes – which they defy.

The point about a more inclusive class dividing a less inclusive refers to the following phenomenon (and its generalization, as stated presently). Consider the fact that 2 divides 4 – *arithmetically*, that is, *without remainder*. Instead of this obvious fact, which leads nowhere if followed into algebraic number fields, we replace 2 by the *class* of *all* its integer multiples, . . . , − 8, − 6, − 4, − 2, 0, 2, 4, 6, 8, . . . As a matter of convenience we denote this class by (2). In the same way (4) denotes the class of *all* integer multiples of 4. Some of the numbers in (4) are . . . , − 16, −12, − 8, − 4, 0, 8, 12, 16, . . . It is now obvious that (2) is the more inclusive class; in fact (2) contains *all* the numbers in (4) and in addition (to mention only two) − 6 and 6. The fact that (2) contains (4) is symbolized by writing $(2)|(4)$. It can be seen quite easily that if m, n are any common whole numbers then $(m) \mid (n)$ *when, and only when*, m *divides* n.

This might suggest that the notion of common arithmetical divisibility be replaced by that of class inclusion as just described. But this replacement would be futile if it failed to preserve the characteristic properties of arithmetical divisibility. That it does so preserve them can be seen in detail, but one instance must suffice. If m divides n, and n divides l, then m divides l – for example, 12 divides 24 and 24 divides 72, and 12 does in fact divide 72. Transferred to classes, as above, this becomes: if $(m) \mid (n)$ and $(n) \mid (l)$, then $(m) \mid (l)$ or, in English, if the class (m) contains the class (n), and if the class (n) contains the class (l), then the class (m) contains the class (l) – which obviously is true. The upshot is that the replacement of numbers by their corresponding classes does what is required when we add the definition of 'multiplication': $(m) \times (n)$ is defined to be the class (mn); $(2) \times (6) = (12)$. Notice that the last is a definition; it is not meant to follow from the meanings of (m) and (n).

Dedekind's ideals for algebraic numbers are a generalization of what precedes. Following his usual custom Dedekind gave an *abstract* definition, that is, a definition based upon essential properties rather than one contingent upon some particular mode of representing, or picturing, the thing defined.

Consider the set (or class) of *all* algebraic *integers* in a given algebraic number field. In this all-inclusive set will be subsets. A subset is called an *ideal* if it has the two following properties.

A. The *sum* and *difference* of any two integers in the subset are also in the subset.

B. If any integer in the subset be multiplied by any integer in the all-inclusive set, the resulting integer is in the subset.

An ideal is thus an infinite *class* of integers. It will be seen readily that (m), (n), . . . , previously defined, are ideals according to A, B. As before, if one ideal contains another, the first is said to divide the second.

It can be proved that every ideal is a class of integers all of which are of the form

$$x_1a_1 + x_2a_2 + \dots + x_na_n,$$

where $a_1, a_2, \dots, a_n$ are *fixed* integers of the field of degree n concerned, and each of $x_1, x_2, \dots, x_n$ may be any integer whatever in the field. This being so, it is convenient to symbolize an ideal by exhibiting only the fixed integers, $a_1, a_2, \dots, a_n$, and this is done by writing $(a_1, a_2, \dots, a_n)$ as the symbol of the ideal. The order in which $a_1, a_2, \dots, a_n$ are written in the symbol is immaterial.

'Multiplication' of ideals must now be defined: the *product* of the two ideals $(a_1, \dots, a_n)$, $(b_1, \dots, b_n)$ is the ideal whose symbol is $(a_1b_1, \dots, a_1b_n, \dots, a_nb_n)$, in which all possible products, a_1b_1, etc., obtained by multiplying an integer in the first symbol by an integer in the second occur. For example, the product of (a_1, a_2) and (b_1, b_2) is $(a_1b_1, a_1b_2, a_2b_1, a_2b_2)$. It is always possible to reduce any such product-symbol (for a field of degree n) to a symbol containing at most n integers.

One final short remark completes the synopsis of the story. An ideal whose symbol contains *but one* integer, such as (a_1), is called a *principal* ideal. Using as before the notation $(a_1) \mid (b_1)$ to signify that (a_1) *contains* (b_1), we can see without difficulty that $(a_1) \mid (b_1)$ *when*, and *only when*, the integer a_1 *divides* the integer b_1. As before, then, the concept of arithmetical divisibility is here – for algebraic integers – completely equivalent to that of class inclusion. A *prime* ideal is one which is not 'divisible by' – included in – any ideal except the all-inclusive ideal which consists of *all* the algebraic integers in the given field. Algebraic integers being now replaced by their corresponding principal ideals, it is proved that a given ideal is a product of prime ideals in one way only, precisely as in the 'fundamental theorem of arithmetic' a rational integer is the product of primes in one way only. By the above equivalence of arithmetical divisibility for algebraic integers and class inclusion, the fundamental theorem of arithmetic has been restored to integers in algebraic number fields.

Anyone who will ponder a little on the foregoing bare outline of Dedekind's creation will see that what he did demanded penetrating insight and a mind gifted far above the ordinary good mathematical mind in the power of abstraction. Dedekind was a mathematician after Gauss' own heart: '*At nostro quidem judicio hujusmodi veritates ex notionibus potius quam ex notationibus hauriri debeant*' (But in our opinion such truths [arithmetical] should be derived from notions rather than from notations). Dedekind always relied on his head rather than on an ingenious symbolism and expert manipulations of formulae to get him forward. If ever a man put notions into mathematics, Dedekind did, and the wisdom of his preference for creative ideas over sterile symbols is now apparent although it may not have been during his lifetime. The longer mathematics lives the more abstract – and therefore, possibly, also the more practical – it becomes.

CHAPTER TWENTY-EIGHT

THE LAST UNIVERSALIST

Poincaré

In the *History of his Life and Times* the astrologer William Lilly (1602–81) records an amusing – if incredible – account of the meeting between John Napier (1550–1617), of Merchiston, the inventor of logarithms, and Henry Briggs (1561–1631) of Gresham College, London, who computed the first table of common logarithms. One John Marr, 'an excellent mathematician and geometrician', had gone 'into Scotland before Mr Briggs, purposely to be there when these two so learned persons should meet. Mr Briggs appoints a certain day when to meet in Edinburgh; but failing thereof, the lord Napier was doubtful he would not come. It happened one day as John Marr and the lord Napier were speaking of Mr Briggs: "Ah John (said Merchiston), Mr Briggs will not now come." At the very moment one knocks at the gate; John Marr hastens down, and it proved Mr Briggs to his great contentment. He brings Mr Briggs up into my lord's chamber, where almost *one quarter of an hour was spent*, each beholding other with admiration, *before one word was spoke*.'

Recalling this legend Sylvester tells how he himself went after Briggs' world record for flabbergasted admiration when, in 1885, he called on the author of numerous astonishingly mature and marvellously original papers on a new branch of analysis which had been swamping the editors of mathematical journals since the early 1880's.

'I quite entered into Briggs' feelings at his interview with Napier', Sylvester confesses, 'when I recently paid a visit to Poincaré [1854–1912] in his airy perch in the Rue Gay-Lussac. ... In the presence of that mighty reservoir of pent-up intellectual force my tongue at first refused its office, and it was not until I had taken some time (it may be two or three minutes) to peruse and absorb as it were the idea of his external youthful lineaments that I found myself in a condition to speak.'

Elsewhere Sylvester records his bewilderment when, after having toiled up the three flights of narrow stairs leading to Poincaré's 'airy perch', he paused, mopping his magnificent bald head, in astonishment at beholding a mere boy, 'so blond, so young', as the author of the deluge of papers which had heralded the advent of a successor to Cauchy.

A second anecdote may give some idea of the respect in which Poincaré's work is held by those in a position to appreciate its scope. Asked by some patriotic British brass hat in the rabidly nationalistic days of World War I – when it was obligatory on all academic patriots to exalt their aesthetic allies and debase their boorish enemies – who was the greatest man France had produced in modern times, Bertrand Russell answered instantly, 'Poincaré.' 'What! *That* man?' his uninformed interlocutor exclaimed, believing Russell meant Raymond Poincaré, President of the French Republic. 'Oh,' Russell explained when he understood the other's dismay, 'I was thinking of Raymond's cousin, *Henri* Poincaré.'

Poincaré was the last man to take practically all mathematics, both pure and applied, as his province. It is generally believed that it would be impossible for any human being starting to-day to understand comprehensively, much less do creative work of high quality in more than two of the four main divisions of mathematics – arithmetic, algebra, geometry, analysis, to say nothing of astronomy and mathematical physics. However, even in the 1880's, when Poincaré's great career opened, it was commonly thought that Gauss was the last of the mathematical universalists, so it may not prove impossible for some future Poincaré once more to cover the entire field.

As mathematics evolves it both expands and contracts, somewhat like one of Lemaître's models of the universe. At present the phase is one of explosive expansion, and it is quite impossible for any man to familiarize himself with the entire inchoate mass of mathematics that has been dumped on the world since the year 1900. But already in certain important sectors a most welcome tendency towards contraction is plainly apparent. This is so, for example, in algebra, where the wholesale introduction of postulational methods is making the subject at once more abstract, more general, and less disconnected. Unexpected similarities – in some instances amounting to disguised identity – are being disclosed by the modern attack, and it is conceivable that the next generation of algebraists will not need to know much that is now considered valuable, as many of these particular, difficult things will have been subsumed under simpler general principles of wider scope. Something of this sort happened in classical mathematical physics when relativity put the complicated mathematics of the ether on the shelf.

Another example of this contraction in the midst of expansion is the rapidly growing use of the tensor calculus in preference to that of numerous special brands of vector analysis. Such generalizations and condensations are often hard for older men to grasp at first and frequently have a severe struggle to survive, but in the end it is usually realized that general methods are essentially simpler and easier to handle than miscellaneous collections of ingenious tricks devised for special problems. When mathematicians assert that such a thing as the tensor calculus is easy – at least in comparison with some of the algorithms that preceded it – they are not trying to appear superior or mysterious but are stating a valuable truth which any student can verify for himself. This quality of inclusive generality was a distinguishing trait of Poincaré's vast output.

If abstractness and generality have obvious advantages of the kind indicated, it is also true that they sometimes have serious drawbacks for those who must be interested in details. Of what immediate use is it to a working physicist to know that a particular differential equation occurring in his work is solvable, because some pure mathematician has proved that it is, when neither he nor the mathematician can perform the Herculean labour demanded by a numerical solution capable of application to specific problems?

To take an example from a field in which Poincaré did some of his most original work, consider a homogeneous, incompressible fluid mass held together by the gravitation of its particles and rotating about an axis. Under what conditions will the motion be stable and what will be the possible shapes of such a stably rotating fluid? MacLaurin, Jacobi, and others proved that certain ellipsoids will be stable; Poincaré, using more intuitive, 'less arithmetical' methods than his predecessors, once thought he had determined the criteria for the stability of a pear-shaped body. But he had made a slip. His methods were not adapted to numerical computation and later workers, including G. H. Darwin, son of the famous Charles, undeterred by the horrific jungles of algebra and arithmetic that must be cleared out of the way before a definite conclusion can be reached, undertook a decisive solution.*

The man interested in the evolution of binary stars is more comfortable if the findings of the mathematicians are presented to him in a form to which he can apply a calculating machine. And since Kronecker's fiat of 'no construction, no existence', some pure mathematicians themselves have been less enthusiastic than they were in Poincaré's day for existence theorems which are not constructive. Poincaré's scorn for the kind of detail that users of mathematics demand and must have before they can get on with their work was one of the most important contributory causes to his universality. Another was his extraordinarily comprehensive grasp of all the machinery of the theory of functions of a complex variable. In this he had no equal. And it may be noted that Poincaré turned his universality to magnificent use in disclosing hitherto unsuspected connexions between distant branches of mathematics, for example between (continuous) groups and linear algebra.

* This famous question of the 'piriform body', of considerable importance in cosmogony, was apparently settled in 1905 by Liapounoff, whose conclusion was confirmed in 1915 by Sir James Jeans: they found that the motion is unstable. Few have had the courage to check the calculations. After 1915 Leon Lichtenstein, a fellow-countryman of Liapounoff, made a general attack on the problem of rotating fluid masses. The problem seems to be unlucky; both L's had violent deaths.

One more characteristic of Poincaré's outlook must be recalled for completeness before we go on to his life: few mathematicians have had the breadth of philosophical vision that Poincaré had, and none is his superior in the gift of clear exposition. Probably he had always been deeply interested in the philosophical implications of science and mathematics, but it was only in 1902, when his greatness as a technical mathematician was established beyond all cavil, that he turned as a side-interest to what may be called the popular appeal of mathematics and let himself go in a sincere enthusiasm to share with non-professionals the meaning and human importance of his subject. Here his liking for the general in preference to the particular aided him in telling intelligent outsiders what is of more than technical importance in mathematics without talking down to his audience. Twenty or thirty years ago workmen and shop-girls could be seen in the parks and cafés of Paris avidly reading one or other of Poincaré's popular masterpieces in its cheap print and shabby paper cover. The same works in a richer format could also be found – well thumbed and evidently read – on the tables of the professedly cultured. These books were translated into English, German, Spanish, Hungarian, Swedish, and Japanese. Poincaré spoke the universal languages of mathematics and science to all in accents which they recognized. His style, peculiarly his own, loses much by translation.

