

LORD KELVIN  
HIS LIFE AND WORK  
A. E. RUSSELL M. A. D. Sc

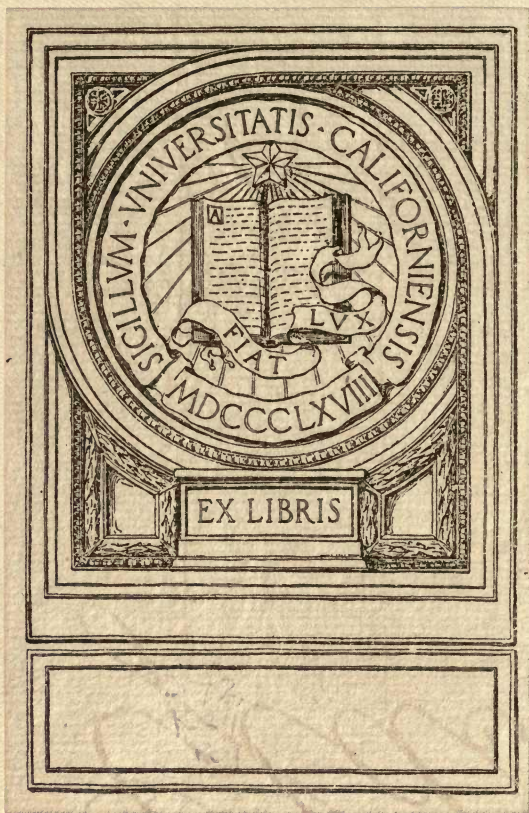
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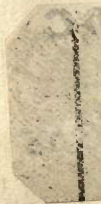
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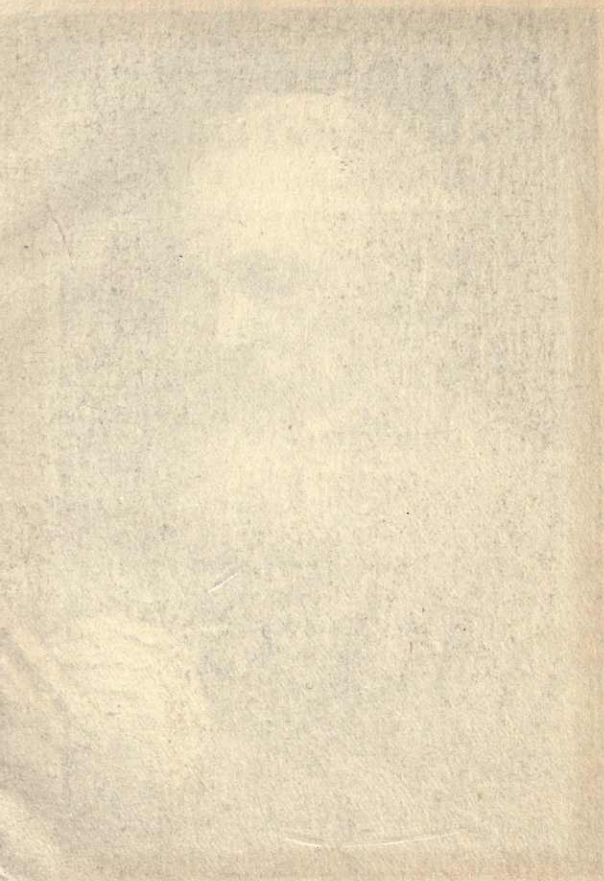


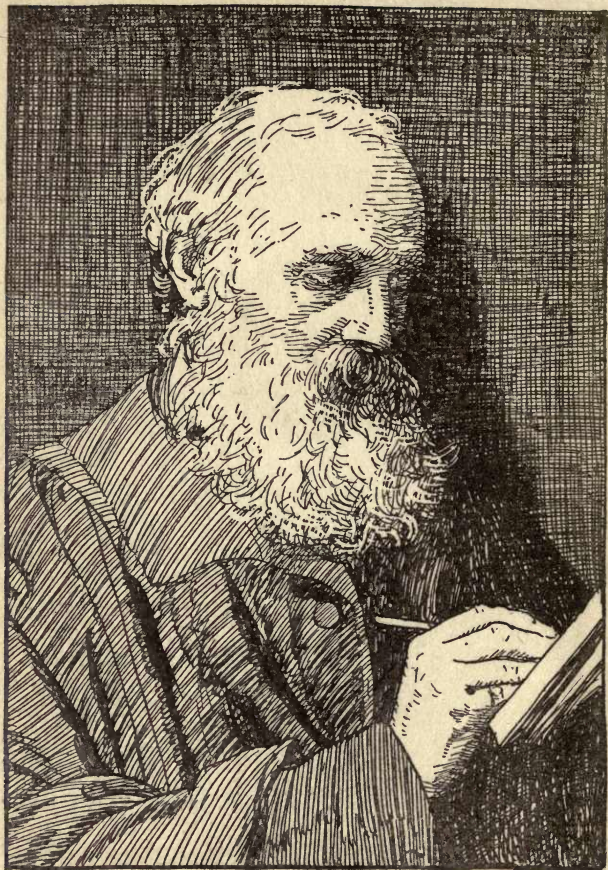
THE PEOPLE'S BOOKS











# LORD KELVIN

HIS LIFE AND WORK

By ALEXANDER RUSSELL

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PRINCIPAL OF FARADAY HOUSE, LONDON



THE  
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MEMORIAL

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## PREFACE

IN the life of a military leader the account of the preparations for his campaigns, his successes and failures, naturally hold the most prominent place. So, also, in giving even a brief description of the life of a leader of science, a relatively large amount of space must be devoted to his work. A whole-hearted devotion to the development of science for the material and intellectual welfare of humanity is the keynote of Kelvin's life. The author has attempted to describe the scientific work in simple language, but, owing to the very advanced and abstruse nature of much of Kelvin's work, he is conscious that some of it will remain obscure to the non-scientific reader. He will be happy, however, if anything he has written induces the reader to make a further study of the subject in Kelvin's original memoirs, which are now accessible to all in his published works.

A. R.

FARADAY HOUSE,  
LONDON, 1912.

