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MATHEMATICAL MONOGRAPHS

EDITED BY
MANSFIELD MERRIMAN AND ROBERT S. WOODWARD

No. 17

LECTURES ON
TEN BRITISH MATHEMATICIANS
OF THE NINETEENTH CENTURY

BY

ALEXANDER MACFARLANE,

Late President for the International Association for Promoting
the Study of Quaternions
1916

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No. 17. Ten British Mathematicians.
By Alexander Macfarlane.

PREFACE

During the years 1901-1904 Dr. Alexander Macfarlane delivered, at Lehigh University, lectures on twenty-five British mathematicians of the nineteenth century. The manuscripts of twenty of these lectures have been found to be almost ready for the printer, although some marginal notes by the author indicate that he had certain additions in view. The editors have felt free to disregard such notes, and they here present ten lectures on ten pure mathematicians in essentially the same form as delivered. In a future volume it is hoped to issue lectures on ten mathematicians whose main work was in physics and astronomy.

These lectures were given to audiences composed of students, instructors and townspeople, and each occupied less than an hour in delivery. It should hence not be expected that a lecture can fully treat of all the activities of a mathematician, much less give critical analyses of his work and careful estimates of his influence. It is felt by the editors, however, that the lectures will prove interesting and inspiring to a wide circle of readers who have no acquaintance at first hand with the works of the men who are discussed, while they cannot fail to be of special interest to older readers who have such acquaintance.

It should be borne in mind that expressions such as "now," "recently," "ten years ago," etc., belong to the year when a lecture was delivered. On the first page of each lecture will be found the date of its delivery.

For six of the portraits given in the frontispiece the editors are indebted to the kindness of Dr. David Eugene Smith, of Teachers College, Columbia University.

Alexander Macfarlane was born April 21, 1851, at Blairgowrie, Scotland. From 1871 to 1884 he was a student, instructor and examiner in physics at the University of Edinburgh, from 1885 to 1894 professor of physics in the University of Texas, and from 1895 to 1908 lecturer in electrical engineering and mathematical physics in Lehigh University. He was the author of papers on algebra of logic, vector analysis and quaternions, and of Monograph No. 8 of this series. He was twice secretary of the section of physics of the American Association for the Advancement of Science, and twice vice-president of the section of mathematics and astronomy. He was one of the founders of the International Association for Promoting the Study of Quaternions, and its president at the time of his death, which occured at Chatham, Ontario, August 28, 1913. His personal acquaintance with British mathematicians of the nineteenth century imparts to many of these lectures a personal touch which greatly adds to their general interest.

Alexander Macfarlane
From a photograph of 1898

Contents

Chapter 1

GEORGE PEACOCK[1]

(1791-1858)

George Peacock was born on April 9, 1791, at Denton in the north of England, 14 miles from Richmond in Yorkshire. His father, the Rev. Thomas Peacock, was a clergyman of the Church of England, incumbent and for 50 years curate of the parish of Denton, where he also kept a school. In early life Peacock did not show any precocity of genius, and was more remarkable for daring feats of climbing than for any special attachment to study. He received his elementary education from his father, and at 17 years of age, was sent to Richmond, to a school taught by a graduate of Cambridge University to receive instruction preparatory to entering that University. At this school he distinguished himself greatly both in classics and in the rather elementary mathematics then required for entrance at Cambridge. In 1809 he became a student of Trinity College, Cambridge.

Here it may be well to give a brief account of that University, as it was the alma mater of four out of the six mathematicians discussed in this course of lectures[2].

At that time the University of Cambridge consisted of seventeen colleges, each of which had an independent endowment, buildings, master, fellows and scholars. The endowments, generally in the shape of lands, have come down from ancient times; for example, Trinity College was founded by Henry VIII in 1546, and at the beginning of the 19th century it consisted of a master,

[1]This Lecture was delivered April 12, 1901.—Editors.

[2]Dr. Macfarlane's first course included the first six lectures given in this volume.—Editors.

60 fellows and 72 scholars. Each college was provided with residence halls, a dining hall, and a chapel. Each college had its own staff of instructors called tutors or lecturers, and the function of the University apart from the colleges was mainly to examine for degrees. Examinations for degrees consisted of a pass examination and an honors examination, the latter called a tripos. Thus, the mathematical tripos meant the examinations of candidates for the degree of Bachelor of Arts who had made a special study of mathematics. The examination was spread over a week, and those who obtained honors were divided into three classes, the highest class being called *wranglers*, and the highest man among the wranglers, *senior wrangler*. In more recent times this examination developed into what De Morgan called a "great writing race;" the questions being of the nature of short problems. A candidate put himself under the training of a coach, that is, a mathematician who made it a business to study the kind of problems likely to be set, and to train men to solve and write out the solution of as many as possible per hour. As a consequence the lectures of the University professors and the instruction of the college tutors were neglected, and nothing was studied except what would pay in the tripos examination. Modifications have been introduced to counteract these evils, and the conditions have been so changed that there are now no senior wranglers. The tripos examination used to be followed almost immediately by another examination in higher mathematics to determine the award of two prizes named the Smith's prizes. "Senior wrangler" was considered the greatest academic distinction in England.

In 1812 Peacock took the rank of second wrangler, and the second Smith's prize, the senior wrangler being John Herschel. Two years later he became a candidate for a fellowship in his college and won it immediately, partly by means of his extensive and accurate knowledge of the classics. A fellowship then meant about £200 a year, tenable for seven years provided the Fellow did not marry meanwhile, and capable of being extended after the seven years provided the Fellow took clerical Orders. The limitation to seven years, although the Fellow devoted himself exclusively to science, cut short and prevented by anticipation the career of many a laborer for the advancement of science. Sir Isaac Newton was a Fellow of Trinity College, and its limited terms nearly deprived the world of the *Principia*.

The year after taking a Fellowship, Peacock was appointed a tutor and lecturer of his college, which position he continued to hold for many years. At that time the state of mathematical learning at Cambridge was discreditable. How could that be? you may ask; was not Newton a professor of mathematics in that University? did he not write the *Principia* in Trinity College? had his influence died out so soon? The true reason was he was worshipped too much as an authority; the University had settled down to the study of Newton instead of Nature, and they had followed him in one grand mistake—the ignoring of the differential notation in the calculus. Students of the differential calculus are more or less familiar with the controversy which raged over the respective claims of Newton and Leibnitz to the invention of the calculus; rather over the question whether Leibnitz was an independent inventor, or appropriated the fundamental ideas from Newton's writings and correspondence, merely giving them a new clothing in the form of the differential notation. Anyhow, Newton's countrymen adopted the latter alternative; they clung to the fluxional notation of Newton; and following Newton, they ignored the notation of Leibnitz and everything written in that notation. The Newtonian notation is as follows: If y denotes a fluent, then $\dot{y}$ denotes its fluxion, and $\ddot{y}$ the fluxion of $\dot{y}$; if y itself be considered a fluxion, then y' denotes its fluent, and y'' the fluent of y' and so on; a differential is denoted by o. In the notation of Leibnitz $\dot{y}$ is written $\frac{dy}{dx}$, $\ddot{y}$ is written $\frac{d^2y}{dx^2}$, y' is $\int y dx$, and so on. The result of this Chauvinism on the part of the British mathematicians of the eighteenth century was that the developments of the calculus were made by the contemporary mathematicians of the Continent, namely, the Bernoullis, Euler, Clairault, Delambre, Lagrange, Laplace, Legendre. At the beginning of the 19th century, there was only one mathematician in Great Britain (namely Ivory, a Scotsman) who was familiar with the achievements of the Continental mathematicians. Cambridge University in particular was wholly given over not merely to the use of the fluxional notation but to ignoring the differential notation. The celebrated saying of Jacobi was then literally true, although it had ceased to be true when he gave it utterance. He visited Cambridge about 1842. When dining as a guest at the high table of one of the colleges he was asked who in his opinion was the greatest of the living mathematicians of England; his reply was "There is none."

Peacock, in common with many other students of his own standing, was profoundly impressed with the need of reform, and while still an undergraduate formed a league with Babbage and Herschel to adopt measures to bring it about. In 1815 they formed what they called the *Analytical Society*, the object of which was stated to be to advocate the *d*'ism of the Continent versus the *dot*-age of the University. Evidently the members of the new society were armed with wit as well as mathematics. Of these three reformers, Babbage afterwards became celebrated as the inventor of an analytical engine, which could not only perform the ordinary processes of arithmetic, but, when set with the proper data, could tabulate the values of any function and print the results. A part of the machine was constructed, but the inventor and the Government (which was supplying the funds) quarrelled, in consequence of which the complete machine exists only in the form of drawings. These are now in the possession of the British Government, and a scientific commission appointed to examine them has reported that the engine could be constructed. The third reformer—Herschel—was a son of Sir William Herschel, the astronomer who discovered Uranus, and afterwards as Sir John Herschel became famous as an astronomer and scientific philosopher.

The first movement on the part of the Analytical Society was to translate from the French the smaller work of Lacroix on the differential and integral calculus; it was published in 1816. At that time the best manuals, as well as the greatest works on mathematics, existed in the French language. Peacock followed up the translation with a volume containing a copious *Collection of Examples of the Application of the Differential and Integral Calculus*, which was published in 1820. The sale of both books was rapid, and contributed materially to further the object of the Society. Then high wranglers of one year became the examiners of the mathematical tripos three or four years afterwards. Peacock was appointed an examiner in 1817, and he did not fail to make use of the position as a powerful lever to advance the cause of reform. In his questions set for the examination the differential notation was for the first time officially employed in Cambridge. The innovation did not escape censure, but he wrote to a friend as follows: "I assure you that I shall never cease to exert myself to the utmost in the cause of reform, and that I will never decline any office which may increase my power to effect it. I am nearly certain of being nominated to the office of Moderator in the year 1818-1819, and as I am an examiner in virtue of my office, for the next year I shall pursue a course even more decided than hitherto, since I shall feel that men have been prepared for the change, and will then be enabled to have acquired a better system by the publication of improved elementary books. I have considerable influence as a lecturer, and I will not neglect it. It is by silent perseverance only, that we can hope to reduce the many-headed monster of prejudice and make the University answer her character as the loving mother of good learning and science." These few sentences give an insight into the character of Peacock: he was an ardent reformer and a few years brought success to the cause of the Analytical Society.

Another reform at which Peacock labored was the teaching of algebra. In 1830 he published a *Treatise on Algebra* which had for its object the placing of algebra on a true scientific basis, adequate for the development which it had received at the hands of the Continental mathematicians. As to the state of the science of algebra in Great Britain, it may be judged of by the following facts. Baron Maseres, a Fellow of Clare College, Cambridge, and William Frend, a second wrangler, had both written books protesting against the use of the negative quantity. Frend published his *Principles of Algebra* in 1796, and the preface reads as follows: "The ideas of number are the clearest and most distinct of the human mind; the acts of the mind upon them are equally simple and clear. There cannot be confusion in them, unless numbers too great for the comprehension of the learner are employed, or some arts are used which are not justifiable. The first error in teaching the first principles of algebra is obvious on perusing a few pages only of the first part of Maclaurin's *Algebra*. Numbers are there divided into two sorts, positive and negative; and an attempt is made to explain the nature of negative numbers by allusion to book debts and other arts. Now when a person cannot explain the principles of a science without reference to a metaphor, the probability is, that he has never thought accurately upon the subject. A number may be greater or less than another number; it may be added to, taken from, multiplied into, or divided by, another number; but in other respects it is very intractable; though the whole world should be destroyed, one will be one, and three will be three, and no art whatever can change their nature. You may put a mark before one, which it will obey; it submits to be taken away from a number greater than itself, but to attempt to take it away from a number less than itself is ridiculous. Yet this is attempted by algebraists who talk of a number less than nothing; of multiplying a negative number into a negative number and thus producing a positive number; of a number being imaginary. Hence they talk of two roots to every equation of the second order, and the learner is to try which will succeed in a given equation; they talk of solving an equation which requires two impossible roots to make it soluble; they can find out some impossible numbers which being multiplied together produce unity. This is all jargon, at which common sense recoils; but from its having been once adopted, like many other figments, it finds the most strenuous supporters among those who love to take things upon trust and hate the colour of a serious thought." So far, Frend. Peacock knew that Argand, Français and Warren had given what seemed to be an explanation not only of the negative quantity but of the imaginary, and his object was to reform the teaching of algebra so as to give it a true scientific basis.

At that time every part of exact science was languishing in Great Britain. Here is the description given by Sir John Herschel: "The end of the 18th and the beginning of the 19th century were remarkable for the small amount of scientific movement going on in Great Britain, especially in its more exact departments. Mathematics were at the last gasp, and Astronomy nearly so—I mean in those members of its frame which depend upon precise measurement and systematic calculation. The chilling torpor of routine had begun to spread itself over all those branches of Science which wanted the excitement of experimental research." To elevate astronomical science the Astronomical Society of London was founded, and our three reformers Peacock, Babbage and Herschel were prime movers in the undertaking. Peacock was one of the most zealous promoters of an astronomical observatory at Cambridge, and one of the founders of the Philosophical Society of Cambridge.

The year 1831 saw the beginning of one of the greatest scientific organizations of modern times. That year the British Association for the Advancement of Science (prototype of the American, French and Australasian Associations) held its first meeting in the ancient city of York. Its objects were stated to be: first, to give a stronger impulse and a more systematic direction to scientific enquiry; second, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers; third, to obtain a more general attention to the objects of science, and the removal of any disadvantages of a public kind which impede its progress. One of the first resolutions adopted was to procure reports on the state and progress of particular sciences, to be drawn up from time to time by competent persons for the information of the annual meetings, and the first to be placed on the list was a report on the progress of mathematical science. Dr. Whewell, the mathematician and philosopher, was a Vice-president of the meeting: he was instructed to select the reporter. He first asked Sir W. R. Hamilton, who declined; he then asked Peacock, who accepted. Peacock had his report ready for the third meeting of the Association, which was held in Cambridge in 1833; although limited to Algebra, Trigonometry, and the Arithmetic of Sines, it is one of the best of the long series of valuable reports which have been prepared for and printed by the Association.

In 1837 he was appointed Lowndean professor of astronomy in the University of Cambridge, the chair afterwards occupied by Adams, the co-discoverer of Neptune, and now occupied by Sir Robert Ball, celebrated for his *Theory of Screws*. In 1839 he was appointed Dean of Ely, the diocese of Cambridge. While holding this position he wrote a text book on algebra in two volumes, the one called *Arithmetical Algebra*, and the other *Symbolical Algebra*. Another object of reform was the statutes of the University; he worked hard at it and was made a member of a commission appointed by the Government for the purpose; but he died on November 8, 1858, in the 68th year of his age. His last public act was to attend a meeting of the Commission.

Peacock's main contribution to mathematical analysis is his attempt to place algebra on a strictly logical basis. He founded what has been called the philological or symbolical school of mathematicians; to which Gregory, De Morgan and Boole belonged. His answer to Maseres and Frend was that the science of algebra consisted of two parts—arithmetical algebra and symbolical algebra—and that they erred in restricting the science to the arithmetical part. His view of arithmetical algebra is as follows: "In arithmetical algebra we consider symbols as representing numbers, and the operations to which they are submitted as included in the same definitions as in common arithmetic; the signs $+$ and $-$ denote the operations of addition and subtraction in their ordinary meaning only, and those operations are considered as impossible in all cases where the symbols subjected to them possess values which would render them so in case they were replaced by digital numbers; thus in expressions such as $a + b$ we must suppose a and b to be quantities of the same kind; in others, like $a - b$, we must suppose a greater than b and therefore homogeneous with it; in products and quotients, like ab and $\frac{a}{b}$ we must suppose the multiplier and divisor to be abstract numbers; all results whatsoever, including negative quantities, which are not strictly deducible as legitimate conclusions from the definitions of the several operations must be rejected as impossible, or as foreign to the science."

Peacock's principle may be stated thus: the elementary symbol of arithmetical algebra denotes a digital, i.e., an integer number; and every combination of elementary symbols must reduce to a digital number, otherwise it is impossible or foreign to the science. If a and b are numbers, then $a + b$ is always a number; but $a - b$ is a number only when b is less than a. Again, under the same conditions, ab is always a number, but $\frac{a}{b}$ is really a number only when b is an exact divisor of a. Hence we are reduced to the following dilemma: Either $\frac{a}{b}$ must be held to be an impossible expression in general, or else the meaning of the fundamental symbol of algebra must be extended so as to include rational fractions. If the former horn of the dilemma is chosen, arithmetical algebra becomes a mere shadow; if the latter horn is chosen, the operations of algebra cannot be defined on the supposition that the elementary symbol is an integer number. Peacock attempts to get out of the difficulty by supposing that a symbol which is used as a multiplier is always an integer number, but that a symbol in the place of the multiplicand may be a fraction. For instance, in ab, a can denote only an integer number, but b may denote a rational fraction. Now there is no more fundamental principle in arithmetical algebra than that $ab = ba$; which would be illegitimate on Peacock's principle.

One of the earliest English writers on arithmetic is Robert Record, who dedicated his work to King Edward the Sixth. The author gives his treatise the form of a dialogue between master and scholar. The scholar battles long over this difficulty,—that multiplying a thing could make it less. The master attempts to explain the anomaly by reference to proportion; that the product due to a fraction bears the same proportion to the thing multiplied that the fraction bears to unity. But the scholar is not satisfied and the master goes on to say: "If I multiply by more than one, the thing is increased; if I take it but once, it is not changed, and if I take it less than once, it cannot be so much as it was before. Then seeing that a fraction is less than one, if I multiply by a fraction, it follows that I do take it less than once." Whereupon the scholar replies, "Sir, I do thank you much for this reason,—and I trust that I do perceive the thing."

The fact is that even in arithmetic the two processes of multiplication and division are generalized into a common multiplication; and the difficulty consists in passing from the original idea of multiplication to the generalized idea of a *tensor*, which idea includes compressing the magnitude as well as stretching it. Let m denote an integer number; the next step is to gain the idea of the reciprocal of m, not as $\frac{1}{m}$ but simply as $/m$. When m and $/n$ are compounded we get the idea of a rational fraction; for in general m/n will not reduce to a number nor to the reciprocal of a number.

Suppose, however, that we pass over this objection; how does Peacock lay the foundation for general algebra? He calls it symbolical algebra, and he passes from arithmetical algebra to symbolical algebra in the following manner: "Symbolical algebra adopts the rules of arithmetical algebra but removes altogether their restrictions; thus symbolical subtraction differs from the same operation in arithmetical algebra in being possible for all relations of value of the symbols or expressions employed. All the results of arithmetical algebra which are deduced by the application of its rules, and which are general in form though particular in value, are results likewise of symbolical algebra where they are general in value as well as in form; thus the product of a^m and a^n which is a^{m+n} when m and n are whole numbers and therefore general in form though particular in value, will be their product likewise when m and n are general in value as well as in form; the series for $(a+b)^n$ determined by the principles of arithmetical algebra when n is any whole number, *if it be exhibited in a general form, without reference to a final term*, may be shown upon the same principle to the equivalent series for $(a+b)^n$ when n is general both in form and value."

The principle here indicated by means of examples was named by Peacock the "principle of the permanence of equivalent forms," and at page 59 of the *Symbolical Algebra* it is thus enunciated: "Whatever algebraical forms are equivalent when the symbols are general in form, but specific in value, will be equivalent likewise when the symbols are general in value as well as in form."

For example, let a, b, c, d denote any integer numbers, but subject to the restrictions that b is less than a, and d less than c; it may then be shown arithmetically that

$$(a-b)(c-d) = ac + bd - ad - bc.$$

Peacock's principle says that the form on the left side is equivalent to the form on the right side, not only when the said restrictions of being less are removed, but when a, b, c, d denote the most general algebraical symbol. It means that a, b, c, d may be rational fractions, or surds, or imaginary quantities, or indeed operators such as $\frac{d}{dx}$. The equivalence is not established by means of the nature of the quantity denoted; the equivalence is assumed to be true, and then it is attempted to find the different interpretations which may be put on the symbol.

It is not difficult to see that the problem before us involves the fundamental problem of a rational logic or theory of knowledge; namely, how are we able to ascend from particular truths to more general truths. If a, b, c, d denote integer numbers, of which b is less than a and d less than c, then

$$(a-b)(c-d) = ac + bd - ad - bc.$$

It is first seen that the above restrictions may be removed, and still the above equation hold. But the antecedent is still too narrow; the true scientific problem consists in specifying the meaning of the symbols, which, and only which, will admit of the forms being equal. It is not to find *some meanings*, but the *most general meaning*, which allows the equivalence to be true. Let us examine some other cases; we shall find that Peacock's principle is not a solution of the difficulty; the great logical process of generalization cannot be reduced to any such easy and arbitrary procedure. When a, m, n denote integer numbers, it can be shown that

$$a^m a^n = a^{m+n}.$$

According to Peacock the form on the left is always to be equal to the form on the right, and the meanings of a, m, n are to be found by interpretation. Suppose that a takes the form of the incommensurate quantity e, the base of the natural system of logarithms. A number is a degraded form of a complex quantity $p + q^{\sqrt{-1}}$ and a complex quantity is a degraded form of a quaternion; consequently one meaning which may be assigned to m and n is that of quaternion. Peacock's principle would lead us to suppose that $e^m e^n = e^{m+n}$, m and n denoting quaternions; but that is just what Hamilton, the inventor of the quaternion generalization, denies. There are reasons for believing that he was mistaken, and that the forms remain equivalent even under that extreme generalization of m and n; but the point is this: it is not a question of conventional definition and formal truth; it is a question of objective definition and real truth. Let the symbols have the prescribed meaning, does or does not the equivalence still hold? And if it does not hold, what is the higher or more complex form which the equivalence assumes?

Chapter 2

AUGUSTUS DE MORGAN[1]

(1806-1871)

Augustus De Morgan was born in the month of June at Madura in the presidency of Madras, India; and the year of his birth may be found by solving a conundrum proposed by himself, "I was x years of age in the year x^2." The problem is indeterminate, but it is made strictly determinate by the century of its utterance and the limit to a man's life. His father was Col. De Morgan, who held various appointments in the service of the East India Company. His mother was descended from James Dodson, who computed a table of anti-logarithms, that is, the numbers corresponding to exact logarithms. It was the time of the Sepoy rebellion in India, and Col. De Morgan removed his family to England when Augustus was seven months old. As his father and grandfather had both been born in India, De Morgan used to say that he was neither English, nor Scottish, nor Irish, but a Briton "unattached," using the technical term applied to an undergraduate of Oxford or Cambridge who is not a member of any one of the Colleges.

When De Morgan was ten years old, his father died. Mrs. De Morgan resided at various places in the southwest of England, and her son received his elementary education at various schools of no great account. His mathematical talents were unnoticed till he had reached the age of fourteen. A friend of the family accidentally discovered him making an elaborate drawing of a figure in Euclid with ruler and compasses, and explained to him the aim of Euclid, and gave him an initiation into demonstration.

[1]This Lecture was delivered April 13, 1901.—Editors.

De Morgan suffered from a physical defect—one of his eyes was rudimentary and useless. As a consequence, he did not join in the sports of the other boys, and he was even made the victim of cruel practical jokes by some schoolfellows. Some psychologists have held that the perception of distance and of solidity depends on the action of two eyes, but De Morgan testified that so far as he could make out he perceived with his one eye distance and solidity just like other people.

He received his secondary education from Mr. Parsons, a Fellow of Oriel College, Oxford, who could appreciate classics much better than mathematics. His mother was an active and ardent member of the Church of England, and desired that her son should become a clergyman; but by this time De Morgan had begun to show his non-grooving disposition, due no doubt to some extent to his physical infirmity. At the age of sixteen he was entered at Trinity College, Cambridge, where he immediately came under the tutorial influence of Peacock and Whewell. They became his life-long friends; from the former he derived an interest in the renovation of algebra, and from the latter an interest in the renovation of logic—the two subjects of his future life work.

At college the flute, on which he played exquisitely, was his recreation. He took no part in athletics but was prominent in the musical clubs. His love of knowledge for its own sake interfered with training for the great mathematical race; as a consequence he came out fourth wrangler. This entitled him to the degree of Bachelor of Arts; but to take the higher degree of Master of Arts and thereby become eligible for a fellowship it was then necessary to pass a theological test. To the signing of any such test De Morgan felt a strong objection, although he had been brought up in the Church of England. About 1875 theological tests for academic degrees were abolished in the Universities of Oxford and Cambridge.

As no career was open to him at his own university, he decided to go to the Bar, and took up residence in London; but he much preferred teaching mathematics to reading law. About this time the movement for founding the London University took shape. The two ancient universities were so guarded by theological tests that no Jew or Dissenter from the Church of England could enter as a student; still less be appointed to any office. A body of liberal-minded men resolved to meet the difficulty by establishing in London a University on the principle of religious neutrality. De Morgan, then 22 years of age, was appointed Professor of Mathematics. His introductory lecture "On the study of mathematics" is a discourse upon mental education of permanent value which has been recently reprinted in the United States.

The London University was a new institution, and the relations of the Council of management, the Senate of professors and the body of students were not well defined. A dispute arose between the professor of anatomy and his students, and in consequence of the action taken by the Council, several of the professors resigned, headed by De Morgan. Another professor of mathematics was appointed, who was accidentally drowned a few years later. De Morgan had shown himself a prince of teachers: he was invited to return to his chair, which thereafter became the continuous center of his labors for thirty years.

The same body of reformers—headed by Lord Brougham, a Scotsman eminent both in science and politics—who had instituted the London University, founded about the same time a Society for the Diffusion of Useful Knowledge. Its object was to spread scientific and other knowledge by means of cheap and clearly written treatises by the best writers of the time. One of its most voluminous and effective writers was De Morgan. He wrote a great work on *The Differential and Integral Calculus* which was published by the Society; and he wrote one-sixth of the articles in the *Penny Cyclopedia*, published by the Society, and issued in penny numbers. When De Morgan came to reside in London he found a congenial friend in William Frend, notwithstanding his mathematical heresy about negative quantities. Both were arithmeticians and actuaries, and their religious views were somewhat similar. Frend lived in what was then a suburb of London, in a country-house formerly occupied by Daniel Defoe and Isaac Watts. De Morgan with his flute was a welcome visitor; and in 1837 he married Sophia Elizabeth, one of Frend's daughters.

The London University of which De Morgan was a professor was a different institution from the University of London. The University of London was founded about ten years later by the Government for the purpose of granting degrees after examination, without any qualification as to residence. The London University was affiliated as a teaching college with the University of London, and its name was changed to University College. The University of London was not a success as an examining body; a teaching University was demanded. De Morgan was a highly successful teacher of mathematics. It was his plan to lecture for an hour, and at the close of each lecture to give out a number of problems and examples illustrative of the subject lectured on; his students were required to sit down to them and bring him the results, which he looked over and returned revised before the next lecture. In De Morgan's opinion, a thorough comprehension and mental assimilation of great principles far outweighed in importance any merely analytical dexterity in the application of half-understood principles to particular cases.

De Morgan had a son George, who acquired great distinction in mathematics both at University College and the University of London. He and another like-minded alumnus conceived the idea of founding a Mathematical Society in London, where mathematical papers would be not only received (as by the Royal Society) but actually read and discussed. The first meeting was held in University College; De Morgan was the first president, his son the first secretary. It was the beginning of the London Mathematical Society. In the year 1866 the chair of mental philosophy in University College fell vacant. Dr. Martineau, a Unitarian clergyman and professor of mental philosophy, was recommended formally by the Senate to the Council; but in the Council there were some who objected to a Unitarian clergyman, and others who objected to theistic philosophy. A layman of the school of Bain and Spencer was appointed. De Morgan considered that the old standard of religious neutrality had been hauled down, and forthwith resigned. He was now 60 years of age. His pupils secured a pension of $500 for him, but misfortunes followed. Two years later his son George—the younger Bernoulli, as he loved to hear him called, in allusion to the two eminent mathematicians of that name, related as father and son—died. This blow was followed by the death of a daughter. Five years after his resignation from University College De Morgan died of nervous prostration on March 18, 1871, in the 65th year of his age.