For the literary excellence of his popular writings Poincaré was accorded the highest honour a French writer can get, membership in the literary section of the Institut. It has been somewhat spitefully said by envious novelists that Poincaré achieved this distinction, unique for a man of science, because one of the functions of the (literary) Academy is the constant compilation of a definitive dictionary of the French language, and the universal Poincaré was obviously the man to help out the poets and grammarians in their struggle to tell the world what automorphic functions are. Impartial opinion, based on a study of Poincaré's writings, agrees that the mathematician deserved no less than he got.

Closely allied to his interest in the philosophy of mathematics was Poincaré's preoccupation with the psychology of mathematical creation. How do mathematicians make their discoveries? Poincaré will tell us later his own observations on this mystery in one of the most interesting narratives of personal discovery that was ever written. The upshot seems to be that mathematical discoveries more or less make themselves after a long spell of hard labour on the part of the mathematician. As in literature – according to Dante Gabriel Rossetti – 'a certain amount of fundamental brainwork' is necessary before a poem can mature, so in mathematics there is no discovery without preliminary drudgery, but this is by no means the whole story. All 'explanations' of creativeness that fail to provide a recipe whereby a gifted human being can create are open to suspicion. Poincaré's excursion into practical psychology, like some others in the same direction, failed to bring back the Golden Fleece, but it did at least suggest that such a thing is not wholly mythical and may some day be found when human beings grow intelligent enough to understand their own bodies.

Poincaré's intellectual heredity on both sides was good. We shall not go farther back than his paternal grandfather. During the Napoleonic campaign of 1814 this grandfather, at the early age of twenty, was attached to the military hospital at Saint-Quentin. On settling in 1817 at Rouen he married and had two sons: Léon Poincaré, born in 1828, who became a first-rate physician and a member of a medical faculty; and Antoine, who rose to the inspector-generalship of the department of roads and bridges. Léon's son Henri, born on 29 April 1854, at Nancy, Lorraine, became the leading mathematician of the early twentieth century; one of Antoine's two sons, Raymond, went in for law and rose to the presidency of the French Republic during World War I; Antoine's other son became director of secondary education. A great-uncle who had followed Napoleon into Russia disappeared and was never heard of after the Moscow fiasco.

From this distinguished list it might be thought that Henri would have exhibited some administrative ability, but he did not, except in his early childhood when he freely invented political games for his sister and young friends to play. In these games he was always fair and scrupulously just, seeing that each of his playmates got his or her full share of office-holding. This perhaps is conclusive evidence that 'the child is father to the man' and that Poincaré was constitutionally incapable of understanding the simplest principle of administration, which his cousin Raymond applied intuitively.

Poincaré's biography was written in great detail by his fellow countryman Gaston Darboux (1842–1917), one of the leading geometers of modern times, in 1913 (the year following Poincaré's death). Something may have escaped the present writer, but it seems that Darboux, after having stated that Poincaré's mother 'coming from a family in the Meuse district whose [the mother's] parents lived in Arrancy, was a very good person, very active and very intelligent', blandly omits to mention her maiden name. Can it be possible that the French took over the doctrine of 'the three big K's' – noted in connexion with Dedekind – from their late instructors after the kultural drives of Germany into France in 1870 and 1914? However, it can be deduced from an anecdote told later by Darboux that the family name *may* have been Lannois. We learn that the mother devoted her entire attention to the education of her two young children, Henri and his younger sister (name not mentioned). The sister was to become the wife of Émile Boutroux and the mother of a mathematician (who died young).

Owing partly to his mother's constant care, Poincaré's mental development as a child was extremely rapid. He learned to talk very early, but also very badly at first because he thought more rapidly than he could get the words out. From infancy his motor co-ordination was poor. When he learned to write it was discovered that he was ambidextrous and that he could write or draw as badly with his left hand as with his right. Poincaré never outgrew this physical awkwardness. As an item of some interest in this connexion it may be recalled that when Poincaré was acknowledged as the foremost mathematician and leading popularizer of science of his time he submitted to the Binet tests and made such a disgraceful showing that, had he been judged as a child instead of as the famous mathematician he was, he would have been rated – by the tests – as an imbecile.

At the age of five Henri suffered a bad setback from diphtheria which left him for nine months with a paralyzed larynx. This misfortune made him for long delicate and timid, but it also turned him back on his own resources as he was forced to shun the rougher games of children his own age.

His principal diversion was reading, where his unusual talents first showed up. A book once read – at incredible speed – became a permanent possession, and he could always state the page and line where a particular thing occurred. He retained this powerful memory all his life. This rare faculty, which Poincaré shared with Euler who had it in a lesser degree, might be called visual or spatial memory. In temporal memory – the ability to recall with uncanny precision a sequence of events long passed – he was also unusually strong. Yet he unblushingly describes his memory as 'bad'. His poor eyesight perhaps contributed to a third peculiarity of his memory. The majority of mathematicians appear to remember theorems and formulae mostly by eye; with Poincaré it was almost wholly by ear. Unable to see the board distinctly when he became a student of advanced mathematics, he sat back and listened, following and remembering perfectly without taking notes – an easy feat for him, but one incomprehensible to most mathematicians. Yet he must have had a vivid memory of the 'inner eye' as well, for much of his work, like a good deal of Riemann's, was of the kind that goes with facile space-intuition and acute visualization. His inability to use his fingers skilfully of course handicapped him in laboratory exercises, which seems a pity, as some of his own work in mathematical physics might have been closer to reality had he mastered the art of experiment. Had Poincaré been as strong in practical science as he was in theoretical he might have made a fourth with the incomparable three, Archimedes, Newton, and Gauss.

Not many of the great mathematicians have been the absent-minded dreamers that popular fancy likes to picture them. Poincaré was one of the exceptions, and then only in comparative trifles, such as carrying off hotel linen in his baggage. But many persons who are anything but absent-minded do the same, and some of the most alert mortals living have even been known to slip restaurant silver into their pockets and get away with it.

One phase of Poincaré's absent-mindedness resembles something quite different. Thus (Darboux does not tell the story, but it should be told, as it illustrates a certain brusqueness of Poincaré's later years), when a distinguished mathematician had come all the way from Finland to Paris to confer with Poincaré on scientific matters, Poincaré did not leave his study to greet his caller when the maid notified him, but continued to pace back and forth – as was his custom when mathematicizing – for three solid hours. All this time the diffident caller sat quietly in the adjoining room, barred from the master only by flimsy portières. At last the drapes parted and Poincaré's buffalo head was thrust for an instant into the room. '*Vous me dérangez beaucoup*' (You are disturbing me greatly), the head exploded, and disappeared. The caller departed without an interview, which was exactly what the 'absent-minded' professor wanted.

Poincaré's elementary school career was brilliant, although he did not at first show any marked interest in mathematics. His earliest passion was for natural history, and all his life he remained a great lover of animals. The first time he tried out a rifle he accidentally shot a bird at which he had not aimed. This mishap affected him so deeply that thereafter nothing (except compulsory military drill) could induce him to touch firearms. At the age of nine he showed the first promise of what was to be one of his major successes. The teacher of French composition declared that a short exercise, original in both form and substance, which young Poincaré had handed in, was 'a little masterpiece', and kept it as one of his treasures. But he also advised his pupil to be more conventional – stupider – if he wished to make a good impression on the school examiners.

Being out of the more boisterous games of his schoolfellows, Poincaré invented his own. He also became an indefatigable dancer. As all his lessons came to him as easily as breathing he spent most of his time on amusements and helping his mother about the house. Even at this early stage of his career Poincaré exhibited some of the more suspicious features of his mature 'absentmindedness': he frequently forgot his meals and almost never remembered whether or not he had breakfasted. Perhaps he did not care to stuff himself as most boys do.

The passion for mathematics seized him at adolescence or shortly before (when he was about fifteen). From the first he exhibited a lifelong peculiarity: his mathematics was done in his head as he paced restlessly about, and was committed to paper only when all had been thought through. Talking or other noise never disturbed him while he was working. In later life he wrote his mathematical memoirs at one dash without looking back to see what he had written and limiting himself to but a very few erasures as he wrote. Cayley also composed in this way, and probably Euler, too. Some of Poincaré's work shows the marks of hasty composition, and he said himself that he never finished a paper without regretting either its form or its substance. More than one man who has written well has felt the same. Poincaré's flair for classical studies, in which he excelled at school, taught him the importance of both form and substance.

The Franco-Prussian war broke over France in 1870 when Poincaré was sixteen. Although he was too young and too frail for active service, Poincaré nevertheless got his full share of the horrors, for Nancy, where he lived, was submerged by the full tide of the invasion, and the young boy accompanied his physician-father on his rounds of the ambulances. Later he went with his mother and sister, under terrible difficulties, to Arrancy to see what had happened to his maternal grandparents, in whose spacious country garden the happiest days of his childhood had been spent during the long school vacations. Arrancy lay near the battlefield of Saint-Privat. To reach the town the three had to pass 'in glacial cold' through burned and deserted villages. At last they reached their destination, only to find that the house had been thoroughly pillaged, 'not only of things of value but of things of no value', and in addition had been defiled in the bestial manner made familiar to the French by the 1914 sequel to 1870. The grandparents had been left nothing; their evening meal on the day they viewed the great purging was supplied by a poor woman who had refused to abandon the ruins of her cottage and who insisted on sharing her meagre supper with them.

Poincaré never forgot this, nor did he ever forget the long occupation of Nancy by the enemy. It was during the war that he mastered German. Unable to get any French news, and eager to learn what the Germans had to say of France and for themselves, Poincaré taught himself the language. What he had seen and what he learned from the official accounts of the invaders themselves made him a flaming patriot for life but, like Hermite, he never confused the mathematics of his country's enemies with their more practical activities. Cousin Raymond, on the other hand, could never say anything about *les Allemands* (the Germans) without an accompanying scream of hate. In the bookkeeping of hell which balances the hate of one patriot against that of another, Poincaré may be checked off against Kummer, Hermite against Gauss, thus producing that perfect zero implied in the scriptural contract 'an eye for an eye and a tooth for a tooth'.

Following the usual French custom Poincaré took the examinations for his first degrees (bachelor of letters, and of science) before specializing. These he passed in 1871 at the age of seventeen – after almost failing in mathematics! He had arrived late and flustered at the examination and had fallen down on the extremely simple proof of the formula giving the sum of a convergent geometrical progression. But his fame had preceded him. 'Any student other than Poincaré would have been plucked', the head examiner declared.

He next prepared for the entrance examinations to the School of Forestry, where he astonished his companions by capturing the first prize in mathematics without having bothered to take any lecture notes. His classmates had previously tested him out, believing him to be a trifler, by delegating a fourth-year student to quiz him on a mathematical difficulty which had seemed particularly tough. Without apparent thought, Poincaré gave the solution immediately and walked off, leaving his crestfallen baiters asking 'How does he do it?' Others were to ask the same question all through Poincaré's career. He never seemed to think when a mathematical difficulty was submitted to him by his colleagues: 'The reply came like an arrow'.

At the end of this year he passed first into the École Polytechnique. Several legends of his unique examination survive. One tells how a certain examiner, forewarned that young Poincaré was a mathematical genius, suspended the examination for three-quarters of an hour in order to devise 'a "nice" question' – a refined torture. But Poincaré got the better of him and the inquisitor 'congratulated the examinee warmly, telling him he had won the highest grade'. Poincaré's experiences with his tormentors would seem to indicate that French mathematical examiners have learned something since they ruined Galois and came within an ace of doing the like by Hermite.

At the Polytechnique Poincaré was distinguished for his brilliance in mathematics, his superb incompetence in all physical exercises, including gymnastics and military drill, and his utter inability to make drawings that resembled anything in heaven or earth. The last was more than a joke; his score of *zero* in the entrance examination in drawing had almost kept him out of the school. This had greatly embarrassed his examiners: '... a zero is eliminatory. In everything else [but drawing] he is absolutely without an equal. If he is admitted, it will be as first; but can he be admitted?' As Poincaré was admitted the good examiners probably put a decimal point before the zero and placed a 1 after it.

In spite of his ineptitude for physical exercises Poincaré was extremely popular with his classmates. At the end of the year they organized a public exhibition of his artistic masterpieces, carefully labelling them in Greek, 'this is a horse', and so on – not always accurately. But Poincaré's inability to draw also had its serious side when he came to geometry, and he lost first place, passing out of the school second in rank.

On leaving the Polytechnique in 1875 at the age of twenty-one Poincaré entered the School of Mines with the intention of becoming an engineer. His technical studies, although faithfully carried out, left him some leisure to do mathematics, and he showed what was in him by attacking a general problem in differential equations. Three years later he presented a thesis, on the same subject, but concerning a more difficult and yet more general question, to the Faculty of Sciences at Paris for the degree of doctor of mathematical sciences. 'At the first glance', says Darboux, who had been asked to examine the work, 'it was clear to me that the thesis was out of the ordinary and amply merited acceptance. Certainly it contained results enough to supply material for several good theses. But, I must not be afraid to say, if an accurate idea of the way Poincaré worked is wanted, many points called for corrections or explanations. Poincaré was an intuitionist. Having once arrived at the summit he never retraced his steps. He was satisfied to have crashed through the difficulties and left to others the pains of mapping the royal roads* destined to lead more easily to the end. He willingly enough made the corrections and tidying-up which seemed to me necessary. But he explained to me when I asked him to do it that he had many other ideas in his head; he was already occupied with some of the great problems whose solution he was to give us.'

Thus young Poincaré, like Gauss, was overwhelmed by the host of ideas which besieged his mind but, unlike Gauss, his motto was not 'Few, but ripe'. It is an open question whether a creative scientist who hoards the fruits of his labour so long that some of them go stale does more for the advancement of science than the more impetuous man who scatters broadcast everything he gathers, green or ripe, to fall where it may to ripen or rot as wind and weather take it. Some believe one way, some another. As a decision is beyond the reach of objective criteria everyone is entitled to his own purely subjective opinion.