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# LORD KELVIN

## CHAPTER I

### EARLY LIFE

THE effects of early upbringing and environment on a man's life and work are always important. In the case of Lord Kelvin these effects are especially apparent. His father, James Thomson, was born at Armaghmore, near Ballynahinch, County Down, Ireland, in 1786. His grandfather was a small farmer, whose ancestors came over from Scotland in covenanting times, probably owing to persecution. Sprung from such a stock, the Thomsons took a stern and serious view of life. The disturbed condition of the country did not tend to mollify this view. James Thomson, when twelve years old, was an eyewitness of some of the horrors of the Irish Rebellion of 1798. These scenes left an ineffaceable impression on his mind.

It is related of James Thomson that when still a boy he discovered for himself the art of making a sundial. As he was practically self-educated before this discovery, his natural ability must have been of a high order. It was not surprising, therefore, that when he went to school his progress was rapid and successful. When he finished his course, he was offered, and accepted, the post of assistant teacher. At this period his ambition was to become a Presbyterian minister. Like all true-hearted young men, he was eager to raise the moral and intellectual ideals of the people, and to ameliorate the lot of the poor. His parents, gratified at the bent of

his incinations, encouraged him to follow them, and so he remained as an assistant schoolmaster, thriftily saving money to pay for his college education.

It was not until 1810, when he was twenty-four years of age, that he was able to enter Glasgow University as a student. In those days it was a long and trying journey to get from County Down to Glasgow. The small vessels which sailed between Belfast and Glasgow were often delayed by storms, and in the summer-time they were sometimes becalmed for days at a time in the Firth of Clyde. Lord Kelvin relates how a smack, laden with a cargo of lime, in which his father was once sailing to Glasgow was becalmed, and was carried by the tide no less than three times round Ailsa Craig, a rocky islet about two miles in circumference, before enough wind arose to enable them to proceed on their voyage. On another occasion, when pressed for time, he and some fellow-student passengers asked the captain to land them on the coast of Ayrshire, so that they might walk to Glasgow—a distance of between thirty and forty miles.

At Glasgow University James Thomson won prizes in classics, mathematics, and natural philosophy. He took the M.A. degree in 1812. Afterwards, he attended all the theological classes and most of the medical. At that time this was not an uncommon practice of Scotch students, whose thirst for knowledge was insatiable. As the session lasted only six months, he was able, like so many others, to work in the summer, and so obtain money to pay, either in whole or in part, the small college fees and the necessary living expenses during the winter.

His success as a mathematical teacher and the high honours gained at the University led James Thomson to abandon the idea of entering the ministry. In 1815 he obtained the post of Professor of Mathematics in

the Royal Academical Institute at Belfast. This appointment he held until he was appointed to the Chair of Mathematics in Glasgow University.

He was a very successful teacher. His treatise on Arithmetic, published in 1819, is an excellent illustration of the lucidity of his teaching and his wide learning. The examples given are chosen with the minutest care. Each is an excellent illustration of some important branch of practical theory. Many of the notes given are of value to the antiquarian. Great stress is very properly laid on all commercial applications. Admirable explanations are given even of such advanced mathematical subjects as the theory of continued fractions. The ingenious method of extracting square roots by their use, which had then only just been discovered by the French mathematicians, is clearly described. References are freely given to eminent mathematicians like Lacroix and Legendre. In many respects, therefore, it is very unlike the books now in use, and puts arithmetic on an altogether higher level. The book ran through seventy editions in sixty years, and has had a great influence on the methods of teaching adopted in this country. In 1880 his sons, Professor James Thomson and Sir William Thomson, edited the seventy-second edition.

When at Belfast, James Thomson also wrote treatises on Geometry, Geography, Astronomy, and the Calculus. They all give a clear logical development of the subject under discussion, and contain many carefully worded definitions. In after years his eldest son, Professor James Thomson, showed a similar love for inventing definitions characterised by minute accuracy of statement. In 1829 the University of Glasgow conferred on him the honorary degree of LL.D.

James Thomson married Miss Gardiner, the daughter

of a Glasgow merchant, and had four sons and three daughters. Whilst at Belfast, he lost his wife and youngest daughter. The blow was a severe one, but he obtained consolation by devoting himself to the education of his young family. When he removed to Glasgow, his wife's sister, Mrs. Gall, kept house for him. As a father, Professor Thomson was admirable. Although a strict disciplinarian, he never alienated the affection of his children. The recollection of the high ideals he set before them was ever an incentive both to them and to his grandchildren to more strenuous exertions and loftier actions.

The education of his elder children he personally superintended with judicious care. In after years his second son, William (Lord Kelvin), used frequently to say that all that he learned as a boy in English, geography, history, mathematics, and classics was taught him, along with his brothers and sisters at home, by their father. He used also to add that he never met a better teacher in anything than his father was in everything. From 1832 to 1849 James Thomson was Mathematical Professor at Glasgow. His death in 1849 from cholera was a great loss to the University.

William Thomson (Lord Kelvin) was born in Belfast on June 26, 1824, and so was only eight years old when his father removed to Glasgow. His elder brother, James, was two years older. The Thomsons lived in the official residence of the mathematical professor, which was in the Old College in the High Street. The surroundings, except on the site of the College Green, were rather squalid. Old students, however, and the families of the professors look back on it with deep affection. A railway station now stands on the site of the Old College, no trace of which has been preserved. It is difficult to realise that the Molendinar stream once



meandered through college groves tenanted by cooing pigeons and cawing rooks. The removal, however, in 1871 of the college to the handsome buildings on Gilmorehill was well advised, from the hygienic point of view.

The thick fogs which sometimes occurred in the winter-time in the neighbourhood of the Old College often made it necessary to have the gas burning indoors all day. This and the cold east winds in the spring made climatic conditions very unfavourable to the delicate. The long summer holiday, however, gave the professor and his family a welcome opportunity for recuperating their health. In 1834, for instance, he arranged with the captain of the *Glenalbin*, a small steamer trading between Glasgow and Londonderry, to take him and his family to Invercloy (Brodict), on the Island of Arran. This is a mountainous and very picturesque island in the Firth of Clyde. At that time it was very sparsely populated. The Duke of Hamilton, who owned practically the whole island, would not allow his tenants to enlarge their cottages so as to make them more attractive to summer visitors. But he could not prevent visitors from coming and living in the little thatched-roof cottages. The primitive arrangements and the difficulties experienced in obtaining even the necessaries of life added much to the zest of the holiday. For example, the only bread obtainable was brought from Saltcoats, on the mainland, by a small sailing vessel, which came, weather permitting, twice a week.