De Morgan was a brilliant and witty writer, whether as a controversialist or as a correspondent. In his time there flourished two Sir William Hamiltons who have often been confounded. The one Sir William was a baronet (that is, inherited the title), a Scotsman, professor of logic and metaphysics in the University of Edinburgh; the other was a knight (that is, won the title), an Irishman, professor of astronomy in the University of Dublin. The baronet contributed to logic the doctrine of the quantification of the predicate; the knight, whose full name was William Rowan Hamilton, contributed to mathematics the geometric algebra called Quaternions. De Morgan was interested in the work of both, and corresponded with both; but the correspondence with the Scotsman ended in a public controversy, whereas that with the Irishman was marked by friendship and terminated only by death. In one of his letters to Rowan, De Morgan says, "Be it known unto you that I have discovered that you and the other Sir W. H. are reciprocal polars with respect to me (intellectually and morally, for the Scottish baronet is a polar bear, and you, I was going to say, are a polar gentleman). When I send a bit of investigation to Edinburgh, the W. H. of that ilk says I took it from him. When I send you one, you take it from me, generalize it at a glance, bestow it thus generalized upon society at large, and make me the second discoverer of a known theorem."

The correspondence of De Morgan with Hamilton the mathematician extended over twenty-four years; it contains discussions not only of mathematical matters, but also of subjects of general interest. It is marked by geniality on the part of Hamilton and by wit on the part of De Morgan. The following is a specimen: Hamilton wrote, "My copy of Berkeley's work is not mine; like Berkeley, you know, I am an Irishman." De Morgan replied, "Your phrase 'my copy is not mine' is not a bull. It is perfectly good English to use the same word in two different senses in one sentence, particularly when there is usage. Incongruity of language is no bull, for it expresses meaning. But incongruity of ideas (as in the case of the Irishman who was pulling up the rope, and finding it did not finish, cried out that somebody had cut off the other end of it) is the genuine bull."

De Morgan was full of personal peculiarities. We have noticed his almost morbid attitude towards religion, and the readiness with which he would resign an office. On the occasion of the installation of his friend, Lord Brougham, as Rector of the University of Edinburgh, the Senate offered to confer on him the honorary degree of LL.D.; he declined the honor as a misnomer. He once printed his name: Augustus De Morgan,

H · O · M · O · P · A · U · C · A · R · U · M · L · I · T · E · R · A · R · U · M.

He disliked the country, and while his family enjoyed the seaside, and men of science were having a good time at a meeting of the British Association in the country he remained in the hot and dusty libraries of the metropolis. He said that he felt like Socrates, who declared that the farther he got from Athens the farther was he from happiness. He never sought to become a Fellow of the Royal Society, and he never attended a meeting of the Society; he said that he had no ideas or sympathies in common with the physical philosopher. His attitude was doubtless due to his physical infirmity, which prevented him from being either an observer or an experimenter. He never voted at an election, and he never visited the House of Commons, or the Tower, or Westminster Abbey.

Were the writings of De Morgan published in the form of collected works, they would form a small library. We have noticed his writings for the Useful

Knowledge Society. Mainly through the efforts of Peacock and Whewell, a Philosophical Society had been inaugurated at Cambridge; and to its Transactions De Morgan contributed four memoirs on the foundations of algebra, and an equal number on formal logic. The best presentation of his view of algebra is found in a volume, entitled *Trigonometry and Double Algebra*, published in 1849; and his earlier view of formal logic is found in a volume published in 1847. His most unique work is styled a *Budget of Paradoxes*; it originally appeared as letters in the columns of the *Athenæum* journal; it was revised and extended by De Morgan in the last years of his life, and was published posthumously by his widow. "If you wish to read something entertaining," said Professor Tait to me, "get De Morgan's *Budget of Paradoxes* out of the library." We shall consider more at length his theory of algebra, his contribution to exact logic, and his Budget of Paradoxes.

In my last lecture I explained Peacock's theory of algebra. It was much improved by D. F. Gregory, a younger member of the Cambridge School, who laid stress not on the permanence of equivalent forms, but on the permanence of certain formal laws. This new theory of algebra as the science of symbols and of their laws of combination was carried to its logical issue by De Morgan; and his doctrine on the subject is still followed by English algebraists in general. Thus Chrystal founds his *Textbook of Algebra* on De Morgan's theory; although an attentive reader may remark that he practically abandons it when he takes up the subject of infinite series. De Morgan's theory is stated in his volume on *Trigonometry and Double Algebra*. In the chapter (of the book) headed "On symbolic algebra" he writes: "In abandoning the meaning of symbols, we also abandon those of the words which describe them. Thus addition is to be, for the present, a sound void of sense. It is a mode of combination represented by $+$; when $+$ receives its meaning, so also will the word addition. It is most important that the student should bear in mind that, with one exception, no word nor sign of arithmetic or algebra has one atom of meaning throughout this chapter, the object of which is symbols, and their laws of combination, giving a symbolic algebra which may hereafter become the grammar of a hundred distinct significant algebras. If any one were to assert that $+$ and $-$ might mean reward and punishment, and A, B, C, etc., might stand for virtues and vices, the reader might believe him, or contradict him, as he pleases, but not out of this chapter. The one exception above noted, which has some share of meaning, is the sign $=$ placed between two symbols as in $A = B$. It indicates that the two symbols have the same resulting meaning, by whatever steps attained. That A and B, if quantities, are the same amount of quantity; that if operations, they are of the same effect, etc."

Here, it may be asked, why does the symbol = prove refractory to the symbolic theory? De Morgan admits that there is one exception; but an exception proves the rule, not in the usual but illogical sense of establishing it, but in the old and logical sense of testing its validity. If an exception can be established, the rule must fall, or at least must be modified. Here I am talking not of grammatical rules, but of the rules of science or nature.

De Morgan proceeds to give an inventory of the fundamental symbols of algebra, and also an inventory of the laws of algebra. The symbols are 0, 1, $+$, $-$, $\times$, $\div$, $(\,)^{(\,)}$, and letters; these only, all others are derived. His inventory of the fundamental laws is expressed under fourteen heads, but some of them are merely definitions. The laws proper may be reduced to the following, which, as he admits, are not all independent of one another:

I. Law of signs. $++=+$, $+-=-$, $-+=-$, $--=+$, $\times\times=\times$, $\times\div=\div$, $\div\times=\div$, $\div\div=\times$.

II. Commutative law. $a+b=b+a$, $ab=ba$.

III. Distributive law. $a(b+c)=ab+ac$.

IV. Index laws. $a^b\times a^c=a^{b+c}$, $(a^b)^c=a^{bc}$, $(ab)^c=a^cb^c$.

V. $a-a=0$, $a\div a=1$.

The last two may be called the rules of reduction. De Morgan professes to give a complete inventory of the laws which the symbols of algebra must obey, for he says, "Any system of symbols which obeys these laws and no others, except they be formed by combination of these laws, and which uses the preceding symbols and no others, except they be new symbols invented in abbreviation of combinations of these symbols, is symbolic algebra." From his point of view, none of the above principles are rules; they are formal laws, that is, arbitrarily chosen relations to which the algebraic symbols must be subject. He does not mention the law, which had already been pointed out by Gregory, namely, $(a+b)+c=a+(b+c)$, $(ab)c=a(bc)$ and to which was afterwards given the name of the *law of association.* If the commutative law fails, the associative may hold good; but not *vice versa.* It is an unfortunate thing for the symbolist or formalist that in universal arithmetic m^n is not equal to n^m; for then the commutative law would have full scope. Why does he not give it full scope? Because the foundations of algebra are, after all, real not formal, material not symbolic. To the formalists the index operations are exceedingly refractory, in consequence of which some take no account of them, but relegate them to applied mathematics. To give an inventory of the laws which the symbols of algebra must obey is an impossible task, and reminds one not a little of the task of those philosophers who attempt to give an inventory of the *a priori* knowledge of the mind.

De Morgan's work entitled *Trigonometry and Double Algebra* consists of two parts; the former of which is a treatise on Trigonometry, and the latter a treatise on generalized algebra which he calls Double Algebra. But what is meant by Double as applied to algebra? and why should Trigonometry be also treated in the same textbook? The first stage in the development of algebra is *arithmetic*, where numbers only appear and symbols of operations such as $+$, $\times$, etc. The next stage is *universal arithmetic*, where letters appear instead of numbers, so as to denote numbers universally, and the processes are conducted without knowing the values of the symbols. Let a and b denote any numbers; then such an expression as $a - b$ may be impossible; so that in universal arithmetic there is always a proviso, *provided the operation is possible*. The third stage is *single algebra*, where the symbol may denote a quantity forwards or a quantity backwards, and is adequately represented by segments on a straight line passing through an origin. Negative quantities are then no longer impossible; they are represented by the backward segment. But an impossibility still remains in the latter part of such an expression as $a + b\sqrt{-1}$ which arises in the solution of the quadratic equation. The fourth stage is *double algebra*; the algebraic symbol denotes in general a segment of a line in a given plane; it is a double symbol because it involves two specifications, namely, length and direction; and $\sqrt{-1}$ is interpreted as denoting a quadrant. The expression $a + b\sqrt{-1}$ then represents a line in the plane having an abscissa a and an ordinate b. Argand and Warren carried double algebra so far; but they were unable to interpret on this theory such an expression as $e^{a\sqrt{-1}}$. De Morgan attempted it by *reducing* such an expression to the form $b + q\sqrt{-1}$, and he considered that he had shown that it could be always so reduced. The remarkable fact is that this double algebra satisfies all the fundamental laws above enumerated, and as every apparently impossible combination of symbols has been interpreted it looks like the complete form of algebra.

If the above theory is true, the next stage of development ought to be *triple* algebra and if $a + b\sqrt{-1}$ truly represents a line in a given plane, it ought to be possible to find a third term which added to the above would represent a line in space. Argand and some others guessed that it was $a + b\sqrt{-1} + c\sqrt{-1}^{\sqrt{-1}}$ although this contradicts the truth established by Euler that $\sqrt{-1}^{\sqrt{-1}} = e^{-\frac{1}{2}\pi}$. De Morgan and many others worked hard at the problem, but nothing came of it until the problem was taken up by Hamilton. We now see the reason clearly: the symbol of double algebra denotes not a length and a direction; but a multiplier and *an angle*. In it the angles are confined to one plane; hence the next stage will be a *quadruple algebra*, when the axis of the plane is made variable. And this gives the answer to the first question; double algebra is nothing but analytical plane trigonometry, and this is the reason why it has been found to be the natural analysis for alternating currents. But De Morgan never got this far; he died with the belief "that double algebra must remain as the full development of the conceptions of arithmetic, so far as those symbols are concerned which arithmetic immediately suggests."

When the study of mathematics revived at the University of Cambridge, so also did the study of logic. The moving spirit was Whewell, the Master of Trinity College, whose principal writings were a *History of the Inductive Sciences*, and *Philosophy of the Inductive Sciences*. Doubtless De Morgan was influenced in his logical investigations by Whewell; but other contemporaries of influence were Sir W. Hamilton of Edinburgh, and Professor Boole of Cork. De Morgan's work on *Formal Logic*, published in 1847, is principally remarkable for his development of the numerically definite syllogism. The followers of Aristotle say and say truly that from two particular propositions such as *Some* M's *are* A's, and *Some* M's *are* B's nothing follows of necessity about the relation of the A's and B's. But they go further and say in order that any relation about the A's and B's may follow of necessity, the middle term must be taken universally in one of the premises. De Morgan pointed out that from *Most* M's *are* A's and *Most* M's *are* B's it follows of necessity that some A's are B's and he formulated the numerically definite syllogism which puts this principle in exact quantitative form. Suppose that the number of the M's is m, of the M's that are A's is a, and of the M's that are B's is b; then there are at least $(a + b - m)$ A's that are B's. Suppose that the number of souls on board a steamer was 1000, that 500 were in the saloon, and 700 were lost; it follows of necessity, that at least $700 + 500 - 1000$, that is, 200, saloon passengers were lost. This single principle suffices to prove the validity of all the Aristotelian moods; it is therefore a fundamental principle in necessary reasoning.

Here then De Morgan had made a great advance by introducing *quantification of the terms.* At that time Sir W. Hamilton was teaching at Edinburgh a doctrine of the quantification of the predicate, and a correspondence sprang up. However, De Morgan soon perceived that Hamilton's quantification was of a different character; that it meant for example, substituting the two forms *The whole of A is the whole of B*, and *The whole of A is a part of B* for the Aristotelian form All A's are B's. Philosophers generally have a large share of intolerance; they are too apt to think that they have got hold of the whole truth, and that everything outside of their system is error. Hamilton thought that he had placed the keystone in the Aristotelian arch, as he phrased it; although it must have been a curious arch which could stand 2000 years without a keystone. As a consequence he had no room for De Morgan's innovations. He accused De Morgan of plagiarism, and the controversy raged for years in the columns of the *Athenæum*, and in the publications of the two writers.

The memoirs on logic which De Morgan contributed to the Transactions of the Cambridge Philosophical Society subsequent to the publication of his book on *Formal Logic* are by far the most important contributions which he made to the science, especially his fourth memoir, in which he begins work in the broad field of the *logic of relatives.* This is the true field for the logician of the twentieth century, in which work of the greatest importance is to be done towards improving language and facilitating thinking processes which occur all the time in practical life. Identity and difference are the two relations which have been considered by the logician; but there are many others equally deserving of study, such as equality, equivalence, consanguinity, affinity, etc.

In the introduction to the *Budget of Paradoxes* De Morgan explains what he means by the word. "A great many individuals, ever since the rise of the mathematical method, have, each for himself, attacked its direct and indirect consequences. I shall call each of these persons a *paradoxer*, and his system a *paradox.* I use the word in the old sense: a paradox is something which is apart from general opinion, either in subject matter, method, or conclusion. Many of the things brought forward would now be called *crotchets*, which is the nearest word we have to old *paradox.* But there is this difference, that by calling a thing a crotchet we mean to speak lightly of it; which was not the necessary sense of paradox. Thus in the 16th century many spoke of the earth's motion as the *paradox of Copernicus* and held the ingenuity of that theory in very high esteem, and some I think who even inclined towards it. In the seventeenth century the depravation of meaning took place, in England at least."

How can the sound paradoxer be distinguished from the false paradoxer? De Morgan supplies the following test: "The manner in which a paradoxer will show himself, as to sense or nonsense, will not depend upon what he maintains, but upon whether he has or has not made a sufficient knowledge of what has been done by others, especially as to the mode of doing it, a preliminary to inventing knowledge for himself.... New knowledge, when to any purpose, must come by contemplation of old knowledge, in every matter which concerns thought; mechanical contrivance sometimes, not very often, escapes this rule. All the men who are now called discoverers, in every matter ruled by thought, have been men versed in the minds of their predecessors and learned in what had been before them. There is not one exception."

I remember that just before the American Association met at Indianapolis in 1890, the local newspapers heralded a great discovery which was to be laid before the assembled savants—a young man living somewhere in the country had squared the circle. While the meeting was in progress I observed a young man going about with a roll of paper in his hand. He spoke to me and complained that the paper containing his discovery had not been received. I asked him whether his object in presenting the paper was not to get it read, printed and published so that everyone might inform himself of the result; to all of which he assented readily. But, said I, many men have worked at this question, and their results have been tested fully, and they are printed for the benefit of anyone who can read; have you informed yourself of their results? To this there was no assent, but the sickly smile of the false paradoxer.

The *Budget* consists of a review of a large collection of paradoxical books which De Morgan had accumulated in his own library, partly by purchase at bookstands, partly from books sent to him for review, partly from books sent to him by the authors. He gives the following classification: squarers of the circle, trisectors of the angle, duplicators of the cube, constructors of perpetual motion, subverters of gravitation, stagnators of the earth, builders of the universe. You will still find specimens of all these classes in the New World and in the new century.

De Morgan gives his personal knowledge of paradoxers. "I suspect that I know more of the English class than any man in Britain. I never kept any reckoning: but I know that one year with another?—and less of late years than in earlier time?—I have talked to more than five in each year, giving more than a hundred and fifty specimens. Of this I am sure, that it is my own fault if they have not been a thousand. Nobody knows how they swarm, except those to whom they naturally resort. They are in all ranks and occupations, of all ages and characters. They are very earnest people, and their purpose is bona fide, the dissemination of their paradoxes. A great many—the mass, indeed—are illiterate, and a great many waste their means, and are in or approaching penury. These discoverers despise one another."

A paradoxer to whom De Morgan paid the compliment which Achilles paid Hector—to drag him round the walls again and again—was James Smith, a successful merchant of Liverpool. He found $\pi = 3\frac{1}{8}$. His mode of reasoning was a curious caricature of the *reductio ad absurdum* of Euclid. He said let $\pi = 3\frac{1}{8}$, and then showed that on that supposition, every other value of π must be absurd; consequently $\pi = 3\frac{1}{8}$ is the true value. The following is a specimen of De Morgan's dragging round the walls of Troy: "Mr. Smith continues to write me long letters, to which he hints that I am to answer. In his last of 31 closely written sides of note paper, he informs me, with reference to my obstinate silence, that though I think myself and am thought by others to be a mathematical Goliath, I have resolved to play the mathematical snail, and keep within my shell. A mathematical *snail*! This cannot be the thing so called which regulates the striking of a clock; for it would mean that I am to make Mr. Smith sound the true time of day, which I would by no means undertake upon a clock that gains 19 seconds odd in every hour by false quadrative value of π. But he ventures to tell me that pebbles from the sling of simple truth and common sense will ultimately crack my shell, and put me *hors de combat.* The confusion of images is amusing: Goliath turning himself into a snail to avoid $\pi = 3\frac{1}{8}$ and James Smith, Esq., of the Mersey Dock Board: and put *hors de combat* by pebbles from a sling. If Goliath had crept into a snail shell, David would have cracked the Philistine with his foot. There is something like modesty in the implication that the crack-shell pebble has not yet taken effect; it might have been thought that the slinger would by this time have been singing—And thrice [and one-eighth] I routed all my foes, And thrice [and one-eighth] I slew the slain."

In the region of pure mathematics De Morgan could detect easily the false from the true paradox; but he was not so proficient in the field of physics. His father-in-law was a paradoxer, and his wife a paradoxer; and in the opinion of the physical philosophers De Morgan himself scarcely escaped. His wife wrote a book describing the phenomena of spiritualism, table-rapping, table-turning, etc.; and De Morgan wrote a preface in which he said that he knew some of the asserted facts, believed others on testimony, but did not pretend to know *whether* they were caused by spirits, or had some unknown and unimagined origin. From this alternative he left out ordinary material causes. Faraday delivered a lecture on *Spiritualism*, in which he laid it down that in the investigation we ought to set out with the idea of what is physically possible, or impossible; De Morgan could not understand this.

Chapter 3

SIR WILLIAM ROWAN HAMILTON[1]

(1805-1865)

William Rowan Hamilton was born in Dublin, Ireland, on the 3d of August, 1805. His father, Archibald Hamilton, was a solicitor in the city of Dublin; his mother, Sarah Hutton, belonged to an intellectual family, but she did not live to exercise much influence on the education of her son. There has been some dispute as to how far Ireland can claim Hamilton; Professor Tait of Edinburgh in the Encyclopaedia Brittanica claims him as a Scotsman, while his biographer, the Rev. Charles Graves, claims him as essentially Irish. The facts appear to be as follows: His father's mother was a Scotch woman; his father's father was a citizen of Dublin. But the name "Hamilton" points to Scottish origin, and Hamilton himself said that his family claimed to have come over from Scotland in the time of James I. Hamilton always considered himself an Irishman; and as Burns very early had an ambition to achieve something for the renown of Scotland, so Hamilton in his early years had a powerful ambition to do something for the renown of Ireland. In later life he used to say that at the beginning of the century people read French mathematics, but that at the end of it they would be reading Irish mathematics.

Hamilton, when three years of age, was placed in the charge of his uncle, the Rev. James Hamilton, who was the curate of Trim, a country town, about twenty miles from Dublin, and who was also the master of the Church

[1] This Lecture was delivered April 16, 1901.—Editors.

of England school. From his uncle he received all his primary and secondary education and also instruction in Oriental languages. As a child Hamilton was a prodigy; at three years of age he was a superior reader of English and considerably advanced in arithmetic; at four a good geographer; at five able to read and translate Latin, Greek, and Hebrew, and liked to recite Dryden, Collins, Milton and Homer; at eight a reader of Italian and French and giving vent to his feelings in extemporized Latin; at ten a student of Arabic and Sanscrit. When twelve years old he met Zerah Colburn, the American calculating boy, and engaged with him in trials of arithmetical skill, in which trials Hamilton came off with honor, although Colburn was generally the victor. These encounters gave Hamilton a decided taste for arithmetical computation, and for many years afterwards he loved to perform long operations in arithmetic in his mind, extracting the square and cube root, and solving problems that related to the properties of numbers. When thirteen he received his initiation into algebra from Clairault's *Algebra* in the French, and he made an epitome, which he ambitiously entitled "A Compendious Treatise on Algebra by William Hamilton."

When Hamilton was fourteen years old, his father died and left his children slenderly provided for. Henceforth, as the elder brother of three sisters, Hamilton had to act as a man. This year he addressed a letter of welcome, written in the Persian language, to the Persian Ambassador, then on a visit to Dublin; and he met again Zerah Colburn. In the interval Zerah had attended one of the great public schools of England. Hamilton had been at a country school in Ireland, and was now able to make a successful investigation of the methods by which Zerah made his lightning calculations. When sixteen, Hamilton studied the Differential Calculus by the help of a French textbook, and began the study of the *Mécanique céleste* of Laplace, and he was able at the beginning of this study to detect a flaw in the reasoning by which Laplace demonstrates the theorem of the parallelogram of forces. This criticism brought him to the notice of Dr. Brinkley, who was then the professor of astronomy in the University of Dublin, and resided at Dunkirk, about five miles from the centre of the city. He also began an investigation for himself of equations which represent systems of straight lines in a plane, and in so doing hit upon ideas which he afterwards developed into his first mathematical memoir to the Royal Irish Academy. Dr. Brinkley is said to have remarked of him at this time: "This young man, I do not say *will be*, but *is*, the first mathematician of his age."

At the age of eighteen Hamilton entered Trinity College, Dublin, the University of Dublin founded by Queen Elizabeth, and differing from the Universities of Oxford and Cambridge in having only one college. Unlike Oxford, which has always given prominence to classics, and Cambridge, which has always given prominence to mathematics, Dublin at that time gave equal prominence to classics and to mathematics. In his first year Hamilton won the very rare honor of *optime* at his examination in Homer. In the old Universities marks used to be and in some cases still are published, descending not in percentages but by means of the scale of Latin adjectives: *optime, valdebene, bene, satis, mediocriter, vix medi, non*; *optime* means passed with the very highest distinction; *vix* means passed but with great difficulty. This scale is still in use in the medical examinations of the University of Edinburgh. Before entering college Hamilton had been accustomed to translate Homer into blank verse, comparing his result with the translations of Pope and Cowper; and he had already produced some original poems. In this, his first year he wrote a poem "On college ambition" which is a fair specimen of his poetical attainments.

Oh! Ambition hath its hour
Of deep and spirit-stirring power;
Not in the tented field alone,
Nor peer-engirded court and throne;
Nor the intrigues of busy life;
But ardent Boyhood's generous strife,
While yet the Enthusiast spirit turns
Where'er the light of Glory burns,
Thinks not how transient is the blaze,
But longs to barter Life for Praise.

Look round the arena, and ye spy
Pallid cheek and faded eye;
Among the bands of rivals, few
Keep their native healthy hue:
Night and thought have stolen away
Their once elastic spirit's play.
A few short hours and all is o'er,
Some shall win one triumph more;
Some from the place of contest go
Again defeated, sad and slow.

What shall reward the conqueror then

For all his toil, for all his pain,
For every midnight throb that stole
So often o'er his fevered soul?
Is it the applaudings loud
Or wond'ring gazes of the crowd;
Disappointed envy's shame,
Or hollow voice of fickle Fame?
These may extort the sudden smile,
May swell the heart a little while;
But they leave no joy behind,
Breathe no pure transport o'er the mind,
Nor will the thought of selfish gladness
Expand the brow of secret sadness.

Yet if Ambition hath its hour
Of deep and spirit-stirring power,
Some bright rewards are all its own,
And bless its votaries alone:
The anxious friend's approving eye;
The generous rivals' sympathy;
And that best and sweetest prize
Given by silent Beauty's eyes!
These are transports true and strong,
Deeply felt, remembered long:
Time and sorrow passing o'er
Endear their memory but the more.

The "silent Beauty" was not an abstraction, but a young lady whose brothers were fellow-students of Trinity College. This led to much effusion of poetry; but unfortunately while Hamilton was writing poetry about her another young man was talking prose to her; with the result that Hamilton experienced a disappointment. On account of his self-consciousness, inseparable probably from his genius, he felt the disappointment keenly. He was then known to the professor of astronomy, and walking from the College to the Observatory along the Royal Canal, he was actually tempted to terminate his life in the water.

In his second year he formed the plan of reading so as to compete for the highest honors both in classics and in mathematics. At graduation two gold medals were awarded, the one for distinction in classics, the other for distinction in mathematics. Hamilton aimed at carrying off both. In his junior year he received an *optime* in mathematical physics; and, as the winner of two *optimes*, the one in classics, the other in mathematics, he immediately became a celebrity in the intellectual circle of Dublin.

In his senior year he presented to the Royal Irish Academy a memoir embodying his research on systems of lines. He now called it a "Theory of Systems of Rays" and it was printed in the *Transactions*. About this time Dr. Brinkley was appointed to the bishopric of Cloyne, and in consequence resigned the professorship of astronomy. In the United Kingdom it is customary when a post becomes vacant for aspirants to lodge a formal application with the appointing board and to supplement their own application by testimonial letters from competent authorities. In the present case quite a number of candidates appeared, among them Airy, who afterwards became Astronomer Royal of England, and several Fellows of Trinity College, Dublin. Hamilton did not become a formal candidate, but he was invited to apply, with the result that he received the appointment while still an undergraduate, and not twenty-two years of age. Thus was his undergraduate career signalized much more than by the carrying off of the two gold medals. Before assuming the duties of his chair he made a tour through England and Scotland, and met for the first time the poet Wordsworth at his home at Rydal Mount, in Cumberland. They had a midnight walk, oscillating backwards and forwards between Rydal and Ambleside, absorbed in converse on high themes, and finding it almost impossible to part. Wordsworth afterwards said that Coleridge and Hamilton were the two most wonderful men, taking all their endowments together, that he had ever met.

In October, 1827, he came to reside at the place which was destined to be the scene of his scientific labors. I had the pleasure of visiting it last summer as the guest of his successor. The Observatory is situated on the top of a hill, Dunsink, about five miles from Dublin. The house adjoins the observatory; to the east is an extensive lawn; to the west a garden with stone wall and shaded walks; to the south a terraced field; at the foot of the hill is the Royal Canal; to the southeast the city of Dublin; while the view is bounded by the sea and the Dublin and Wicklow Mountains; a fine home for a poet or a philosopher or a mathematician, and in Hamilton all three were combined.

Settled at the Observatory he started out diligently as an observer, but he found it difficult to stand the low temperatures incident to the work. He never attained skill as an observer, and unfortunately he depended on a very poor assistant. Himself a brilliant computer, with a good observer for assistant, the work of the observatory ought to have flourished. One of the first distinguished visitors at the Observatory was the poet Wordsworth, in commemoration of which one of the shaded walks in the garden was named Wordsworth's walk. Wordsworth advised him to concentrate his powers on science; and, not long after, wrote him as follows: "You send me showers of verses which I receive with much pleasure, as do we all: yet have we fears that this employment may seduce you from the path of science which you seem destined to tread with so much honor to yourself and profit to others. Again and again I must repeat that the composition of verse is infinitely more of an art than men are prepared to believe, and absolute success in it depends upon innumerable *minutiæ* which it grieves me you should stoop to acquire a knowledge of... Again I do venture to submit to your consideration, whether the poetical parts of your nature would not find a field more favorable to their exercise in the regions of prose; not because those regions are humbler, but because they may be gracefully and profitably trod, with footsteps less careful and in measures less elaborate."