Poincaré was not destined to become a mining engineer, but during his apprenticeship he showed that he had at least the courage of a real engineer. After a mine explosion and fire which had claimed sixteen victims he went down at once with the rescue crew. But the calling was uncongenial and he welcomed the opportunity to become a professional mathematician which his thesis and other early work opened up to him. His first academic appointment was at Caen on 1 December 1879, as

* 'There is no royal road to geometry', as Menaechmus is said to have told Alexander the Great when the latter wished to conquer geometry in a hurry.

Professor of Mathematical Analysis. Two years later he was promoted (at the age of twenty-seven) to the University of Paris where, in 1886, he was again promoted, taking charge of the course in mechanics and experimental physics (the last seems rather strange, in view of Poincaré's exploits as a student in the laboratory). Except for trips to scientific congresses in Europe and a visit to the United States in 1904 as an invited lecturer at the St Louis Exposition, Poincaré spent the rest of his life in Paris as the ruler of French mathematics.

Poincaré's creative period opened with the thesis of 1878 and closed with his death in 1912 – when he was at the apex of his powers. Into this comparatively brief span of thirty-four years he crowded a mass of work that is sheerly incredible when we consider the difficulty of most of it. His record is nearly 500 papers on *new* mathematics, many of them extensive memoirs, and more than thirty books covering practically all branches of mathematical physics, theoretical physics, and theoretical astronomy as they existed in his day. This leaves out of account his classics on the philosophy of science and his popular essays. To give an adequate idea of this immense labour one would have to be a second Poincaré, so we shall presently select two or three of his most celebrated works for brief description, apologizing here once for all for the necessary inadequacy.

Poincare's first successes were in the theory of differential equations, to which he applied all the resources of the analysis of which he was absolute master. This early choice for a major effort already indicates Poincaré's leaning toward the applications of mathematics, for differential equations have attracted swarms of workers since the time of Newton chiefly because they *are* of great importance in the exploration of the physical universe. 'Pure' mathematicians sometimes like to imagine that all their activities are dictated by their own tastes and that the applications of science suggest nothing of interest to them. Nevertheless some of the purest of the pure drudge away their lives over differential equations that first appeared in the translation of physical situations into mathematical symbolism, and it is precisely these practically suggested equations which are the heart of the theory. A particular equation suggested by science may be generalized by the mathematicians and then be turned back to the scientists (frequently without a solution in any form that they can use) to be applied to new physical problems, but first and last the motive is scientific. Fourier summed up this thesis in a famous passage which irritates one type of mathematician, but which Poincaré endorsed and followed in much of his work.

'The profound study of nature', Fourier declared, 'is the most fecund source of mathematical discoveries. Not only does this study, by offering a definite goal to research, have the advantage of excluding vague questions and futile calculations, but it is also a sure means of moulding analysis itself and discovering those elements in it which it is essential to know and which science ought always to conserve. These fundamental elements are those which recur in all natural phenomena.' To which some might retort: No doubt, but what about arithmetic in the sense of Gauss? However, Poincaré followed Fourier's advice whether he believed in it or not – even his researches in the theory of numbers were more or less remotely inspired by others closer to the mathematics of physical science.

The investigations on differential equations led out in 1880, when Poincaré was twenty-six, to one of his most brilliant discoveries, a generalization of the elliptic functions (and of some others). The nature of a (uniform) periodic function of a single variable has frequently been described in preceding chapters, but to bring out what Poincaré did, we may repeat the essentials. The trigonometric function $\sin z$ has the period 2π, namely, $\sin (z + 2\pi) = \sin z$; that is, when the variable z is increased by 2π, the sine function of z returns to its initial value. For an elliptic function, say $E(z)$, there are *two* distinct periods, say p_1 and p_2, such that $E(z + p_1) = E(z)$, $E(z + p_2) = E(z)$. Poincaré found that *periodicity* is merely a special case of a more general property: the value of certain functions is restored when the variable is replaced by any one of a *denumerable* infinity of linear fractional transformations of itself, and all these transformations form a group. A few symbols will clarify this statement.

Let z be replaced by $\dfrac{az+b}{cz+d}$. Then, for a *denumerable infinity* of sets of values of a,b,c,d, there are uniform functions of z, say $F(z)$ is one of them, such that

$$F\left(\frac{az+b}{cz+d}\right) = F(z).$$

Further, if a_1,b_1,c_1,d_1, and $a_2b_2c_2d_2$ are any two of the sets of values of a,b,c,d, and if z be replaced first by $\dfrac{a_1z+b_1}{c_1z+d_1}$, and then, in this, z be replaced by $\dfrac{a_2z+b_2}{c_2z+d_2}$, giving, say, $\dfrac{Az+B}{Cz+D}$, then not only do we have

$$F\left(\frac{a_1z+b_1}{c_1z+d_1}\right) = F(z), \quad F\left(\frac{a_2z+b_2}{c_2z+d_2}\right) = F(z),$$

but also

$$F\left(\frac{Az+B}{Cz+D}\right) = F(z).$$

Further the set of all the substitutions

$$z \rightarrow \frac{az+b}{cz+d}$$

(the arrow is read 'is replaced by') which leave the value of $F(z)$ unchanged as just explained *form a group*: the result of the successive performance of two substitutions in the set,

$$z \rightarrow \frac{a_1z+b_1}{c_1z+d_1}, \quad z \rightarrow \frac{a_2z+b_2}{c_2z+d_2},$$

is in the set; there is an 'identity substitution' in the set, namely $z \rightarrow z$ (here $a = 1, b = 0, c = 0, d = 1$); and finally each substitution has a unique 'inverse' – that is, for each substitution in the set there is a single other one which, if applied to the first, will produce the identity substitution. In summary, using the terminology of previous chapters, we see that $F(z)$ is *a function which is invariant under an infinite group of linear fractional transformations.* Note that the infinity of substitutions is a *denumerable* infinity, as first stated: the substitutions can be counted off 1,2,3, . . . , and are *not* as numerous as the points on a line. Poincaré actually constructed such functions and developed their most important properties in a series of papers in the 1880's. Such functions are called *automorphic.*

Only two remarks need be made here to indicate what Poincaré achieved by this wonderful creation. First, his theory includes that of the elliptic functions as a detail. Second, as the distinguished French mathematician Georges Humbert said, Poincaré found two memorable propositions which 'gave him the keys of the algebraic cosmos':

Two automorphic functions* invariant under the same group are connected by an algebraic equation;

Conversely, the co-ordinates of a point on any algebraic curve can be expressed in terms of automorphic functions, and hence by uniform functions of a single parameter (variable).

An algebraic curve is one whose equation is of the type $P(x,y) = 0$, where $P(x,y)$ is a polynomial in x and y. As a simple example, the equation of the circle whose centre is at the origin – (0,0) – and whose radius is a, is $x^2 + y^2 = a^2$. According to the second of Poincaré's 'keys', it must be possible to express x,y as automorphic functions of a single parameter, say t. It is; for if $x = a \cos t$ and $y = a \sin t$, then, squaring and adding, we get rid of t (since $\cos^2 t + \sin^2 t = 1$), and find $x^2 + y^2 = a^2$. But the trigonometric functions $\cos t$, $\sin t$ are special cases of elliptic functions, which in turn are special cases of automorphic functions.

The creation of this vast theory of automorphic functions was but one of many astonishing things in analysis which Poincaré did before he was thirty. Nor was all his time devoted to analysis; the theory of numbers, parts of algebra, and mathematical astronomy also shared his attention. In the first he recast the Gaussian theory of binary quadratic forms (see chapter on

* Poincaré called some of his functions 'Fuchsian', after the German mathematician Lazarus Fuchs (1833–1902) one of the creators of the modern theory of differential equations, for reasons that need not be gone into here. Others he called 'Kleinian' after Felix Klein – in ironic acknowledgement of disputed priority.

Gauss) in a geometrical shape which appeals particularly to those who, like Poincaré, prefer the intuitive approach. This of course was not all that he did in the higher arithmetic, but limitations of space forbid further details.

Work of this calibre did not pass unappreciated. At the unusually early age of thirty-two (in 1887) Poincaré was elected to the Academy. His proposer said some pretty strong things, but most mathematicians will subscribe to their truth: '[Poincaré's] work is above ordinary praise and reminds us inevitably of what Jacobi wrote of Abel – that he had settled questions which, before him, were unimagined. It must indeed be recognized that we are witnessing a revolution in Mathematics comparable in every way to that which manifested itself, half a century ago, by the accession of elliptic functions.'

To leave Poincaré's work in pure mathematics here is like rising from a banquet table after having just sat down, but we must turn to another side of his universality.

Since the time of Newton and his immediate successors astronomy has generously supplied mathematicians with more problems than they can solve. Until the late nineteenth century the weapons used by mathematicians in their attack on astronomy were practically all immediate improvements of those invented by Newton himself, Euler, Lagrange, and Laplace. But all through the nineteenth century, particularly since Cauchy's development of the theory of functions of a complex variable and the investigations of himself and others on the convergence of infinite series, a huge arsenal of untried weapons had been accumulating from the labours of pure mathematicians. To Poincaré, to whom analysis came as naturally as thinking, this vast pile of unused mathematics seemed the most natural thing in the world to use in a new offensive on the outstanding problems of celestial mechanics and planetary evolution. He picked and chose what he liked out of the heap, improved it, invented new weapons of his own, and assaulted theoretical astronomy in a grand fashion it had not been assaulted in for a century. He *modernized* the attack; indeed his campaign was so extremely modern to the majority of experts in celestial mechanics that even to-day, forty years or more after Poincaré opened his offensive, few have mastered his weapons and some, unable to bend his bow, insinuate that it is worthless in a practical attack. Nevertheless Poincaré is not without forceful champions whose conquests would have been impossible to the men of the pre-Poincaré era.

Poincaré's first (1889) great success in mathematical astronomy grew out of an unsuccessful attack on 'the problem of n bodies.' For $n = 2$ the problem was completely solved by Newton; the famous 'problem of three bodies' ($n = 3$) will be noticed later; when n exceeds 3 some of the reductions applicable to the case $n = 3$ can be carried over.

According to the Newtonian law of gravitation two particles of masses m, M at a distance D apart attract one another with a force proportional to $\frac{m \times M}{D^2}$. Imagine n material particles distributed in any manner in space; the masses, initial motions' and the mutual distances of all the particles are assumed known at a given instant. If they attract one another according to the Newtonian law, *what will be their positions and motions (velocities) after any stated lapse of time?* For the purposes of mathematical astronomy the stars in a cluster, or in a galaxy, or in a cluster of galaxies, may be thought of as material particles attracting one another according to the Newtonian law. The 'problem of n bodies' thus amounts – in one of its applications – to asking what will be the aspect of the heavens a year from now, or a billion years hence, it being assumed that we have sufficient observational data to describe the general configuration *now*. The problem of course is tremendously complicated by radiation – the masses of the stars do not remain constant for millions of years; but a complete, calculable solution of the problem of n bodies in its Newtonian form would probably give results of an accuracy sufficient for all human purposes – the human race will likely be extinct long before radiation can introduce observable inaccuracies.

This was substantially the problem proposed for the prize offered by King Oscar II of Sweden in 1887. Poincaré did not solve the problem, but in 1889 he was awarded the prize anyhow by a jury consisting of Weierstrass, Hermite, and Mittag-

Leffler for his general discussion of the differential equations of dynamics and an attack on the problem of three bodies. The last is usually considered the most important case of the n-body problem, as the Earth, Moon, and Sun furnish an instance of the case $n = 3$. In his report to Mittag-Leffler, Weierstrass wrote, 'You may tell your Sovereign that this work cannot indeed be considered as furnishing the complete solution of the question proposed, but that it is nevertheless of such importance that *its publication will inaugurate a new era in the history of Celestial Mechanics*. The end which His Majesty had in view in opening the competition may therefore be considered as having been attained.' Not to be outdone by the King of Sweden, the French Government followed up the prize by making Poincaré a Knight of the Legion of Honour – a much less expensive acknowledgement of the young mathematician's genius than the King's 2,500 crowns and gold medal.

As we have mentioned the problem of three bodies we may now report one item from its fairly recent history; since the time of Euler it has been considered one of the most difficult problems in the whole range of mathematics. Stated mathematically, the problem boils down to solving a system of nine simultaneous differential equations (all linear, each of the second order). Lagrange succeeded in reducing this system to a simpler. As in the majority of physical problems, the solution is not to be expected in *finite* terms; *if a solution exists at all* it will be given by *infinite series*. The solution will 'exist' if these series satisfy the equations (formally) and moreover *converge* for certain values of the variables. The central difficulty is to prove the convergence. Up till 1905 various special solutions had been found, but the existence of anything that could be called general had not been proved.

In 1906 and 1909 a considerable advance came from a rather unexpected quarter – Finland, a country which sophisticated Europeans even to-day consider barely civilized, especially for its queer custom of paying its debts, and which few Americans thought advanced beyond the Stone Age till Paavo Nurmi ran the legs off the United States. Excepting only the rare case when all three bodies collide simultaneously, Karl Frithiof

Sundman of Helsingfors, utilizing analytical methods due to the Italian Levi-Civita and the French Painlevé, and making an ingenious transformation of his own, *proved* the existence of a solution in the sense described above. Sundman's solution is not adapted to numerical computation, nor does it give much information regarding the actual motion, but that is not the point of interest here: a problem which had not been known to be solvable was proved to be so. Many had struggled desperately to prove this much; when the proof was forthcoming, some, humanly enough, hastened to point out that Sundman had done nothing much because he had not solved some problem other than the one he had. This kind of criticism is as common in mathematics as it is in literature and art, showing once more that mathematicians are as human as anybody.