It can be readily understood how the children revelled in the freedom from restraint, and how the excitement of exploring glens and hills and visiting waterfalls kept them constantly in the open air. The explorations were real, for, with the exception of the curlews and the plovers, they had the hills for a playground practically

to themselves. Elizabeth, the eldest daughter, tells how on one occasion she was so ill when she left Glasgow that she had to be carried on board the steamer. And yet, so great was the recuperative power of the air, in a week or two she could go for long walks with her brothers.

The encyclopædic knowledge of Professor Thomson naturally led him to take a great interest in the geology of the island, which has now for three or four generations been a place of pilgrimage for geologists from all parts of the world. The northern part of the island is of volcanic origin, and exceedingly precipitous. The lofty precipices and deep ravines within easy reach of Invercloy must have aroused the curiosity of the children, and led them to ask many questions of their father. It is easy to understand the fascination which problems in connection with Plutonic action and the physics of the earth's crust always had for his son William in after years.

James even at this early age showed his bent as an inventor and an engineer. He displayed much ability in making a model boat, which the children, with all due ceremony, christened the *St. Patrick*. This shows that they were proud of their Irish nationality. Playing with this boat and associating with the native seafaring men on the beach gave to William a lifelong love for nautical matters. The following extract from the great treatise on Natural Philosophy, which he wrote in conjunction with Professor Tait, proves that the observations of childhood often help the formulating of important rules in later life.

“That the course of a symmetrical square-rigged ship sailing in the direction of the wind with the rudder amidships is unstable, and can only be kept by manipulating the rudder to check infinitesimal deviations;—

and that a child's toy-boat, whether 'square-rigged' or 'fore-and-aft-rigged,' cannot be got to sail permanently before the wind by any permanent adjustment of rudder and sails, and that (without a wind vane, or a weighted tiller, acting on the rudder to do the part of steersman) it always, after running a few yards before the wind, turns round till nearly in a direction perpendicular to the wind (either 'jibing' first, or 'luffing' without jibing if it is a cutter or a schooner)."

We can almost picture James and William discussing why the *St. Patrick* would not sail steadily with the wind. In later years William's schooner yacht, the *Lalla Rookh*, was well known to yachtsmen on the Clyde and in the Solent. He took long voyages in it, sometimes as far as Madeira, and had, as we shall see, a reputation as an expert in navigation.

At the early age of ten years William matriculated at Glasgow University—and he and James went through the arts classes together. The younger brother was always first, but the elder was a good second. Their fellow-students were astounded at William's quick perception and his wide knowledge. The prizes were not always given on examinational results. Some of them, in accordance with an ancient custom, which, taking all things into consideration, worked in some cases remarkably well, were voted by his classmates.

Some idea of the subjects of the lectures can be got from the University calendar of that period. For example, in order to get the highest distinction in mathematics in the degree examinations, the candidate must profess Lagrange's *Theory of Functions* and "The Analytical Works of Apollonius and the other Ancient Geometricians." In natural philosophy the whole of Newton's *Principia* and Laplace's *Mécanique Céleste* must be known. The calendar makes the somewhat

alarming statement that candidates must answer all the questions set with perfect accuracy.

In 1839, when only fifteen, William gained a University medal for an essay on the figure of the earth. He also gained prizes in classics and logic, beating several very formidable competitors, one of whom—John Caird—was afterwards Principal of the University. Amongst the many friends he made with his fellow-students was Francis Sandford, son of Sir Daniel Sandford, the Professor of Greek. Francis Sandford went to Oxford as a Snell exhibitioner, and afterwards was well known as a great educationalist. As Lord Sandford of Sandford, he was one of Kelvin's sponsors when he entered the House of Lords.

Lord Kelvin always looked back with pride and admiration to the University of his young days. When giving his inaugural address, as Chancellor of the University, in 1904, he made a spirited reply to the accusation that the Glasgow University of his young days was only a stagnant survival of mediævalism.

“The University of Adam Smith, James Watt, and Thomas Reid was never stagnant.” Nearly two centuries ago it had a laboratory of human anatomy. Seventy-five years ago it had the first students' chemical laboratory, and sixty-five years ago it had the first professorship of engineering in the British Empire. Kelvin himself started the first physical laboratory in this country, using a deserted wine-cellar in an old professorial house for this purpose. This shows the pointlessness of the accusation.

In 1840 Professor Thomson, with some of his family, made a tour in Germany. During this tour William read for the first time Fourier's great treatise on the Conduction of Heat, which had been published eighteen years previously. The many results obtained by mathe-

mathematical analysis from a few fundamental principles, and the elegance of the mathematical methods used, excited his admiration to the highest degree. During all his life he often talked and wrote about the transcendent interest and perennial importance of Fourier's solutions in all branches of physical science. Some of his most important theoretical and practical work was done with their help. Even in 1907, the year of his death, he was busy applying these solutions to investigate the growth of a train of waves in water.

The reading of this book led him to write his first paper, which is headed "Frankfort, July 1840, and Glasgow, April 1841." In this paper he justifies Fourier's method against the strictures passed on it by Kelland in his *Theory of Heat* (1837). Kelland, who was professor at Edinburgh University, says, "There can be little doubt to any one who carefully examines the subject that nearly all M. Fourier's Series in this branch of the subject are erroneous." Thomson's paper was sent by his father to Professor Kelland, and, after being toned down somewhat, was published. His second paper, also dated April 1841, discusses the cooling of a heated sphere in space. In August of the same year he published an important paper, showing the equivalence of certain problems in heat and electricity. The paper was written in Arran a few months before he left with his father for Cambridge.

## CHAPTER II

### CAMBRIDGE

DURING last century the Cambridge School of Mathematics attracted students from all parts of the world. A good position in the Tripos affixed the hall-mark

to mathematical attainments. A Glasgow student—Archibald Smith of Jordanhill—was Senior Wrangler and first Smith's prizeman in 1836. It was not surprising, therefore, that Professor James Thomson encouraged his son William to go to Cambridge. He knew that William would do extremely well in his examinations, and that the possession of a brilliant Cambridge degree would be a great help in applying for a Scotch professorship. In particular, he had in view the Professorship of Natural Philosophy at Glasgow University, which in all probability would be vacant before long, as Professor Meikleham was in precarious health.