Hamilton possessed the poetic imagination; what he was deficient in was the technique of the poet. The imagination of the poet is kin to the imagination of the mathematician; both extract the ideal from a mass of circumstances. In this connection De Morgan wrote: "The moving power of mathetical *invention* is not reasoning but imagination. We no longer apply the homely term *maker* in literal translation of *poet*; but discoverers of all kinds, whatever may be their lines, are makers, or, as we mow say, have the creative genius." Hamilton spoke of the *Mécanique analytique* of Lagrange as a "scientific poem"; Hamilton himself was styled the Irish Lagrange. Engineers venerate Rankine, electricians venerate Maxwell; both were scientific discoverers and likewise poets, that is, amateur poets. The proximate cause of the shower of verses was that Hamilton had fallen in love for the second time. The young lady was Miss de Vere, daughter of an accomplished Irish baronet, and who like Tennyson's Lady Clara Vere de Vere could look back on a long and illustrious descent. Hamilton had a pupil in Lord Adare, the eldest son of the Earl of Dunraven, and it was while visiting Adare Manor that he was introduced to the De Vere family, who lived near by at Curragh Chase. His suit was encouraged by the Countess of Dunraven, it was favorably received by both father and mother, he had written many sonnets of which Ellen de Vere was the inspiration, he had discussed with her astronomy, poetry and philosophy; and was on the eve of proposing when he gave up because the young lady incidentally said to him that "she could not live happily anywhere but at Curragh." His action shows the working of a too self-conscious mind, proud of his own intellectual achievements, and too much awed by her long descent. So he failed for the second time; but both of these ladies were friends of his to the last.

At the age of 27 he contributed to the Irish Academy a supplementary paper on his Theory of Systems of Rays, in which he predicted the phenomenon of conical refraction; namely, that under certain conditions a single ray incident on a biaxial crystal would be broken up into a cone of rays, and likewise that under certain conditions a single emergent ray would appear as a cone of rays. The prediction was made by Hamilton on Oct. 22nd; it was experimentally verified by his colleague Prof. Lloyd on Dec. 14th. It is not experiment alone or mathematical reasoning alone which has built up the splendid temple of physical science, but the two working together; and of this we have a notable exemplification in the discovery of conical refraction.

Twice Hamilton chose well but failed; now he made another choice and succeeded. The lady was a Miss Bayly, who visited at the home of her sister near Dunsink hill. The lady had serious misgivings about the state of her health; but the marriage took place. The kind of wife which Hamilton needed was one who could govern him and efficiently supervise all domestic matters; but the wife he chose was, from weakness of body and mind, incapable of doing it. As a consequence, Hamilton worked for the rest of his life under domestic difficulties of no ordinary kind.

At the age of 28 he made a notable addition to the theory of Dynamics by extending to it the idea of a Characteristic Function, which he had previously applied with success to the science of Optics in his Theory of Systems of Rays. It was contributed to the Royal Society of London, and printed in their *Philosophical Transactions*. The Royal Society of London is the great scientific society of England, founded in the reign of Charles II, and of which Newton was one of the early presidents; Hamilton was invited to become a fellow but did not accept, as he could not afford the expense.

At the age of 29 he read a paper before the Royal Irish Academy, which set forth the result of long meditation and investigation on the nature of Algebra as a science; the paper is entitled "Algebra as the Science of Pure Time." The main idea is that as Geometry considered as a science is founded upon the pure intuition of space, so algebra as a science is founded upon the pure intuition of time. He was never satisfied with Peacock's theory of algebra as a "System of Signs and their Combinations"; nor with De Morgan's improvement of it; he demanded a more real foundation. In reading Kant's

Critique of Pure Reason he was struck by the following passage: "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 cognitions of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetical propositions *a priori* possible." Thus, according to Kant, space and time are forms of the intellect; and Hamilton reasoned that, as geometry is the science of the former, so algebra must be the science of the latter. When algebra is based on any unidimensional subject, such as time, or a straight line, a difficulty arises in explaining the roots of a quadratic equation when they are imaginary. To get over this difficulty Hamilton invented a theory of algebraic couplets, which has proved a conundrum in the mathematical world. Some 20 years ago there nourished in Edinburgh a mathematician named Sang who had computed the most elaborate tables of logarithms in existence—which still exist in manuscript. On reading the theory in question he first judged that either Hamilton was crazy, or else that he (Sang) was crazy, but eventually reached the more comforting alternative. On the other hand, Prof. Tait believes in its soundness, and endeavors to bring it down to the ordinary comprehension.

We have seen that the British Association for the Advancement of Science was founded in 1831, and that its first meeting was in the ancient city of York. It was a policy of the founders not to meet in London, but in the provincial cities, so that thereby greater interest in the advance of science might be produced over the whole land. The cities chosen for the place of meeting in following years were the University towns: Oxford, Cambridge, Edinburgh, Dublin. Hamilton was the only representative of Ireland present at the Oxford meeting; and at the Oxford, Cambridge, and Edinburgh meetings he not only contributed scientific papers, but he acquired renown as a scientific orator. In the case of the Dublin meeting he was chief organizer beforehand, and chief orator when it met. The week of science was closed by a grand dinner given in the library of Trinity College; and an incident took place which is thus described by an American scientist:

"We assembled in the imposing hall of Trinity Library, two hundred and eighty feet long, at six o'clock. When the company was principally assembled, I observed a little stir near the place where I stood, which nobody could explain, and which, in fact, was not comprehended by more than two or three persons present. In a moment, however, I perceived myself standing near the Lord Lieutenant and his suite, in front of whom a space had been cleared, and by whom was Professor Hamilton, looking very much embarrassed. The Lord Lieutenant then called him by name, and he stepped into the vacant space. 'I am,' said his Excellency, 'about to exercise a prerogative of royalty, and it gives me great pleasure to do it, on this splendid public occasion, which has brought together so many distinguished men from all parts of the empire, and from all parts even of the world where science is held in honor. But, in exercising it, Professor Hamilton, I do not confer a distinction. I but set the royal, and therefore the national mark on a distinction already acquired by your genius and labors.' He went on in this way for three of four minutes, his voice very fine, rich and full; his manner as graceful and dignified as possible; and his language and allusions appropriate and combined into very ample flowing sentences. Then, receiving the State sword from one of his attendants, he said, 'Kneel down, Professor Hamilton'; and laying the blade gracefully and gently first on one shoulder, and then on the other, he said, 'Rise up, Sir William Rowan Hamilton.' The Knight rose, and the Lord Lieutenant then went up, and with an appearance of great tact in his manner, shook hands with him. No reply was made. The whole scene was imposing, rendered so, partly by the ceremony itself, but more by the place in which it passed, by the body of very distinguished men who were assembled there, and especially by the extraordinarily dignified and beautiful manner in which it was performed by the Lord Lieutenant. The effect at the time was great, and the general impression was that, as the honor was certainly merited by him who received it, so the words by which it was conferred were so graceful and appropriate that they constituted a distinction by themselves, greater than the distinction of knighthood. I was afterwards told that this was the first instance in which a person had been knighted by a Lord Lieutenant either for scientific or literary merit."

Two years after another great honor came to Hamilton—the presidency of the Royal Irish Academy. While holding this office, in the year 1843, when 38 years old, he made the discovery which will ever be considered his highest title to fame. The story of the discovery is told by Hamilton himself in a letter to his son: "On the 16th day of October, which happened to be a Monday, and Council day of the Royal Irish Academy, I was walking in to attend and preside, and your mother was walking with me along the Royal Canal, to which she had perhaps driven; and although she talked with me now and then, yet an undercurrent of thought was going on in my mind, which gave at last a result, whereof it is not too much to say that I felt at once the importance. An electric circuit seemed to close; and a spark flashed forth, the herald (as I foresaw immediately) of many long years to come of definitely directed thought and work, by myself if spared, and at all events on the part of others, if I should even be allowed to live long enough distinctly to communicate the discovery. Nor could I resist the impulse—unphilosophical as it may have been—to cut with a knife on a stone of Brougham Bridge, as we passed it, the fundamental formula with the symbols i,j,k; namely,

$$i^2 = j^2 = k^2 = ijk = -1,$$

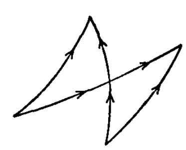
which contains the solution of the problem, but of course as an inscription has long since mouldered away. A more durable notice remains, however, in the Council Book of the Academy for that day, which records the fact that I then asked for and obtained leave to read a paper on Quaternions, at the first general meeting of the session, which reading took place accordingly on Monday the 13th of November following."

Last summer Prof. Joly and I took the walk here described. We started from the Observatory, walked down the terraced field, then along the path by the side of the Royal Canal towards Dublin until we came to the second bridge spanning the canal. The path of course goes under the Bridge, and the inner side of the Bridge presents a very convenient surface for an inscription. I have seen this incident quoted as an example of how a genius strikes on a discovery all of a sudden. No doubt a problem was solved then and there, but the problem had engaged Hamilton's thoughts and researches for fifteen years. It is rather an illustration of how genius is patience, or a faculty for infinite labor. What was Hamilton struggling to do all these years? To emerge from Flatland into Space; in other words, Algebra had been extended so as to apply to lines in a plane; but no one had been able to extend it so as to apply to lines in space. The greatness of the feat is made evident by the fact that most analysts are still crawling in Flatland. The same year in which he discovered Quaternions the Government granted him a pension of £200 per annum for life, on account of his scientific work.

We have seen how Hamilton gained two *optimes*, one in classics, the other in physics, the highest possible distinction in his college course; how he was appointed professor of astronomy while yet an undergraduate; how he was a scientific chief in the British Association at 27; how he was knighted for his scientific achievements at 30; how he was appointed president of the Royal Irish Academy at 32; how he discovered Quaternions and received a Government pension at 38; can you imagine that this brilliant and successful genius would fall a victim to intemperance? About this time at a dinner of a scientific society in Dublin he lost control of himself, and was so mortified that, on the advice of friends he resolved to abstain totally. This resolution he kept for two years; when happening to be a member of a scientific party at the castle of Lord Rosse, an amateur astronomer then the possessor of the largest telescope in existence, he was taunted for sticking to water, particularly by Airy the Greenwich astronomer. He broke his good resolution, and from that time forward the craving for alcoholic stimulants clung to him. How could Hamilton with all his noble aspirations fall into such a vice? The explanation lay in the want of order which reigned in his home. He had no regular times for his meals; frequently had no regular meals at all, but resorted to the sideboard when hunger compelled him. What more natural in such condition than that he should refresh himself with a quaff of that beverage for which Dublin is famous—porter labelled X^3? After Hamilton's death the dining-room was found covered with huge piles of manuscript, with convenient walks between the piles; when these literary remains were wheeled out and examined, china plates with the relics of food upon them were found between the sheets of manuscript, plates sufficient in number to furnish a kitchen. He used to carry on, says his eldest son, long trains of algebraical and arithmetical calculations in his mind, during which he was unconscious of the earthly necessity of eating; "we used to bring in a 'snack' and leave it in his study, but a brief nod of recognition of the intrusion of the chop or cutlet was often the only result, and his thoughts went on soaring upwards."

In 1845 Hamilton attended the second Cambridge meeting of the British Association; and after the meeting he was lodged for a week in the rooms in Trinity College which tradition points out as those in which Sir Isaac Newton composed the *Principia*. This incident was intended as a compliment and it seems to have impressed Hamilton powerfully. He came back to the Observatory with the fixed purpose of preparing a work on Quaternions which might not unworthily compare with the *Principia* of Newton, and in order to obtain more leisure for this undertaking he resigned the office of president of the Royal Irish Academy. He first of all set himself to the preparation of a course of lectures on Quaternions, which were delivered in Trinity College, Dublin, in 1848, and were six in number. Among his hearers were George Salmon, now well known for his highly successful series of manuals on Analytical Geometry; and Arthur Cayley, then a Fellow of Trinity College, Cambridge. These lectures were afterward expanded and published in 1853, under the title of *Lectures on Quaternions*, at the expense of Trinity College, Dublin.

Hamilton had never had much experience as a teacher; the volume was criticised for diffuseness of style, and certainly Hamilton sometimes forgot the expositor in the orator. The book was a paradox—a sound paradox, and of his experience as a paradoxer Hamilton wrote: "It required a certain capital of scientific reputation, amassed in former years, to make it other than dangerously imprudent to hazard the publication of a work which has, although at bottom quite conservative, a highly revolutionary air. It was part of the ordeal through which I had to pass, an episode in the battle of life, to know that even candid and friendly people secretly or, as it might happen, openly, censured or ridiculed me, for what appeared to them my monstrous innovations." One of these monstrous innovations was the principle that ij is not $= ji$ but $= -ji$; the truth of which is evident from the diagram. Critics said that he held that 3×4 is not $= 4 \times 3$; which proceeds on the assumption that only numbers can be represented by letter symbols.



Soon after the publication of the Lectures, he became aware of its imperfection as a manual of instruction, and he set himself to prepare a second book on the model of Euclid's *Elements*. He estimated that it would fill 400 pages and take two years to prepare; it amounted to nearly 800 closely printed pages and took seven years. At times he would work for twelve hours on a stretch; and he also suffered from anxiety as to the means of publication. Trinity College advanced £200, he paid £50 out of his own pocket, but when illness came upon him the expense of paper and printing had mounted up to £400. He was seized by an acute attack of gout, from which, after several months of suffering, he died on Sept. 2, 1865, in the 61st year of his age.

It is pleasant to know that this great mathematician received during his last illness an honor from the United States, which made him feel that he had realized the aim of his great labors. While the war between the North and South was in progress, the National Academy of Sciences was founded, and the news which came to Hamilton was that he had been elected one of ten foreign members, and that his name had been voted to occupy the specially honorable position of first on the list. Sir William Rowan Hamilton was thus the first foreign associate of the National Academy of Sciences of the United States.

As regards religion Hamilton was deeply reverential in nature. He was born and brought up in the Church of England, which was then the established Church in Ireland. He lived in the time of the Oxford movement, and for some time he sympathized with it; but when several of his friends, among them the brother of Miss De Vere, passed over into the Roman Catholic Church, he modified his opinion of the movement and remained Protestant to the end.

The immense intellectual activity of Hamilton, especially during the years when he was engaged on the enormous labor of writing the *Elements of Quaternions*, made him a recluse, and necessarily took away from his power of attending to the practical affairs of life. Some said that however great a master of pure time he might be he was not a master of sublunary time. His neighbors also took advantage of his goodness of heart. Surrounding the house there is an extensive lawn affording good pasture, and on it Hamilton pastured a cow. A neighbor advised Hamilton that his cow would be much better contented by having another cow for company and bargained with Hamilton to furnish the companion provided Hamilton paid something like a dollar per month.

Here is Hamilton's own estimate of himself. "I have very long admired Ptolemy's description of his great astronomical master, Hipparchus, as ἀνήρ φιλόπονος καὶ φιλαλήθης; a labor-loving and truth-loving man. Be such my epitaph."

Hamilton's family consisted of two sons and one daughter. At the time of his death, the *Elements of Quaternions* was all finished excepting one chapter. His eldest son, William Edwin Hamilton, wrote a preface, and the volume was published at the expense of Trinity College, Dublin. Only 500 copies were printed, and many of those were presented. In consequence it soon became a scarce book, and as much as $35.00 has been paid for a copy. A new edition, in two volumes, is now being published by Prof. Joly, his successor in Dunsink Observatory.

Chapter 4

GEORGE BOOLE[1]

(1815-1864)

George Boole was born at Lincoln, England, on the 2d of November, 1815. His father, a tradesman of very limited means, was attached to the pursuit of science, particularly of mathematics, and was skilled in the construction of optical instruments. Boole received his elementary education at the National School of the city, and afterwards at a commercial school; but it was his father who instructed him in the elements of mathematics, and also gave him a taste for the construction and adaptation of optical instruments. However, his early ambition did not urge him to the further prosecution of mathematical studies, but rather to becoming proficient in the ancient classical languages. In this direction he could receive no help from his father, but to a friendly bookseller of the neighborhood he was indebted for instruction in the rudiments of the Latin Grammar. To the study of Latin he soon added that of Greek without any external assistance; and for some years he perused every Greek or Latin author that came within his reach. At the early age of twelve his proficiency in Latin made him the occasion of a literary controversy in his native city. He produced a metrical translation of an ode of Horace, which his father in the pride of his heart inserted in a local journal, stating the age of the translator. A neighboring school-master wrote a letter to the journal in which he denied, from internal evidence, that the version could have been the work of one so young. In his early thirst for knowledge of languages and ambition to excel in verse he was like Hamilton,

[1]This Lecture was delivered April 19, 1901.—Editors.

but poor Boole was much more heavily oppressed by the *res angusta domi*—the hard conditions of his home. Accident discovered to him certain defects in his methods of classical study, inseparable from the want of proper early training, and it cost him two years of incessant labor to correct them.

Between the ages of sixteen and twenty he taught school as an assistant teacher, first at Doncaster in Yorkshire, afterwards at Waddington near Lincoln; and the leisure of these years he devoted mainly to the study of the principal modern languages, and of patristic literature with the view of studying to take orders in the Church. This design, however, was not carried out, owing to the financial circumstances of his parents and some other difficulties. In his twentieth year he decided on opening a school on his own account in his native city; thenceforth he devoted all the leisure he could command to the study of the higher mathematics, and solely with the aid of such books as he could procure. Without other assistance or guide he worked his way onward, and it was his own opinion that he had lost five years of educational progress by his imperfect methods of study, and the want of a helping hand to get him over difficulties. No doubt it cost him much time; but when he had finished studying he was already not only learned but an experienced investigator.

We have seen that at this time (1835) the great masters of mathematical analysis wrote in the French language; and Boole was naturally led to the study of the *Mécanique celeste* of Laplace, and the *Mécanique analytique* of Lagrange. While studying the latter work he made notes from which there eventually emerged his first mathematical memoir, entitled, "On certain theorems in the calculus of variations." By the same works his attention was attracted to the transformation of homogeneous functions by linear substitutions, and in the course of his subsequent investigations he was led to results which are now regarded as the foundation of the modern Higher Algebra. In the publication of his results he received friendly assistance from D. F. Gregory, a younger member of the Cambridge school, and editor of the newly founded *Cambridge Mathematical Journal.* Gregory and other friends suggested that Boole should take the regular mathematical course at Cambridge, but this he was unable to do; he continued to teach school for his own support and that of his aged parents, and to cultivate mathematical analysis in the leisure left by a laborious occupation.

Duncan F. Gregory was one of a Scottish family already distinguished in the annals of science. His grandfather was James Gregory, the inventor of the refracting telescope and discoverer of a convergent series for π. A cousin of his father was David Gregory, a special friend and fellow worker of Sir Isaac Newton. D. F. Gregory graduated at Cambridge, and after graduation he immediately turned his attention to the logical foundations of analysis. He had before him Peacock's theory of algebra, and he knew that in the analysis as developed by the French school there were many remarkable phenomena awaiting explanation; particularly theorems which involved what was called the separation of symbols. He embodied his results in a paper "On the real Nature of symbolical Algebra" which was printed in the *Transactions* of the Royal Society of Edinburgh.

Boole became a master of the method of separation of symbols, and by attempting to apply it to the solution of differential equations with variable coefficients was led to devise a general method in analysis. The account of it was printed in the *Transactions* of the Royal Society of London, and brought its author a Royal medal. Boole's study of the separation of symbols naturally led him to a study of the foundations of analysis, and he had before him the writings of Peacock, Gregory and De Morgan. He was led to entertain very wide views of the domain of mathematical analysis; in fact that it was coextensive with exact analysis, and so embraced formal logic. In 1848, as we have seen, the controversy arose between Hamilton and De Morgan about the quantification of terms; the general interest which that controversy awoke in the relation of mathematics to logic induced Boole to prepare for publication his views on the subject, which he did that same year in a small volume entitled *Mathematical Analysis of Logic*.

About this time what are denominated the Queen's Colleges of Ireland were instituted at Belfast, Cork and Galway; and in 1849 Boole was appointed to the chair of mathematics in the Queen's College at Cork. In this more suitable environment he set himself to the preparation of a more elaborate work on the mathematical analysis of logic. For this purpose he read extensively books on psychology and logic, and as a result published in 1854 the work on which his fame chiefly rests—"An Investigation of the Laws of Thought, on which are founded the mathematical theories of logic and probabilities." Subsequently he prepared textbooks on *Differential Equations* and *Finite Differences*; the former of which remained the best English textbook on its subject until the publication of Forsyth's *Differential Equations*.

Prefixed to the *Laws of Thought* is a dedication to Dr. Ryall, Vice-President and Professor of Greek in the same College. In the following year, perhaps as a result of the dedication, he married Miss Everest, the niece of that colleague. Honors came: Dublin University made him an LL.D., Oxford a D.C.L.; and the Royal Society of London elected him a Fellow. But Boole's career was cut short in the midst of his usefulness and scientific labors. One day in 1864 he walked from his residence to the College, a distance of two miles, in a drenching rain, and lectured in wet clothes. The result was a feverish cold which soon fell upon his lungs and terminated his career on December 8, 1864, in the 50th year of his age.

De Morgan was the man best qualified to judge of the value of Boole's work in the field of logic; and he gave it generous praise and help. In writing to the Dublin Hamilton he said, "I shall be glad to see his work (*Laws of Thought*) out, for he has, I think, got hold of the true connection of algebra and logic." At another time he wrote to the same as follows: "All metaphysicians except you and I and Boole consider mathematics as four books of Euclid and algebra up to quadratic equations." We might infer that these three contemporary mathematicians who were likewise philosophers would form a triangle of friends. But it was not so; Hamilton was a friend of De Morgan, and De Morgan a friend of Boole; but the relation of *friend*, although convertible, is not necessarily transitive. Hamilton met De Morgan only once in his life, Boole on the other hand with comparative frequency; yet he had a voluminous correspondence with the former extending over 20 years, but almost no correspondence with the latter. De Morgan's investigations of double algebra and triple algebra prepared him to appreciate the quaternions, whereas Boole was too much given over to the symbolic theory to appreciate geometric algebra.

Hamilton's biography has appeared in three volumes, prepared by his friend Rev. Charles Graves; De Morgan's biography has appeared in one volume, prepared by his widow; of Boole no biography has appeared. A biographical notice of Boole was written for the *Proceedings* of the Royal Society of London by his friend the Rev. Robert Harley, and it is to it that I am indebted for most of my biographical data. Last summer when in England I learned that the reason why no adequate biography of Boole had appeared was the unfortunate temper and lack of sound judgment of his widow. Since her husband's death Mrs. Boole has published a paradoxical book of the false kind worthy of a notice in De Morgan's *Budget*.

The work done by Boole in applying mathematical analysis to logic necessarily led him to consider the general question of how reasoning is accomplished by means of symbols. The view which he adopted on this point is stated at page 68 of the *Laws of Thought*. "The conditions of valid reasoning by the aid of symbols, are: *First*, that a fixed interpretation be assigned to the symbols employed in the expression of the data; and that the laws of the combination of those symbols be correctly determined from that interpretation; *Second*, that the formal processes of solution or demonstration be conducted throughout in obedience to all the laws determined as above, without regard to the question of the interpretability of the particular results obtained; *Third*, that the final result be interpretable in form, and that it be actually interpreted in accordance with that system of interpretation which has been employed in the expression of the data." As regards these conditions it may be observed that they are very different from the formalist view of Peacock and De Morgan, and that they incline towards a realistic view of analysis, as held by Hamilton. True he speaks of interpretation instead of meaning, but it is a fixed interpretation; and the rules for the processes of solution are not to be chosen arbitrarily, but are to be found out from the particular system of interpretation of the symbols.

It is Boole's second condition which chiefly calls for study and examination; respecting it he observes as follows: "The principle in question may be considered as resting upon a general law of the mind, the knowledge of which is not given to us *a priori*, that is, antecedently to experience, but is derived, like the knowledge of the other laws of the mind, from the clear manifestation of the general principle in the particular instance. A single example of reasoning, in which symbols are employed in obedience to laws founded upon their interpretation, but without any sustained reference to that interpretation, the chain of demonstration conducting us through intermediate steps which are not interpretable to a final result which is interpretable, seems not only to establish the validity of the particular application, but to make known to us the general law manifested therein. No accumulation of instances can properly add weight to such evidence. It may furnish us with clearer conceptions of that common element of truth upon which the application of the principle depends, and so prepare the way for its reception. It may, where the immediate force of the evidence is not felt, serve as a verification, *a posteriori*, of the practical validity of the principle in question. But this does not affect the position affirmed, viz., that the general principle must be seen in the particular instance—seen to be general in application as well as true in the special example. The employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry furnishes an illustration of what has been said. I apprehend that there is no mode of explaining that application which does not covertly assume the very principle in question. But that principle, though not, as I conceive, warranted by formal reasoning based upon other grounds, seems to deserve a place among those axiomatic truths which constitute in some sense the foundation of general knowledge, and which may properly be regarded as expressions of the mind's own laws and constitution."

We are all familiar with the fact that algebraic reasoning may be conducted through intermediate equations without requiring a sustained reference to the meaning of these equations; but it is paradoxical to say that these equations can, in any case, have no meaning or interpretation. It may not be necessary to consider their meaning, it may even be difficult to find their meaning, but that they have a meaning is a dictate of common sense. It is entirely paradoxical to say that, as a general process, we can start from equations having a meaning, and arrive at equations having a meaning by passing through equations which have no meaning. The particular instance in which Boole sees the truth of the paradoxical principle is the successful employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry. So soon then as this symbol is interpreted, or rather, so soon as its meaning is demonstrated, the evidence for the principle fails, and Boole's transcendental logic falls.

In the algebra of quantity we start from elementary symbols denoting numbers, but are soon led to compound forms which do not reduce to numbers; so in the algebra of logic we start from elementary symbols denoting classes, but are soon introduced to compound expressions which cannot be reduced to simple classes. Most mathematical logicians say, Stop, we do not know what this combination means. Boole says, It may be meaningless, go ahead all the same. The design of the *Laws of Thought* is stated by the author to be to investigate the fundamental laws of those operations of the mind by which reasoning is performed; to give expression to them in the symbolical 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.

Boole's inventory of the symbols required in the algebra of logic is as follows: *first*, Literal symbols, as x, y, etc., representing things as subjects of our conceptions; *second*, Signs of operation, as $+$, $-$, $\times$, standing for those operations of the mind by which the conceptions of things are combined or resolved so as to form new conceptions involving the same elements;

third, The sign of identity =; not equality merely, but identity which involves equality. The symbols x, y, etc., are used to denote classes; and it is one of Boole's maxims that substantives and adjectives alike denote classes. "They may be regarded," he says, "as differing only in this respect, that the former expresses the substantive existence of the individual thing or things to which it refers, the latter implies that existence. If we attach to the adjective the universally understood subject, 'being' or 'thing,' it becomes virtually a substantive, and may for all the essential purposes of reasoning be replaced by the substantive." Let us then agree to represent the class of individuals to which a particular name is applicable by a single letter as x. If the name is *men* for instance, let x represent *all men*, or the class *men*. Again, if an adjective, as *good*, is employed as a term of description, let us represent by a letter, as y, all things to which the description *good* is applicable, that is, *all good things* or the class *good things*. Then the combination yx will represent *good men*.