Poincaré's most original work in mathematical astronomy was summed up in his great treatise *Les méthodes nouvelles de la mécanique céleste* (New Methods of Celestial Mechanics; three volumes, 1892, 1893, 1899). This was followed by another three-volume work in 1905–10 of a more immediately practical nature, *Leçons de mécanique céleste*, and a little later by the publication of his course of lectures *Sur les figures d'équilibre d'une masse fluide* (On the Figures of Equilibrium of a Fluid Mass), and a historical-critical book *Sur les hypothèses cosmogoniques* (On Cosmological Hypotheses).

Of the first of these works Darboux (seconded by many others) declares that it did indeed start a new era in celestial mechanics and that it is comparable to the *Mécanique céleste* of Laplace and the earlier work of D'Alembert on the precession of the equinoxes. 'Following the road in analytical mechanics opened up by Lagrange,' Darboux says, '... Jacobi had established a theory which appeared to be one of the most complete in dynamics. For fifty years we lived on the theorems of the illustrious German mathematician, applying them and studying them from all angles, but without adding anything essential. It was Poincaré who first shattered these rigid frames in which the theory seemed to be encased and contrived for it vistas and new windows on the external world. He introduced or used, in the study of dynamical problems, different notions: the first, which had been given before and which, moreover, is applicable not solely to mechanics, is that of *variational equations*, namely, linear differential equations that determine solutions of a problem infinitely near to a given solution; the second, that of *integral invariants*, which belong entirely to him and play a capital part in these researches. Further fundamental notions were added to these, notably those concerning so-called "periodic" solutions, for which the bodies whose motion is studied return after a certain time to their initial positions and original relative velocities.'

The last started a whole department of mathematics, the investigation of *periodic orbits*: given a system of planets, or of stars, say, with a complete specification of the initial positions and relative velocities of all members of the system at a stated epoch, it is required to determine under what conditions the system will return to its initial state at some later epoch, and hence continue to repeat the cycle of its motions indefinitely. For example, is the solar system of this recurrent type, or if not, would it be were it isolated and not subject to perturbations by external bodies? Needless to say the general problem has not yet been solved completely.

Much of Poincaré's work in his astronomical researches was qualitative rather than quantitative, as befitted an intuitionist, and this characteristic led him, as it had Riemann, to the study of analysis situs. On this he published six famous memoirs which revolutionized the subject as it existed in his day. The work on analysis situs in its turn was freely applied to the mathematics of astronomy.

We have already alluded to Poincaré's work on the problem of rotating fluid bodies – of obvious importance in cosmogony, one brand of which assumes that the planets were once sufficiently like such bodies to be treated as if they actually were without patent absurdity. Whether they were or not is of no importance for the mathematics of the situation, which is of interest in itself. A few extracts from Poincaré's own summary will indicate more clearly than any paraphrase the nature of what he mathematicized about in this difficult subject.

'Let us imagine a [rotating] fluid body contracting by cooling, but slowly enough to remain homogeneous and for the rotation to be the same in all its parts.

'At first very approximately a sphere, the figure of this mass will become an ellipsoid of revolution which will flatten more and more, then, at a certain moment, it will be transformed into an ellipsoid with three unequal axes. Later, the figure will cease to be an ellipsoid and will become pear-shaped until at last the mass, hollowing out more and more at its "waist", will separate into two distinct and unequal bodies.

'The preceding hypothesis certainly cannot be applied to the solar system. Some astronomers have thought that it might be true for certain double stars and that double stars of the type of Beta Lyrae might present transitional forms analogous to those we have spoken of.'

He then goes on to suggest an application to Saturn's rings, and he claims to have proved that the rings can be stable only if their density exceeds 1/16 that of Saturn. It may be remarked that these questions were not considered as fully settled as late as 1935. In particular a more drastic mathematical attack on poor old Saturn seemed to show that he had not been completely vanquished by the great mathematicians, including Clerk Maxwell, who have been firing away at him off and on for the past seventy years.

Once more we must leave the banquet having barely tasted anything and pass on to Poincaré's voluminous work in mathematical physics. Here his luck was not so good. To have cashed in on his magnificent talents he should have been born thirty years later or have lived twenty years longer. He had the misfortune to be in his prime just when physics had reached one of its recurrent periods of senility, and he was so thoroughly saturated with nineteenth-century theories when physics began to recover its youth – after Planck, in 1900, and Einstein, in 1905, had performed the difficult and delicate operation of endowing the decrepit *roué* with its first pair of new glands – that he had barely time to digest the miracle before his death in 1912. All his mature life Poincaré seemed to absorb knowledge through his pores without a conscious effort. Like Cayley, he was not only a prolific creator but also a profoundly erudite scholar. His range was probably wider than even Cayley's, for Cayley never professed to be able to understand everything that was going on in applied mathematics. This unique erudition may have been a disadvantage when it came to a question of living science as opposed to classical.

Everything that boiled up in the melting pots of physics was grasped instantly as it appeared by Poincaré and made the topic of several purely mathematical investigations. When wireless telegraphy was invented he seized on the new thing and worked out its mathematics. While others were either ignoring Einstein's early work on the (special) theory of relativity or passing it by as a mere curiosity, Poincaré was already busy with its mathematics, and he was the first scientific man of high standing to tell the world what had arrived and urge it to watch Einstein as probably the most significant phenomenon of the new era which he foresaw but could not himself usher in. It was the same with Planck's early form of the quantum theory. Opinions differ, of course; but at this distance it is beginning to look as if mathematical physics did for Poincaré what Ceres did for Gauss; and although Poincaré accomplished enough in mathematical physics to make half a dozen great reputations, it was not the trade to which he had been born and science would have got more out of him if he had stuck to pure mathematics – his astronomical work was nothing else. But science got enough, and a man of Poincaré's genius is entitled to his hobbies.

We pass on now to the last phase of Poincaré's universality for which we have space: his interest in the rationale of mathematical creation. In 1902 and 1904 the Swiss mathematical periodical *L'Enseignement Mathématique* undertook an enquiry into the working habits of mathematicians. Questionnaires were issued to a number of mathematicians, of whom over a hundred replied. The answers to the questions and an analysis of general trends were published in final form in 1912.* Anyone wishing to look into the 'psychology' of mathematicians will

* *Enquête de 'L'Enseignement Mathematique' sur la méthode de travail des mathématiciens.* Available either in the periodical or in book form (8 + 137 pp.) from Gauthier-Villars, Paris.

find much of interest in this unique work and many confirmations of the views at which Poincaré had arrived independently before he saw the results of the questionnaire. A few points of general interest may be noted before we quote from Poincaré.

The early interest in mathematics of those who were to become great mathematicians has been frequently exemplified in preceding chapters. To the question 'At what period ... and under what circumstances did mathematics seize you?' ninety-three replies to the first part were received: thirty-five said before the age of ten; forty-three said eleven to fifteen; eleven said sixteen to eighteen; three said nineteen to twenty; and the lone laggard said twenty-six.

Again, anyone with mathematical friends will have noticed that some of them like to work early in the morning (I know one very distinguished mathematician who begins his day's work at the inhuman hour of five a.m.), while others do nothing till after dark. The replies on this point indicated a curious trend – possibly significant, although there are numerous exceptions: mathematicians of the northern races prefer to work at night, while the Latins favour the morning. Among night-workers prolonged concentration often brings on insomnia as they grow older and they change – reluctantly – to the morning. Felix Klein, who worked day and night as a young man, once indicated a possible way out of this difficulty. One of his American students complained that he could not sleep for thinking of his mathematics. 'Can't sleep, eh?' Klein snorted. 'What's chloral for?' However, this remedy is not to be recommended indiscriminately; it probably had something to do with Klein's own tragic breakdown.

Probably the most significant of the replies were those received on the topic of inspiration versus drudgery as the source of mathematical discoveries. The conclusion is that 'Mathematical discoveries, small or great ... are never born of spontaneous generation. They always presuppose a soil seeded with preliminary knowledge and well prepared by labour, both conscious and subconscious.'

Those who, like Thomas Alva Edison, have declared that genius is 99 per cent perspiration and only 1 per cent inspiration, are not contradicted by those who would reverse the figures. Both are right; one man remembers the drudgery while another forgets it all in the thrill of apparently sudden discovery, but both, when they analyse their impressions, admit that without drudgery and a flash of 'inspiration' discoveries are not made. If drudgery alone sufficed, how is it that many gluttons for hard work who seem to know everything about some branch of science, while excellent critics and commentators, never themselves make even a small discovery? On the other hand, those who believe in 'inspiration' as the sole factor in discovery or invention – scientific or literary – may find it instructive to look at an early draft of any of Shelley's 'completely spontaneous' poems (so far as these have been preserved and reproduced), or the successive versions of any of the greater novels that Balzac inflicted on his maddened printer.

Poincaré stated his views on mathematical discovery in an essay first published in 1908 and reproduced in his *Science et Méthode*. The genesis of mathematical discovery, he says, is a problem which should interest psychologists intensely, for it is the activity in which the human mind seems to borrow least from the external world, and by understanding the process of mathematical thinking we may hope to reach what is most essential in the human mind.

How does it happen, Poincaré asks, that there are persons who do not understand mathematics? 'This should surprise us, or rather it would surprise us if we were not so accustomed to it.' If mathematics is based only on the rules of logic, such as all normal minds accept, and which only a lunatic would deny (according to Poincaré), how is it that so many are mathematically impermeable? To which it may be answered that no exhaustive set of experiments substantiating mathematical incompetence as the normal human mode has yet been published. 'And further', he asks, 'how is error possible in mathematics?' Ask Alexander Pope: 'To err is human', which is as unsatisfactory a solution as any other. The chemistry of the digestive system may have something to do with it, but Poincaré prefers a more subtle explanation – one which could not be tested by feeding the 'vile body' hasheesh and alcohol.

'The answer seems to me evident', he declares. Logic has very little to do with discovery or invention, and memory plays tricks. Memory however is not so important as it might be. His own memory, he says without a blush, is bad: 'Why then does it not desert me in a difficult piece of mathematical reasoning where most chess players [whose 'memories' he assumes to be excellent] would be lost? Evidently because it is guided by the general course of the reasoning. A mathematical proof is not a mere juxtaposition of syllogisms; it is syllogisms *arranged in a certain order*, and the order is more important than the elements themselves.' If he has the 'intuition' of this order, memory is at a discount, for each syllogism will take its place automatically in the sequence.

Mathematical creation, however, does not consist merely in making new combinations of things already known; 'anyone could do that, but the combinations thus made would be infinite in number and most of them entirely devoid of interest. To create consists precisely in avoiding useless combinations and in making those which are useful and which constitute only a small minority. Invention is discernment, selection.' But has not all this been said thousands of times before? What artist does not know that selection – an intangible – is one of the secrets of success? We are exactly where we were before the investigation began.

To conclude this part of Poincaré's observations it may be pointed out that much of what he says is based on an assumption which may indeed be true but for which there is not a particle of scientific evidence. To put it bluntly he assumes that the majority of human beings are mathematical imbeciles. Granting him this, we need not even then accept his purely romantic theories. They belong to inspirational literature and not to science. Passing to something less controversial, we shall now quote the famous passage in which Poincaré describes how one of his own greatest 'inspirations' came to him. It is meant to substantiate his theory of mathematical creation. Whether it does or not may be left to the reader.

He first points out that the technical terms need not be understood in order to follow his narrative: 'What is of interest to the psychologist is not the theorem but the circumstances.'

For fifteen days I struggled to prove that no functions analogous to those I have since called *Fuchsian functions* could exist; I was then very ignorant. Every day I sat down at my work table where I spent an hour or two; I tried a great number of combinations and arrived at no result. One evening, contrary to my custom, I took black coffee; I could not go to sleep; ideas swarmed up in clouds; I sensed them clashing until, to put it so, a pair would hook together to form a stable combination. By morning I had established the existence of a class of Fuchsian functions, those derived from the hypergeometric series. I had only to write up the results, which took me a few hours.

Next I wished to represent these functions by the quotient of two series; this idea was perfectly conscious and thought out; analogy with elliptic functions guided me. I asked myself what must be the properties of these series if they existed, and without difficulty I constructed the series which I called thetafuchsian.

I then left Caen, where I was living at the time, to participate in a geological trip sponsored by the School of Mines. The exigencies of travel made me forget my mathematical labours; reaching Coutances we took a bus for some excursion or another. The instant I put my foot on the step the idea came to me, apparently with nothing whatever in my previous thoughts having prepared me for it, that the transformations which I had used to define Fuchsian functions were identical with those of non-Euclidean geometry. I did not make the verification; I should not have had the time, because once in the bus I resumed an interrupted conversation; but I felt an instant and complete certainty. On returning to Caen, I verified the result at my leisure to satisfy my conscience.

I then undertook the study of certain arithmetical questions without much apparent success and without suspecting that such matters could have the slightest connexion with my previous studies. Disgusted at my lack of success, I went to spend a few days at the seaside and thought of something else. One day, while walking along the cliffs, the idea came to me, again with the same characteristics of brevity, suddenness, and immediate certainty, that the transformations of indefinite ternary quadratic forms were identical with those of non-Euclidean geometry.