Before William Thomson went up to Cambridge, he was perfectly competent to understand, and even to criticise, the writings of the great mathematical physicists. The training he had received at Glasgow, however, was not exactly suited to qualify him to win the senior wranglership—the blue ribbon of the mathematical world. The sturdy, independent character of the training given in Scotch universities of that day, and the love they inculcated of knowledge for its own sake, made the mental training which he had to undergo during his undergraduate course, in order to accelerate his pace in solving problems, some of which, although extremely difficult, were of little, if any, practical value, very irksome to him. They can be well described in the words of Pope :

“ Tricks to shew the stretch of human brain,  
Mere curious pleasure, or ingenious pain.”

William Thomson entered St. Peter's College as a freshman in 1841, five years after Archibald Smith's Tripos. Amongst the undergraduates he soon obtained the reputation of being the future Senior Wrangler, and

several of the dons who had noticed his original papers in the *Cambridge Mathematical Journal* recognised the advent of a mathematical physicist of superior ability.

Thomson was popular with his fellow-undergraduates, and made many lifelong friends. One of these was Hugh Blackburn of Trinity, who was fifth wrangler in Thomson's year, and was afterwards his colleague as Professor of Mathematics at Glasgow for many years. Another great friend was G. G. Stokes of Pembroke, who was Senior Wrangler just before Thomson came into residence. He was very proud also of his acquaintance with Archibald Smith, who encouraged him to proceed with his original investigations.

In his second year Thomson read privately with William Hopkins of St. Peter's College, whose reputation as a mathematical coach was only equalled in after years by E. J. Routh of the same college. Hopkins took his Tripos when thirty years old, and his position of seventh wrangler represents most inadequately his ability. He was not a mere crammer who studies the idiosyncrasies of the examiners for the year and makes his students specialise in those subjects that will pay. He did not want them merely to limit their aspirations to mathematical honours, and strove to impart to them a disinterested love of their studies. Thomson must have been a pupil after Hopkins's own heart. During his undergraduate course he wrote no less than sixteen original papers, some of which are of great merit and importance. We can well imagine that Thomson spent little time over the book-work papers, engrossed as he was in problems of absorbing physical interest.

Thomson was careful always to keep his body in perfect physical condition by taking the necessary amount of exercise. Swimming and rowing were the athletic exercises which attracted him most. With

Hemming of Trinity, who was Senior Wrangler in 1844, and became afterwards a distinguished lawyer, he went shares in a "funny," and practised rowing assiduously. He became an excellent oarsman, and won the Colquhoun silver sculls—a prize open to all undergraduates. Distinction in athletics, however, was only a very secondary ambition.

He always enjoyed an out-of-door life. In a letter to his sister, he says that the early mornings at Cambridge remind him of the May mornings they used to enjoy in the Isle of Arran. In the summer months Thomson frequently went for a walk into the country round Cambridge with one of his friends. He sometimes bathed in a pool in the upper Cam, well known to undergraduates as Byron's Pool. It is in the middle of a cowslip-covered meadow, and water-lilies grow near the banks. It is a favourite spot for good swimmers who like to take a run on the meadow and then make a flying plunge into the pool.

In his last undergraduate year the shadow of the coming Tripos began to affect the pleasure of his existence. He knew that he had an excellent chance of being Senior Wrangler, but the chapter of accidents has always to be considered, especially in such a severe competitive examination. A bad headache or the expenditure of too much time over a wrong problem would upset his chances. Knowing his father's hopes, and desirous to do everything to please him, a very natural anxiety began to affect him. At the same time he was brimming with ideas—soon to give an enormous impetus to science, but which would be of little help to him during the examinations.

When the Tripos list was published, and Parkinson of St. John's was declared Senior, Thomson being second, he took his defeat philosophically. He sympathised



most keenly with the natural disappointment of his father, who had done so much for him, but he had too much common sense to attach much real importance to a slight difference in the number of marks between himself and his rival. At the Smith's prize examination their relative positions were reversed, Thomson being first. The papers set in this examination were admirably fair, and might still be set to mathematical physicists. One of the questions in Earnshaw's paper was to give a physical analogy between fluid motion, attraction of bodies, and temperature. This seems to have been suggested by one of Thomson's own papers which had only recently been published. Other questions about elastic solids and waves in canals must have directly appealed to Thomson. In after years he was fond of discussing some of these in his senior mathematical class.

The papers set about this period in the Tripos and Smith's prize examinations are exceptionally good. Later on, when the various branches of science got more specialised, the questions became much more lengthy, and the element of luck entered more largely into the competition. The later questions also are much less interesting. Compare, for instance, Whewell's question, "Does the attraction of the Moon affect the position of a plumb-line?" and the kindly hint attached that you are to take into account the motion of the tides, with the long questions sometimes set later in the century, the meaning of which is not always clear. The answer to Whewell's question is, that the plumb-line is affected owing to the variation in the height of the ocean, causing a small and variable declination of a plumb-line situated, for instance, on the shore. The complete discussion of the problem is by no means easy, but it gives plenty of scope to the student to show his ability.

Canon Wordsworth gives an amusing description of the origin of the word "Tripos." In the days long before written examinations, the senior bachelor had to sit upon a three-legged stool before the proctors. The three-legged stool was the only tripos at this period. Later on the bachelor was called the tripos, just as judges are sometimes called the "bench." Subsequently the name was given to tripos speeches, then to tripos verses, and finally to the tripos lists.

In Thomson's time there was a trio of most distinguished senior wranglers in consecutive years. In 1841 there was Sir George Gabriel Stokes, who was Thomson's immediate predecessor as President of the Royal Society. In 1842 Arthur Cayley, a mathematician of European eminence, was Senior. Thomson half humorously used to say that he often tried to get Cayley to study "useful" problems. In 1843 the Senior was J. C. Adams, the discoverer of the planet Uranus. Twenty years afterwards, Lord Rayleigh, who, like Stokes and Thomson, became a president of the Royal Society, was Senior.

There are many eminent men besides Thomson who have been second wrangler. Whewell, of encyclopædic genius, Sylvester, one of the greatest of mathematicians, and Clerk Maxwell, who wrote a marvellously clever treatise on electricity, were all second wranglers.

After his examinations were over, Hopkins presented Thomson with a copy of an essay, written by George Green, on the mathematical theory of electricity and magnetism, which was published by private subscription at Nottingham in 1828. Thomson learned from this essay that the very important theorem in attractions which he had published in 1842 had been anticipated by Green. It is extraordinary how few people

at Cambridge or elsewhere seem to have been aware of the existence of this essay, but the facts of Green's life partly explain it.