Boole's symbolic logic was brought to my notice by Professor Tait, when I was a student in the physical laboratory of Edinburgh University. I studied the *Laws of Thought* and I found that those who had written on it regarded the method as highly mysterious; the results wonderful, but the processes obscure. I reduced everything to diagram and model, and I ventured to publish my views on the subject in a small volume called *Principles of the Algebra of Logic*; one of the chief points I made is the philological and analytical difference between the substantive and the adjective. What I said was that the word *man* denotes a class, but the word *white* does not; in the former a definite unit-object is specified, in the latter no unit-object is specified. We can exhibit a type of a *man*, we cannot exhibit a type of a *white*.

The identification of the substantive and adjective on the one hand and their discrimination on the other hand, lead to different conceptions of what De Morgan called the *universe*. Boole's conception of the Universe is as follows (*Laws of Thought*, p. 42): "In every discourse, whether of the mind conversing with its own thoughts, or of the individual in his intercourse with others, there is an assumed or expressed limit within which the subjects of its operation are confined. The most unfettered discourse is that in which the words we use are understood in the widest possible application, and for them the limits of discourse are coextensive with those of the universe itself. But more usually we confine ourselves to a less spacious field. Sometimes in discoursing of men we imply (without expressing the limitation) that it is of men only under certain circumstances and conditions that we speak, as of civilized men, or of men in the vigor of life, or of men under some other condition or relation. Now, whatever may be the extent of the field within which all the objects of our discourse are found, that field may properly be termed the universe of discourse."

Another view leads to the conception of the Universe as a collection of homogeneous units, which may be finite or infinite in number; and in a particular problem the mind considers the relation of identity between different groups of this collection. This *universe* corresponds to *the series of events*, in the theory of Probability; and the characters correspond to the different ways in which the event may happen. The difference is that the Algebra of Logic considers necessary data and relations; while the theory of Probability considers probable data and relations. I will explain the elements of Boole's method on this theory.

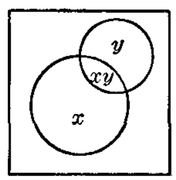


FIG. 1.

The square is a collection of points: it may serve to represent any collection of homogeneous units, whether finite or infinite in number, that is, the universe of the problem. Let x denote *inside the left-hand circle*, and y *inside the right-hand circle*. Uxy will denote the points inside both circles (Fig. 1). In arithmetical value x may range from 1 to 0; so also y; while xy cannot be greater than x or y, or less than 0 or $x + y - 1$. This last is the principle of the syllogism. From the co-ordinate nature of the operations x and y, it is evident that $Uxy = Uyx$; but this is a different thing from commuting, as Boole does, the relation of U and x, which is not that of co-ordination, but of subordination of x to U, and which is properly denoted by writing U first.

Suppose y to be the same character as x; we will then always have $Uxx = Ux$; that is, an elementary selective symbol x is always such that $x^2 = x$. These are but the symbols of ordinary algebra which satisfy this relation, namely 1 and 0; these are also the extreme selective symbols *all* and *none*. The law in question was considered Boole's paradox; it plays a very great part in the development of his method.

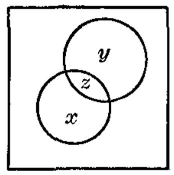


FIG. 2.

Let $Uxy = Uz$, where $=$ means *identical with*, not *equal to*; we may write $xy = z$, leaving the U to be understood. It does not mean that the combination of characters xy is identical with the character z; but that those points which have the characters x and y are identical with the points which have the character z (Fig. 2). From $xy = z$, we derive $x = \frac{1}{y}z$; what is the meaning of this expression? We shall return to the question, after we have considered $+$ and $-$.

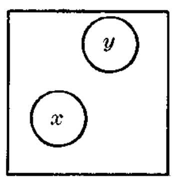


FIG. 3

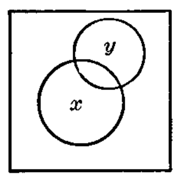


FIG. 4

Let us now consider the expression $U(x + y)$. If the x points and the y points are outside of one another, it means the sum of the x points and the y points (Fig. 3). So far all are agreed. But suppose that the x points and the y points are partially identical (Fig. 4); then there arises difference of opinion. Boole held that the common points must be taken twice over, or in other words that the symbols x and y must be treated all the same as if they were independent of one another; otherwise, he held, no general analysis is possible. $U(x + y)$ will not in general denote a single class of points; it will involve in general a duplication.

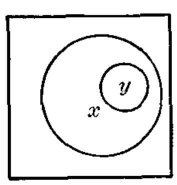


FIG. 5.

Similarly, Boole held that the expression $U(x-y)$ does not involve the condition of the Uy being wholly included in the Ux (Fig. 5). If that condition is satisfied, $U(x-y)$ denotes a simple class; namely, the Ux's *without* the Uy's. But when there is partial coincidence (as in Fig. 4), the common points will be cancelled, and the result will be the Ux's which are not y taken positively and the Uy's which are not x taken negatively. In Boole's view $U(x-y)$ was in general an intermediate uninterpretable form, which might be used in reasoning the same way as analysts used $\sqrt{-1}$.

Most of the mathematical logicians who have come after Boole are men who would have stuck at the impossible subtraction in ordinary algebra. They say virtually, "How can you throw into a heap the same things twice over; and how can you take from a heap things that are not there." Their great principle is the impossibility of taking the pants from a Highlander. Their only conception of the analytical processes of addition and subtraction is throwing into a heap and taking out of a heap. It does not occur to them that the processes of algebra are *ideal*, and not subject to gross material restrictions.

If $x+y$ denotes a quality without duplication, it will satisfy the condition

$$\begin{aligned} (x+y)^2 &= x+y, \\ x^2+2xy+y^2 &= x+y, \\ \text{but } x^2 = x, y^2 &= y, \\ \therefore 2xy &= 0. \end{aligned}$$

Similarly, if $x-y$ denote a simple quality, then

$$\begin{aligned} (x-y)^2 &= x-y, \\ x^2+y^2-2xy &= x-y, \\ x^2 = x, \quad y^2 &= y, \\ \text{therefore, } y-2xy &= -y, \\ \therefore y &= xy. \end{aligned}$$

In other words, the Uy must be included in the Ux (Fig. 5). Here we have assumed that the law of signs is the same as in ordinary algebra, and the result comes out correct.

Suppose Uz=Uxy; then $Ux = U\frac{1}{y}z$. How are the Ux's related to the Uy's, and the Uz's? From the diagram (in Fig. 2) we see that the Ux's are identical with all the Uyz's together with an indefinite portion of the U's, which are neither y nor z. Boole discovered a general method for finding the meaning of any function of elementary logical symbols, which applied to the above case, is as follows:

When y is an elementary symbol,

$$1 = y + (1-y).$$
$$\text{Similarly } 1 = z + (1-z).$$
$$\therefore 1 = yz + y(1-z) + (1-y)z + (1-y)(1-z),$$

which means that the U's either have both qualities y and z, or y but not z, or z but not y, or neither y and z. Let

$$\frac{1}{y}z = Ayz + By(1-z) + C(1-y)z + D(1-y)(1-z),$$

it is required to determine the coefficients A, B, C, D. Suppose $y = 1$, $z = 1$; then $1 = A$. Suppose $y = 1$, $z = 0$, then $0 = B$. Suppose $y = 0$, $z = 1$; then $\frac{1}{0} = C$, and C is infinite; therefore $(1-y)z = 0$; which we see to be true from the diagram. Suppose $y = 0$, $z = 0$; then $\frac{0}{0} = D$, or D is indeterminate. Hence

$$\frac{1}{y}z = yz + \text{an indefinite portion of } (1-y)(1-z).$$

*   *   *   *   *

Boole attached great importance to the index law $x^2 = x$. He held that it expressed a law of thought, and formed the characteristic distinction of the operations of the mind in its ordinary discourse and reasoning, as compared with its operations when occupied with the general algebra of quantity. It makes possible, he said, the solution of a quintic or equation of higher degree, when the symbols are logical. He deduces from it the axiom of metaphysicians which is termed the principle of contradiction, and which affirms that it is impossible for any being to possess a quality, and at the same time not to possess it. Let x denote an elementary quality applicable to the universe U; then $1 - x$ denotes the absence of that quality. But if $x^2 = x$, then $0 = x - x^2, 0 = x(1-x)$, that is, from $Ux^2 = Ux$ we deduce $Ux(1-x) = 0$.

He considers $x(1-x) = 0$ as an expression of the principle of contradiction. He proceeds to remark: "The above interpretation has been introduced not on account of its immediate value in the present system, but as an illustration of a significant fact in the philosophy of the intellectual powers, viz., that what has been commonly regarded as the fundamental axiom of metaphysics is but the consequence of a law of thought, mathematical in its form. I desire to direct attention also to the circumstance that the equation in which that fundamental law of thought is expressed is an equation of the second degree. Without speculating at all in this chapter upon the question whether that circumstance is necessary in its own nature, we may venture to assert that if it had not existed, the whole procedure of the understanding would have been different from what it is."

We have seen that De Morgan investigated long and published much on mathematical logic. His logical writings are characterized by a display of many symbols, new alike to logic and to mathematics; in the words of Sir W. Hamilton of Edinburgh, they are "horrent with mysterious spiculæ." It was the great merit of Boole's work that he used the immense power of the ordinary algebraic notation as an exact language and proved its power for making ordinary language more exact. De Morgan could well appreciate the magnitude of the feat, and he gave generous testimony to it as follows:

"Boole's system of logic is but one of many proofs of genius and patience combined. I might legitimately have entered it among my *paradoxes*, or things counter to general opinion: but it is a paradox which, like that of Copernicus, excited admiration from its first appearance. That the symbolic processes of algebra, invented as tools of numerical calculation, should be competent to express every act of thought, and to furnish the grammar and dictionary of an all-containing system of logic, would not have been believed until it was proved. When Hobbes, in the time of the Commonwealth, published his "Computation or Logique" he had a remote glimpse of some of the points which are placed in the light of day by Mr. Boole. The unity of the forms of thought in all the applications of reason, however remotely separated, will one day be matter of notoriety and common wonder: and Boole's name will be remembered in connection with one of the most important steps towards the attainment of this knowledge."

Chapter 5

ARTHUR CAYLEY[1]

(1821-1895)

Arthur Cayley was born at Richmond in Surrey, England, on August 16, 1821. His father, Henry Cayley, was descended from an ancient Yorkshire family, but had settled in St. Petersburg, Russia, as a merchant. His mother was Maria Antonia Doughty, a daughter of William Doughty; who, according to some writers, was a Russian; but her father's name indicates an English origin. Arthur spent the first eight years of his life in St. Petersburg. In 1829 his parents took up their permanent abode at Blackheath, near London; and Arthur was sent to a private school. He early showed great liking for, and aptitude in, numerical calculations. At the age of 14 he was sent to King's College School, London; the master of which, having observed indications of mathematical genius, advised the father to educate his son, not for his own business, as he had at first intended, but to enter the University of Cambridge.

At the unusually early age of 17 Cayley began residence at Trinity College, Cambridge. As an undergraduate he had generally the reputation of being a mere mathematician; his chief diversion was novel-reading. He was also fond of travelling and mountain climbing, and was a member of the Alpine Club. The cause of the Analytical Society had now triumphed, and the *Cambridge Mathematical Journal* had been instituted by Gregory and Leslie Ellis. To this journal, at the age of twenty, Cayley contributed three papers, on subjects which had been suggested by reading the *Mécanique analytique*

[1]This Lecture was delivered April 20, 1901.—EDITORS.

of Lagrange and some of the works of Laplace. We have already noticed how the works of Lagrange and Laplace served to start investigation in Hamilton and Boole. Cayley finished his undergraduate course by winning the place of Senior Wrangler, and the first Smith's prize. His next step was to take the M.A. degree, and win a Fellowship by competitive examination. He continued to reside at Cambridge for four years; during which time he took some pupils, but his main work was the preparation of 28 memoirs to the *Mathematical Journal*. On account of the limited tenure of his fellowship it was necessary to choose a profession; like De Morgan, Cayley chose the law, and at 25 entered at Lincoln's Inn, London. He made a specialty of conveyancing and became very skilled at the work; but he regarded his legal occupation mainly as the means of providing a livelihood, and he reserved with jealous care a due portion of his time for mathematical research. It was while he was a pupil at the bar that he went over to Dublin for the express purpose of hearing Hamilton's lectures on Quaternions. He sat alongside of Salmon (now provost of Trinity College, Dublin) and the readers of Salmon's books on Analytical Geometry know how much their author was indebted to his correspondence with Cayley in the matter of bringing his textbooks up to date. His friend Sylvester, his senior by five years at Cambridge, was then an actuary, resident in London; they used to walk together round the courts of Lincoln's Inn, discussing the theory of invariants and covariants. During this period of his life, extending over fourteen years, Cayley produced between two and three hundred papers.

At Cambridge University the ancient professorship of pure mathematics is denominated the Lucasian, and is the chair which was occupied by Sir Isaac Newton. About 1860 certain funds bequeathed by Lady Sadleir to the University, having become useless for their original purpose, were employed to establish another professorship of pure mathematicas, called the Sadlerian. The duties of the new professor were defined to be "to explain and teach the principles of pure mathematics and to apply himself to the advancement of that science." To this chair Cayley was elected when 42 years old. He gave up a lucrative practice for a modest salary; but he never regretted the exchange, for the chair at Cambridge enabled him to end the divided allegiance between law and mathematics, and to devote his energies to the pursuit which he liked best. He at once married and settled down in Cambridge. More fortunate than Hamilton in his choice, his home life was one of great happiness. His friend and fellow investigator, Sylvester, once remarked that Cayley had been much more fortunate than himself; that they both lived as bachelors in London, but that Cayley had married and settled down to a quiet and peaceful life at Cambridge; whereas he had never married, and had been fighting the world all his days. The remark was only too true (as may be seen in the lecture on Sylvester).

At first the teaching duty of the Sadlerian professorship was limited to a course of lectures extending over one of the terms of the academic year; but when the University was reformed about 1886, and part of the college funds applied to the better endowment of the University professors, the lectures were extended over two terms. For many years the attendance was small, and came almost entirely from those who had finished their career of preparation for competitive examinations; after the reform the attendance numbered about fifteen. The subject lectured on was generally that of the memoir on which the professor was for the time engaged.

The other duty of the chair—the advancement of mathematical science was—discharged in a handsome manner by the long series of memoirs which he published, ranging over every department of pure mathematics. But it was also discharged in a much less obtrusive way; he became the standing referee on the merits of mathematical papers to many societies both at home and abroad. Many mathematicians, of whom Sylvester was an example, find it irksome to study what others have written, unless, perchance, it is something dealing directly with their own line of work. Cayley was a man of more cosmopolitan spirit; he had a friendly sympathy with other workers, and especially with young men making their first adventure in the field of mathematical research. Of referee work he did an immense amount; and of his kindliness to young investigators I can speak from personal experience. Several papers which I read before the Royal Society of Edinburgh on the Analysis of Relationships were referred to him, and he recommended their publication. Soon after I was invited by the Anthropological Society of London to address them on the subject, and while there, I attended a meeting of the Mathematical Society of London. The room was small, and some twelve mathematicians were assembled round a table, among whom was Prof. Cayley, as became evident to me from the proceedings. At the close of the meeting Cayley gave me a cordial handshake and referred in the kindest terms to my papers which he had read. He was then about 60 years old, considerably bent, and not filling his clothes. What was most remarkable about him was the active glance of his gray eyes and his peculiar boyish smile.

In 1876 he published a *Treatise on Elliptic Functions*, which was his only book. He took great interest in the movement for the University education of women. At Cambridge the women's colleges are Girton and Newnham. In the early days of Girton College he gave direct help in teaching, and for some years he was chairman of the council of Newnham College, in the progress of which he took the keenest interest to the last. His mathematical investigations did not make him a recluse; on the contrary he was of great practical usefulness, especially from his knowledge of law, in the administration of the University.

In 1872 he was made an honorary fellow of Trinity College, and three years later an ordinary fellow, which meant stipend as well as honor. About this time his friends subscribed for a presentation portrait, which now hangs on the side wall of the dining hall of Trinity College, next to the portrait of James Clerk Maxwell, while on the end wall, behind the high table, hang the more ancient portraits of Sir Isaac Newton and Lord Bacon of Verulam. In the portrait Cayley is represented as seated at a desk, quill in hand, after the mode in which he used to write out his mathematical investigations. The investigation, however, was all thought out in his mind before he took up the quill.

Maxwell was one of the greatest electricians of the nineteenth century. He was a man of philosophical insight and poetical power, not unlike Hamilton, but differing in this, that he was no orator. In that respect he was more like Goldsmith, who "could write like an angel, but only talked like poor poll." Maxwell wrote an address to the committee of subscribers who had charge of the Cayley portrait fund, wherein the scientific poet with his pen does greater honor to the mathematician than the artist, named Dickenson, could do with his brush. Cayley had written on space of n dimensions, and the main point in the address is derived from the artist's business of depicting on a plane what exists in space:

> O wretched race of men, to space confined!
> What honor can ye pay to him whose mind
> To that which lies beyond hath penetrated?
> The symbols he hath formed shall sound his praise,
> And lead him on through unimagined ways
> To conquests new, in worlds not yet created.
>
> First, ye Determinants, in ordered row
> And massive column ranged, before him go,
> To form a phalanx for his safe protection.

Ye powers of the nth root of -1!
Around his head in endless cycles run,
As unembodied spirits of direction.

And you, ye undevelopable scrolls!
Above the host where your emblazoned rolls,
Ruled for the record of his bright inventions.
Ye cubic surfaces! by threes and nines
Draw round his camp your seven and twenty lines
The seal of Solomon in three dimensions.

March on, symbolic host! with step sublime,
Up to the flaming bounds of Space and Time!
There pause, until by Dickenson depicted
In two dimensions, we the form may trace
Of him whose soul, too large for vulgar space,
In n dimensions flourished unrestricted.

The verses refer to the subjects investigated in several of Cayley's most elaborate memoirs; such as, Chapters on the Analytical Geometry of n dimensions; On the theory of Determinants; Memoir on the theory of Matrices; Memoirs on skew surfaces, otherwise Scrolls; On the delineation of a Cubic Scroll, etc.

In 1881 he received from the Johns Hopkins University, Baltimore, where Sylvester was then professor of mathematics, an invitation to deliver a course of lectures. He accepted the invitation, and lectured at Baltimore during the first five months of 1882 on the subject of the *Abelian and Theta Functions*.

The next year Cayley came prominently before the world, as President of the British Association for the Advancement of Science. The meeting was held at Southport, in the north of England. As the President's address is one of the great popular events of the meeting, and brings out an audience of general culture, it is usually made as little technical as possible. Hamilton was the kind of mathematician to suit such an occasion, but he never got the office, on account of his occasional breaks. Cayley had not the oratorical, the philosophical, or the poetical gifts of Hamilton, but then he was an eminently safe man. He took for his subject the Progress of Pure Mathematics; and he opened his address in the following naïve manner: "I wish to speak to you to-night upon Mathematics. I am quite aware of the difficulty arising from the abstract nature of my subject; and if, as I fear, many or some of you, recalling the providential addresses at former meetings, should wish that you were now about to have from a different President a discourse on a different subject, I can very well sympathize with you in the feeling. But be that as it may, I think it is more respectful to you that I should speak to you upon and do my best to interest you in the subject which has occupied me, and in which I am myself most interested. And in another point of view, I think it is right that the address of a president should be on his own subject, and that different subjects should be thus brought in turn before the meetings. So much the worse, it may be, for a particular meeting: but the meeting is the individual, which on evolution principles, must be sacrificed for the development of the race." I daresay that after this introduction, all the evolution philosophers listened to him attentively, whether they understood him or not. But Cayley doubtless felt that he was addressing not only the popular audience then and there before him, but the mathematicians of distant places and future times; for the address is a valuable historical review of various mathematical theories, and is characterized by freshness, independence of view, suggestiveness, and learning.

In 1889 the Cambridge University Press requested him to prepare his mathematical papers for publication in a collected form—a request which he appreciated very much. They are printed in magnificent quarto volumes, of which seven appeared under his own editorship. While editing these volumes, he was suffering from a painful internal malady, to which he succumbed on January 26, 1895, in the 74th year of his age. When the funeral took place, a great assemblage met in Trinity Chapel, comprising members of the University, official representatives of Russia and America, and many of the most illustrious philosophers of Great Britain.

The remainder of his papers were edited by Prof. Forsyth, his successor in the Sadlerian chair. The Collected Mathematical papers number thirteen quarto volumes, and contain 967 papers. His writings are his best monument, and certainly no mathematician has ever had his monument in grander style. De Morgan's works would be more extensive, and much more useful, but he did not have behind him a University Press. As regards fads, Cayley retained to the last his fondness for novel-reading and for travelling. He also took special pleasure in paintings and architecture, and he practised water-color painting, which he found useful sometimes in making mathematical diagrams.

To the third edition of Tait's *Elementary Treatise on Quaternions*, Cayley contributed a chapter entitled "Sketch of the analytical theory of quaternions." In it the $\sqrt{-1}$ reappears in all its glory, and in entire, so it is said, independence of i, j, k. The remarkable thing is that Hamilton started with a quaternion theory of analysis, and that Cayley should present instead an analytical theory of quaternions. I daresay that Prof. Tait was sorry that he allowed the chapter to enter his book, for in 1894 there arose a brisk discussion between himself and Cayley on "Coordinates versus Quaternions," the record of which is printed in the Proceedings of the Royal Society of Edinburgh. Cayley maintained the position that while coordinates are applicable to the whole science of geometry and are the natural and appropriate basis and method in the science, quaternions seemed a particular and very artificial method for treating such parts of the science of three-dimensional geometry as are most naturally discussed by means of the rectangular coordinates x, y, z. In the course of his paper Cayley says: "I have the highest admiration for the notion of a quaternion; but, as I consider the full moon far more beautiful than any moonlit view, so I regard the notion of a quaternion as far more beautiful than any of its applications. As another illustration, I compare a quaternion formula to a pocket-map—a capital thing to put in one's pocket, but which for use must be unfolded: the formula, to be understood, must be translated into coordinates." He goes on to say, "I remark that the imaginary of ordinary algebra—for distinction call this θ—has no relation whatever to the quaternion symbols i, j, k; in fact, in the general point of view, all the quantities which present themselves, are, or may be, complex values $a+\theta b$, or in other words, say that a scalar quantity is in general of the form $a + \theta b$. Thus quaternions do not properly present themselves in plane or two-dimensional geometry at all; but they belong essentially to solid or three-dimensional geometry, and they are most naturally applicable to the class of problems which in coordinates are dealt with by means of the three rectangular coordinates x, y, z."

To the pocketbook illustration it may be replied that a set of coordinates is an immense wall map, which you cannot carry about, even though you should roll it up, and therefore is useless for many important purposes. In reply to the arguments, it may be said, *first*, $\sqrt{-1}$ has a relation to the symbols i, j, k, for each of these can be analyzed into a unit axis multiplied by $\sqrt{-1}$; *second*, as regards plane geometry, the ordinary form of complex quantity is a degraded form of the quaternion in which the constant axis of the plane is left unspecified. Cayley took his illustrations from his experience as a traveller. Tait brought forward an illustration from which you might imagine he had visited the Bethlehem Iron Works, and hunted tigers in India. He says, "A much more natural and adequate comparison would, it seems to me, liken Coordinate Geometry to a steam-hammer, which an expert may employ on any destructive or constructive work of one general kind, say the cracking of an eggshell, or the welding of an anchor. But you must have your expert to manage it, for without him it is useless. He has to toil amid the heat, smoke, grime, grease, and perpetual din of the suffocating engine-room. The work has to be brought to the hammer, for it cannot usually be taken to its work. And it is not in general, transferable; for each expert, as a rule, knows, fully and confidently, the working details of his own weapon only. Quaternions, on the other hand, are like the elephant's trunk, ready at *any* moment for *anything*, be it to pick up a crumb or a field-gun, to strangle a tiger, or uproot a tree; portable in the extreme, applicable anywhere—alike in the trackless jungle and in the barrack square—directed by a little native who requires no special skill or training, and who can be transferred from one elephant to another without much hesitation. Surely this, which adapts itself to its work, is the grander instrument. But then, *it* is the natural, the other, the artificial one."

The reply which Tait makes, so far as it is an argument, is: There are two systems of quaternions, the i, j, k one, and another one which Hamilton developed from it; Cayley knows the first only, he himself knows the second; the former is an intensely artificial system of imaginaries, the latter is the natural organ of expression for quantities in space. Should a fourth edition of his *Elementary Treatise* be called for i, j, k will disappear from it, excepting in Cayley's chapter, should it be retained. Tait thus describes the first system: "Hamilton's extraordinary *Preface* to his first great book shows how from Double Algebras, through Triplets, Triads, and Sets, he finally reached Quaternions. This was the genesis of the Quaternions of the forties, and the creature thus produced is still essentially the Quaternion of Prof. Cayley. It is a magnificent analytical conception; but it is nothing more than the full development of the system of imaginaries i, j, k; defined by the equations,

$$i^2 = j^2 = k^2 = ijk = -1,$$

with the associative, but *not* the commutative, law for the factors. The novel and splendid points in it were the treatment of all directions in space as essentially alike in character, and the recognition of the unit vector's claim to rank also as a quadrantal versor. These were indeed inventions of the first magnitude, and of vast importance. And here I thoroughly agree with Prof. Cayley in his admiration. Considered as an analytical system, based throughout on pure imaginaries, the Quaternion method is elegant in the extreme. But, unless it had been also something more, something very different and much higher in the scale of development, I should have been content to admire it;—and to pass it by."

From "the most intensely artificial of systems, arose, as if by magic, an absolutely natural one" which Tait thus further describes. "To me Quaternions are primarily a Mode of Representation:—immensely superior to, but of essentially the same kind of usefulness as, a diagram or a model. They are, virtually, the thing represented; and are thus antecedent to, and independent of, coordinates; giving, in general, all the main relations, in the problem to which they are applied, without the necessity of appealing to coordinates at all. Coordinates may, however, easily be read into them:—when anything (such as metrical or numerical detail) is to be gained thereby. Quaternions, in a word, exist in space, and we have only to recognize them:—but we have to invent or imagine coordinates of all kinds."

To meet the objection why Hamilton did not throw i, j, k overboard, and expound the developed system, Tait says: "Most unfortunately, alike for himself and for his grand conception, Hamilton's nerve failed him in the composition of his first great volume. Had he then renounced, for ever, all dealings with i, j, k, his triumph would have been complete. He spared Agog, and the best of the sheep, and did not utterly destroy them. He had a paternal fondness for i, j, k; perhaps also a not unnatural liking for a meretricious title such as the mysterious word *Quaternion*; and, above all, he had an earnest desire to make the utmost return in his power for the liberality shown him by the authorities of Trinity College, Dublin. He had fully recognized, and proved to others, that his i, j, k, were mere excrescences and blots on his improved method:—but he unfortunately considered that their continued (if only partial) recognition was indispensable to the reception of his method by a world steeped in—Cartesianism! Through the whole compass of each of his tremendous volumes one can find traces of his desire to avoid even an allusion to i, j, k, and along with them, his sorrowful conviction that, should he do so, he would be left without a single reader."

To Cayley's presidential address we are indebted for information about the view which he took of the foundations of exact science, and the philosophy which commended itself to his mind. He quoted Plato and Kant with approval, J. S. Mill with faint praise. Although he threw a sop to the empirical philosophers at the beginning of his address, he gave them something to think of before he finished.

He first of all remarks that the connection of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space; in which he, of course, made a hit at Hamilton's theory of Algebra as the science of pure time. Further on he discusses the theory directly, and concludes as follows: "Hamilton uses the term algebra in a very wide sense, but whatever else he includes under it, he includes all that in contradistinction to the Differential Calculus would be called algebra. Using the word in this restricted sense, I cannot myself recognize the connection of algebra with the notion of time; granting that the notion of continuous progression presents itself and is of importance, I do not see that it is in anywise the fundamental notion of the science. And still less can I appreciate the manner in which the author connects with the notion of time his algebraical couple, or imaginary magnitude, $a + b\sqrt{-1}$." So you will observe that doctors differ—Tait and Cayley—about the soundness of Hamilton's theory of couples. But it can be shown that a couple may not only be represented on a straight line, but actually means a portion of a straight line; and as a line is unidimensional, this favors the truth of Hamilton's theory.