On returning to Caen, I reflected on this result and deduced its consequences; the example of quadratic forms showed me that there were Fuchsian groups other than those corresponding to the hypergeometric series; I saw that I could apply to them the theory of thetafuchsian functions, and hence that there existed thetafuchsian functions other than those derived from the hypergeometric series, the only ones I had known up till then. Naturally I set myself the task of constructing all these functions. I conducted a systematic siege and, one after another, carried all the outworks; there was however one which still held out and whose fall would bring about that of the whole position. But all my efforts served only to make me better acquainted with the difficulty, which in itself was something. All this work was perfectly conscious.

At this point I left for Mont-Valérien, where I was to discharge my military service. I had therefore very different preoccupations. One day, while crossing the boulevard, the solution of the difficulty which had stopped me appeared to me all of a sudden. I did not seek to go into it immediately, and it was only after my service that I resumed the question. I had all the elements, and had only to assemble and order them. So I wrote out my definitive memoir at one stroke and with no difficulty.

Many other examples of this sort of thing could be given from his own work, he says, and from that of other mathematicians as reported in *L'Enseignement Mathématique*. From his experiences he believes that this semblance of 'sudden illumination [is] a manifest sign of previous long subconscious work', and he proceeds to elaborate his theory of the subconscious mind and its part in mathematical creation. Conscious work is necessary as a sort of trigger to fire off the accumulated dynamite which the subconscious has been excreting – he does not put it so, but what he says amounts to the same. But what is gained in the way of rational explanation if, following Poincaré, we foist off on the 'subconscious mind', or the 'subliminal self', the very activities which it is our object to understand? Instead of endowing this mysterious agent with a hypothetical tact enabling it to discriminate between the 'exceedingly numerous' possible combinations presented (how, Poincaré does not say) for its inspection, and calmly saying that the 'subconscious' rejects all but the 'useful' combinations because it has a feeling for symmetry and beauty, sounds suspiciously like solving the initial problem by giving it a more impressive name. Perhaps this is exactly what Poincaré intended, for he once defined mathematics as the art of giving the same name to different things; so here he may be rounding out the symmetry of his view by giving different names to the same thing. It seems strange that a man who could have been satisfied with such a 'psychology' of mathematical invention was the complete sceptic in religious matters that Poincaré was. After Poincaré's brilliant lapse into psychology sceptics may well despair of ever disbelieving anything.

During the first decade of the twentieth century Poincaré's fame increased rapidly and he came to be looked upon, especially in France, as an oracle on all things mathematical. His pronouncements on all manner of questions, from politics to ethics, were usually direct and brief, and were accepted as final by the majority. As almost invariably happens after a great man's extinction, Poincaré's dazzling reputation during his lifetime passed through a period of partial eclipse in the decade following his death. But his intuition for what was likely to be of interest to a later generation is always justifying itself. To take but one instance of many, Poincaré was a vigorous opponent of the theory that all mathematics can be rewritten in terms of the most elementary notions of classical logic; something more than logic, he believed, makes mathematics what it is. Although he did not go quite so far as the current intuitionist school, he seems to have believed, as that school does, that at least some mathematical notions precede logic, and if one is to be derived from the other it is logic which must come out of mathematics, not the other way about. Whether this is to be the ultimate creed remains to be seen, but at present it appears as if the theory which Poincaré assailed with all the irony at his command is not the final one, whatever may be its merits.

Except for a distressing illness during his last four years

Poincaré's busy life was tranquil and happy. Honours were showered upon him by all the leading societies of the world, and in 1906, at the age of fifty-two, he achieved the highest distinction possible to a French scientist, the Presidency of the Academy of Sciences. None of all this inflated his ego, for Poincaré was truly humble and unaffectedly simple. He knew of course that he was without a close rival in the days of his maturity, but he could also say without a trace of affectation that he knew nothing compared to what is to be known. He was happily married and had a son and three daughters in whom he took much pleasure, especially when they were children. His wife was a great-granddaughter of Étienne-Geoffroy Saint-Hilaire, remembered as the antagonist of that pugnacious comparative anatomist Cuvier. One of Poincaré's passions was symphonic music.

At the International Mathematical Congress of 1908, held at Rome, Poincaré was prevented by illness from reading his stimulating (if premature) address on *The Future of Mathematical Physics*. His trouble was hypertrophy of the prostate, which was relieved by the Italian surgeons, and it was thought that he was permanently cured. On his return to Paris he resumed his work as energetically as ever. But in 1911 he began to have presentiments that he might not live long, and on 9 December wrote asking the editor of a mathematical journal whether he would accept an unfinished memoir – contrary to the usual custom – on a problem which Poincaré considered of the highest importance: '... at my age, I may not be able to solve it, and the results obtained, susceptible of putting researchers on a new and unexpected path, seem to me too full of promise, in spite of the deceptions they have caused me, that I should resign myself to sacrificing them. ...' He had spent the better part of two fruitless years trying to overcome his difficulties.

A proof of the theorem which he conjectured would have enabled him to make a striking advance in the problem of three bodies; in particular it would have permitted him to prove the existence of an infinity of periodic solutions in cases more general than those hitherto considered. The desired proof was given shortly after the publication of Poincaré's 'unfinished symphony' by a young American mathematician, George David Birkhoff (1884–1944).

In the spring of 1912 Poincaré fell ill again and underwent a second operation on 9 July. The operation was successful, but on 17 July he died very suddenly from an embolism while dressing. He was in the fifty-ninth year of his age and at the height of his powers – 'the living brain of the rational sciences', in the words of Painlevé.

CHAPTER TWENTY-NINE

PARADISE LOST?

Cantor

THE controversial topic of *Mengenlehre* (theory of sets, or classes, particularly of infinite sets) created in 1874–95 by Georg Cantor (1845–1918) may well be taken, out of its chronological order, as the conclusion of the whole story. This topic typifies for mathematics the general collapse of those principles which the prescient seers of the nineteenth century, foreseeing everything but the grand debacle, believed to be fundamentally sound in all things from physical science to democratic government.

If 'collapse' is perhaps too strong to describe the transition the world is doing its best to enjoy, it is nevertheless true that the evolution of scientific ideas is now proceeding so vertiginously that evolution is barely distinguishable from revolution.

Without the errors of the past as a deep-seated focus of disturbance the present upheaval in physical science would perhaps not have happened; but to credit our predecessors with all the inspiration for what our own generation is doing, is to give them more than their due. This point is worth a moment's consideration, as some may be tempted to say that the corresponding 'revolution' in mathematical thinking, whose beginnings are now plainly apparent, is merely an echo of Zeno and other doubters of ancient Greece.

The difficulties of Pythagoras over the square root of 2 and the paradoxes of Zeno on continuity (or 'infinite divisibility') are – so far as we know – the origins of our present mathematical schism. Mathematicians to-day who pay any attention to the philosophy (or foundations) of their subject are split into at least two factions, apparently beyond present hope of reconciliation, over the validity of the reasoning used in mathematical analysis, and this disagreement can be traced back through the centuries to the Middle Ages and thence to ancient Greece. All sides have had their representatives in all ages of mathematical thought, whether that thought was disguised in provocative paradoxes, as with Zeno, or in logical subtleties, as with some of the most exasperating logicians of the Middle Ages. The root of these differences is commonly accepted by mathematicians as being a matter of temperament: any attempt to convert an analyst like Weierstrass to the scepticism of a doubter like Kronecker is bound to be as futile as trying to convert a Christian fundamentalist to rabid atheism.

A few dated quotations from leaders in the dispute may serve as a stimulant – or sedative, according to taste – for our enthusiasm over the singular intellectual career of Georg Cantor, whose 'positive theory of the infinite' precipitated, in our own generation, the fiercest frog-mouse battle (as Einstein once called it) in history over the validity of traditional mathematical reasoning.

In 1831 Gauss expressed his 'horror of the actual infinite' as follows. 'I protest against the use of infinite magnitude as something completed, which is never permissible in mathematics. Infinity is merely a way of speaking, the true meaning being a limit which certain ratios approach indefinitely close, while others are permitted to increase without restriction.'

Thus, if x denotes a real number, the fraction $1/x$ diminishes as x increases, and we can find a value of x such that $1/x$ differs from zero by any preassigned amount (other than zero) which may be as small as we please, and as x continues to increase, the difference *remains* less than this preassigned amount; the *limit* of $1/x$, 'as x tends to infinity,' is zero. The symbol of infinity is ∞; the assertion $1/\infty = 0$ is nonsensical for two reasons: 'division by infinity' is an operation which is *undefined*, and hence has no meaning; the second reason was stated by Gauss. Similarly $1/0 = \infty$ is meaningless.

Cantor agrees and disagrees with Gauss. Writing in 1886 on the problem of the actual (what Gauss called completed) infinite, Cantor says that 'in spite of the essential difference between the concepts of the *potential* and the *actual* "infinite", the former meaning a *variable* finite magnitude increasing beyond all finite limits (like x in $1/x$ above), while the latter is a *fixed, constant* magnitude lying beyond all finite magnitudes, it happens only too often that they are confused.'

Cantor goes on to state that misuse of the infinite in mathematics had justly inspired a horror of the infinite among careful mathematicians of his day, precisely as it did in Gauss. Nevertheless he maintains that the resulting 'uncritical rejection of the legitimate actual infinite is no less a violation of the nature of things [whatever that may be – it does not appear to have been revealed to mankind as a whole], which must be taken as they are' – however that may be. Cantor thus definitely aligns himself with the great theologians of the Middle Ages, of whom he was a deep student and an ardent admirer.

Absolute certainties and complete solutions of age-old problems always go down better if well salted before swallowing. Here is what Bertrand Russell had to say in 1901 about Cantor's Promethean attack on the infinite.

'Zeno was concerned with three problems. . . . These are the problem of the infinitesimal, the infinite, and continuity. . . . From his day to our own, the finest intellects of each generation in turn attacked these problems, but achieved, broadly speaking, nothing. . . . Weierstrass, Dedekind, and Cantor . . . have completely solved them. Their solutions . . . are so clear as to leave no longer the slightest doubt of difficulty. This achievement is probably the greatest of which the age can boast. . . . The problem of the infinitesimal was solved by Weierstrass, the solution of the other two was begun by Dedekind and definitely accomplished by Cantor.'*

The enthusiasm of this passage warms us even to-day, although we know that Russell in the second edition (1924) of his and A. N. Whitehead's *Principia Mathematica* admitted that all was not well with the Dedekind 'cut' (see Chapter 27), which is the spinal cord of analysis. Nor is it well to-day. More is done for or against a particular creed in science or mathe-

* Quoted from R. E. Moritz' *Memorabilia Mathematica*, 1914. The original source is not accessible to me.

matics in a decade than was accomplished in a century of antiquity, the Middle Ages, or the late renaissance. More good minds attack an outstanding scientific or mathematical problem to-day than ever before, and finality has become the private property of fundamentalists. Not one of the finalities in Russell's remarks of 1901 has survived. A quarter of a century ago those who were unable to see the great light which the prophets assured them was blazing overhead like the noonday sun in a midnight sky were called merely stupid. To-day for every competent expert on the side of the prophets there is an equally competent and opposite expert against them. If there is stupidity anywhere it is so evenly distributed that it has ceased to be a mark of distinction. We are entering a new era, one of doubt and decent humility.

On the doubtful side about the same time (1905) we find Poincaré. 'I have spoken ... of our need to return continually to the first principles of our science, and of the advantages of this for the study of the human mind. This need has inspired two enterprises which have assumed a very prominent place in the most recent development of mathematics. The first is Cantorism. ... Cantor introduced into science a new way of considering the mathematical infinite ... but it has come about that we have encountered certain paradoxes, certain apparent contradictions that would have delighted Zeno the Eleatic and the school of Megara. So each must seek the remedy. I for my part – and I am not alone – think that the important thing is never to introduce entities not completely definable in a finite number of words. Whatever be the cure adopted, we may promise ourselves the joy of the physician called in to treat a beautiful pathologic case.'

A few years later Poincaré's interest in pathology for its own sake had abated somewhat. At the International Mathematical Congress of 1908 at Rome, the satiated physician delivered himself of this prognosis: 'Later generations will regard *Mengenlehre* as a disease from which one has recovered.'

It was Cantor's greatest merit to have discovered in spite of himself and against his own wishes in the matter that the 'body mathematic' is profoundly diseased and that the sickness with which Zeno infected it has not yet been alleviated. His disturbing discovery is a curious echo of his own intellectual life. We shall first glance at the facts of his material existence, not of much interest in themselves, perhaps, but singularly illuminative in their later aspects of his theory.

Of pure Jewish descent on both sides, Georg Ferdinand Ludwig Philipp Cantor was the first child of the prosperous merchant Georg Waldemar Cantor and his artistic wife Maria Bohm. The father was born in Copenhagen, Denmark, but migrated as a young man to St Petersburg, Russia, where the mathematician Georg Cantor was born on 3 March 1845. Pulmonary disease caused the father to move in 1856 to Frankfurt, Germany, where he lived in comfortable retirement till his death in 1863. From this curious medley of nationalities it is possible for several fatherlands to claim Cantor as their son. Cantor himself favoured Germany, but it cannot be said that Germany favoured him very cordially.