The father of George Green was a miller, possessed of private means, who lived at Sneinton, in Yorkshire. The son was an entirely self-educated mathematician. When thirty-five years old he published his essay, which proves that he was thoroughly familiar with the writings of the French mathematicians, and more especially with Poisson's work. In 1833 Murphy, a tutor of Caius College, published a small treatise on Electricity, in which he mentions Green as the originator of the term potential, but he gives no reference to his theorem. At the age of forty, Green, probably attracted by Murphy's reputation, entered Caius College, and graduated as fourth wrangler in 1835. He did not mix much with the students, and entered little into the life of the college. He was elected to a fellowship, and died two years afterwards. Thomson used his influence successfully to get the importance of Green's work recognised. His papers have been published by Caius College in a volume edited by Norman Macleod Ferrers, a Senior Wrangler, who himself took no inconsiderable part in advancing our electrical knowledge on the lines laid down by Green and Murphy.

In the early part of 1845 Thomson made the acquaintance of the great Michael Faraday, and visited his laboratory at the Royal Institution. In later years he was very proud of his acquaintance with Faraday, and used to show his class the piece of heavy glass by means of which Faraday had first shown the connection between light and electricity. This piece of glass had been presented to him by Faraday himself, and he treasured it as one of his choicest possessions. In the summer he went to Paris with his friend, Hugh Black-

burn, to study physics in the laboratory of the great Regnault, who was then engaged in making his classical determinations of the constants in the theory of heat. The devices he used to secure accuracy and eliminate sources of error were much appreciated by Thomson. Some ten years later, when he started a physical laboratory of his own, he found this Paris experience invaluable. He often gave reminiscences to the class of Regnault's skill. He related how sceptical he and Blackburn were at first when they saw Regnault freezing mercury in a red-hot crucible. In order to effect this, he utilised the spheroidal state of liquid ether and the low temperature caused by its rapid evaporation.

Dr. Meikleham, the Professor of Natural Philosophy at Glasgow University, died in May 1846, William Thomson being, to his father's great delight, unanimously appointed as his successor. A gloom, however, was cast over the Thomson family later in the year by the death of his younger brother, John, the resident assistant at the Glasgow Royal Infirmary. He had greatly distinguished himself at the medical classes at the University, but he had not the robust physique of William. He died of a fever contracted when in discharge of his duty at the hospital. In 1849, three years later, his father, Professor James Thomson, died of cholera. Epidemics of this disease were by no means infrequent at this period. Thomson's friend, Hugh Blackburn, was elected to succeed him in the mathematical chair.

## CHAPTER III

## PROFESSOR OF NATURAL PHILOSOPHY

ON the 3rd of November 1846 Professor William Thomson gave his first lecture to the natural philosophy class at Glasgow. He had spent a considerable time preparing it, and had written it out in full. Owing to nervousness, however, he read it much too quickly, and he felt that his first lecture had not been a success. He was considerably depressed in consequence. Very shortly afterwards, his enthusiasm for his subject made him forget the trammels which his preconceived notions about lecturing had put on his delivery, and he developed a more natural style, which suited his genius. Attendance at the natural philosophy lecture was compulsory for all students who desired to take an arts degree. The scientific knowledge of the bulk of the class was therefore very limited. As a professor, he was too apt to forget this, and address his class as if he were speaking before a learned society. The habit also of making digressions, which sometimes so interested and amused audiences at the London Royal Institution, was rather trying to those students whose ambitions were bounded by the degree examinations. But they were all very proud of him, and felt that it was a rare privilege to be one of his students.

He opened his class every morning by saying, with his eyes shut, the third collect from the morning service of the Church of England. He then began his lecture, the students taking notes and looking with interest at the apparatus, sometimes very elaborate, set out for demonstrational purposes. Being often deeply

absorbed in physical problems, it was sometimes hard for him during lecture to keep his mind from straying back to his own private difficulties. One felt that he was looking forward to renewing the attack on the problem when the lecture was over. In the senior class he was more open. Sometimes he would branch off during the lecture into the problem in which he was absorbed. After a hasty *résumé* of it to the class, he would write down the equations on the blackboard and proceed to study them. The class had the thrilling experience of watching a great scientist attack an unsolved problem in physics, and could see him try one mathematical method after another in his attempts to wrest the secret from Nature. Even those of the students who could not follow him looked on with the greatest interest. But physical discoveries are evolved very slowly, and when at the next lecture he told us the result, we could tell, judging by the progress he had made during class, that he must have expended many hours of hard thought on it in the interval. Sometimes he made an apparent discovery during lecture, but he would generally find out afterwards in his library that he had been anticipated by others. On these occasions he would always tell the result of his researches for the benefit of his class.

The writer remembers that on one occasion, when discussing the motion of gyrostats linked together, he discovered certain algebraical theorems in connection with determinants. He asked us all to verify them and try to expand them, and finished the lecture in the highest spirits. On the next occasion he gave a list of books in which he had found the theorem, and ended up by saying that it was even given in Todhunter's *Theory of Equations*!

Todhunter was an excellent mathematician, who was

Senior Wrangler three years after Thomson's year. He was the author of very numerous text-books, mainly on mathematical subjects, which at that time were almost universally used. Probably owing to their academic nature, Thomson had an antipathy to them.

The author remembers that, when he was asked in class to give the meaning of a symbolical expression written on the board, he said with much complacency that it was the limiting value of the ratio of the increment of  $x$  to the increment of  $t$  when the latter increment was indefinitely diminished. As a matter of fact he had learned this definition from Dr. Muir of the High School—a mathematician of European reputation—some years previously. His satisfaction, however, was short-lived. Thomson's comment was, "That's what Todhunter would say. Does nobody know that it represents a velocity?" The general definition savoured too much of "cut-and-dried" mathematics. He wanted a physical meaning for the expression.

Thomson was most enthusiastic about the convenience of the French metrical system. He rarely let an opportunity pass of running down what he called the British "no-system." The people of this country, he would say, have for their unit of mass, the grain, the scruple, the gunmaker's drachm, the apothecary's drachm, the ounce troy, the ounce avoirdupois, the pound troy, the pound avoirdupois, the stone (Imperial, Ayrshire, Lanarkshire, Dumbartonshire), the stone for hay, the stone for corn, the quarter (of a hundredweight), the quarter (of corn), the hundredweight, the ton, and several other units. This he contrasted with the beautiful French system. He considered it a remarkable phenomenon that the British people, who pride themselves on their common sense, should condemn themselves to so much unnecessary

hard labour. The strong prejudices of many engineers in favour of our system he put down as a strange phenomenon depending more on moral and social science than on physical.