As to the nature of mathematical science Cayley quoted with approval from an address of Hamilton's:

"These purely mathematical sciences of algebra and geometry are sciences of the pure reason, deriving no weight and no assistance from experiment, and isolated or at least isolable from all outward and accidental phenomena. The idea of order with its subordinate ideas of number and figure, we must not call innate ideas, if that phrase be defined to imply that all men must possess them with equal clearness and fulness; they are, however, ideas which seem to be so far born with us that the possession of them in any conceivable degree is only the development of our original powers, the unfolding of our proper humanity."

It is the aim of the evolution philosopher to reduce all knowledge to the empirical status; the only intuition he grants is a kind of instinct formed by the experience of ancestors and transmitted cumulatively by heredity. Cayley first takes him up on the subject of arithmetic: "Whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic; mathematician, or not, we have each of us, in its most abstract form, the idea of number; we can each of us appreciate the truth of a proposition in numbers; and we cannot but see that a truth in regard to numbers is something different in kind from an experimental truth generalized from experience. Compare, for instance, the proposition, that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on; and the proposition that even and odd numbers succeed each other alternately *ad infinitum*; the latter at least seems to have the characters of universality and necessity. Or again, suppose a proposition observed to hold good for a long series of numbers, one thousand numbers, two thousand numbers, as the case may be: this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition, holding good for all numbers whatever; there are in the Theory of Numbers very remarkable instances of propositions observed to hold good for very long series of numbers which are nevertheless untrue."

Then he takes him up on the subject of geometry, where the empiricist rather boasts of his success. "It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration; and that Lobatschewsky constructed a perfectly consistent theory, wherein this axiom was assumed not to hold good, or say a system of non-Euclidean plane geometry. My own view is that Euclid's twelfth axiom in Playfair's form of it does not need demonstration, but is part of our notion of space, of the physical space of our experience—the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. Riemann's view before referred to may I think be said to be that, having *in intellectu* a more general notion of space (in fact a notion of non-Euclidean space), we learn by experience that space (the physical space of our experience) is, if not exactly, at least to the highest degree of approximation, Euclidean space. But suppose the physical space of our experience to be thus only approximately Euclidean space, what is the consequence which follows? *Not* that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidean space which has been so long regarded as being the physical space of our experience."

In his address he remarks that the fundamental notion which underlies and pervades the whole of modern analysis and geometry is that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry. In the case of two given curves there are two equations satisfied by the coordinates (x, y) of the several points of intersection, and these give rise to an equation of a certain order for the coordinate x or y of a point of intersection. In the case of a straight line and a circle this is a quadratic equation; it has two roots real or imaginary. There are thus two values, say of x, and to each of these corresponds a single value of y. There are therefore two points of intersection, viz., a straight line and a circle intersect always in two points, real or imaginary. It is in this way we are led analytically to the notion of imaginary points in geometry. He asks, What is an imaginary point? Is there in a plane a point the coordinates of which have given imaginary values? He seems to say No, and to fall back on the notion of an imaginary space as the *locus in quo* of the imaginary point.

Chapter 6

WILLIAM KINGDON CLIFFORD[1]

(1845-1879)

William Kingdon Clifford was born at Exeter, England, May 4, 1845. His father was a well-known and active citizen and filled the honorary office of justice of the peace; his mother died while he was still young. It is believed that Clifford inherited from his mother not only some of his genius, but a weakness in his physical constitution. He received his elementary education at a private school in Exeter, where examinations were annually held by the Board of Local Examinations of the Universities of Oxford and Cambridge; at these examinations Clifford gained numerous distinctions in widely different subjects. When fifteen years old he was sent to King's College, London, where he not only demonstrated his peculiar mathematical abilities, but also gained distinction in classics and English literature.

When eighteen, he entered Trinity College, Cambridge; the college of Peacock, De Morgan, and Cayley. He already had the reputation of possessing extraordinary mathematical powers; and he was eccentric in appearance, habits and opinions. He was reported to be an ardent High Churchman, which was then a more remarkable thing at Cambridge than it is now. His undergraduate career was distinguished by eminence in mathematics, English literature and gymnastics. One who was his companion in gymnastics wrote: "His neatness and dexterity were unusually great, but the most remarkable thing was his great strength as compared with his weight, as shown

[1]This Lecture was delivered April 23, 1901.—Editors.

in some exercises. At one time he would pull up on the bar with either hand, which is well known to be one of the greatest feats of strength. His nerve at dangerous heights was extraordinary." In his third year he won the prize awarded by Trinity College for declamation, his subject being Sir Walter Raleigh; as a consequence he was called on to deliver the annual oration at the next Commemoration of Benefactors of the College. He chose for his subject, Dr. Whewell, Master of the College, eminent for his philosophical and scientific attainments, whose death had occurred but recently. He treated it in an original and unexpected manner; Dr. Whewell's claim to admiration and emulation being put on the ground of his intellectual life exemplifying in an eminent degree the active and creating faculty. "Thought is powerless, except it make something outside of itself; the thought which conquers the world is not contemplative but active. And it is this that I am asking you to worship to-day."

To obtain high honors in the Mathematical Tripos, a student must put himself in special training under a mathematican, technically called a coach, who is not one of the regular college instructors, nor one of the University professors, but simply makes a private business of training men to pass that particular examination. Skill consists in the rate at which one can solve and more especially write out the solution of problems. It is excellent training of a kind, but there is no time for studying fundamental principles, still less for making any philosophical investigations. Mathematical insight is something higher than skill in solving problems; consequently the senior wrangler has not always turned out the most distinguished mathematician in after life. We have seen that De Morgan was fourth wrangler. Clifford also could not be kept to the dust of the race-course; but such was his innate mathematical insight that he came out second wrangler. Other instances of the second wrangler turning out the better mathematician are Whewell, Sylvester, Kelvin, Maxwell.

In 1868, when he was 23 years old, he was elected a Fellow of his College; and while a resident fellow, he took part in the eclipse expedition of 1870 to Italy, and passed through the experience of a shipwreck near Catania on the coast of the island of Sicily. In 1871 he was appointed professor of Applied Mathematics and Mechanics in University College, London; De Morgan's college, but not De Morgan's chair. Henceforth University College was the centre of his labors.

He was now urged by friends to seek admission into the Royal Society of London. This is the ancient scientific society of England, founded in the time of Charles II, and numbering among its first presidents Sir Isaac Newton. About the middle of the nineteenth century the admission of new members was restricted to fifteen each year; and from applications the Council recommends fifteen names which are posted up, and subsequently balloted for by the Fellows. Hamilton and De Morgan never applied. Clifford did not apply immediately, but he became a Fellow a few years later. He joined the London Mathematical Society—for it met in University College—and he became one of its leading spirits. Another metropolitan Society in which he took much interest was the Metaphysical Society; like Hamilton, De Morgan, and Boole, Clifford was a scientific philosopher.

In 1875 Clifford married; the lady was Lucy, daughter of Mr. John Lane, formerly of Barbadoes. His home in London became the meeting-point of a numerous body of friends, in which almost every possible variety of taste and opinion was represented, and many of whom had nothing else in common. He took a special delight in amusing children, and for their entertainment wrote a collection of fairy tales called *The Little People*. In this respect he was like a contemporary mathematician, Mr. Dodgson—"Lewis Carroll"—the author of *Alice in Wonderland*. A children's party was one of Clifford's greatest pleasures. At one such party he kept a waxwork show, children doing duty for the figures; but I daresay he drew the line at walking on all fours, as Mr. Dodgson was accustomed to do. A children's party was to be held in a house in London and it happened that there was a party of adults held simultaneously in the neighboring house; to give the children a surprise Dodgson resolved to walk in on all fours; unfortunately he crawled into the parlor of the wrong house!

Clifford possessed unsurpassed power as a teacher. Mr. Pollock, a fellow student, gives an instance of Clifford's theory of what teaching ought to be, and his constant way of carrying it out in his discourses and conversations on mathematical and scientific subjects. "In the analytical treatment of statics there occurs a proposition called Ivory's Theorem concerning the attractions of an ellipsoid. The textbooks demonstrate it by a formidable apparatus of coordinates and integrals, such as we were wont to call a *grind*. On a certain day in the Long Vacation of 1866, which Clifford and I spent at Cambridge, I was not a little exercised by the theorem in question, as I suppose many students have been before and since. The chain of symbolic proof seemed artificial and dead; it compelled the understanding, but failed to satisfy the reason. After reading and learning the proposition one still failed to see what it was all about. Being out for a walk with Clifford, I opened my perplexities to him; I think that I can recall the very spot. What he said I do not remember in detail; which is not surprising, as I have had no occasion to remember anything about Ivory's Theorem these twelve years. But I know that as he spoke he appeared not to be working out a question, but simply telling what he saw. Without any diagram or symbolic aid he described the geometrical conditions on which the solution depended, and they seemed to stand out visibly in space. There were no longer consequences to be deduced, but real and evident facts which only required to be seen."

Clifford inherited a constitution in which nervous energy and physical strength were unequally balanced. It was in his case specially necessary to take good care of his health, but he did the opposite; he would frequently sit up most of the night working or talking. Like Hamilton he would work twelve hours on a stretch; but, unlike Hamilton, he had laborious professional duties demanding his personal attention at the same time. The consequence was that five years after his appointment to the chair in University College, his health broke down; indications of pulmonary disease appeared. To recruit his health he spent six months in Algeria and Spain, and came back to his professional duties again. A year and a half later his health broke down a second time, and he was obliged to leave again for the shores of the Mediterranean. In the fall of 1878 he returned to England for the last time, when the winter came he left for the Island of Madeira; all hope of recovery was gone; he died March 3, 1879 in the 34th year of his age.

On the title page of the volume containing his collected mathematical papers I find a quotation, "If he had lived we might have known something." Such is the feeling one has when one looks at his published works and thinks of the shortness of his life. In his lifetime there appeared *Elements of Dynamic, Part I*. Posthumously there have appeared *Elements of Dynamic, Part II; Collected Mathematical Papers; Lectures and Essays; Seeing and Thinking; Common Sense of the Exact Sciences*. The manuscript of the last book was left in a very incomplete state, but the design was filled up and completed by two other mathematicians.

In a former lecture I had occasion to remark on the relation of Mathematics to Poetry—on the fact that in mathematical investigation there is needed a higher power of imagination akin to the creative instinct of the poet. The matter is discussed by Clifford in a discourse on "Some of the conditions of mental development," which he delivered at the Royal Institution in 1868 when he was 23 years of age. This institution was founded by Count Rumford, an American, and is located in London. There are Professorships of Chemistry, Physics, and Physiology; its professors have included Davey, Faraday, Young, Tyndall, Rayleigh, Dewar. Their duties are not to teach the elements of their science to regular students, but to make investigations, and to lecture to the members of the institution, who are in general wealthy and titled people.

In this discourse Clifford said "Men of science have to deal with extremely abstract and general conceptions. By constant use and familiarity, these, and the relations between them, become just as real and external as the ordinary objects of experience, and the perception of new relations among them is so rapid, the correspondence of the mind to external circumstances so great, that a real scientific sense is developed, by which things are perceived as immediately and truly as I see you now. Poets and painters and musicians also are so accustomed to put outside of them the idea of beauty, that it becomes a real external existence, a thing which they see with spiritual eyes and then describe to you, but by no means create, any more than we seem to create the ideas of table and forms and light, which we put together long ago. There is no scientific discoverer, no poet, no painter, no musician, who will not tell you that he found ready made his discovery or poem or picture—that it came to him from outside, and that he did not consciously create it from within. And there is reason to think that these senses or insights are things which actually increase among mankind. It is certain, at least, that the scientific sense is immensely more developed now than it was three hundred years ago; and though it may be impossible to find any absolute standard of art, yet it is acknowledged that a number of minds which are subject to artistic training will tend to arrange themselves under certain great groups and that the members of each group will give an independent and yet consentient testimony about artistic questions. And this arrangement into schools, and the definiteness of the conclusions reached in each, are on the increase, so that here, it would seem, are actually two new senses, the scientific and the artistic, which the mind is now in the process of forming for itself."

Clifford himself wrote a good many poems, but only a few have been published. The following verses were sent to George Eliot, the novelist, with a presentation copy of *The Little People*:

Baby drew a little house,
  Drew it all askew;
Mother saw the crooked door

And the window too.

Mother heart, whose wide embrace
Holds the hearts of men,
Grows with all our growing hopes,
Gives them birth again,

Listen to this baby-talk:
'Tisn't wise or clear;
But what baby-sense it has
Is for you to hear.

An amusement in which Clifford took pleasure even in his maturer years was the flying of kites. He made some mathematical investigations in the subject, anticipating, as it were, the interest which has been taken in more recent years in the subject of motion through the atmosphere. Clifford formed a project of writing a series of textbooks on Mathematics beginning at the very commencement of each subject and carrying it on rapidly to the most advanced stages. He began with the *Elements of Dynamic*, of which three books were printed in his lifetime, and a fourth book, in a supplementary volume, after his death. The work is unique for the clear ideas given of the science; ideas and principles are more prominent than symbols and formulae. He takes such familiar words as *spin, twist, squirt, whirl*, and gives them an exact meaning. The book is an example of what he meant by scientific insight, and from its excellence we can imagine what the complete series of textbooks would have been.

In Clifford's lifetime it was said in England that he was the only mathematician who could discourse on mathematics to an audience composed of people of general culture and make them think that they understood the subject. In 1872 he was invited to deliver an evening lecture before the members of the British Association, at Brighton; he chose for his subject "The aims and instruments of scientific thought." The main theses of the lecture are *First*, that scientific thought is the application of past experience to new circumstances by means of an observed order of events. *Second*, this order of events is not theoretically or absolutely exact, but only exact enough to correct experiments by. As an instance of what is, and what is not scientific thought, he takes the phenomenon of double refraction. "A mineralogist, by measuring the angles of a crystal, can tell you whether or no it possesses the property of double refraction without looking through it. He requires no scientific thought to do that. But Sir William Rowan Hamilton, knowing these facts and also the explanation of them which Fresnel had given, thought about the subject, and he predicted that by looking through certain crystals in a particular direction we should see not two dots but a continuous circle. Mr. Lloyd made the experiment, and saw the circle, a result which had never been even suspected. This has always been considered one of the most signal instances of scientific thought in the domain of physics. It is most distinctly an application of experience gained under certain circumstances to entirely different circumstances."

In physical science there are two kinds of law—distinguished as "empirical" and "rational." The former expresses a relation which is sufficiently true for practical purposes and within certain limits; for example, many of the formulas used by engineers. But a rational law states a connection which is accurately true, without any modification of limit. In the theorems of geometry we have examples of scientific exactness; for example, in the theorem that the sum of the three interior angles of a plane triangle is equal to two right angles. The equality is one not of approximation, but of exactness. Now the philosopher Kant pointed to such a truth and said: We know that it is true not merely here and now, but everywhere and for all time; such knowledge cannot be gained by experience; there must be some other source of such knowledge. His solution was that space and time are forms of the sensibility; that truths about them are not obtained by empirical induction, but by means of intuition; and that the characters of necessity and universality distinguished these truths from other truths. This philosophy was accepted by Sir William Rowan Hamilton, and to him it was not a barren philosophy, for it served as the starting point of his discoveries in algebra which culminated in the discovery of quaternions.

This philosophy was admired but not accepted by Clifford; he was, so long as he lived, too strongly influenced by the philosophy which has been built upon the theory of evolution. He admits that the only way of escape from Kant's conclusions is by denying the theoretical exactness of the proposition referred to. He says, "About the beginning of the present century the foundations of geometry were criticised independently by two mathematicians, Lobatchewsky and Gauss, whose results have been extended and generalized more recently by Riemann and Helmholtz. And the conclusion to which these investigations lead is that, although the assumptions which were very properly made by the ancient geometers are practically exact—that is to say, more exact than experiment can be—for such finite things as we have to deal with, and such portions of space as we can reach; yet the truth of them for very much larger things, or very much smaller things, or parts of space which are at present beyond our reach, is a matter to be decided by experiment, when its powers are considerably increased. I want to make as clear as possible the real state of this question at present, because it is often supposed to be a question of words or metaphysics, whereas it is a very distinct and simple question of fact. I am supposed to know that the three angles of a rectilinear triangle are exactly equal to two right angles. Now suppose that three points are taken in space, distant from one another as far as the Sun is from α Centauri, and that the shortest distances between these points are drawn so as to form a triangle. And suppose the angles of this triangle to be very accurately measured and added together; this can at present be done so accurately that the error shall certainly be less than one minute, less therefore than the five-thousandth part of a right angle. Then I do not know that this sum would differ at all from two right angles; but also I do not know that the difference would be less than ten degrees or the ninth part of a right angle."

You will observe that Clifford's philosophy depends on the validity of Lobatchewsky's ideas. Now it has been shown by an Italian mathematician, named Beltrami, that the plane geometry of Lobatchewsky corresponds to trigonometry on a surface called the *pseudosphere*. Clifford and other followers of Lobatchewsky admit Beltrami's interpretation, an interpretation which does not involve any paradox about geometrical space, and which leaves the trigonometry of the plane alone as a different thing. If that interpretation is true, the Lobatchewskian plane triangle is after all a triangle on a special surface, and the *straight* lines joining the points are not the shortest absolutely, but only the *shortest* with respect to the surface, whatever that may mean. If so, then Clifford's argument for the empirical nature of the proposition referred to fails; and nothing prevents us from falling back on Kant's position, namely, that there is a body of knowledge characterized by absolute exactness and possessing universal application in time and space; and as a particular case thereof we believe that the sum of the three angles of Clifford's gigantic triangle is precisely two right angles.

Trigonometry on a spherical surface is a generalized form of plane trigonometry, from the theorems of the former we can deduce the theorems of the latter by supposing the radius of the sphere to be infinite. The sum of the three angles of a spherical triangle is greater than two right angles; the sum of the angles of a plain triangle is equal to two right angles; we infer that there is another surface, complementary to the sphere, such that the angles of any triangle on it are less than two right angles. The complementary surface to which I refer is not the pseudosphere, but the equilateral hyperboloid. As the plane is the transition surface between the sphere and the equilateral hyperboloid, and a triangle on it is the transition triangle between the spherical triangle and the equilateral hyperboloidal triangle, the sum of the angles of the plane triangle must be exactly equal to two right angles.

In 1873, the British Association met at Bradford; on this occasion the evening discourse was delivered by Maxwell, the celebrated physicist. He chose for his subject "Molecules." The application of the method of spectrum-analysis assures the physicist that he can find out in his laboratory truths of universal validity in space and time. In fact, the chief maxim of physical science, according to Maxwell is, that physical changes are independent of the conditions of space and time, and depend only on conditions of configuration of bodies, temperature, pressure, etc. The address closed with a celebrated passage in striking contrast to Clifford's address: "In the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another; and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which are found on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in precisely the same time. No theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction. None of the processes of Nature since the time when Nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to any of the causes which we call natural. On the other hand, the exact equality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent."

What reply could Clifford make to this? In a discourse on the "First and last catastrophe" delivered soon afterwards, he said "If anyone not possessing the great authority of Maxwell, had put forward an argument, founded upon a scientific basis, in which there occurred assumptions about what things can and what things cannot have existed from eternity, and about the exact similarity of two or more things established by experiment, we would say: 'Past eternity; absolute exactness; won't do'; and we should pass on to another book. The experience of all scientific culture for all ages during which it has been a light to men has shown us that we never do get at any conclusions of that sort. We do not get at conclusions about infinite time, or infinite exactness. We get at conclusions which are as nearly true as experiment can show, and sometimes which are a great deal more correct than direct experiment can be, so that we are able actually to correct one experiment by deductions from another, but we never get at conclusions which we have a right to say are absolutely exact."

Clifford had not faith in the exactness of mathematical science nor faith in that maxim of physical science which has built up the new astronomy, and extended all the bounds of physical science. Faith in an exact order of Nature was the characteristic of Faraday, and he was by unanimous consent the greatest electrician of the nineteenth century. What is the general direction of progress in science? Physics is becoming more and more mathematical; chemistry is becoming more and more physical, and I daresay the biological sciences are moving in the same direction. They are all moving towards exactness; consequently a true philosophy of science will depend on the principles of mathematics much more than upon the phenomena of biology. Clifford, I believe, had he lived longer, would have changed his philosophy for a more mathematical one. In 1874 there appeared in *Nature* among the letters from correspondents one to the following effect:

An anagram: The practice of enclosing discoveries in sealed packets and sending them to Academies seems so inferior to the old one of Huyghens, that the following is sent you for publication in the old conservated form:

$$A^8C^3DE^{12}F^4GH^6J^6L^3M^3N^5O^6PR^4S^5T^{14}U^6V^2WXY^2.$$

This anagram was explained in a book entitled *The Unseen Universe*, which was published anonymously in 1875; and is there translated, "Thought conceived to affect the matter of another universe simultaneously with this may explain a future state." The book was evidently a work of a physicist or physicists, and as physicists were not so numerous then as they are now, it was not difficult to determine the authorship from internal evidence. It was attributed to Tait, the professor of physics at Edinburgh University, and Balfour Stewart, the professor of physics at Owens College, Manchester. When the fourth edition appeared, their names were given on the title page.

The kernel of the book is the above so-called discovery, first published in the form of an anagram. Preliminary chapters are devoted to a survey of the beliefs of ancient peoples on the subject of the immortality of the soul; to physical axioms; to the physical doctrine of energy, matter, and ether; and to the biological doctrine of development; in the last chapter we come to the unseen universe. What is meant by the *unseen universe*? Matter is made up of molecules, which are supposed to be vortex-rings of an imperfect fluid, namely, the luminiferous ether; the luminous ether is made up of much smaller molecules, which are vortex-rings in a second ether. These smaller molecules with the ether in which they float are the unseen universe. The authors see reason to believe that the unseen universe absorbs energy from the visible universe and *vice versa*. The soul is a frame which is made of the refined molecules and exists in the unseen universe. In life it is attached to the body. Every thought we think is accompanied by certain motions of the coarse molecules of the brain, these motions are propagated through the visible universe, but a part of each motion is absorbed by the fine molecules of the soul. Consequently the soul has an organ of memory as well as the body; at death the soul with its organ of memory is simply set free from association with the coarse molecules of the body. In this way the authors consider that they have shown the physical possibility of the immortality of the soul.

The curious part of the book follows: the authors change their possibility into a theory and apply it to explain the main doctrines of Christianity; and it is certainly remarkable to find in the same book a discussion of Carnot's heat-engine and extensive quotations from the apostles and prophets. Clifford wrote an elaborate review which he finished in one sitting occupying twelve hours. He pointed out the difficulties to which the main speculation, which he admitted to be ingenious, is liable; but his wrath knew no bounds when he proceeded to consider the application to the doctrines of Christianity; for from being a High Churchman in youth he became an agnostic in later years; and he could not write on any religious question without using language which was offensive even to his friends.

The *Phaedo* of Plato is more satisfying to the mind than the *Unseen Universe* of Tait and Stewart. In it, Socrates discusses with his friends the immortality of the soul, just before taking the draught of poison. One argument he advances is, How can the works of an artist be more enduring than the artist himself? This is a question which comes home in force when we peruse the works of Peacock, De Morgan, Hamilton, Boole, Cayley and

Clifford.

Chapter 7

HENRY JOHN STEPHEN SMITH[1]

(1826-1883)

Henry John Stephen Smith was born in Dublin, Ireland, on November 2, 1826. His father, John Smith, was an Irish barrister, who had graduated at Trinity College, Dublin, and had afterwards studied at the Temple, London, as a pupil of Henry John Stephen, the editor of Blackstone's *Commentaries*; hence the given name of the future mathematician. His mother was Mary Murphy, an accomplished and clever Irishwoman, tall and beautiful. Henry was the youngest of four children, and was but two years old when his father died. His mother would have been left in straitened circumstances had she not been successful in claiming a bequest of £10,000 which had been left to her husband but had been disputed. On receiving this money, she migrated to England, and finally settled in the Isle of Wight.

Henry as a child was sickly and very near-sighted. When four years of age he displayed a genius for mastering languages. His first instructor was his mother, who had an accurate knowledge of the classics. When eleven years of age, he, along with his brother and sisters, was placed in the charge of a private tutor, who was strong in the classics; in one year he read a large portion of the Greek and Latin authors commonly studied. His tutor was impressed with his power of memory, quickness of perception, indefatigable diligence, and intuitive grasp of whatever he studied. In their leisure hours

[1]This Lecture was delivered March 15, 1902.—Editors.

the children would improvise plays from Homer, or Robinson Crusoe; and they also became diligent students of animal and insect life. Next year a new tutor was strong in the mathematics, and with his aid Henry became acquainted with advanced arithmetic, and the elements of algebra and geometry. The year following, Mrs. Smith moved to Oxford, and placed Henry under the care of Rev. Mr. Highton, who was not only a sound scholar, but an exceptionally good mathematician. The year following Mr. Highton received a mastership at Rugby with a boardinghouse attached to it (which is important from a financial point of view) and he took Henry Smith with him as his first boarder. Thus at the age of fifteen Henry Smith was launched into the life of the English public school, and Rugby was then under the most famous headmaster of the day, Dr. Arnold. Schoolboy life as it was then at Rugby has been depicted by Hughes in "Tom Brown's Schooldays."

Here he showed great and all-around ability. It became his ambition to crown his school career by carrying off an entrance scholarship at Balliol College, Oxford. But as a sister and brother had already died of consumption, his mother did not allow him to complete his third and final year at Rugby, but took him to Italy, where he continued his reading privately. Notwithstanding this manifest disadvantage, he was able to carry off the coveted scholarship; and at the age of nineteen he began residence as a student of Balliol College. The next long vacation was spent in Italy, and there his health broke down. By the following winter he had not recovered enough to warrant his return to Oxford; instead, he went to Paris, and took several of the courses at the Sorbonne and the Collège de France. These studies abroad had much influence on his future career as a mathematician. Thereafter he resumed his undergraduate studies at Oxford, carried off what is considered the highest classical honor, and in 1849, when 23 years old, finished his undergraduate career with a double-first; that is, in the honors examination for bachelor of arts he took first-class rank in the classics, and also first-class rank in the mathematics.

It is not very pleasant to be a double first, for the outwardly envied and distinguished recipient is apt to find himself in the position of the ass between two equally inviting bundles of hay, unless indeed there is some external attraction superior to both. In the case of Smith, the external attraction was the bar, for which he was in many respects well suited; but the feebleness of his constitution led him to abandon that course. So he had a difficulty in deciding between classics and mathematics, and there is a story to the effect that he finally solved the difficulty by tossing up a penny. He certainly used the expression: but the reasons which determined his choice in favor of mathematics were first, his weak sight, which made thinking preferable to reading, and secondly, the opportunity which presented itself.

At that time Oxford was recovering from the excitement which had been produced by the Tractarian movement, and which had ended in Newman going over to the Church of Rome. But a Parliamentary Commission had been appointed to inquire into the working of the University. The old system of close scholarships and fellowships was doomed, and the close preserves of the Colleges were being either extinguished or thrown open to public competition. Resident professors, married tutors or fellows were almost or quite unknown; the heads of the several colleges, then the governing body of the University, formed a little society by themselves. Balliol College (founded by John Balliol, the unfortunate King of Scotland who was willing to sell its independence) was then the most distinguished for intellectual eminence; the master was singular among his compeers for keeping steadily in view the true aim of a college, and he reformed the abuses of privilege and close endowment as far as he legally could. Smith was elected a fellow with the hope that he would consent to reside, and take the further office of tutor in mathematics, which he did. Soon after he became one of the mathematical tutors of Balliol he was asked by his college to deliver a course of lectures on chemistry. For this purpose he took up the study of chemical analysis, and exhibited skill in manipulation and accuracy in work. He had an idea of seeking numerical relations connecting the atomic weights of the elements, and some mathematical basis for their properties which might enable experiments to be predicted by the operation of the mind.