Georg had a brother Constantin, who became a German army officer (what a career for a Jew!), and a sister, Sophie Nobiling. The brother was a fine pianist; the sister an accomplished designer. Georg's pent-up artistic nature found its turbulent outlet in mathematics and philosophy, both classical and scholastic. The marked artistic temperaments of the children were inherited from their mother, whose grandfather was a musical conductor, one of whose brothers, living in Vienna, taught the celebrated violinist Joachim. A brother of Maria Cantor was a musician, and one of her nieces a painter. If it is true, as claimed by the psychological proponents of drab mediocrity, that normality and phlegmatic stability are equivalent, all this artistic brilliance in his family may have been the root of Cantor's instability.

The family were Christians, the father having been converted to Protestantism; the mother was born a Roman Catholic. Like his arch-enemy Kronecker, Cantor favoured the Protestant side and acquired a singular taste for the endless hairsplitting of medieval theology. Had he not become a mathematician it is quite possible that he would have left his mark on philosophy or theology. As an item of interest that may be noted in this connexion, Cantor's theory of the infinite was eagerly pounced on by the Jesuits, whose keen logical minds detected in the mathematical imagery beyond their theological comprehension indubitable proofs of the existence of God and the self-consistency of the Holy Trinity with its three-in-one, one-in-three, co-equal and co-eternal. Mathematics has strutted to some pretty queer tunes in the past 2,500 years, but this takes the cake. It is only fair to say that Cantor, who had a sharp wit and a sharper tongue when he was angered, ridiculed the pretentious absurdity of such 'proofs', devout Christian and expert theologian though he himself was.

Cantor's school career was like that of most highly gifted mathematicians – an early recognition (before the age of fifteen) of his greatest talent and an absorbing interest in mathematical studies. His first instruction was under a private tutor, followed by a course in an elementary school in St Petersburg. When the family moved to Germany, Cantor first attended private schools at Frankfurt and the Darmstadt non-classical school, entering the Wiesbaden Gymnasium in 1860 at the age of fifteen.

Georg was determined to become a mathematician, but his practical father, recognizing the boy's mathematical ability, obstinately tried to force him into engineering as a more promising bread-and-butter profession. On the occasion of Cantor's confirmation in 1860 his father wrote to him expressing the high hopes he and all Georg's numerous aunts, uncles, and cousins in Germany, Denmark, and Russia had placed on the gifted boy: 'They expect from you nothing less than that you become a Theodor Schaeffer and later, perhaps, if God so wills, a shining star in the engineering firmament.' When will parents recognize the presumptuous stupidity of trying to make a cart horse out of a born racer?

The pious appeal to God which was intended to blackjack the sensitive, religious boy of fifteen into submission in 1860 would to-day (thank God!) rebound like a tennis ball from the harder heads of our own younger generation. But it hit Cantor pretty hard. In fact it knocked him out cold. Loving his father devotedly and being of a deeply religious nature, young Cantor could not see that the old man was merely rationalizing his own greed for money. Thus began the first warping of Georg Cantor's acutely sensitive mind. Instead of rebelling, as a gifted boy to-day might do with some hope of success, Georg submitted till it became apparent even to the obstinate father that he was wrecking his son's disposition. But in the process of trying to please his father against the promptings of his own instincts Georg Cantor sowed the seeds of the self-distrust which was to make him an easy victim for Kronecker's vicious attack in later life and cause him to doubt the value of his work. Had Cantor been brought up as an independent human being he would never have acquired the timid deference to men of established reputation which made his life wretched.

The father gave in when the mischief was already done. On Georg's completion of his school course with distinction at the age of seventeen, he was permitted by 'dear papa' to seek a university career in mathematics. 'My dear papa!' Georg writes in his boyish gratitude: 'You can realize for yourself how greatly your letter delighted me. The letter fixes my future. . . . Now I am happy when I see that it will not displease you if I follow my feelings in the choice. I hope you will live to find joy in me, dear father; since my soul, my whole being, lives in my vocation; what a man desires to do, and that to which an inner compulsion drives him, that will he accomplish!' Papa no doubt deserves a vote of thanks, even if Georg's gratitude is a shade too servile for a modern taste.

Cantor began his university studies at Zurich in 1862, but migrated to the University of Berlin the following year, on the death of his father. At Berlin he specialized in mathematics, philosophy, and physics. The first two divided his interests about equally; for physics he never had any sure feeling. In mathematics his instructors were Kummer, Weierstrass, and his future enemy Kronecker. Following the usual German custom, Cantor spent a short time at another university, and was in residence for one semester of 1866 at Göttingen.

With Kummer and Kronecker at Berlin the mathematical atmosphere was highly charged with arithmetic. Cantor made a profound study of the *Disquisitiones Arithmeticae* of Gauss and wrote his dissertation, accepted for the Ph.D. degree in 1867, on a difficult point which Gauss had left aside concerning the solution in integers x, y, z of the indeterminate equation

$$ax^2 + by^2 + cz^2 = 0,$$

where a, b, c are any given integers. This was a fine piece of work, but it is safe to say that no mathematician who read it anticipated that the conservative author of twenty-two was to become one of the most radical originators in the history of mathematics. Talent no doubt is plain enough in this first attempt, but genius – no. There is not a single hint of the great originator in this severely classical dissertation.

The like may be said for all of Cantor's earliest work published before he was twenty-nine. It was excellent, but might have been done by any brilliant man who had thoroughly absorbed, as Cantor had, the doctrine of rigorous proof from Gauss and Weierstrass. Cantor's first love was the Gaussian theory of numbers, to which he was attracted by the hard, sharp, clear perfection of the proofs. From this, under the influence of the Weierstrassians, he presently branched off into rigorous analysis, particularly in the theory of trigonometric series (Fourier series).

The subtle difficulties of this theory (where questions of convergence of infinite series are less easily approachable than in the theory of power series) seem to have inspired Cantor to go deeper for the foundations of analysis than any of his contemporaries had cared to look, and he was led to his grand attack on the mathematics and philosophy of the infinite itself, which is at the bottom of all questions concerning continuity, limits, and convergence. Just before he was thirty, Cantor published his first revolutionary paper (in Crelle's *Journal*) on the theory of infinite sets. This will be described presently. The unexpected and paradoxical result concerning the set of *all* algebraic numbers which Cantor established in this paper and the complete novelty of the methods employed immediately marked the young author as a creative mathematician of extraordinary originality. Whether all agreed that the new methods were sound or not is beside the point; it was universally admitted that a man had arrived with something fundamentally new in mathematics. He should have been given an influential position at once.

Cantor's material career was that of any of the less eminent German professors of mathematics. He never achieved his ambition of a professorship at Berlin, possibly the highest German distinction during the period of Cantor's greatest and most original productivity (1874–84, age twenty-nine to thirty-nine). All his active professional career was spent at the University of Halle, a distinctly third-rate institution, where he was appointed *Privatdozent* (a lecturer who lives by what fees he can collect from his students) in 1869 at the age of twenty-four. In 1872 he was made assistant professor and in 1879 – before the criticism of his work had begun to assume the complexion of a malicious personal attack on himself – he was appointed full professor. His earliest teaching experience was in a girl's school in Berlin. For this curiously inappropriate task he had qualified himself by listening to dreary lectures on pedagogy by an uninspired mathematical mediocrity before securing his state licence to teach children. More social waste.

Rightly or wrongly, Cantor blamed Kronecker for his failure to obtain the coveted position at Berlin. The aggressive clannishness of Jews has often been remarked, sometimes as an argument against employing them in academic work, but it has not been so generally observed that there is no more vicious academic hatred than that of one Jew for another when they disagree on purely scientific matters or when one is jealous or afraid of another. Gentiles either laugh these hatreds off or go at them in an efficient, underhand way which often enables them to accomplish their spiteful ends under the guise of sincere friendship. When two intellectual Jews fall out they disagree all over, throw reserve to the dogs, and do everything in their power to cut one another's throats or stab one another in the back. Perhaps after all this is a more decent way of fighting – if men must fight – than the sanctimonious hypocrisy of the other. The object of any war is to destroy the enemy, and being sentimental or chivalrous about the unpleasant business is the mark of an incompetent fighter. Kronecker was one of the most competent warriors in the history of scientific controversy;

Cantor, one of the least competent. Kronecker won. But, as will appear later, Kronecker's bitter animosity towards Cantor was not wholly personal but at least partly scientific and disinterested.

The year 1874 which saw the appearance of Cantor's first revolutionary paper on the theory of sets was also that of his marriage, at the age of twenty-nine, to Vally Guttmann. Two sons and four daughters were born of this marriage. None of the children inherited their father's mathematical ability.

On their honeymoon at Interlaken the young couple saw a lot of Dedekind, perhaps the one first-rate mathematician of the time who made a serious and sympathetic attempt to understand Cantor's subversive doctrine.

Himself somewhat of a *persona non grata* to the leading German overlords of mathematics in the last quarter of the nineteenth century, the profoundly original Dedekind was in a position to sympathize with the scientifically disreputable Cantor. It is sometimes imagined by outsiders that originality is always assured of a cordial welcome in science. The history of mathematics contradicts this happy fantasy: the way of the transgressor in a well-established science is likely to be as hard as it is in any other field of human conservatism, even when the transgressor is admitted to have found something valuable by overstepping the narrow bounds of bigoted orthodoxy.

Both Dedekind and Cantor got what they might have expected had they paused to consider before striking out in new directions. Dedekind spent his entire working life in mediocre positions; the claim – now that Dedekind's work is recognized as one of the most important contributions to mathematics that Germany has ever made – that Dedekind *preferred* to stay in obscure holes while men who were in no sense his intellectual superiors shone like tin plates in the glory of public and academic esteem, strikes observers who are themselves 'Aryans' but not Germans as highly diluted eyewash.

The ideal of German scholarship in the nineteenth century was the lofty one of a thoroughly co-ordinated 'safety first', and perhaps rightly it showed an extreme Gaussian caution towards radical originality – the new thing might conceivably be not quite right. After all an honestly edited encyclopaedia is in general a more reliable source of information about the soaring habits of skylarks than a poem, say Shelley's, on the same topic.

In such an atmosphere of cloying alleged fact, Cantor's theory of the infinite – one of the most disturbingly original contributions to mathematics in the past 2,500 years – felt about as much freedom as a skylark trying to soar up through an atmosphere of cold glue. Even if the theory was totally wrong – and there are some who believe it cannot be salvaged in any shape resembling the thing Cantor thought he had launched – it deserved something better than the brickbats which were hurled at it chiefly because it was new and unbaptized in the holy name of orthodox mathematics.

The pathbreaking paper of 1874 undertook to establish a totally unexpected and highly paradoxical property of the set of *all* algebraic numbers. Although such numbers have been frequently described in preceding chapters, we shall state once more what they are, in order to bring out clearly the nature of the astounding fact which Cantor proved – in saying 'proved' we deliberately ignore for the present all doubts as to the soundness of the reasoning used by Cantor.

If r satisfies an algebraic equation of degree n with rational integer (common whole number) coefficients, and if r satisfies no such equation of degree less than n, then r is an algebraic number of degree n.

This can be generalized. For it is easy to prove that any root of an equation of the type

$$c_0x^n + c_1x^{n-1} + \dots + c_{n-1}x + c_n = 0,$$

in which the c's are any given *algebraic* numbers (as defined above), is itself an algebraic number. For example, according to this theorem, all roots of

$$(1 - 3\sqrt{-1})x^3 - (2 + 5\sqrt{17})\,x + \sqrt[3]{90} = 0$$

are algebraic numbers, since the coefficients are. (The first coefficient satisfies $x^2 - 2x + 10 = 0$, the second, $x^2 - 4x - 421 = 0$, the third, $x^3 - 90 = 0$, of the respective degrees 2, 2, 3).

Imagine (if you can) the set of *all* algebraic numbers. *Among* these will be *all* the positive rational integers 1, 2, 3, . . . , since any one of them, say n, satisfies an algebraic equation, $x - n = 0$, in which the coefficients (1, and $-n$) are rational integers. But *in addition to these* the set of *all* algebraic numbers will include *all* roots of *all* quadratic equations with rational integer coefficients, and *all* roots of *all* cubic equations with rational integer coefficients, and so on, indefinitely. Is it not *intuitively evident* that the set of *all* algebraic numbers will contain *infinitely more* members than its *sub*-set of the rational integers 1, 2, 3, . . . ? It might indeed be so, but it happens to be false.

Cantor proved that the set of all rational integers 1, 2, 3, . . . contains precisely as many members as the 'infinitely more inclusive' set of *all* algebraic numbers.

A proof of this paradoxical statement cannot be given here, but the kind of device – that of 'one-to-one correspondence' – upon which the proof is based can easily be made intelligible. This should induce in the philosophical mind an understanding of what a *cardinal number* is. Before describing this simple but somewhat elusive concept it will be helpful to glance at an expression of opinion on this and other definitions of Cantor's theory which emphasizes a distinction between the attitudes of some mathematicians and many philosophers toward all questions regarding 'number' or 'magnitude'.

'A mathematician never defines magnitudes in themselves, as a philosopher would be tempted to do; he defines their equality, their sum, and their product, and these definitions determine, or rather constitute, all the mathematical properties of magnitudes. In a yet more abstract and more formal manner he *lays down* symbols and at the same time *prescribes* the rules according to which they must be combined; these rules suffice to characterize these symbols and to give them a mathematical value. Briefly, he creates mathematical entities by means of arbitrary conventions, in the same way that the several chessmen are defined by the conventions which govern their moves and the relations between them.'* Not all schools of mathematical thought would subscribe to these opinions, but they suggest at least one 'philosophy' responsible for the following *definition* of cardinal numbers.

Note that the initial stage in the definition is the description of 'same cardinal number', in the spirit of Couturat's opening remarks; 'cardinal number' then arises phoenix-like from the ashes of its 'sameness'. It is all a matter of *relations* between concepts not explicitly defined.