Even in his introductory lectures Thomson soared to heights which made many of his class feel giddy and helpless. He would say, for example, that all motion is relative motion. We can calculate from astronomical data the direction in which and the velocity with which we are moving at any instant. We first compound the known velocity of rotation of the Earth round its axis with its motion round the Sun. This resultant motion having been accurately determined, we have then to compound it with the roughly known velocity of the Sun in space. But even if this were accurately known, it would not give us our absolute velocity in space. For it is only the Sun's relative motion among the stars that we can observe. In all human probability the Sun, Moon, and stars are moving with inconceivably great velocities relatively to other bodies in the universe. Having thus unsettled the ideas of his class and awakened their interest, he would point out how easy it is to get the relative motion, by the simple device of impressing upon all the moving bodies a velocity equal and opposite to the velocity of the one about which the relative motion is to be found.

Similarly, when he defined what a second of time is, many of his class realised for the first time how extremely difficult it is to give a rigorous definition. To say that it is a definite fraction of the period of the earth's rotation round its axis is only scientifically correct, provided that you give the date. Observations made on ancient eclipses, dating as far back as 720 B.C., make it highly probable that the period of the earth's rotation has lengthened by about the three-hundredth part of a



second. As a timekeeper, therefore, the earth is not ideally perfect. He stated that a carefully arranged metallic spring hermetically sealed in an exhausted glass vessel would be a more accurate measurer of time. Even in two thousand years tidal friction has quite an appreciable effect on the length of the day. If we were legislating for fifty million years ahead, we should also have to take into account the effects produced by the shrinking of the earth, due to its cooling.

In his lectures he made numerous references to Thomson and Tait's *Natural Philosophy*. He was thus enabled to avoid wearying the junior class by long mathematical proofs on the board. He frequently made use of terms employed in navigation and astronomy when explaining physical principles. References to parallax and aberration, azimuthal and precessional motion, right ascension, fore and aft, starboard and many other technical words and phrases made a large demand on the general knowledge of the class. A student once asked what he meant by the weather side of a ship. His reply that it was the side towards the wind made the student feel that he had asked an unintelligent question. To remove this impression, he explained how a ship carries a weather helm when it is necessary to hold the helm on the weather side of its middle position to keep the ship on its course. This suggested that it would be useful to point out to the class that the natural tendency of a body moving in a liquid is to turn its length across the direction of its motion. This explains why an elongated rifle bullet requires rapid rotation about its axis to keep its point foremost.

Towards the end of the session, owing to the very comprehensive programme that had to be got through, the pace had to be quickened. The last day was always an eventful one, the professor sometimes lecturing and

showing experiments to those of the class who could remain, long after the hour was up. The writer was one of those who remained to the end—a period of over four hours—in 1878. The whole of the theory of light had to be given in this time. Newton's spectrum was first explained and illustrated by coloured diagrams. Stokes's anticipation of Kirchoff's discovery of the method of spectrum analysis was then related. The phenomena of fluorescence and phosphorescence were explained, Stokes's theory being given, and Becquerel's recent experiments described. The practical application of phosphorescent material to clock faces was next shown. The students were invited to come down and see for themselves the coloured images formed by polarised light passing through glass under compression, quartz, &c. But even the most enthusiastic of us were beginning to get fagged before the professor gave any signs of concluding. The memory of that last lecture is a treasured possession.

Some years afterwards, the writer attended Stokes's lectures on the same subject. His calm, reflective style was a great contrast to Thomson's impetuosity, and the primitive apparatus he used, although admirably adapted for its purpose, was very different from the elaborate apparatus Thomson generally employed. Both had singularly winning smiles when lecturing, both were actuated by the same enthusiasm for science, and the relations of both to their students were marked with the most perfect old-world courtesy. Stokes was a scholar and a scientific man, but Thomson was, in addition, a man of affairs.

In the natural philosophy class a thorough knowledge of Kepler's laws and Newton's deductions from them was regarded as essential. Thomson used to mention several astronomical treatises, but advised students to

read Sir John Herschel's, not because it was the most accurate or contained the latest discoveries, but because of its literary charm. The first paragraph of the introduction especially excited his admiration.

“In entering upon any scientific pursuit, one of the student's first endeavours ought to be to prepare his mind for the reception of truth, by dismissing, or at least loosening his hold on, all such crude and hastily adopted notions respecting the objects and relations he is about to examine as may tend to embarrass or mislead him ; and to strengthen himself, by something of an effort and a resolve, for the unprejudiced admission of any conclusion which shall appear to be supported by careful observation and logical argument, even should it prove of a nature adverse to notions he had previously formed for himself, or taken up, without examination on the credit of others. Such an effort is, in fact, a commencement of that intellectual discipline which forms one of the most important ends of all science. It is the ‘euphrasy and rue’ with which we must ‘purge our sight’ before we can receive and contemplate as they are the lineaments of truth and nature.”

Sir John Herschel was another of the long list of distinguished senior wranglers. It is highly probable that Sir Isaac Newton was first in his degree examination, but no record of his place has survived. Thomson regarded a knowledge of Newton's *Principia* and Herschel's *Astronomy* as essential to a liberal education. It is interesting to remember that his grave in Westminster Abbey is very near the graves of the two men whose works he delighted to praise.

## CHAPTER IV

## EARLY ELECTRICAL RESEARCHES

PROFESSOR JAMES THOMSON, acting on the advice of his colleague, William Thomson, the Professor of Chemistry at Glasgow University, had pointed out to his son the importance of acquiring experimental skill so as to qualify himself better for a professorship of natural philosophy. It was, in fact, with this end in view that he went to Regnault's laboratory. During this time he was successful in solving some very important and fundamental problems in electricity.

As far back as 1834, W. Snow Harris had published a paper in the *Philosophical Transactions*, in which he made a careful and thorough study of the elementary laws of electricity. The results he obtained led him to cast doubts on the laws which Coulomb had deduced from his experiments, notwithstanding the strong mathematical evidence which Poisson had given in their favour. Snow Harris found, for instance, that in several cases the laws of attraction between electrified bodies did not follow the law of inverse squares—that is, that the magnitude of the attraction was not diminished to a quarter the value when the distance between the centres was doubled. In some cases he even found that the attraction between the electrified bodies changed to repulsion at a certain distance. At a particular distance he found that there was neither attraction nor repulsion between them, although both were electrified. As an experimentalist, Snow Harris's reputation was of the highest, and the experiments were all made with the minutest care. It required,

therefore, no little courage for William Thomson to deny the truth of his conclusions, and to suggest that he had omitted to take certain necessary precautions.