About this time Whewell, the master of Trinity College, Cambridge, wrote *The Plurality of Worlds*, which was at first published anonymously. Whewell pointed out what he called law of waste traceable in the Divine economy; and his argument was that the other planets were waste effects, the Earth the only oasis in the desert of our system, the only world inhabited by intelligent beings; Sir David Brewster, a Scottish physicist, inventor of the kaleidoscope, wrote a fiery answer entitled "More worlds than one, the creed of the philosopher and the hope of the Christian." In 1855 Smith wrote an essay on this subject for a volume of Oxford and Cambridge Essays in which the fallibility both of men of science and of theologians was impartially exposed. It was his first and only effort at popular writing.

His two earliest mathematical papers were on geometrical subjects, but the third concerned that branch of mathematics in which he won fame—the theory of numbers. How he was led to take up this branch of mathematics is not stated on authority, but it was probably as follows: There was then no school of mathematics at Oxford; the symbolical school was flourishing at Cambridge; and Hamilton was lecturing on Quaternions at Dublin. Smith did not estimate either of these very highly; he had studied at Paris under some of the great French analysts; he had lived much on the Continent, and was familiar with the French, German and Italian languages. As a scholar he was drawn to the masterly disquisitions of Gauss, who had made the theory of numbers a principal subject of research. I may quote here his estimate of Gauss and of his work: "If we except the great name of Newton (and the exception is one which Gauss himself would have been delighted to make) it is probable that no mathematician of any age or country has ever surpassed Gauss in the combination of an abundant fertility of invention with an absolute vigorousness in demonstration, which the ancient Greeks themselves might have envied. It may be admitted, without any disparagement to the eminence of such great mathematicians as Euler and Cauchy that they were so overwhelmed with the exuberant wealth of their own creations, and so fascinated by the interest attaching to the results at which they arrived, that they did not greatly care to expend their time in arranging their ideas in a strictly logical order, or even in establishing by irrefragable proof propositions which they instinctively felt, and could almost see to be true. With Gauss the case was otherwise. It may seem paradoxical, but it is probably nevertheless true that it is precisely the effort after a logical perfection of form which has rendered the writings of Gauss open to the charge of obscurity and unnecessary difficulty. The fact is that there is neither obscurity nor difficulty in his writings, as long as we read them in the submissive spirit in which an intelligent schoolboy is made to read his Euclid. Every assertion that is made is fully proved, and the assertions succeed one another in a perfectly just analogical order; there nothing so far of which we can complain. But when we have finished the perusal, we soon begin to feel that our work is but begun, that we are still standing on the threshold of the temple, and that there is a secret which lies behind the veil and is as yet concealed from us. No vestige appears of the process by which the result itself was obtained, perhaps not even a trace of the considerations which suggested the successive steps of the demonstration. Gauss says more than once that for brevity, he gives only the synthesis, and suppresses the analysis of his propositions. *Pauca sed matura*—few but well-matured—were the words with which he delighted to describe the character which he endeavored to impress upon his mathematical writings. If, on the other hand, we turn to a memoir of Euler's, there is a sort of free and luxuriant gracefulness about the whole performance, which tells of the quiet pleasure which Euler must have taken in each step of his work; but we are conscious nevertheless that we are at an immense distance from the severe grandeur of design which is characteristic of all Gauss's greater efforts."

Following the example of Gauss, he wrote his first paper on the theory of numbers in Latin: "De compositione numerorum primorum formæ $4^n + 1$ ex duobus quadratis." In it he proves in an original manner the theorem of Fermat—"That every prime number of the form $4^n + 1$ (n being an integer number) is the sum of two square numbers." In his second paper he gives an introduction to the theory of numbers. "It is probable that the Pythagorean school was acquainted with the definition and nature of prime numbers; nevertheless the arithmetical books of the elements of Euclid contain the oldest extant investigations respecting them; and, in particular the celebrated yet simple demonstration that the number of the primes is infinite. To Eratosthenes of Alexandria, who is for so many other reasons entitled to a place in the history of the sciences, is attributed the invention of the method by which the primes may successively be determined in order of magnitude. It is termed, after him, 'the sieve of Eratosthenes'; and is essentially a method of exclusion, by which all composite numbers are successively erased from the series of natural numbers, and the primes alone are left remaining. It requires only one kind of arithmetical operation; that is to say, the formation of the successive multiples of given numbers, or in other words, addition only. Indeed it may be said to require no arithmetical operation whatever, for if the natural series of numbers be represented by points set off at equal distances along a line, by using a geometrical compass we can determine without calculation the multiples of any given number. And in fact, it was by a mechanical contrivance of this nature that M. Burckhardt calculated his table of the least divisors of the first three millions of numbers."

In 1857 Mrs. Smith died; as the result of her cares and exertions she had seen her son enter Balliol College as a scholar, graduate a double-first, elected a fellow of his college, appointed tutor in mathematics, and enter on his career as an independent mathematician. The brother and sister that were left arranged to keep house in Oxford, the two spending the terms together, and each being allowed complete liberty of movement during the vacations. Thereafter this was the domestic arrangement in which Smith lived and worked; he never married. As the owner of a house, instead of living in rooms in college he was able to satisfy his fondness for pet animals, and also to extend Irish hospitality to visiting friends under his own roof. He had no household cares to destroy the needed serenity for scientific work, excepting that he was careless in money matters, and trusted more to speculation in mining shares than to economic management of his income. Though addicted to the theory of numbers, he was not in any sense a recluse; on the contrary he entered with zest into every form of social enjoyment in Oxford, from croquet parties and picnics to banquets. He had the rare power of utilizing stray hours of leisure, and it was in such odd times that he accomplished most of his scientific work. After attending a picnic in the afternoon, he could mount to those serene heights in the theory of numbers

> "Where never creeps a cloud or moves a wind,
> Nor ever falls the least white star of snow,
> Nor ever lowest roll of thunder moans,
> Nor sound of human sorrow mounts, to mar
> Their sacred everlasting calm."

Then he could of a sudden come down from these heights to attend a dinner, and could conduct himself there, not as a mathematical genius lost in reverie and pointed out as a poor and eccentric mortal, but on the contrary as a thorough man of the world greatly liked by everybody.

In 1860, when Smith was 34 years old, the Savilian professor of geometry at Oxford died. At that time the English universities were so constituted that the teaching was done by the college tutors. The professors were officers of the University; and before reform set in, they not only did not teach, they did not even reside in Oxford. At the present day the lectures of the University professors are in general attended by only a few advanced students. Henry Smith was the only Oxford candidate; there were other candidates from the outside, among them George Boole, then professor of mathematics at Queens College, Cork. Smith's claims and talents were considered so conspicuous by the electors, that they did not consider any other candidates. He did not resign as tutor at Balliol, but continued to discharge the arduous duties, in order that the income of his Fellowship might be continued. With proper financial sense he might have been spared from labors which militated against the discharge of the higher duties of professor.

His freedom during vacation gave him the opportunity of attending the meetings of the British Association, where he was not only a distinguished savant, but an accomplished member of the social organization known as the Red Lions. In 1858 he was selected by that body to prepare a report upon the Theory of Numbers. It was prepared in five parts, extending over the years 1859-1865. It is neither a history nor a treatise, but something intermediate. The author analyzes with remarkable clearness and order the works of mathematicians for the preceding century upon the theory of congruences, and upon that of binary quadratic forms. He returns to the original sources, indicates the principle and sketches the course of the demonstrations, and states the result, often adding something of his own. The work has been pronounced to be the most complete and elegant monument ever erected to the theory of numbers, and the model of what a scientific report ought to be.

During the preparation of the Report, and as a logical consequence of the researches connected therewith, Smith published several original contributions to the higher arithmetic. Some were in complete form and appeared in the *Philosophical Transactions* of the Royal Society of London; others were incomplete, giving only the results without the extended demonstrations, and appeared in the Proceedings of that Society. One of the latter, entitled "On the orders and genera of quadratic forms containing more than three indeterminates," enunciates certain general principles by means of which he solves a problem proposed by Eisenstein, namely, the decomposition of integer numbers into the sum of five squares; and further, the analogous problem for seven squares. It was also indicated that the four, six, and eight-square theorems of Jacobi, Eisenstein and Lionville were deducible from the principles set forth.

In 1868 he returned to the geometrical researches which had first occupied his attention. For a memoir on "Certain cubic and biquadratic problems" the Royal Academy of Sciences of Berlin awarded him the Steiner prize. On account of his ability as a man of affairs, Smith was in great demand for University and scientific work of the day. He was made Keeper of the University Museum; he accepted the office of Mathematical Examiner to the University of London; he was a member of a Royal Commission appointed to report on Scientific Education; a member of the Commission appointed to reform the University of Oxford; chairman of the committee of scientists who were given charge of the Meteorological Office, etc. It was not till 1873, when offered a Fellowship by Corpus Christi College, that he gave up his tutorial duties at Balliol. The demands of these offices and of social functions upon his time and energy necessarily reduced the total output of mathematical work of the highest order; the results of long research lay buried in notebooks, and the necessary time was not found for elaborating them into a form suitable for publication. Like his master, Gauss, he had a high ideal of what a scientific memoir ought to be in logical order, vigor of demonstration and literary execution; and it was to his mathematical friends matter of regret that he did not reserve more of his energy for the work for which he was exceptionally fitted.

He was a brilliant talker and wit. Working in the purely speculative region of the theory of numbers, it was perhaps natural that he should take an anti-utilitarian view of mathematical science, and that he should express it in exaggerated terms as a defiance to the grossly utilitarian views then popular. It is reported that once in a lecture after explaining a new solution of an old problem he said, "It is the peculiar beauty of this method, gentlemen, and one which endears it to the really scientific mind, that under no circumstances can it be of the smallest possible utility." I believe that it was at a banquet of the Red Lions that he proposed the toast "Pure mathematics; may it never be of any use to any one."

I may mention some other specimens of his wit. "You take tea in the morning," was the remark with which he once greeted a friend; "if I did that I should be awake all day." Some one mentioned to him the enigmatical motto of Marischal College, Aberdeen: "They say; what say they; let them say." "Ah," said he, "it expresses the three stages of an undergraduate's career. 'They say'—in his first year he accepts everything he is told as if it were inspired. 'What say they'—in his second year he is skeptical and asks that question. 'Let them say' expresses the attitude of contempt characteristic of his third year." Of a brilliant writer but illogical thinker he said "He is never right and never wrong; he is never to the point." Of Lockyer, the astronomer, who has been for many years the editor of the scientific journal *Nature*, he said, "Lockyer sometimes forgets that he is only the editor, not the author, of Nature." Speaking to a newly elected fellow of his college he advised him in a low whisper to write a little and to save a little, adding "I have done neither."

At the jubilee meeting of the British Association held at York in 1881, Prof. Huxley and Sir John Lubbock (now Lord Avebury) strolled down one afternoon to the Minster, which is considered the finest cathedral in England. At the main door they met Prof. Smith coming out, who made a mock movement of surprise. Huxley said, "You seem surprised to see me here." "Yes," said Smith, "going in, you know; I would not have been surprised to see you on one of the pinnacles." Once I was introduced to him at a garden party, given in the grounds of York Minster. He was a tall man, with sandy hair and beard, decidedly good-looking, with a certain intellectual distinction in his features and expression. He was everywhere and known to everyone, the life and soul of the gathering. He retained to the day of his death the simplicity and high spirits of a boy. Socially he was an embodiment of Irish blarney modified by Oxford dignity.

In 1873 the British Association met at Bradford; at which meeting Maxwell delivered his famous "Discourse on Molecules." At the same meeting Smith was the president of the section of mathematics and physics. He did not take up any technical subject in his address; but confined himself to matters of interest in the exact sciences. He spoke of the connection between mathematics and physics, as evidenced by the dual province of the section. "So intimate is the union between mathematics and physics that probably by far the larger part of the accessions to our mathematical knowledge have been obtained by the efforts of mathematicians to solve the problems set to them by experiment, and to create for each successive class of phenomena a new calculus or a new geometry, as the case might be, which might prove not wholly inadequate to the subtlety of nature. Sometimes indeed the mathematician has been before the physicist, and it has happened that when some great and new question has occurred to the experimenter or the observer, he has found in the armory of the mathematician the weapons which he has needed ready made to his hand. But much oftener the questions proposed by the physicist have transcended the utmost powers of the mathematics of the time, and a fresh mathematical creation has been needed to supply the logical instrument required to interpret the new enigma." As an example of the rule he points out that the experiments of Faraday called forth the mathematical theory of Maxwell; as an example of the exception that the work of Apollonius on the conic sections was ready for Kepler in investigating the orbits of the planets.

At the time of the Bradford meeting, education in the public schools and universities of England was practically confined to the classics and pure mathematics. In his address Smith took up the importance of science as an educational discipline in schools; and the following sentences, falling as they did from a profound scholar, produced a powerful effect: "All knowledge of natural science that is imparted to a boy, is, or may be, useful to him in the business of his after-life; but the claim of natural science to a place in education cannot be rested upon its usefulness only. The great object of education is to expand and to train the mental faculties, and it is because we believe that the study of natural science is eminently fitted to further these two objects that we urge its introduction into school studies. Science expands the minds of the young, because it puts before them great and ennobling objects of contemplation; many of its truths are such as a child can understand, and yet such that while in a measure he understands them, he is made to feel something of the greatness, something of the sublime regularity and something of the impenetrable mystery, of the world in which he is placed. But science also trains the growing faculties, for science proposes to itself truth as its only object, and it presents the most varied, and at the same time the most splendid examples of the different mental processes which lead to the attainment of truth, and which make up what we call reasoning. In science error is always possible, often close at hand; and the constant necessity for being on our guard against it is one important part of the education which science supplies. But in science sophistry is impossible; science knows no love of paradox; science has no skill to make the worse appear the better reason; science visits with a not long deferred exposure all our fondness for preconceived opinions, all our partiality for views which we have ourselves maintained; and thus teaches the two best lessons that can well be taught—on the one hand, the love of truth; and on the other, sobriety and watchfulness in the use of the understanding."

The London Mathematical Society was founded in 1865. By going to the meetings Prof. Smith was induced to prepare for publication a number of papers from the materials of his notebooks. He was for two years president, and at the end of his term delivered an address "On the present state and prospects of some branches of pure mathematics." He began by referring to a charge which had been brought against the Society, that its Proceedings showed a partiality in favor of one or two great branches of mathematical science to the comparative neglect and possible disparagement of others. He replies in the language of a miner. "It may be rejoined with great plausibility that ours is not a blamable partiality, but a well-grounded preference. So great (we might contend) have been the triumphs achieved in recent times by that combination of the newer algebra with the direct contemplation of space which constitutes the modern geometry—so large has been the portion of these triumphs, which is due to the genius of a few great English mathematicians; so vast and so inviting has been the field thus thrown open to research, that we do well to press along towards a country which has, we might say, been 'prospected' for us, and in which we know beforehand we cannot fail to find something that will repay our trouble, rather than adventure ourselves into regions where, soon after the first step, we should have no beaten tracks to guide us to the lucky spots, and in which (at the best) the daily earnings of the treasure-seeker are small, and do not always make a great show, even after long years of work. Such regions, however, there are in the realm of pure mathematics, and it cannot be for the interest of science that they should be altogether neglected by the rising generation of English mathematicians. I propose, therefore, in the first instance to direct your attention to some few of these comparatively neglected spots." Since then quite a number of the neglected spots pointed out have been worked.

In 1878 Oxford friends urged him to come forward as a candidate for the representation in Parliament of the University of Oxford, on the principle that a University constituency ought to have for its representative not a mere party politician, but an academic man well acquainted with the special needs of the University. The main question before the electors was the approval or disapproval of the Jingo war policy of the Conservative Government. Henry Smith had always been a Liberal in politics, university administration, and religion. The voting was influenced mainly by party considerations—Beaconsfield or Gladstone—with the result that Smith was defeated by more than 2 to 1; but he had the satisfaction of knowing that his support came mainly from the resident and working members of the University. He did not expect success and he hardly desired it, but he did not shrink when asked to stand forward as the representative of a principle in which he believed. The election over, he devoted himself with renewed energy to the publication of his mathematical researches. His report on the theory of numbers had ended in elliptic functions; and it was this subject which now engaged his attention.

In February, 1882, he was surprised to see in the *Comptes rendus* that the subject proposed by the Paris Academy of Science for the *Grand prix des sciences mathématiques* was the theory of the decomposition of integer numbers into a sum of five squares; and that the attention of competitors was directed to the results announced without demonstration by Eisenstein, whereas nothing was said about his papers dealing with the same subject in the Proceedings of the Royal Society. He wrote to M. Hermite calling his attention to what he had published; in reply he was assured that the members of the commission did not know of the existence of his papers, and he was advised to complete his demonstrations and submit the memoir according to the rules of the competition. According to the rules each manuscript bears a motto, and the corresponding envelope containing the name of the successful author is opened. There were still three months before the closing of the *concours* (1 June, 1882) and Smith set to work, prepared the memoir and despatched it in time.

Meanwhile a political agitation had grown up in favor of extending the franchise in the county constituencies. In the towns the mechanic had received a vote; but in the counties that power remained with the squire and the farmer; poor Hodge, as he is called, was left out. Henry Smith was not merely a Liberal; he felt a genuine sympathy for the poor of his own land. At a meeting in the Oxford Town Hall he made a speech in favor of the movement, urging justice to all classes. From that platform he went home to die. When he spoke he was suffering from a cold. The exposure and excitement were followed by congestion of the liver, to which he succumbed on February 9, 1883, in the 57th year of his age.

Two months after his death the Paris Academy made their award. Two of the three memoirs sent in were judged worthy of the prize. When the envelopes were opened, the authors were found to be Prof. Smith and M. Minkowski, a young mathematician of Koenigsberg, Prussia. No notice was taken of Smith's previous publication on the subject, and M. Hermite on being written to, said that he forgot to bring the matter to the notice of the commission. It was admitted that there was considerable similarity in the course of the investigation in the two memoirs. The truth seems to be that M. Minkowski availed himself of whatever had been published on the subject, including Smith's paper, but to work up the memoir from that basis cost Smith himself much intellectual labor, and must have cost Minkowski much more. Minkowski is now the chief living authority in that high region of the theory of numbers. Smith's work remains the monument of one of the greatest British mathematicians of the nineteenth century.

Chapter 8

JAMES JOSEPH SYLVESTER[1]

(1814-1897)

James Joseph Sylvester was born in London, on the 3d of September, 1814. He was by descent a Jew. His father was Abraham Joseph Sylvester, and the future mathematician was the youngest but one of seven children. He received his elementary education at two private schools in London, and his secondary education at the Royal Institution in Liverpool. At the age of twenty he entered St. John's College, Cambridge; and in the tripos examination he came out second wrangler. The senior wrangler of the year did not rise to any eminence; the fourth wrangler was George Green, celebrated for his contributions to mathematical physics; the fifth wrangler was Duncan F. Gregory, who subsequently wrote on the foundations of algebra. On account of his religion Sylvester could not sign the thirty-nine articles of the Church of England; and as a consequence he could neither receive the degree of Bachelor of Arts nor compete for the Smith's prizes, and as a further consequence he was not eligible for a fellowship. To obtain a degree he turned to the University of Dublin. After the theological tests for degrees had been abolished at the Universities of Oxford and Cambridge in 1872, the University of Cambridge granted him his well-earned degree of Bachelor of Arts and also that of Master of Arts.

On leaving Cambridge he at once commenced to write papers, and these were at first on applied mathematics. His first paper was entitled "An ana-

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lytical development of Fresnel's optical theory of crystals," which was published in the *Philosophical Magazine*. Ere long he was appointed Professor of Physics in University College, London, thus becoming a colleague of De Morgan. At that time University College was almost the only institution of higher education in England in which theological distinctions were ignored. There was then no physical laboratory at University College, or indeed at the University of Cambridge; which was fortunate in the case of Sylvester, for he would have made a sorry experimenter. His was a sanguine and fiery temperament, lacking the patience necessary in physical manipulation. As it was, even in these pre-laboratory days he felt out of place, and was not long in accepting a chair of pure mathematics.

In 1841 he became professor of mathematics at the University of Virginia. In almost all notices of his life nothing is said about his career there; the truth is that after the short space of four years it came to a sudden and rather tragic termination. Among his students were two brothers, fully imbued with the Southern ideas about honor. One day Sylvester criticised the recitation of the younger brother in a wealth of diction which offended the young man's sense of honor; he sent word to the professor that he must apologize or be chastised. Sylvester did not apologize, but provided himself with a sword-cane; the young man provided himself with a heavy walking-stick. The brothers lay in wait for the professor; and when he came along the younger brother demanded an apology, almost immediately knocked off Sylvester's hat, and struck him a blow on the bare head with his heavy stick. Sylvester drew his sword-cane, and pierced the young man just over the heart; who fell back into his brother's arms, calling out "I am killed." A spectator, coming up, urged Sylvester away from the spot. Without waiting to pack his books the professor left for New York, and took the earliest possible passage for England. The student was not seriously hurt; fortunately the point of the sword had struck fair against a rib.

Sylvester, on his return to London, connected himself with a firm of actuaries, his ultimate aim being to qualify himself to practice conveyancing. He became a student of the Inner Temple in 1846, and was called to the bar in 1850. He chose the same profession as did Cayley; and in fact Cayley and Sylvester, while walking the law-courts, discoursed more on mathematics than on conveyancing. Cayley was full of the theory of invariants; and it was by his discourse that Sylvester was induced to take up the subject. These two men were life-long friends; but it is safe to say that the permanence of the friendship was due to Cayley's kind and patient disposition. Recognized as the leading mathematicians of their day in England, they were yet very different both in nature and talents.

Cayley was patient and equable; Sylvester, fiery and passionate. Cayley finished off a mathematical memoir with the same care as a legal instrument; Sylvester never wrote a paper without foot-notes, appendices, supplements; and the alterations and corrections in his proofs were such that the printers found their task well-nigh impossible. Cayley was well-read in contemporary mathematics, and did much useful work as referee for scientific societies; Sylvester read only what had an immediate bearing on his own researches, and did little, if any, work as a referee. Cayley was a man of sound sense, and of great service in University administration; Sylvester satisfied the popular idea of a mathematician as one lost in reflection, and high above mundane affairs. Cayley was modest and retiring; Sylvester, courageous and full of his own importance. But while Cayley's papers, almost all, have the stamp of pure logical mathematics, Sylvester's are full of human interest. Cayley was no orator and no poet; Sylvester was an orator, and if not a poet, he at least prided himself on his poetry. It was not long before Cayley was provided with a chair at Cambridge, where he immediately married, and settled down to work as a mathematician in the midst of the most favorable environment. Sylvester was obliged to continue what he called "fighting the world" alone and unmarried.

There is an ancient foundation in London, named after its founder, Gresham College. In 1854 the lectureship of geometry fell vacant and Sylvester applied. The trustees requested him and I suppose also the other candidates, to deliver a probationary lecture; with the result that he was not appointed. The professorship of mathematics in the Royal Military Academy at Woolwich fell vacant; Sylvester was again unsuccessful; but the appointee died in the course of a year, and then Sylvester succeeded on a second application. This was in 1855, when he was 41 years old.

He was a professor at the Military Academy for fifteen years; and these years constitute the period of his greatest scientific activity. In addition to continuing his work on the theory of invariants, he was guided by it to take up one of the most difficult questions in the theory of numbers. Cayley had reduced the problem of the enumeration of invariants to that of the partition of numbers; Sylvester may be said to have revolutionized this part of mathematics by giving a complete analytical solution of the problem, which was in effect to enumerate the solutions in positive integers of the indeterminate equation:

$$ax + by + cz + \ldots + ld = m.$$

Thereafter he attacked the similar problem connected with two such simultaneous equations (known to Euler as the problem of the Virgins) and was partially and considerably successful. In June, 1859, he delivered a series of seven lectures on compound partition in general at King's College, London. The outlines of these lectures have been published by the Mathematical Society of London.

Five years later (1864) he contributed to the Royal Society of London what is considered his greatest mathematical achievement. Newton, in his lectures on algebra, which he called "Universal Arithmetic" gave a rule for calculating an inferior limit to the number of imaginary roots in an equation of any degree, but he did not give any demonstration or indication of the process by which he reached it. Many succeeding mathematicians such as Euler, Waring, Maclaurin, took up the problem of investigating the rule, but they were unable to establish either its truth or inadequacy. Sylvester in the paper quoted established the validity of the rule for algebraic equations as far as the fifth degree inclusive. Next year in a communication to the Mathematical Society of London, he fully established and generalized the rule. "I owed my success," he said, "chiefly to merging the theorem to be proved in one of greater scope and generality. In mathematical research, reversing the axiom of Euclid and controverting the proposition of Hesiod, it is a continual matter of experience, as I have found myself over and over again, that the whole is less than its part."

Two years later he succeeded De Morgan as president of the London Mathematical Society. He was the first mathematician to whom that Society awarded the Gold medal founded in honor of De Morgan. In 1869, when the British Association met in Exeter, Prof. Sylvester was president of the section of mathematics and physics. Most of the mathematicians who have occupied that position have experienced difficulty in finding a subject which should satisfy the two conditions of being first, cognate to their branch of science; secondly, interesting to an audience of general culture. Not so Sylvester. He took up certain views of the nature of mathematical science which Huxley the great biologist had just published in *Macmillan's Magazine* and the *Fortnightly Review*. He introduced his subject by saying that he was himself like a great party leader and orator in the House of Lords, who, when requested to make a speech at some religious or charitable, at-all-events non-political meeting declined the honor on the ground that he could not speak unless he saw an adversary before him. I shall now quote from the address, so that you may hear Sylvester's own words.

"In obedience," he said, "to a somewhat similar combative instinct, I set to myself the task of considering certain utterances of a most distinguished member of the Association, one whom I no less respect for his honesty and public spirit, than I admire for his genius and eloquence, but from whose opinions on a subject he has not studied I feel constrained to differ. I have no doubt that had my distinguished friend, the probable president-elect of the next meeting of the Association, applied his uncommon powers of reasoning, induction, comparison, observation and invention to the study of mathematical science, he would have become as great a mathematician as he is now a biologist; indeed he has given public evidence of his ability to grapple with the practical side of certain mathematical questions; but he has not made a study of mathematical science as such, and the eminence of his position, and the weight justly attaching to his name, render it only the more imperative that any assertion proceeding from such a quarter, which may appear to be erroneous, or so expressed as to be conducive to error should not remain unchallenged or be passed over in silence.

"Huxley says 'mathematical training is almost purely deductive. The mathematician starts with a few simple propositions, the proof of which is so obvious that they are called self-evident, and the rest of his work consists of subtle deductions from them. The teaching of languages at any rate as ordinarily practised, is of the same general nature—authority and tradition furnish the data, and the mental operations are deductive.' It would seem from the above somewhat singularly juxtaposed paragraphs, that according to Prof. Huxley, the business of the mathematical student is, from a limited number of propositions (bottled up and labelled ready for use) to deduce any required result by a process of the same general nature as a student of languages employs in declining and conjugating his nouns and verbs—that to make out a mathematical proposition and to construe or parse a sentence are equivalent or identical mental operations. Such an opinion scarcely seems to need serious refutation. The passage is taken from an article in *Macmillan's Magazine* for June last, entitled, 'Scientific Education—Notes of an after-dinner speech'; and I cannot but think would have been couched in more guarded terms by my distinguished friend, had his speech been made *before* dinner instead of *after*.