Two sets are said to have *the same cardinal number* when all the things in the sets can be *paired off* one-to-one. After the pairing there are to be no unpaired things in either set.

Some examples will clarify this esoteric definition. It is one of those trivially obvious and fecund nothings which are so profound that they are overlooked for thousands of years. The sets (x, y, z), (a, b, c) have *the same cardinal number* (we shall not commit the blunder of saying 'Of course! Each contains *three letters*') *because* we can *pair off* the things x, y, z in the first set with those, a, b, c in the second as follows, x with a, y with b, z with c, and having done so, find that none remain unpaired in either set. Obviously there are other ways for effecting the pairing. Again, in a Christian community practising technical monogamy, if twenty married couples sit down together to dinner, the set of husbands will have the same cardinal number as the set of wives.

As another instance of this 'obvious' sameness, we recall

* L. Couturat, *De l'infini mathématique*, Paris, 1896, p. 49. With the caution that much of this work is now hopelessly out of date, it can be recommended for its clarity to the general reader. An account of the elements of Cantorism by a leading Polish expert which is within the comprehension of anyone with a grade-school education and a taste for abstract reasoning is the *Leçons sur les nombres transfinis*, by Waclaw Sierpinski, Paris, 1928. The preface by Borel supplies the necessary danger signal. The above extract from Couturat is of some historical interest in connexion with Hilbert's programme. It anticipates by thirty years Hilbert's statement of his formalist creed.

Galileo's example of the set of all squares of positive integers and the set of all positive integers:

$$1^2, 2^2, 3^2, 4^2, \ldots, n^2, \ldots$$
$$1, 2, 3, 4, \ldots, n, \ldots$$

The 'paradoxical' distinction between this and the preceding examples is apparent. If all the wives retire to the drawing room, leaving their spouses to sip port and tell stories, there will be precisely twenty human beings sitting at the table, just half as many as there were before. But if all the squares desert the natural numbers, there are just as many left as there were before. Dislike it or not as we may (we should not, if we are rational animals), the crude miracle stares us in the face that *a part of a set may have the same cardinal number as the entire set.* If anyone dislikes the 'pairing' definition of 'same cardinal number', he may be challenged to produce a comelier. Intuition (male, female, or mathematical) has been greatly overrated. Intuition is the root of all superstition.

Notice at this stage that a difficulty of the first magnitude has been glossed. *What is a set, or a class?* 'That', in the words of Hamlet, 'is the question'. We shall return to it, but we shall not answer it. Whoever succeeds in answering that innocent question to the entire satisfaction of Cantor's critics will quite likely dispose of the more serious objections against his ingenious theory of the infinite and at the same time establish mathematical analysis on a non-emotional basis. To see that the difficulty is not trivial, try to imagine the set of *all* positive rational integers, 1, 2, 3, . . . , and ask yourself whether, with Cantor, you can hold this totality – which is a 'class' – in your mind as a definite object of thought, as easily apprehended as the class x, y, z of three letters. Cantor requires us to do just this thing in order to reach the *transfinite* numbers which he created.

Proceeding now to the definition of 'cardinal number', we introduce a convenient technical term: two sets or classes whose members can be paired off one-to-one (as in the examples given previously) are said to be *similar. How many* things are there in the set (or class) x, y, z? Obviously three. But what is 'three'? An answer is contained in the following definition: 'The *number* of things in a given class is the *class* of all classes that are similar to the given class.'

This definition gains nothing from attempted explanation: it must be grasped as it is. It was proposed in 1879 by Gottlob Frege, and again (independently) by Bertrand Russell in 1901. One advantage which it has over other definitions of 'cardinal number of a class' is its applicability to both finite and infinite classes. Those who believe the definition too mystical for mathematics can avoid it by following Couturat's advice and not attempting to *define* 'cardinal number'. However, that way also leads to difficulties.

Cantor's spectacular result that the class of all algebraic numbers is similar (in the technical sense defined above) to its sub-class of all the positive rational integers was but the first of many wholly unexpected properties of infinite classes. Granting for the moment that his reasoning in reaching these properties is sound, or, if not unobjectionable in the form in which Cantor left it, that it can be made rigorous, we must admit its power.

Consider for example the 'existence' of transcendental numbers. In an earlier chapter we saw what a tremendous effort it cost Hermite to prove the transcendence of *a particular* number of this kind. Even to-day there is no general method known whereby the transcendence of any number which we suspect is transcendental can be proved; each new type requires the invention of special and ingenious methods. It is suspected, for example, that the number (it is a constant, although it looks as if it might be a variable from its definition) which is defined as the limit of

$$1 + \frac{1}{2} + \frac{1}{3} + \dots + \frac{1}{n} - \log n$$

as n tends to infinity, is transcendental, but we cannot prove that it is. What is required is to show that this constant is not a root of *any* algebraic equation with rational integer coefficients.

All this suggests the question 'How many transcendental numbers are there? Are they *more* numerous than the integers, or the rationals, or the algebraic numbers as a whole, or are they *less* numerous? Since (by Cantor's theorem) the integers, the rationals, and *all* algebraic numbers are equally numerous, the question amounts to this: can the transcendental numbers be counted off 1, 2, 3, ...? Is the class of all transcendental numbers *similar* to the class of all positive rational integers? The answer is no; the transcendentals are *infinitely more numerous than the integers.*

Here we begin to get into the controversial aspects of the theory of sets. The conclusion just stated was like a challenge to a man of Kronecker's temperament. Discussing Lindemann's proof that π is transcendental (see Chapter 24), Kronecker asked, 'Of what use is your beautiful investigation regarding π? Why study such problems, since irrational [and hence transcendental] numbers do not exist?' We can imagine the effect on such a scepticism of Cantor's proof that the transcendentals are infinitely more numerous than the integers 1, 2, 3, ... which, according to Kronecker, are the noblest work of God and the *only* numbers that *do* 'exist'.

Even a summary of Cantor's proof is out of the question here, but something of the kind of reasoning he used can be seen from the following simple considerations. If a class is similar (in the above technical sense) to the class of all positive rational integers, the class is said to be *denumerable.* The things in a denumerable class can be counted off 1, 2, 3, ...; the things in a non-denumerable class can *not* be counted off 1, 2, 3, ... : there will be more things in a non-denumerable class than in a denumerable class. Do non-denumerable classes exist? Cantor proved that they do. In fact the class of all points on any line-segment, no matter how small the segment is (provided it is more than a single point), is non-denumerable.

From this we see a hint of why the transcendentals are non-denumerable. In the chapter on Gauss we saw that any root of any algebraic equation is representable by a point on the plane of Cartesian geometry. All these roots constitute the set of all algebraic numbers, which Cantor proved to be denumerable. But if the points on a mere line-segment are non-denumerable, it follows that *all* the points on the Cartesian plane are likewise non-denumerable. The algebraic numbers are spotted over the plane like stars against a black sky; the dense blackness is the firmament of the transcendentals.

The most remarkable thing about Cantor's proof is that it provides no means whereby a single one of the transcendentals can be constructed. To Kronecker any such proof was sheer nonsense. Much milder instances of 'existence proofs' roused his wrath. One of these in particular is of interest as it prophesied Brouwer's objection to the full use of classical (Aristotelian) logic in reasoning about infinite sets.

A polynomial $ax^n + bx^{n-1} + \ldots + l$, in which the coefficients $a, b, \ldots l$ are rational numbers is said to be *irreducible* if it cannot be factored into a product of two polynomials both of which have rational number coefficients. Now, it is a meaningful statement to most human beings to assert, as Aristotle would, that a given polynomial either *is* irreducible or *is not* irreducible.

Not so for Kronecker. Until some definite process, capable of being carried out in a *finite* number of non-tentative steps, is provided whereby we can settle the reducibility of any given polynomial, we have no logical right, according to Kronecker, to use the concept of irreducibility in our mathematical proofs. To do otherwise, according to him, is to court inconsistencies in our conclusions and, at best, the use of 'irreducibility' without the process described can give us only a Scotch verdict of 'not proven'. All such *non-constructive* reasoning is – according to Kronecker – illegitimate.

As Cantor's reasoning in his theory of infinite classes is largely non-constructive, Kronecker regarded it as a dangerous type of mathematical insanity. Seeing mathematics headed for the madhouse under Cantor's leadership, and being passionately devoted to what he considered the truth of mathematics, Kronecker attacked 'the positive theory of infinity' and its hypersensitive author vigorously and viciously with every weapon that came to his hand, and the tragic outcome was that not the theory of sets went to the asylum, but Cantor. Kronecker's attack broke the creator of the theory.

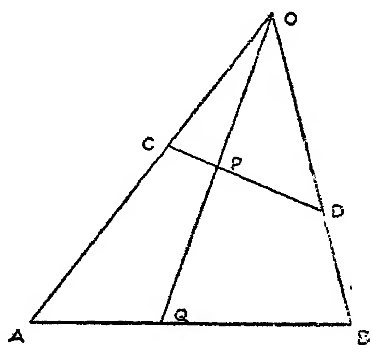
In the spring of 1884, in his fortieth year, Cantor experienced the first of those complete breakdowns which were to recur with varying intensity throughout the rest of his long life and drive him from society to the shelter of a mental clinic. His explosive temper aggravated his difficulty. Profound fits of depression humbled himself in his own eyes and he came to doubt the soundness of his work. During one lucid interval he begged the authorities at Halle to transfer him from his professorship of mathematics to a chair of philosophy. Some of his best work on the positive theory of the infinite was done in the intervals between one attack and the next. On recovering from a seizure he noticed that his mind became extraordinarily clear.

Kronecker perhaps has been blamed too severely for Cantor's tragedy; his attack was but one of many contributing causes. Lack of recognition embittered the man who believed he had taken the first – and last – steps toward a rational theory of the infinite and he brooded himself into melancholia and irrationality. Kronecker, however, does appear to have been largely responsible for Cantor's failure to obtain the position he craved in Berlin. It is usually considered not quite sporting for one scientist to deliver a savage attack on the work of a contemporary to his students. The disagreement can be handled objectively in scientific papers. Kronecker laid himself out in 1891 to criticize Cantor's work to his students at Berlin, and it became obvious that there was no room for both under one roof. As Kronecker was already in possession, Cantor resigned himself to staying out in the cold.

However, he was not without some comfort. The sympathetic Mittag-Leffler not only published some of Cantor's work in his journal (*Acta Mathematica*) but comforted Cantor in his fight against Kronecker. In one year alone Mittag-Leffler received no less than fifty-two letters from the suffering Cantor. Of those who believed in Cantor's theories, the genial Hermite was one of the most enthusiastic. His cordial acceptance of the new doctrine warmed Cantor's modest heart: 'The praises which Hermite pours out to me in this letter ... on the subject of the theory of sets are so high in my eyes, so unmerited, that I should not care to publish them lest I incur the reproach of being dazzled by them.'

With the opening of the new century Cantor's work gradually came to be accepted as a fundamental contribution to all mathematics and particularly to the foundations of analysis. But unfortunately for the theory itself the paradoxes and antinomies which still infect it began to appear simultaneously. These may in the end be the greatest contribution which Cantor's theory is destined to make to mathematics, for their unsuspected existence in the very rudiments of logical and mathematical reasoning about the infinite was the direct inspiration of the present critical movement in all deductive reasoning. Out of this we hope to derive a mathematics which is both richer and 'truer' – freer from inconsistency – than the mathematics of the pre-Cantor era.

Cantor's most striking results were obtained in the theory of *non-denumerable* sets, the simplest example of which is the set of all points on a line-segment. Only one of the simplest of his conclusions can be stated here. Contrary to what intuition would predict, two unequal line-segments contain the *same number* of points. Remembering that two sets contain the same number of things if, and only if, the things in them can be paired off one-to-one, we easily see the reasonableness of



Cantor's conclusion. Place the unequal segments AB, CD as in the figure. The line OPQ cuts CD in the point P, and AB in Q; P and Q are thus paired off. As OPQ rotates about O, the point P traverses CD, while Q simultaneously traverses AB, and each point of CD has one, and only one, 'paired' point of AB.

An even more unexpected result can be proved. Any line-segment, no matter how small, contains as many points as an infinite straight line. Further, the segment contains as many points as there are in an entire plane, or in the whole of three-dimensional space, or in the whole of space of n dimensions (where n is *any* integer greater than zero) or, finally, in a space of a denumerably infinite number of dimensions.

In all this we have not yet attempted to define a *class* or a *set*. Possibly (as Russell held in 1912) it is not necessary to do so in order to have a clear conception of Cantor's theory or for that theory to be consistent with itself – which is enough to demand of any mathematical theory. Nevertheless present disputes seem to require that some clear, self-consistent definition be given. The following used to be thought satisfactory.

A set is characterized by three qualities: it contains all things to which a certain definite property (say redness, or volume, or taste) belongs; no thing not having this property belongs to the set; each thing in the set is recognizable as the same thing and as different from all other things in the set – briefly, each thing in the set has a permanently recognizable individuality. The set itself is to be grasped as a whole. This definition may be too drastic for use. Consider, for example, what happens to Cantor's set of all transcendental numbers under the third demand.

At this point we may glance back over the whole history of mathematics – or as much of it as is revealed by the treatises of the master mathematicians in their purely technical works – and note two modes of expression which recur constantly in nearly all mathematical exposition. The reader perhaps has been irritated by the repetitious use of phrases such as 'we can find a whole number greater than 2', or 'we can choose a number less than n and greater than $n - 2$.' The choice of such phraseology is not merely stereotyped pedantry. There is a reason for its use, and careful writers mean exactly what they say when they assert that 'we can find, etc'. They mean that *they can do what they say*.