Snow Harris measured the electrical attraction between spheres by weighing them, and deduced an empirical law for this attraction. Thomson suggests that there must have been conducting bodies in the neighbourhood of the bodies the attraction between which was being weighed. In experiments of this nature many precautions are necessary. "None of these precautions, however, have been taken in the experiments described in Mr. Harris's memoir, and the results are accordingly unavailable for the accurate quantitative verification of any law, on account of the numerous unknown disturbing circumstances by which they are affected." This plain speaking shows how convinced Thomson was of the truth of the fundamental laws.

He gives in this paper, without proof, the theorem of the attraction between two equal electrified spheres when one of them is connected with the earth. Undeterred by the numerical labour involved, he computes the forces that Snow Harris ought to have observed. Instead of the attractions being 8·25, 4·6, and 3·5 grains weight respectively, he calculates that they should have been 7·94, 4·18, and 3·00 respectively. If Thomson's results are correct, therefore, the experiments must have been done in a very careless manner.

Sir W. Snow Harris, however, did not admit the accuracy of Thomson's criticisms, although his analytical skill filled him with admiration. He could not point out what was wrong, but he knew that his experiments had been made with the greatest care, and that he had tried the effect on the weight required to balance the attraction of bringing conductors in the neighbourhood of the electrified bodies. His opinions, therefore,

were unchanged. In his *Rudiments of Electricity*, published in 1848, he says, "The advance of modern researches certainly renders the views of electricity entertained by the French mathematicians somewhat questionable. It is not that they cast the least doubt on the intellectual ingenuity and profound thought of the great experimentalist upon which their particular theory is built, but only on the hypothetical evidence upon which some of the experiments are based."

In the author's opinion, Thomson was not justified in assuming that one of the spheres in Snow Harris's experiment was at the same electrical pressure as the earth. Making the much more probable assumption that the electric charges on the spheres were equal and opposite, and computing the attractions in Thomson's way, he finds that the calculated values agree within the limits of experimental error with the values observed by Snow Harris. It is curious that no one seems to have pointed this out before.

In 1849 Thomson sent a complete solution of the problem of the mutual attraction or repulsion between any two electrified spherical conductors to M. Liouville, and in 1853 the solution was published in the *Philosophical Magazine*. He employs an exceedingly ingenious method, which he called the method of electrical images. He says that it was suggested by Murphy's "principle of successive influences." It is analogous to the optical problem of calculating the illumination produced by a candle placed between two spherical mirrors. The illumination is not only due to the direct rays from the candle itself, but also to the rays proceeding apparently from the infinite number of the images of the candle in the two spheres. In an appendix to this paper he publishes tables, which facilitate the utilisation of his results in electrometers—that is in instruments for measuring

electrical pressures. The results obtained in this paper give a perfect vindication of Coulomb's theory, and are a marvellous illustration of Thomson's skill in bringing the most difficult physical problems within the domain of mathematical analysis.

In after years, Thomson made several successful and some unsuccessful attempts to utilise his formulæ for the attractions between electrified conductors. At the Crystal Palace Electrical Exhibition in 1890 he was busy perfecting an instrument of this type for measuring very high voltages. Unfortunately, at that period there was practically no demand for an instrument of this nature, and so he did not continue the experiments. Professor W. Buchanan, his assistant at that time, relates how Sir William Thomson frequently called to find out the progress that was being made, and invariably tested the experimental results obtained by the formulæ in his green-backed note-book.

Thomson's results give an absolute method of measuring electrical pressure in volts. We have simply to weigh the attraction between the conductors, measure their dimensions and their distance apart, and then Thomson's formulæ give us the voltage. It will be seen, therefore, that the volt is independent of all the other electrical units. Similarly, he showed that the ampere (the unit of current) could be found by weighing the attraction between two coils of wire in which the same current was flowing. If we now adjust a coil of wire so that when the current flowing through it is one ampere (the difference of pressure between its ends is one volt), we get a coil the resistance to the flow of current through which is the unit of resistance—that is, the ohm. In determining the ohm in this way, any error due to a faulty knowledge of the value of gravity at the place of observation cancels out. It will

be seen that if we alter either our unit of length (the centimetre) or our unit of time (the second), we must also alter the three electrical units.

In 1853 Thomson read a remarkable paper to the Glasgow Philosophical Society, on the "Oscillatory Discharge of a Leyden Jar." Six years previously, Helmholtz had discussed a puzzling phenomenon he had noticed when a knitting-needle was magnetised by the discharge current from a Leyden jar passing through a wire twisted round it. In some cases the needle was left magnetised with the north pole at one end, and sometimes with the north pole at the other. A possible explanation of these results was that the discharge was oscillatory. Thomson proved mathematically that this was the case, and obtained a formula by means of which the rapidity of the oscillations can be computed.

A Leyden jar consists of a glass bottle coated inside and outside with layers of tinfoil. When the inner and outer layers are each connected with a terminal of a frictional machine, charges of electricity are induced in the two coatings. These charges are equal in magnitude but have opposite signs. If the extremity of a wire connected with the outer coating be brought near a wire connected with the inner coating, when they are sufficiently close together a spark which makes a loud, snapping noise ensues, and the quantities of electricity in the two coatings are neutralised, a rush of electric current taking place in the wires.

Helmholtz showed that an electric current cannot rise instantaneously to a finite value. It requires time. When an electric current flows, there is energy stored up all round it. The energy due to the currents in the discharge wires of a Leyden jar has been obtained from the energy originally stored up by the charged coatings.



This transference of energy cannot be done instantaneously. When the coatings have lost all their electric energy, then, if the wires had no resistance and no energy was dissipated at the spark, the current in them would be a maximum, and the energy of this current would be exactly equal to the energy originally stored in the coatings. As the current diminishes, this energy is restored to the coatings, which are now charged in the reverse direction, and, when the current stops, the whole energy will have been stored up again in the jar. It will now discharge in the reverse direction, the cycle of operations being performed in the same order. In practice, however, energy is dissipated at the spark and in heating the wires, and so the oscillations only ensue in certain cases, and get feebler and feebler until there is not enough energy to start the current flowing across the gap.