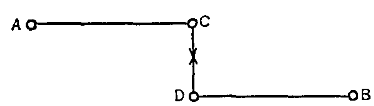
"The notion that mathematical truth rests on the narrow basis of a limited number of elementary propositions from which all others are to be derived by a process of logical inference and verbal deduction has been stated still more strongly and explicitly by the same eminent writer in an article of even date with the preceeding in the *Fortnightly Review*; where we are told that 'Mathematics is that study which knows nothing of observation, nothing of experiment, nothing of induction, nothing of causation.' I think no statement could have been made more opposite to the undoubted facts of the case, which are that mathematical analysis is constantly invoking the aid of new principles, new ideas and new methods not capable of being defined by any form of words, but springing direct from the inherent powers and activity of the human mind, and from continually renewed introspection of that inner world of thought of which the phenomena are as varied and require as close attention to discern as those of the outer physical world; that it is unceasingly calling forth the faculties of observation and comparison; that one of its principal weapons is induction; that is has frequent recourse to experimental trial and verification; and that it affords a boundless scope for the exercise of the highest efforts of imagination and invention."

Huxley never replied; convinced or not, he had sufficient sagacity to see that he had ventured far beyond his depth. In the portion of the address quoted, Sylvester adds parenthetically a clause which expresses his theory of mathematical knowledge. He says that the inner world of thought in each individual man (which is the world of observation to the mathematician) may be conceived to stand in somewhat the same general relation of correspondence to the outer physical world as an object to the shadow projected from it. To him the mental order was more real than the world of sense, and the foundation of mathematical science was ideal, not experimental.

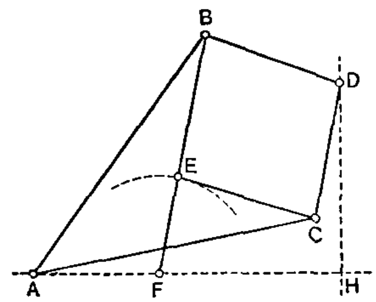
By this time Sylvester had received most of the high distinctions, both domestic and foreign, which are usually awarded to a mathematician of the first rank in his day. But a discontinuity was at hand. The War Office issued a regulation whereby officers of the army were obliged to retire on half pay on reaching the age of 55 years. Sylvester was a professor in a Military College; in a few months, on his reaching the prescribed age, he was retired on half pay. He felt that though no longer fit for the field he was still fit for the classroom. And he felt keenly the diminution in his income. It was about this time that he issued a small volume—the only book he ever published; not on mathematics, as you may suppose, but entitled *The Laws of Verse.* He must have prided himself a good deal on this composition, for one of his last letters in *Nature* is signed "J. J. Sylvester, author of The Laws of Verse."

He made some excellent translations from Horace and from German poets; and like Sir W. R. Hamilton he was accustomed to express his feelings in sonnets.

The break in his life appears to have discouraged Sylvester for the time being from engaging in any original research. But after three years a Russian mathematician named Tschebicheff, a professor in the University of Saint Petersburg, visiting Sylvester in London, drew his attention to the discovery by a Russian student named Lipkin, of a mechanism for drawing a perfect straight line. Mr. Lipkin received from the Russian Government a substantial award. It was found that the same discovery had been made several years before by M. Peaucellier, an officer in the French army, but failing to be recognized at its true value had dropped into oblivion. Sylvester introduced the subject into England in the form of an evening lecture before the Royal Institution, entitled "On recent discoveries in mechanical conversion of motion." The Royal Institution of London was founded to promote scientific research; its professors have been such men as Davy, Faraday, Tyndall, Dewar. It is not a teaching institution, but it provides for special courses of lectures in the afternoons and for Friday evening lectures by investigators of something new in science. The evening lectures are attended by fashionable audiences of ladies and gentlemen in full dress.



Euclid bases his *Elements* on two postulates; first, that a straight line can be drawn, second, that a circle can be described. It is sometimes expressed in this way; he postulates a ruler and compass. The latter contrivance is not difficult to construct, because it does not involve the use of a ruler or a compass in its own construction. But how is a ruler to be made straight, unless you already have a ruler by which to test it? The problem is to devise a mechanism which shall assume the second postulate only, and be able to satisfy the first. It is the mechanical problem of converting motion in a circle into motion in a straight line, without the use of any guide. James Watt, the inventor of the steam-engine, tackled the problem with all his might, but gave it up as impossible. However, he succeeded in finding a contrivance which solves the problem very approximately. Watt's parallelogram, employed in nearly every beam-engine, consists of three links; of which AC and BD are equal, and have fixed pivots at A and B respectively. The link CD is of such a length that AC and BD are parallel when horizontal. The tracing point is attached to the middle point of CD. When C and D move round their pivots, the tracing point describes a straight line very approximately, so long as the arc of displacement is small. The complete figure which would be described is the figure of 8, and the part utilized is near the point of contrary flexure.



A linkage giving a closer approximation to a straight line was also invented by the Russian mathematician before mentioned—Tschebicheff; it likewise made use of three links. But the linkage invented by Peaucellier and later by Lipkin had seven pieces. The arms AB and AC are of equal length, and have a fixed pivot at A. The links DB, BE, EC, CD are of equal length. EF is an arm connecting E with the fixed pivot F and is equal in length to the distance between A and F. It is readily shown by geometry that, as the point E describes a circle around the center F, the point D describes an exact straight line perpendicular to the line joining it and F. The exhibition of this contrivance at work was the climax of Sylvester's lecture.

In Sylvester's audience were two mathematicians, Hart and Kempe, who took up the subject for further investigation. Hart perceived that the contrivances of Watt and of Tschebicheff consisted of three links, whereas Peaucellier's consisted of seven. Accordingly he searched for a contrivance of five links which would enable a tracing point to describe a perfect straight line; and he succeeded in inventing it. Kempe was a London barrister whose specialty was ecclesiastical law. He and Sylvester worked up the theory of linkages together, and discovered among other things the skew pantograph. Kempe became so imbued with linkage that he contributed to the Royal Society of London a paper on the "Theory of Mathematical Form," in which he explains all reasoning by means of linkages.

About this time (1877) the Johns Hopkins University was organized at Baltimore, and Sylvester, at the age of 63, was appointed the first professor of mathematics. Of his work there as a teacher, one of his pupils, Dr. Fabian Franklin, thus spoke in an address delivered at a memorial meeting in that University: "The one thing which constantly marked Sylvester's lectures was enthusiastic love of the thing he was doing. He had in the fullest possible degree, to use the French phrase, the defect of this quality; for as he almost always spoke with enthusiastic ardor, so it was almost never possible for him to speak on matters incapable of evoking this ardor. In other words, the substance of his lectures had to consist largely of his own work, and, as a rule, of work hot from the forge. The consequence was that a continuous and systematic presentation of any extensive body of doctrine already completed was not to be expected from him. Any unsolved difficulty, any suggested extension, such as would have been passed by with a mention by other lecturers, became inevitably with him the occasion of a digression which was sure to consume many weeks, if indeed it did not take him away from the original object permanently. Nearly all of the important memoirs which he published, while in Baltimore, arose in this way. We who attended his lectures may be said to have seen these memoirs in the making. He would give us on the Friday the outcome of his grapplings with the enemy since the Tuesday lecture. Rarely can it have fallen to the lot of any class to follow so completely the workings of the mind of the master. Not only were all thus privileged to see 'the very pulse of the machine,' to learn the spring and motive of the successive steps that led to his results, but we were set aglow by the delight and admiration which, with perfect naïveté and with that luxuriance of language peculiar to him, Sylvester lavished upon these results. That in this enthusiastic admiration he sometimes lacked the sense of proportion cannot be denied. A result announced at one lecture and hailed with loud acclaim as a marvel of beauty was by no means sure of not being found before the next lecture to have been erroneous; but the Esther that supplanted this Vashti was quite certain to be found still more supremely beautiful. The fundamental thing, however, was not this occasional extravagance, but the deep and abiding feeling for truth and beauty which underlay it. No young man of generous mind could stand before that superb grey head and hear those expositions of high and dear-bought truths, testifying to a passionate devotion undimmed by years or by arduous labors, without carrying away that which ever after must give to the pursuit of truth a new and deeper significance in his mind."

One of Sylvester's principal achievements at Baltimore was the founding of the *American Journal of Mathematics*, which, at his suggestion, took the quarto form. He aimed at establishing a mathematical journal in the English language, which should equal Liouville's *Journal* in France, or Crelle's *Journal* in Germany. Probably his best contribution to the *American Journal* consisted in his "Lectures on Universal Algebra"; which, however, were left unfinished, like a great many other projects of his.

Sylvester had that quality of absent-mindedness which is popularly supposed to be, if not the essence, at least an invariable accompaniment, of a distinguished mathematician. Many stories are related on this point, which, if not all true, are at least characteristic. Dr. Franklin describes an instance which actually happened in Baltimore. To illustrate a theory of versification contained in his book *The Laws of Verse*, Sylvester prepared a poem of 400 lines, all rhyming with the name Rosalĭnd or Rosalīnd; and it was announced that the professor would read the poem on a specified evening at a specified hour at the Peabody Institute. At the time appointed there was a large turn-out of ladies and gentlemen. Prof. Sylvester, as usual, had a number of footnotes appended to his production; and he announced that in order to save interruption in reading the poem itself, he would first read the footnotes. The reading of the footnotes suggested various digressions to his imagination; an hour had passed, still no poem; an hour and a half passed and the striking of the clock or the unrest of his audience reminded him of the promised poem. He was astonished to find how time had passed, excused all who had engagements, and proceeded to read the Rosalind poem.

In the summer of 1881 I visited London to see the Electrical Exhibition in the Crystal Palace—one of the earliest exhibitions devoted to electricity exclusively. I had made some investigations on the electric discharge, using a Holtz machine where De LaRue used a large battery of cells. Mr. De LaRue was Secretary of the Royal Institution; he gave me a ticket to a Friday evening discourse to be delivered by Mr. Spottiswoode, then president of the Royal Society, on the phenomena of the intensive discharge of electricity through gases; also an invitation to a dinner at his own house to be given prior to the lecture. Mr. Spottiswoode, the lecturer for the evening, was there; also Prof. Sylvester. He was a man rather under the average height, with long gray beard and a profusion of gray locks round his head surmounted by a great dome of forehead. He struck me as having the appearance of an artist or a poet rather than of an exact scientist. After dinner he conversed very eloquently with an elderly lady of title, while I conversed with her daughter.

Then cabs were announced to take us to the Institution. Prof. Sylvester and I, being both bachelors, were put in a cab together. The professor, who had been so eloquent with the lady, said nothing; so I asked him how he liked his work at the Johns Hopkins University. "It is very pleasant work indeed," said he, "and the young men who study there are all so enthusiastic." We had not exhausted that subject before we reached our destination. We went up the stairway together, then Sylvester dived into the library to see the last number of *Comptes Rendus* (in which he published many of his results at that time) and I saw him no more. I have always thought it very doubtful whether he came out to hear Spottiswoode's lecture.

We have seen that H. J. S. Smith, the Savilian professor of Geometry at Oxford, died in 1883. Sylvester's friends urged his appointment, with the result that he was elected. After two years he delivered his inaugural lecture; of which the subject was differential invariants, termed by him reciprocants. An elementary reciprocant is $\frac{d^2y}{dx^2}$, for if $\frac{d^2y}{dx^2} = 0$ then $\frac{d^2x}{dy^2} = 0$. He looked upon this as the "grub" form, and developed from it the "chrysalis"

$$\begin{vmatrix} \frac{d^2\phi}{dx^2} & \frac{d^2\phi}{dxdy} & \frac{d\phi}{dx}, \\ \frac{d^2\phi}{dxdy} & \frac{d^2\phi}{dy^2} & \frac{d\phi}{dy}, \\ \frac{d\phi}{dx} & \frac{d\phi}{dy} & . \end{vmatrix}$$

and the "imago"

$$\begin{vmatrix} \frac{d^2\Phi}{dx^2} & \frac{d^2\Phi}{dxdy} & \frac{d^2\Phi}{dxdr}, \\ \frac{d^2\Phi}{dxdy} & \frac{d^2\Phi}{dy^2} & \frac{d^2\Phi}{dydr}, \\ \frac{d^2\Phi}{dxdr} & \frac{d^2\Phi}{dydr} & \frac{d^2\Phi}{dr^2}. \end{vmatrix}$$

You will observe that the chrysalis expression is unsymmetrical; the place of a ninth term is vacant. It moved Sylvester's poetic imagination, and into his inaugural lecture he interjected the following sonnet:

To a Missing Member of a Family Group of Terms in an Algebraical Formula:

Lone and discarded one! divorced by fate,
Far from thy wished-for fellows—whither art flown?
Where lingerest thou in thy bereaved estate,
Like some lost star, or buried meteor stone?
Thou minds't 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 Astræa, 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."

This inaugural lecture was the beginning of his last great contribution to mathematics, and the subsequent lectures of that year were devoted to his researches in that line. Smith and Sylvester were akin in devoting attention to the theory of numbers, and also in being eloquent speakers. But in other respects the Oxonians found a great difference. Smith had been a painstaking tutor; Sylvester could lecture only on his own researches, which were not popular in a place so wholly given over to examinations. Smith was an incessantly active man of affairs; Sylvester became the subject of melancholy and complained that he had no friends.

In 1872 a deputy professor was appointed. Sylvester removed to London, and lived mostly at the Athenæum Club. He was now 78 years of age, and suffered from partial loss of sight and memory. He was subject to melancholy, and his condition was indeed "forlorn and desolate." His nearest relatives were nieces, but he did not wish to ask their assistance. One day, meeting a mathematical friend who had a home in London, he complained of the fare at the Club, and asked his friend to help him find suitable private apartments where he could have better cooking. They drove about from place to place for a whole afternoon, but none suited Sylvester. It grew late: Sylvester said, "You have a pleasant home: take me there," and this was done. Arrived, he appointed one daughter his reader and another daughter his amanuensis. "Now," said he, "I feel comfortably installed; don't let my relatives know where I am." The fire of his temper had not dimmed with age, and it required all the Christian fortitude of the ladies to stand his exactions. Eventually, notice had to be sent to his nieces to come and take charge of him. He died on the 15th of March, 1897, in the 83d year of his age, and was buried in the Jewish cemetery at Dalston.

As a theist, Sylvester did not approve of the destructive attitude of such men as Clifford, in matters of religion. In the early days of his career he suffered much from the disabilities attached to his faith, and they were the prime cause of so much "fighting the world." He was, in all probability, a greater mathematical genius than Cayley; but the environment in which he lived for some years was so much less favorable that he was not able to accomplish an equal amount of solid work. Sylvester's portrait adorns St. John's College, Cambridge. A memorial fund of £1500 has been placed in the charge of the Royal Society of London, from the proceeds of which a medal and about £100 in money is awarded triennially for work done in pure mathematics. The first award has been made to M. Henri Poincaré of Paris, a mathematician for whom Sylvester had a high professional and personal regard.

Chapter 9

THOMAS PENYNGTON KIRKMAN[1]

(1806-1895)

Thomas Penyngton Kirkman was born on March 31, 1806, at Bolton in Lancashire. He was the son of John Kirkman, a dealer in cotton and cotton waste; he had several sisters but no brother. He was educated at the Grammar School of Bolton, where the tuition was free. There he received good instruction in Latin and Greek, but no instruction in geometry or algebra; even Arithmetic was not then taught in the headmaster's upper room. He showed a decided taste for study and was by far the best scholar in the school. His father, who had no taste for learning and was succeeding in trade, was determined that his only son should follow his own business, and that without any loss of time. The schoolmaster tried to persuade the father to let his son remain at school; and the vicar also urged the father, saying that if he would send his son to Cambridge University, he would guarantee for sixpence that the boy would win a fellowship. But the father was obdurate; young Kirkman was removed from school, when he was fourteen years of age, and placed at a desk in his father's office. While so engaged, he continued of his own accord his study of Latin and Greek, and added French and German.

After ten years spent in the counting room, he tore away from his father, secured the tuition of a young Irish baronet, Sir John Blunden, and entered the University of Dublin with the view of passing the examinations for the

[1]This Lecture was delivered April 20, 1903.—EDITORS.

degree of B.A. There he never had instruction from any tutor. It was not until he entered Trinity College, Dublin, that he opened any mathematical book. He was not of course abreast with men who had good preparation. What he knew of mathematics, he owed to his own study, having never had a single hour's instruction from any person. To this self-education is due, it appears to me, both the strength and the weakness to be found in his career as a scientist. However, in his college course he obtained honors, or premiums as they are called, and graduated as a moderator, something like a wrangler.

Returning to England in 1835, when he was 29 years old, he was ordained as a minister in the Church of England. He was a curate for five years, first at Bury, afterwards at Lymm; then he became the vicar of a newly-formed parish—Croft with Southworth in Lancashire. This parish was the scene of his life's labors. The income of the benefice was not large, about £200 per annum; for several years he supplemented this by taking pupils. He married, and property which came to his wife enabled them to dispense with the taking of pupils. His father became poorer, but was able to leave some property to his son and daughters. His parochial work, though small, was discharged with enthusiasm; out of the roughest material he formed a parish choir of boys and girls who could sing at sight any four-part song put before them. After the private teaching was over he had the leisure requisite for the great mathematical researches in which he now engaged.

Soon after Kirkman was settled at Croft, Sir William Rowan Hamilton began to publish his quaternion papers and, being a graduate of Dublin University, Kirkman was naturally one of the first to study the new analysis. As the fruit of his meditations he contributed a paper to the *Philosophical Magazine* "On pluquaternions and homoid products of sums of n squares." He proposed the appellation "pluquaternions" for a linear expression involving more than three imaginaries (the i, j, k of Hamilton), "not dreading" he says, "the pluperfect criticism of grammarians, since the convenient barbarism is their own." Hamilton, writing to De Morgan, remarked "Kirkman is a very clever fellow," where the adjective has not the American colloquial meaning but the English meaning.

For his own education and that of his pupils he devoted much attention to mathematical mnemonics, studying the *Memoria Technica* of Grey. In 1851 he contributed a paper on the subject to the Literary and Philosophical Society of Manchester, and in 1852 he published a book, *First Mnemonical Lessons in Geometry, Algebra, and Trigonometry*, which is dedicated to his former pupil, Sir John Blunden. De Morgan pronounced it "the most curious crochet I ever saw," which was saying a great deal, for De Morgan was familiar with many quaint books in mathematics. In the preface he says that much of the distaste for mathematical study springs largely from the difficulty of retaining in the memory the previous results and reasoning. "This difficulty is closely connected with the unpronounceableness of the formulæ; the memory of the tongue and the ear are not easily turned to account; nearly everything depends on the thinking faculty or on the practice of the eye alone. Hence many, who see hardly anything formidable in the study of a language, look upon mathematical acquirements as beyond their power, when in truth they are very far from being so. My object is to enable the learner to 'talk to himself,' in rapid, vigorous and suggestive syllables, about the matters which he must digest and remember. I have sought to bring the memory of the vocal organs and the ear to the assistance of the reasoning faculty and have never scrupled to sacrifice either good grammar or good English in order to secure the requisites for a useful *mnemonic*, which are smoothness, condensation, and jingle."

As a specimen of his mnemonics we may take the cotangent formula in spherical trigonometry:

$$\cot A \sin C + \cos b \cos C = \cot a \sin b$$

To remember this formula most masters then required some aid to the memory; for instance the following: If in any spherical triangle four parts be taken in succession, such as $AbCa$, consisting of two means bC and two extremes Aa, then the product of the cosines of the two means is equal to the sine of the mean side $\times$ cotangent of the extreme side minus sine of the mean angle $\times$ cotangent of the extreme angle, that is

$$\cos b \cos C = \sin b \cot a - \sin C \cot A.$$

This is an appeal to the reason. Kirkman, however, proceeds on the principle of appealing to the memory of the ear, of the tongue, and of the lips altogether; a true *memoria technica*. He distinguishes the large letter from the small by calling them *Ang, Bang, Cang* (*ang* from angle in contrast to side). To make the formula more euphoneous he drops the s from cos and the n from sin. Hence the formula is

cot *Ang* si *Cang* and co *b* co *Cang* are cot *a* si *b* which is to be chanted till it becomes perfectly familiar to the ear and the lips. The former rule is a hint offered to the judgment; Kirkman's method is something to be taught by rote. In his book Kirkman makes much use of verse, in the turning of which he was very skillful.

In the early part of the nineteenth century a publication named the *Lady's and Gentlemen's Diary* devoted several columns to mathematical problems. In 1844 the editor offered a prize for the solution of the following question: "Determine the number of combinations that can be made out of n symbols, each combination having p symbols, with this limitation, that no combination of q symbols which may appear in any one of them, may be repeated in any other." This is a problem of great difficulty; Kirkman solved it completely for the special case of $p = 3$ and $q = 2$ and printed his results in the second volume of the *Cambridge and Dublin Mathematical Journal*. As a chip off this work he published in the *Diary* for 1850 the famous problem of the fifteen schoolgirls as follows: "Fifteen young ladies of a school walk out three abreast for seven days in succession; it is required to arrange them daily so that no two shall walk abreast more than once." To form the schedules for seven days is not difficult; but to find all the possible schedules is a different matter. Kirkman found all the possible combinations of the fifteen young ladies in groups of three to be 35, and the problem was also considered and solved by Cayley, and has been discussed by many later writers; Sylvester gave 91 as the greatest number of days; and he also intimated that the principle of the puzzle was known to him when an undergraduate at Cambridge, and that he had given it to fellow undergraduates. Kirkman replied that up to the time he proposed the problem he had neither seen Cambridge nor met Sylvester, and narrated how he had hit on the question.

The Institute of France offered several times in succession a prize for a memoir on the theory of the polyedra; this fact together with his work in combinations led Kirkman to take up the subject. He always writes *polyedron* not *polyhedron*; for he says we write *periodic* not *perihodic*. When Kirkman began work nothing had been done beyond the very ancient enumeration of the five regular solids and the simple combinations of crystallography. His first paper, "On the representation and enumeration of the polyedra," was communicated in 1850 to the Literary and Philosophical Society of Manchester. He starts with the well-known theorem $P + S = L + 2$, where P is the number of points or summits, S the number of plane bounding surfaces and L the number of linear edges in a geometrical solid. "The question—how many n-edrons are there?—has been asked, but it is not likely soon to receive a definite answer. It is far from being a simple question, even when reduced to the narrower compass—how many n-edrons are there whose summits are all trihedral"? He enumerated and constructed the fourteen 8-edra whose faces are all triangles.

In 1858 the French Institute modified its prize question. As the subject for the *concours* of 1861 was announced: "Perfectionner en quelque point important la théorie géométrique des polyèdres," where the indefiniteness of the question indicates the very imperfect state of knowledge on the subject. The prize offered was 3000 francs. Kirkman appears to have worked at it with a view of competing, but he did not send in his memoir. Cayley appears to have intended to compete. The time was prolonged for a year, but there was no award and the prize was taken down. Kirkman communicated his results to the Royal Society through his friend Cayley, and was soon elected a Fellow. Then he contributed directly an elaborate paper entitled "Complete theory of the Polyedra." In the preface he says, "The following memoir contains a complete solution of the classification and enumeration of the P-edra Q-acra. The actual construction of the solids is a task impracticable from its magnitude, but it is here shown that we can enumerate them with an accurate account of their symmetry to any values of P and Q." The memoir consisted of 21 sections; only the two introductory sections, occupying 45 quarto pages, were printed by the Society, while the others still remain in manuscript. During following years he added many contributions to this subject.

In 1858 the French Academy also proposed a problem in the Theory of Groups as the subject for competition for the grand mathematical prize in 1860: "Quels peuvent être les nombres de valeurs des fonctions bien définies qui contiennent un nombre donné de lettres, et comment peut on former les fonctions pour lesquelles il existe un nombre donné de valeurs?" Three memoirs were presented, of which Kirkman's was one, but no prize was awarded. Not the slightest summary was vouchsafed of what the competitors had added to science, although it was confessed that all had contributed results both new and important; and the question, though proposed for the first time for the year 1860, was withdrawn from competition contrary to the usual custom of the Academy. Kirkman contributed the results of his investigation to the Manchester Society under the title "The complete theory of groups, being the solution of the mathematical prize question of the French Academy for 1860." In more recent years the theory of groups has engaged the attention of many mathematicians in Germany and America; so far as British contributors are concerned Kirkman was the first and still remains the greatest.

In 1861 the British Association met at Manchester; it was the last of its meetings which Sir William Rowan Hamilton attended. After the meeting Hamilton visited Kirkman at his home in the Croft rectory, and that meeting was no doubt a stimulus to both. As regards pure mathematics they were probably the two greatest in Britain; both felt the loneliness of scientific work, both were metaphysicians of penetrating power, both were good versifiers if not great poets. Of nearly the same age, they were both endowed with splendid physique; but the care which was taken of their health was very different; in four years Hamilton died but Kirkman lived more than 30 years longer.

About 1862 the *Educational Times*, a monthly periodical published in London, began to devote several columns to the proposing and solving of mathematical problems, taking up the work after the demise of the *Diary*. This matter was afterwards reprinted in separate volumes, two for each year. In these reprints are to be found many questions proposed by Kirkman; they are generally propounded in quaint verse, and many of them were suggested by his study of combinations. A good specimen is "The Revenge of Old King Cole"

> "Full oft ye have had your fiddler's fling,
> For your own fun over the wine;
> And now" quoth Cole, the merry old king,
> "Ye shall have it again for mine.
> My realm prepares for a week of joy
> At the coming of age of a princely boy—
> Of the grand six days procession in square,
> In all your splendour dressed,
> Filling the city with music rare
> From fiddlers five abreast," etc.

The problem set forth by this and other verses is that of 25 men arranged in five rows on Monday. Shifting the second column one step upward, the third two steps, the fourth three steps, and the fifth four steps gives the arrangement for Tuesday. Applying the same rule to Tuesday gives Wednesday's array, and similarly are found those for Thursday and Friday. In none of these can the same two men be found in one row. But the rule fails to work for Saturday, so that a special arrangement must be brought in which

I leave to my hearers to work out. This problem resembles that of the fifteen schoolgirls.

Monday						Tuesday				
A	B	C	D	E		A	G	M	S	Y
F	G	H	I	J		F	L	R	X	E
K	L	M	N	O		K	Q	W	D	J
P	Q	R	S	T		P	V	C	I	O
U	V	W	X	Y		U	B	H	N	T

Wednesday						Thursday				
A	L	W	I	T		A	Q	H	X	O
F	Q	C	N	Y		F	V	M	D	T
K	V	H	S	E		K	B	R	I	Y
P	B	M	X	J		P	G	W	N	E
N	G	R	D	O		U	L	C	S	J

The Rev. Kirkman became at an early period of his life a broad churchman. About 1863 he came forward in defense of the Bishop of Colenso, a mathematician, and later he contributed to a series of pamphlets published in aid of the cause of "Free Enquiry and Free Expression." In one of his letters to me Kirkman writes as follows: "*The Life of Colenso* by my friend Rev. Sir George Cox, Bart., is a most charming book; and the battle of the Bishops against the lawyers in the matter of the vacant see of Natal, to which Cox is the bishop-elect, is exciting. Canterbury refuses to ask, as required, the Queen's mandate to consecrate him. The Natal churchmen have just petitioned the Queen to make the Primate do his duty according to law. Natal was made a See with perpetual succession, and is endowed. The endowment has been lying idle since Colenso's death in 1883; and the bishops who have the law courts dead against them here are determined that no successor to Colenso shall be consecrated. There is a Bishop of South African Church there, whom they thrust in while Colenso lived, on pretense that Colenso was excommunicate. We shall soon see whether the lawyers or the bishops are to win." It was Kirkman's own belief that his course in this matter injured his chance of preferment in the church; he never rose above being rector of Croft.