In sharp distinction to this is the other phrase which is reiterated over and over again in mathematical writing: 'There exists.' For example, some would say 'there exists a whole number greater than 2', or 'there exists a number less than n and greater than $n - 2$.' The use of such phraseology definitely commits its user to the creed which Kronecker held to be untenable, *unless*, of course, the 'existence' is proved by a *construction*. The existence is not proved for the sets (as defined above) which appear in Cantor's theory.

These two ways of speaking divide mathematicians into two types: the 'we can' men believe (possibly subconsciously) that mathematics is a purely human invention; the 'there exists' men believe that mathematics has an extra-human 'existence' of its own, and that 'we' merely come upon the 'eternal truths' of mathematics in our journey through life, in much the same way that a man taking a walk in a city comes across a number of streets with whose planning he had nothing whatever to do.

Theologians are 'exist' men; cautious sceptics for the most part 'we' men. 'There exist an infinity of even numbers, or of primes', say the advocates of extra-human 'existence'; 'produce them', say Kronecker and the 'we' men.

That the distinction is not trivial can be seen from a famous instance of it in the New Testament. Christ asserted that the Father 'exists'; Philip demanded 'Show us the Father and it sufficeth us.' Cantor's theory is almost wholly on the 'existence' side. Is it possible that Cantor's passion for theology determined his allegiance? If so, we shall have to explain why Kronecker, also a connoisseur of Christian theology, was the rabid 'we' man that he was. As in all such questions ammunition for either side can be filched from any pocket.

A striking and important instance of the 'existence' way of looking at the theory of sets is afforded by what is known as Zermelo's postulate (stated in 1904). 'For every set M whose elements are sets P (that is, M is a set of *sets*, or a class of *classes*), the sets P being non-empty and non-overlapping (no two contain things in common), there exists at least one set N which contains precisely one element from each of the sets P which constitute M.' Comparison of this with the previously stated definition of a set (or class) will show that the 'we' men would not consider the postulate self-evident if the set M consisted, say, of an infinity of non-overlapping line segments. Yet the postulate seems reasonable enough. Attempts to prove it have failed. It is of considerable importance in all questions relating to continuity.

A word as to how this postulate came to be introduced into mathematics will suggest another of the unsolved problems of Cantor's theory. A set of distinct, *countable* things, like all the bricks in a certain wall, can easily be *ordered*; we need only count them off 1, 2, 3, ... in any of dozens of different ways that will suggest themselves. But how would we go about *ordering* all the points on a straight line? They cannot be counted off 1, 2, 3, ... The task appears hopeless when we consider that between *any* two points of the line 'we can find', or 'there exists' *another* point of the line. If every time we counted two adjacent bricks another sprang into being between them in the wall our counting would become slightly confused. Nevertheless the points on a straight line do appear to have some sort of order; we can say whether one point is to the right or the left of another, and so on. Attempts to order the points of a line have not succeeded. Zermelo proposed his postulate as a means for making the attempt easier, but it itself is not universally accepted as a reasonable assumption or as one which it is safe to use.

Cantor's theory contains a great deal more about the actual infinite and the 'arithmetic' of transfinite (infinite) numbers than what has been indicated here. But as the theory is still in the controversial stage, we may leave it with the statement of a last riddle. Does there 'exist', or can we 'construct', an infinite set which is not similar (technical sense of one-to-one matching) either to the set of all the positive rational integers or to the set of all points of a line? The answer is unknown.

Cantor died in a mental hospital in Halle on 6 January 1918 at the age of seventy-three. Honours and recognition were his at the last, and even the old bitterness against Kronecker was forgotten. It was no doubt a satisfaction to Cantor to recall that he and Kronecker had become at least superficially reconciled some years before Kronecker's death in 1891. Could Cantor have lived till to-day he might have taken a just pride in the movement toward more rigorous thinking in *all* mathematics for which his own efforts to found analysis (and the infinite) on a sound basis were largely responsible.

Looking back over the long struggle to make the concepts of *real number*, *continuity*, *limit*, and *infinity* precise and consistently usable in mathematics, we see that Zeno and Eudoxus were not so far in time from Weierstrass, Dedekind, and Cantor as the twenty-four or twenty-five centuries which separate modern Germany from ancient Greece might seem to imply. There is no doubt that we have a clearer conception of the nature of the difficulties involved than our predecessors had, because we see the same unsolved problems cropping up in new guises and in fields the ancients never dreamed of, but to say that we have disposed of those hoary old difficulties is a gross mis-statement of fact. Nevertheless the net score records a greater gain than any which our predecessors could rightfully claim. We are going deeper than they ever imagined necessary, and we are discovering that some of the 'laws' – for instance those of Aristotelian logic – which they accepted in their reasoning are better replaced by others – pure conventions – in our attempts to correlate our experiences. As has already been said, Cantor's revolutionary work gave our present activity its initial impulse. But it was soon discovered – twenty-one years before Cantor's death – that his revolution was either too revolutionary or not revolutionary enough. The latter now appears to be the case.

The first shot in the counter-revolution was fired in 1897 by the Italian mathematician Burali-Forti who produced a flagrant contradiction by reasoning of the type used by Cantor in his theory of infinite sets. This particular paradox was only the first of several, and as it would require lengthy explanations to make it intelligible, we shall state instead Russell's of 1908.

We have already mentioned Frege, who gave the 'class of all classes similar to a given class' definition of the cardinal number of the given class. Frege had spent years trying to put the mathematics of numbers on a sound logical basis. His life work is his *Grundgesetze der Arithmetik* (The Fundamental Laws of Arithmetic), of which the first volume was published in 1893, the second in 1903. In this work the concept of sets is used. There is also a considerable use of more or less sarcastic invective against previous writers on the foundations of arithmetic for their manifest blunders and manifold stupidities. The second volume closes with the following acknowledgement.

> A scientist can hardly encounter anything more undesirable than to have the foundation collapse just as the work is finished. I was put in this position by a letter from Mr Bertrand Russell when the work was almost through the press.

Russell had sent Frege his ingenious paradox of 'the set of all sets which are not members of themselves.' Is this set a member of itself? Either answer can be puzzled out with a little thought to be wrong. Yet Frege had freely used 'sets of all sets'.

Many ways were proposed for evading or eliminating the contradictions which began exploding like a barrage in and over the Frege-Dedekind-Cantor theory of the real numbers, continuity, and the infinite. Frege, Cantor, and Dedekind quit the field, beaten and disheartened. Russell proposed his 'vicious circle principle' as a remedy: 'Whatever involves all of a collection must not be one of the collection'; later he put forth his 'axiom of reducibility', which, as it is now practically abandoned, need not be described. For a time these restoratives were brilliantly effective (except in the opinion of the German mathematicians, who never swallowed them). Gradually, as the critical examination of all mathematical reasoning gained headway, physic was thrown to the dogs and a concerted effort was begun to find out what really ailed the patient in his irrational and real number system before administering further nostrums.

The present effort to understand our difficulties originated in the work of David Hilbert (1862–1943) of Göttingen in 1899 and in that of L. E. J. Brouwer (1881–) of Amsterdam in 1912. Both of these men and their numerous followers have the common purpose of putting mathematical reasoning on a sound basis, although in several respects their methods and philosophies are violently opposed. It seems unlikely that both can be as wholly right as each appears to believe he is.

Hilbert returned to Greece for the beginning of his philosophy of mathematics. Resuming the Pythagorean programme of a rigidly and fully stated set of postulates from which a mathematical argument must proceed by strict deductive reasoning, Hilbert made the programme of the *postulational* development of mathematics more precise than it had been with the Greeks, and in 1899 issued the first edition of his classic on the foundations of geometry. One demand which Hilbert made, and which the Greeks do not seem to have thought of, was that the proposed postulates for geometry shall be *proved* to be self-consistent (free of internal, concealed contradictions). To produce such a proof for geometry it is shown that any contradiction in the geometry developed from the postulates would imply a contradiction in arithmetic. The problem is thus shoved back to proving the consistency of arithmetic, and there it remains to-day.

Thus we are back once more asking the sphinx to tell us what a number is. Both Dedekind and Frege fled to the infinite – Dedekind with his infinite classes defining irrationals, Frege with his class of all classes similar to a given class defining a cardinal number – to interpret the numbers that puzzled Pythagoras. Hilbert, too, would seek the answer in the infinite which, he believes, is necessary for an understanding of the finite. He is quite emphatic in his belief that Cantorism will ultimately be redeemed from the purgatory in which it now tosses. 'This [Cantor's theory] seems to me the most admirable fruit of the mathematical mind and indeed one of the highest achievements of man's intellectual processes.' But he admits that the paradoxes of Burali-Forti, Russell, and others are not resolved. However, his faith surmounts all doubts: 'No one shall expel us from the paradise which Cantor has created for us.'

But at this moment of exaltation Brouwer appears with something that looks suspiciously like a flaming sword in his strong right hand. The chase is on: Dedekind, in the role of Adam, and Cantor disguised as Eve at his side, are already eyeing the gate apprehensively under the stern regard of the uncompromising Dutchman. The postulational method for securing freedom from contradiction proposed by Hilbert will, says, Brouwer, accomplish its end – produce no contradictions, *but* 'nothing of mathematical value will be attained in this manner; a false theory which is not stopped by a contradiction is none the less false, just as a criminal policy unchecked by a reprimanding court is none the less criminal.'

The root of Brouwer's objection to the 'criminal policy' of his opponents is something new – at least in mathematics. He objects to an unrestricted use of Aristotelian logic, particularly in dealing with *infinite* sets, and he maintains that such logic is bound to produce contradictions when applied to sets which cannot be definitely *constructed* in Kronecker's sense (a rule of procedure must be given whereby the things in the set can be produced). The law of 'excluded middle' (a thing must have a certain property or must not have that property, as for example in the assertion that a number is prime or is not prime) is legitimately usable only when applied to *finite* sets. Aristotle devised his logic as a body of working rules for finite sets, basing his method on human experience of *finite* sets, and there is no reason whatever for supposing that a logic which is adequate for the *finite* will continue to produce consistent (not contradictory) results when applied to the *infinite*. This seems reasonable enough when we recall that the very definition of an infinite set emphasizes that a *part* of an *infinite* set may contain precisely *as many* things as the *whole* set (as we have illustrated many times), a situation which *never* happens for a finite set when 'part' means *some, but not all* (as it does in the definition of an infinite set).

Here we have what some consider the root of the trouble in Cantor's theory of the actual infinite. For the *definition* of a set (as stated some time back), by which *all* things having a certain quality are 'united' to form a 'set' (or 'class'), is not suitable as a basis for the theory of sets, in that the definition either is *not constructive* (in Kronecker's sense) or *assumes* a constructibility which no mortal can produce. Brouwer claims that the use of the law of excluded middle in such a situation is at best merely a heuristic guide to propositions which *may be* true, but which are not necessarily so, even when they have been deduced by a rigid application of Aristotelian logic, and he says that numerous false theories (including Cantor's) have been erected on this rotten foundation during the past half century.

Such a revolution in the rudiments of mathematical thinking does not go unchallenged. Brouwer's radical move to the left is speeded by an outraged roar from the reactionary right. 'What Weyl and Brouwer are doing [Brouwer is the leader, Weyl his companion in revolt] is mainly following in the steps of Kronecker', according to Hilbert, the champion of the *status quo*. 'They are trying to establish mathematics by jettisoning everything which does not suit them and setting up an embargo. The effect is to dismember our science and to run the risk of losing part of our most valuable possessions. Weyl and Brouwer condemn the general notions of irrational numbers, of functions – even of such functions as occur in the theory of numbers – Cantor's transfinite numbers, etc., the theorem that an infinite set of positive integers has a least, and even the "law of excluded middle", as for example the assertion: Either there is only a finite number of primes or there are infinitely many. These are examples of [to them] forbidden theorems and modes of reasoning. I believe that impotent as Kronecker was to abolish irrational numbers (Weyl and Brouwer do permit us to retain a torso), no less impotent will their efforts prove to-day. No! Brouwer's programme is not a revolution, but merely the repetition of a futile *coup de main* with old methods, but which was then undertaken with greater verve, yet failed utterly. To-day the State [mathematics] is thoroughly armed and strengthened through the labours of Frege, Dedekind, and Cantor. The efforts of Brouwer and Weyl are foredoomed to futility.'

To which the other side replies by a shrug of the shoulders and goes ahead with its great and fundamentally new task of re-establishing mathematics (particularly the foundations of analysis) on a firmer basis than any laid down by the men of the past 2,500 years from Pythagoras to Weierstrass.

What will mathematics be like a generation hence when – we hope – these difficulties will have been cleared up? Only a prophet or the seventh son of a prophet sticks his head into the noose of prediction. But if there is any continuity at all in the evolution of mathematics – and the majority of dispassionate observers believe that there is – we shall find that the mathematics which is to come will be broader, firmer, and richer in content than that which we or our predecessors have known.

Already the controversies of the past third of a century have added new fields – including totally new logics – to the vast domain of mathematics, and the new is being rapidly consolidated and co-ordinated with the old. If we may rashly venture a prediction, what is to come will be fresher, younger in every respect, and closer to human thought and human needs – freer of appeal for its justification to extra-human 'existences' – than what is now being vigorously refashioned. The spirit of mathematics is eternal youth. As Cantor said, 'The essence of mathematics resides in its freedom'; the present 'revolution' is but another assertion of that freedom.

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