While a broad churchman the Rev. Mr. Kirkman was very vehement against the leaders of the materialistic philosophy. Two years after Tyndall's Belfast address, in which he announced that he could discern in matter the promise and potency of every form of life, Kirkman published a volume entitled *Philosophy without Assumptions*, in which he criticises in very vigorous style the materialistic and evolutional philosophy advocated by Mill, Spencer, Tyndall, and Huxley. In ascribing everything to matter and its powers or potencies he considers that they turn philosophy upside down. He has, he writes, first-hand knowledge of himself as a continuous person, endowed with will; and he infers that there are will forces around; but he sees no evidence of the existence of matter. Matter is an assumption and forms no part of his philosophy. He relies on Boscovich's theory of an atom as simply the center of forces. Force he understands from his knowledge of will, but any other substance he does not understand. The obvious difficulty in this philosophy is to explain the belief in the existence of other conscious beings—other will forces. Is it not the *great* assumption which everyone is obliged to make; verified by experience, but still in its nature an assumption? Kirkman tries to get over this difficulty by means of a syllogism, the major premise of which he has to manufacture, and which he presents to his reason for adoption or rejection. How can a universal proposition be easier to grasp than the particular case included in it? If the mind doubts about an individual case, how can it be sure about an infinite number of such cases? It is a *petitio principii.*

As a critic of the materialistic philosophy Kirkman is more successful. He criticises Herbert Spencer on free will as follows: "The short chapter of eight pages on Will cost more philosophical toil than all the two volumes on Psychology. The author gets himself in a heat, he runs himself into a corner, and brings himself dangerously to bay. Hear him: 'To reduce the general question to its simplest form; psychical changes either conform to law, or they do not. If they do not conform to law, this work, in common with all other works on the subject, is sheer nonsense; no science of Psychology is possible. If they do conform to law, there cannot be any such thing as free will.' Here we see the horrible alternative. If the assertors of free will refuse to commit suicide, they must endure the infinitely greater pang of seeing Mr. Spencer hurl himself and his books into that yawning gulf, a sacrifice long devoted, and now by pitiless Fate consigned, to the abysmal gods of nonsense. Then pitch him down say I. Shall I spare him who tells me that my movements in this orbit of conscious thought and responsibility are made under 'parallel conditions' with those of yon driven moon? Shall I spare him who has juggled me out of my Will, my noblest attribute; who has hocuspocused me out of my subsisting personality; and then, as a refinement of cruelty, has frightened me out of the rest of my wits by forcing me to this terrific alternative that either the testimony of this Being, this Reason and this Conscience is one ever-thundering lie, or else he, even he, has talked nonsense? He has talked nonsense, I say it because I have proved it. And every man must of course talk nonsense who begins his philosophy with abstracts in the clouds instead of building on the witness of his own self-consciousness. 'If they do conform to law,' says Spencer, 'there cannot be any such thing as free will.' The force of this seems to depend on his knowledge of 'law.' When I ask, What does this writer know of law—definite working law in the Cosmos?—the only answer I can get is—Nothing, except a very little which he has picked up, often malappropriately, as we have seen, among the mathematicians. When I ask—What does he know *about* law?—there is neither beginning nor end to the reply. I am advised to read his books *about* law, and to master the differentiations and integrations of the coherences, the correlations, the uniformities, and universalities which he has established in the abstract over all space and all time by his vast experience and miraculous penetration. I have tried to do this, and have found all pretty satisfactory, except the lack of one thing—something like proof of his competence to decide all that scientifically. When I persist in my demand for such proof, it turns out at last—that he knows by heart the whole Hymn Book, the Litanies, the Missal, and the Decretals of the Must-be-ite religion! 'Conform to law.' Shall I tell you what he means by that? Exactly ninety-nine hundredths of his meaning under the word *law* is *must be*."

Kirkman points out that the kind of proof offered by these philosophers is a bold assertion of *must-be-so*. For instance he mentions Spencer's evolution of consciousness out of the unconscious: "That an effectual adjustment may be made they (the separate impressions or constituent changes of a complex correspondence to be coordinated) *must be* brought into relation with each other. But this implies some center of communication common to them all, through which they severally pass; and as they *cannot* pass through it simultaneously, they *must* pass through it in succession. So that as the external phenomena responded to become greater in number and more complicated in kind, the variety and rapidity of the changes to which this common center of communication is subject *must* increase, there *must* result an unbroken series of those changes, there *must* arise a consciousness."

The paraphrase which Kirkman gave of Spencer's definition of Evolution commended itself to such great minds as Tait and Clerk-Maxwell. Spencer's definition is: "Evolution is a change from an indefinite incoherent homogeneity to a definite coherent heterogeneity, through continuous differentiations and integrations." Kirkman's paraphrase is "Evolution is a change from a nohowish untalkaboutable all-likeness, to a somehowish and in-general-talkaboutable not-all-likeness, by continuous somethingelseifications and sticktogetherations." The tone of Kirkman's book is distinctly polemical and full of sarcasm. He unfortunately wrote as a theologian rather than as a mathematician. The writers criticised did not reply, although they felt the edge of his sarcasm; and they acted wisely, for they could not successfully debate any subject involving exact science against one of the most penetrating mathematicians of the nineteenth century.

We have seen that Hamilton appreciated Kirkman's genius; so did Cayley, De Morgan, Clerk-Maxwell, Tait. One of Tait's most elaborate researches was the enumeration and construction of the knots which can be formed in an endless cord—a subject which he was induced to take up on account of its bearing on the vortex theory of atoms. If the atoms are vortex filaments their differences in kind, giving rise to differences in the spectra of the elements, must depend on a greater or less complexity in the form of the closed filament, and this difference would depend on the knottiness of the filament. Hence the main question was "How many different forms of knots are there with any given small number of crossings?" Tait made the investigation for three, four, five, six, seven, eight crossings. Kirkman's investigations on the polyedra were much allied. He took up the problem and, with some assistance from Tait, solved it not only for nine but for ten crossings. An investigation by C. N. Little, a graduate of Yale University, has confirmed Kirkman's results.

Through Professor Tait I was introduced to Rev. Mr. Kirkman; and we discussed the mathematical analysis of relationships, formal logic, and other subjects. After I had gone to the University of Texas, Kirkman sent me through Tait the following question which he said was current in society: "Two boys, Smith and Jones, of the same age, are each the nephew of the other; how many legal solutions?" I set the analysis to work, wrote out the solutions, and the paper is printed in the *Proceedings* of the Royal Society of Edinburgh. There are four solutions, provided Smith and Jones are taken to be mere arbitrary, names; if the convention about surnames holds there are only two legal solutions. On seeing my paper Kirkman sent the question to the *Educational Times* in the following improved form:

> Baby Tom of baby Hugh
> The nephew is and uncle too;
> In how many ways can this be true?

Thomas Penyngton Kirkman died on February 3, 1895, having very nearly reached the age of 89 years. I have found only one printed notice of his career, but all his writings are mentioned in the new German Encyclopædia of Mathematics. He was an honorary member of the Literary and Philosophical Societies of Manchester and of Liverpool, a Fellow of the Royal Society, and a foreign member of the Dutch Society of Sciences at Haarlem. I may close by a quotation from one of his letters: "What I have done in helping busy Tait in knots is, like the much more difficult and extensive things I have done in polyedra or groups, not at likely to be talked about intelligently by people so long as I live. But it is a faint pleasure to think it will one day win a little praise."

Chapter 10

ISAAC TODHUNTER[1]

(1820-1884)

Isaac Todhunter was born at Rye, Sussex, 23 Nov., 1820. He was the second son of George Todhunter, Congregationalist minister of the place, and of Mary his wife, whose maiden name was Hume, a Scottish surname. The minister died of consumption when Isaac was six years old, and left his family, consisting of wife and four boys, in narrow circumstances. The widow, who was a woman of strength, physically and mentally, moved to the larger town of Hastings in the same county, and opened a school for girls. After some years Isaac was sent to a boys' school in the same town kept by Robert Carr, and subsequently to one newly opened by a Mr. Austin from London; for some years he had been unusually backward in his studies, but under this new teacher he made rapid progress, and his career was then largely determined.

After his school days were over, he became an usher or assistant master with Mr. Austin in a school at Peckham; and contrived to attend at the same time the evening classes at University College, London. There he came under the great educating influence of De Morgan, for whom in after years he always expressed an unbounded admiration; to De Morgan "he owed that interest in the history and bibliography of science, in moral philosophy and logic which determined the course of his riper studies." In 1839 he passed the matriculation examination of the University of London, then a merely examining body, winning the exhibition for mathematics (£30 for two years);

[1]This Lecture was delivered April 13, 1904.—Editors.

in 1842 he passed the B.A. examination carrying off a mathematical scholarship (of £50 for three years); and in 1844 obtained the degree of Master of Arts with the gold medal awarded to the candidate who gained the greatest distinction in that examination.

Sylvester was then professor of natural philosophy in University College, and Todhunter studied under him. The writings of Sir John Herschel also had an influence; for Todhunter wrote as follows (*Conflict of Studies*, p. 66): "Let me at the outset record my opinion of mathematics; I cannot do this better than by adopting the words of Sir J. Herschel, to the influence of which I gratefully attribute the direction of my own early studies. He says of Astronomy, 'Admission to its sanctuary can only be gained by one means,—sound and sufficient knowledge of mathematics, the great instrument of all exact inquiry, without which no man can ever make such advances in this or any other of the higher departments of science as can entitle him to form an independent opinion on any subject of discussion within their range.'"

When Todhunter graduated as M.A. he was 24 years of age. Sylvester had gone to Virginia, but De Morgan remained. The latter advised him to go through the regular course at Cambridge; his name was now entered at St. John's College. Being somewhat older, and much more brilliant than the honor men of his year, he was able to devote a great part of his attention to studies beyond those prescribed. Among other subjects he took up Mathematical Electricity. In 1848 he took his B.A. degree as senior wrangler, and also won the first Smith's prize.

While an undergraduate Todhunter lived a very secluded life. He contributed along with his brothers to the support of their mother, and he had neither money nor time to spend on entertainments. The following legend was applied to him, if not recorded of him: "Once on a time, a senior wrangler gave a wine party to celebrate his triumph. Six guests took their seats round the table. Turning the key in the door, he placed one bottle of wine on the table asseverating with unction, 'None of you will leave this room while a single drop remains.'"

At the University of Cambridge there is a foundation which provides for what is called the Burney prize. According to the regulations the prize is to be awarded to a graduate of the University who is not of more than three years' standing from admission to his degree and who shall produce the best English essay "On some moral or metaphysical subject, or on the existence, nature and attributes of God, or on the truth and evidence of the Christian religion." Todhunter in the course of his first postgraduate year submitted an essay on the thesis that "The doctrine of a divine providence is inseparable from the belief in the existence of an absolutely perfect Creator." This essay received the prize, and was printed in 1849.

Todhunter now proceeded to the degree of M.A., and unlike his mathematical instructors in University College, De Morgan and Sylvester, he did not parade his non-conformist principles, but submitted to the regulations with as good grace as possible. He was elected a fellow of his college, but not immediately, probably on account of his being a non-conformist, and appointed lecturer on mathematics therein; he also engaged for some time in work as a private tutor, having for one of his pupils P. G. Tait, and I believe E. J. Routh also.

For a space of 15 years he remained a fellow of St. John's College, residing in it, and taking part in the instruction. He was very successful as a lecturer, and it was not long before he began to publish textbooks on the subjects of his lectures. In 1853 he published a textbook on *Analytical Statics*; in 1855 one on *Plane Coordinate Geometry*; and in 1858 *Examples of Analytical Geometry of Three Dimensions*. His success in these subjects induced him to prepare manuals on elementary mathematics; his *Algebra* appeared in 1858, his *Trigonometry* in 1859, his *Theory of Equations* in 1861, and his *Euclid* in 1862. Some of his textbooks passed through many editions and have been widely used in Great Britain and North America. Latterly he was appointed principal mathematical lecturer in his college, and he chose to drill the freshmen in Euclid and other elementary mathematics.

Within these years he also labored at some works of a more strictly scientific character. Professor Woodhouse (who was the forerunner of the Analytical Society) had written a history of the calculus of variations, ending with the eighteenth century; this work was much admired for its usefulness by Todhunter, and as he felt a decided taste for the history of mathematics, he formed and carried out the project of continuing the history of that calculus during the nineteenth century. It was the first of the great historical works which has given Todhunter his high place among the mathematicians of the nineteenth century. This history was published in 1861; in 1862 he was elected a Fellow of the Royal Society of London. In 1863 he was a candidate for the Sadlerian professorship of Mathematics, to which Cayley was appointed. Todhunter was not a mere mathematical specialist. He was an excellent linguist; besides being a sound Latin and Greek scholar, he was familiar with French, German, Spanish, Italian and also Russian, Hebrew and Sanskrit. He was likewise well versed in philosophy, and for the two years

1863-5 acted as an Examiner for the Moral Science Tripos, of which the chief founders were himself and Whewell.

By 1864 the financial success of his books was such that he was able to marry, a step which involved the resigning of his fellowship. His wife was a daughter of Captain George Davies of the Royal Navy, afterwards Admiral Davies.

As a fellow and tutor of St. John's College he had lived a very secluded life. His relatives and friends thought he was a confirmed bachelor. He had sometimes hinted that the grapes were sour. For art he had little eye; for music no ear. "He used to say he knew two tunes; one was 'God save the Queen,' the other wasn't. The former he recognized by the people standing up." As owls shun the broad daylight he had shunned the glare of parlors. It was therefore a surprise to his friends and relatives when they were invited to his marriage in 1864. Prof. Mayor records that Todhunter wrote to his fiancée, "You will not forget, I am sure, that I have always been a student, and always shall be; but books shall not come into even distant rivalry with you," and Prof. Mayor insinuated that thus forearmed, he calmly introduced to the inner circle of their honeymoon Hamilton on *Quaternions*.

It was now (1865) that the London Mathematical Society was organized under the guidance of De Morgan, and Todhunter became a member in the first year of its existence. The same year he discharged the very onerous duties of examiner for the mathematical tripos—a task requiring so much labor and involving so much interference with his work as an author that he never accepted it again. Now (1865) appeared his *History of the Mathematical Theory of Probability*, and the same year he was able to edit a new edition of Boole's *Treatise on Differential Equations*, the author having succumbed to an untimely death. Todhunter certainly had a high appreciation of Boole, which he shared in common with De Morgan. The work involved in editing the successive editions of his elementary books was great; he did not proceed to stereotype until many independent editions gave ample opportunity to correct all errors and misprints. He now added two more textbooks; *Mechanics* in 1867 and *Mensuration* in 1869.

About 1847 the members of St. John's College founded a prize in honor of their distinguished fellow, J. C. Adams. It is awarded every two years, and is in value about £225. In 1869 the subject proposed was "A determination of the circumstances under which Discontinuity of any kind presents itself in the solution of a problem of maximum or minimum in the Calculus of Variations." There had been a controversy a few years previous on this subject in the pages of *Philosophical Magazine* and Todhunter had there advocated his view of the matter. This view is found in the opening sentences of his essay: "We shall find that, generally speaking, discontinuity is introduced, by virtue of some restriction which we impose, either explicitly or implicitly in the statement of the problems which we propose to solve." This thesis he supported by considering in turn the usual applications of the calculus, and pointing out where he considers the discontinuities which occur have been introduced into the conditions of the problem. This he successfully proves in many instances. In some cases, the want of a distinct test of what discontinuity is somewhat obscures the argument. To his essay the prize was awarded; it is published under the title "Researches in the Calculus of Variations"—an entirely different work from his *History of the Calculus of Variations.*

In 1873 he published his *History of the Mathematical Theories of Attraction.* It consists of two volumes of nearly 1000 pages altogether and is probably the most elaborate of his histories. In the same year (1873) he published in book form his views on some of the educational questions of the day, under the title of *The Conflict of Studies, and other essays on subjects connected with education.* The collection contains six essays; they were originally written with the view of successive publication in some magazine, but in fact they were published only in book form. In the first essay, that on the Conflict of Studies—Todhunter gave his opinion of the educative value in high schools and colleges of the different kinds of study then commonly advocated in opposition to or in addition to the old subjects of classics and mathematics. He considered that the Experimental Sciences were little suitable, and that for a very English reason, because they could not be examined on adequately. He says:

"Experimental Science viewed in connection with education, rejoices in a name which is unfairly expressive. A real experiment is a very valuable product of the mind, requiring great knowledge to invent it and great ingenuity to carry it out. When Perrier ascended the Puy de Dôme with a barometer in order to test the influence of change of level on the height of the column of mercury, he performed an experiment, the suggestion of which was worthy of the genius of Pascal and Descartes. But when a modern traveller ascends Mont Blanc, and directs one of his guides to carry a barometer, he cannot be said to perform an experiment in any very exact or very meritorious sense of the word. It is a repetition of an observation made thousands of times before, and we can never recover any of the interest which belonged to the first trial, unless indeed, without having ever heard of it, we succeeded in reconstructing the process of ourselves. In fact, almost always he who first plucks an experimental flower thus appropriates and destroys its fragrance and its beauty."

At the time when Todhunter was writing the above, the Cavendish Laboratory for Experimental Physics was just being built at Cambridge, and Clerk-Maxwell had just been appointed the professor of the new study; from Todhunter's utterance we can see the state of affairs then prevailing. Consider the corresponding experiment of Torricelli, which can be performed inside a classroom; to every fresh student the experiment retains its fragrance; the sight of it, and more especially the performance of it imparts a kind of knowledge which cannot be got from description or testimony; it imparts accurate conceptions and is a necessary preparative for making a new and original experiment. To Todhunter it may be replied that the flowers of Euclid's *Elements* were plucked at least 2000 years ago, yet, he must admit, they still possess, to the fresh student of mathematics, even although he becomes acquainted with them through a textbook, both fragrance and beauty.

Todhunter went on to write another passage which roused the ire of Professor Tait. "To take another example. We assert that if the resistance of the air be withdrawn a sovereign and a feather will fall through equal spaces in equal times. Very great credit is due to the person who first imagined the well-known experiment to illustrate this; but it is not obvious what is the special benefit now gained by seeing a lecturer repeat the process. It may be said that a boy takes more interest in the matter by seeing for himself, or by performing for himself, that is, by working the handle of the air-pump; this we admit, while we continue to doubt the educational value of the transaction. The boy would also probably take much more interest in football than in Latin grammar; but the measure of his interest is not identical with that of the importance of the subjects. It may be said that the fact makes a stronger impression on the boy through the medium of his sight, that he believes it the more confidently. I say that this ought not to be the case. If he does not believe the statements of his tutor—probably a clergyman of mature knowledge, recognized ability and blameless character—his suspicion is irrational, and manifests a want of the power of appreciating evidence, a want fatal to his success in that branch of science which he is supposed to be cultivating."

Clear physical conceptions cannot be got by tradition, even from a clergyman of blameless character; they are best got directly from Nature, and this is recognized by the modern laboratory instruction in physics. Todhunter would reduce science to a matter of authority; and indeed his mathematical manuals are not free from that fault. He deals with the characteristic difficulties of algebra by authority rather than by scientific explanation. Todhunter goes on to say: "Some considerable drawback must be made from the educational value of experiments, so called, on account of their failure. Many persons must have been present at the exhibitions of skilled performers, and have witnessed an uninterrupted series of ignominious reverses,—they have probably longed to imitate the cautious student who watched an eminent astronomer baffled by Foucault's experiment for proving the rotation of the Earth; as the pendulum would move the wrong way the student retired, saying that he wished to retain his faith in the elements of astronomy."

It is not unlikely that the series of ignominious reverses Todhunter had in his view were what he had seen in the physics classroom of University College when the manipulation was in the hands of a pure mathematician—Prof. Sylvester. At the University of Texas there is a fine clear space about 60 feet high inside the building, very suitable for Foucault's experiment. I fixed up a pendulum, using a very heavy ball, and the turning of the Earth could be seen in two successive oscillations. The experiment, although only a repetition according to Todhunter, was a live and inspiring lesson to all who saw it, whether they came with previous knowledge about it or no. The repetition of any such great experiment has an educative value of which Todhunter had no conception.

Another subject which Todhunter discussed in these essays is the suitability of Euclid's *Elements* for use as the elementary textbook of Geometry. His experience as a college tutor for 25 years; his numerous engagements as an examiner in mathematics; his correspondence with teachers in the large schools gave weight to the opinion which he expressed. The question was raised by the first report of the Association for the Improvement of Geometrical Teaching; and the points which Todhunter made were afterwards taken up and presented in his own unique style by Lewis Carroll in "Euclid and his modern rivals." Up to that time Euclid's manual was, and in a very large measure still is, the authorized introduction to geometry; it is not as in this country where there is perfect liberty as to the books and methods to be employed. The great difficulty in the way of liberty in geometrical teaching is the universal tyranny of competitive examinations. Great Britain is an examination-ridden country. Todhunter referred to one of the most distinguished professors of Mathematics in England; one whose pupils had likewise gained a high reputation as investigators and teachers; his "venerated master and friend," Prof. De Morgan; and pointed out that he recommended the study of Euclid with all the authority of his great attainments and experience.

Another argument used by Todhunter was as follows: In America there are the conditions which the Association desires; there is, for example, a textbook which defines parallel lines as those which *have the same direction.* Could the American mathematicians of that day compare with those of England? He answered no.

While Todhunter could point to one master—De Morgan—as in his favor, he was obliged to quote another master—Sylvester—as opposed. In his presidential address before section A of the British Association at Exeter in 1869, Sylvester had said: "I should rejoice to see . . . Euclid honorably shelved or buried 'deeper than did ever plummet sound' out of the schoolboy's reach; morphology introduced into the elements of algebra; projection, correlation, and motion accepted as aids to geometry; the mind of the student quickened and elevated and his faith awakened by early initiation into the ruling ideas of polarity, continuity, infinity, and familiarization with the doctrine of the imaginary and inconceivable." Todhunter replied: "Whatever may have produced the dislike to Euclid in the illustrious mathematician whose words I have quoted, there is no ground for supposing that he would have been better pleased with the substitutes which are now offered and recommended in its place. But the remark which is naturally suggested by the passage is that nothing prevents an enthusiastic teacher from carrying his pupils to any height he pleases in geometry, even if he starts with the use of Euclid."

Todhunter also replied to the adverse opinion, delivered by some professor (doubtless Tait) in an address at Edinburgh which was as follows: "From the majority of the papers in our few mathematical journals, one would almost be led to fancy that British mathematicians have too much pride to use a simple method, while an unnecessarily complex one can be had. No more telling example of this could be wished for than the insane delusion under which they permit 'Euclid' to be employed in our elementary teaching. They seem voluntarily to weight alike themselves and their pupils for the race." To which Todhunter replied: "The British mathematical journals with the titles of which I am acquainted are the Quarterly Journal of Mathematics, the Mathematical Messenger, and the Philosophical Magazine; to which may be added the Proceedings of the Royal Society and the Monthly Notices of the Astronomical Society. I should have thought it would have been an adequate employment, for a person engaged in teaching, to read and master these periodicals regularly; but that a single mathematician should be able to improve more than half the matter which is thus presented to him fills me with amazement. I take down some of these volumes, and turning over the pages I find article after article by Profs. Cayley, Salmon and Sylvester, not to mention many other highly distinguished names. The idea of amending the elaborate essays of these eminent mathematicians seems to me something like the audacity recorded in poetry with which a superhuman hero climbs to the summit of the Indian Olympus and overturns the thrones of Vishnu, Brahma and Siva. While we may regret that such ability should be exerted on the revolutionary side of the question, here is at least one mournful satisfaction: the weapon with which Euclid is assailed was forged by Euclid himself. The justly celebrated professor, from whose address the quotation is taken, was himself trained by those exercises which he now considers worthless; twenty years ago his solutions of mathematical problems were rich with the fragrance of the Greek geometry. I venture to predict that we shall have to wait some time before a pupil will issue from the reformed school, who singlehanded will be able to challenge more than half the mathematicians of England." Professor Tait, in what he said, had, doubtless, reference to the avoidance of the use of the Quaternion method by his contemporaries in mathematics.

More than half of the Essays is taken up with questions connected with competitive examinations. Todhunter explains the influence of Cambridge in this matter: "Ours is an age of examination; and the University of Cambridge may claim the merit of originating this characteristic of the period. When we hear, as we often do, that the Universities are effete bodies which have lost their influence on the national character, we may point with real or affected triumph to the spread of examinations as a decisive proof that the humiliating assertion is not absolutely true. Although there must have been in schools and elsewhere processes resembling examinations before those of Cambridge had become widely famous, yet there can be little chance of error in regarding our mathematical tripos as the model for rigor, justice and importance, of a long succession of institutions of a similar kind which have since been constructed." Todhunter makes the damaging admission that "We cannot by our examinations, *create* learning or genius; it is uncertain whether we can infallibly *discover* them; what we detect is simply the examination-passing power."

In England education is for the most part directed to training pupils for examination. One direct consequence is that the memory is cultivated at the expense of the understanding; knowledge instead of being assimilated is crammed for the time being, and lost as soon as the examination is over.

Instead of a rational study of the principles of mathematics, attention is directed to problem-making,—to solving ten-minute conundrums. Textbooks are written with the view not of teaching the subject in the most scientific manner, but of passing certain specified examinations. I have seen such a textbook on trigonometry where all the important theorems which required the genius of Gregory and others to discover, are put down as so many definitions. Nominal knowledge, not real, is the kind that suits examinations.

Todhunter possessed a considerable sense of humour. We see this in his Essays; among other stories he tells the following: A youth who was quite unable to satisfy his examiners as to a problem, endeavored to mollify them, as he said, "by writing out book work bordering on the problem." Another youth who was rejected said "if there had been fairer examiners and better papers I should have passed; I knew many things which were not set." Again: "A visitor to Cambridge put himself under the care of one of the self-constituted guides who obtrude their services. Members of the various ranks of the academical state were pointed out to the stranger—heads of colleges, professors and ordinary fellows; and some attempt was made to describe the nature of the functions discharged by the heads and professors. But an inquiry as to the duties of fellows produced and reproduced only the answer, 'Them's fellows I say.' The guide had not been able to attach the notion of even the pretense of duty to a fellowship."

In 1874 Todhunter was elected an honorary fellow of his college, an honor which he prized very highly. Later on he was chosen as an elector to three of the University professorships—Moral Philosophy, Astronomy, Mental Philosophy and Logic. When the University of Cambridge established its new degree of Doctor of Science, restricted to those who have made original contributions to the advancement of science or learning, Todhunter was one of those whose application was granted within the first few months. In 1875 he published his manual *Functions of Laplace, Bessel and Legendre*. Next year he finished an arduous literary task—the preparation of two volumes, the one containing an account of the writings of Whewell, the other containing selections from his literary and scientific correspondence. Todhunter's task was marred to a considerable extent by an unfortunate division of the matter: the scientific and literary details were given to him, while the writing of the life itself was given to another.

In the summer of 1880 Dr. Todhunter first began to suffer from his eyesight, and from that date he gradually and slowly became weaker. But it was not till September, 1883, when he was at Hunstanton, that the worst symptoms came on. He then partially lost by paralysis the use of the right arm; and, though he afterwards recovered from this, he was left much weaker. In January of the next year he had another attack, and he died on March 1, 1884, in the 64th year of his age.

Todhunter left a *History of Elasticity* nearly finished. The manuscript was submitted, to Cayley for report; it was in 1886 published under the editorship of Karl Pearson. I believe that he had other histories in contemplation; I had the honor of meeting him once, and in the course of conversation on mathematical logic, he said that he had a project of taking up the history of that subject; his interest in it dated from his study under De Morgan. Todhunter had the same ruling passion as Airy—love of order—and was thus able to achieve an immense amount of mathematical work. Prof. Mayor wrote, "Todhunter had no enemies, for he neither coined nor circulated scandal; men of all sects and parties were at home with him, for he was many-sided enough to see good in every thing. His friendship extended even to the lower creatures. The canaries always hung in his room, for he never forgot to see to their wants."

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