The phenomenon is analogous to the motion of a pendulum swinging freely. If it be swinging in air, the oscillations will gradually get smaller and smaller until it stops. In this case the damping is said to be small. If it were swinging in water, the damping would be much larger, and if it were swinging in a heavy, viscous liquid like treacle, it would not oscillate at all, but, when displaced, would gradually move back to its middle position without passing it. The resistance of the electric circuit is analogous to the viscosity of the liquid. When the resistance is very small the damping of the oscillations is small, and when the resistance exceeds a certain value we do not get an oscillatory discharge at all.

Thomson suggested that an experimental verification might be obtained by means of Wheatstone's revolving mirror. Feddersen successfully did this in 1859. Recently the invention of the oscillograph has enabled the

discharge currents to be studied in detail, and Thomson's mathematical formulæ have been shown to be very accurately true.

Thomson also suggested that as a lightning-flash was the same as a Leyden jar discharge, but only on an enormously greater scale, it was highly probable that a lightning-flash is an oscillatory phenomenon. This would help to explain why lightning-flashes which apparently last for an appreciable time are sometimes seen.

The main practical importance of Thomson's paper, however, lies in an application which at that time was undreamt of—namely to radio-telegraphy. When the oscillations are very rapid, a large amount of the energy stored in the jar can be radiated into space. Hertz showed that these radiations can be detected by means of a device called a detector. When the rays fall on the detector, sparks ensue between two points of it. This detector can only be used for short distances. The discovery by Branly, in 1890, of another device called a coherer enabled the radiated waves to be detected to a much greater distance. The further discoveries by Lodge, Marconi, and others have made possible the everyday use of this system of signalling, which is now a serious rival to telegraphy, and may possibly even rival telephony in popularity. Thomson's beautiful theory which predicted the oscillatory discharge of a Leyden jar induced many physicists to study this phenomenon most carefully, and radio-telegraphy is an immediate outcome of their labours.

## CHAPTER V

INVESTIGATIONS INTO THE RELATIONS BETWEEN  
HEAT AND WORK

At one of the meetings of the British Association in 1847 Thomson heard Joule read a short abstract of a paper on the Mechanical Value of Heat. He was so impressed by this paper that, without waiting for an invitation, he rose and pointed out to the meeting the great interest of the new theory and its practical importance. This was the beginning of a lifelong friendship between the two great physicists, and for many years they jointly carried out experiments on the heat effects of fluid motion.

Thomson worked assiduously for many years to perfect the theory of heat engines. He perfected the theory of the ideal heat engine first enunciated by the great French engineer Carnot. This engine he looked on as a device which takes a quantity of heat from a source maintained at a constant high temperature, converts part of this heat into work, and then ejects the remainder of the heat into a condenser kept at a constant low temperature. In order that this engine be ideally perfect, Thomson saw that it must be reversible. That is, it must be able theoretically to take a quantity of heat from the condenser, and after the expenditure of a definite amount of work on the engine it must be able to eject the original heat, together with the heat representing the mechanical equivalent of the work expended into the source. He recognised that the amount of work that has to be done depends not only on the difference of temperature between the source and the

condenser, but also on the absolute values of these temperatures. For example, the efficiency of a perfect heat engine working between the temperatures of  $200^{\circ}\text{C}$ . and  $100^{\circ}\text{C}$ . is not the same as that of a perfect heat engine working between  $100^{\circ}\text{C}$ . and  $0^{\circ}\text{C}$ ., but is decidedly less.

He was thus led to invent his absolute scale of thermometry (measurement of temperature). The absolute temperature of the source is proportional to the quantity of heat taken from it, and the absolute temperature of the condenser is proportional to the quantity of heat ejected into it during a cycle. If these quantities are measured, the relative magnitudes of the absolute temperatures can be found, and hence also, by measuring the difference of temperature between the source and condenser, the zero from which they are reckoned. This scale of thermometry is quite independent of the physical properties of any physical substance, as, for instance, the way in which mercury expands when heated or the way in which the electric resistance of pure platinum varies with the temperature. The scale is defined in terms of the ideally perfect heat engine alone. It is known as Thomson's absolute thermodynamic scale.

Having found a true absolute scale to measure temperature, the next thing to be done was to find the relation between this theoretical scale and some easily constructed practical scale. A gas thermometer would be a suitable standard, provided that a gas which exactly obeys Boyle's<sup>1</sup> and Charles's<sup>2</sup> law can be found. Thomson and Joule, therefore, carried out many tests to find how far any given gas obeys the ideal conditions.

<sup>1</sup> When the volume of a given quantity of gas is doubled, its temperature remaining the same, the pressure is halved.

<sup>2</sup> The volume of a given quantity of gas increases by a definite fraction of its volume for each degree of rise in temperature.

The method of experimenting devised by Thomson was to force the gas in a steady current through a porous plug and observe very accurately the temperature of the gas on the two sides of the plug. In the case of most of the gases that he and Joule examined, a slight cooling effect of the gas was produced by forcing it through the plug. It follows theoretically that the absolute zero of temperature on Thomson's scale is slightly higher than would be found by thermometers formed by utilising the expansions of these gases to construct a scale. With carbonic acid gas the cooling effect was much greater than with air, nitrogen, or oxygen. This might have been anticipated, as Regnault had previously found that the ratio of increase in volume for a given rise in temperature is greater for carbonic acid gas than for the constituents of air. They also found that the higher the temperature of the gases on which they experimented the smaller was the cooling effect. Hence at high temperatures the dilatation of the gas was more accurately proportional to the temperature on Thomson's scale than at low temperatures.

With hydrogen, however, they found that a contrary effect was produced. When this gas was used, a slight heating effect was produced by forcing it through the plug. The experimental investigation of the reasons for these small differences of temperature proved to be very difficult and laborious. The experiments were carried out in Manchester. They were cut short by the action of the owners of adjacent property, who threatened Joule with legal proceedings, owing to the vibration and noise caused by the experiments.

The results of these experiments were communicated in a series of papers to the Royal Society. They proved that the temperature of melting ice was  $273.7^{\circ}$  on Thom-

son's scale, and the temperature of boiling water was  $373.7^{\circ}$ . The experiments show that when a gas expands at constant temperature it absorbs an amount of heat, the mechanical equivalent of which is very nearly the same as the work done during the expansion. In 1842 Mayer of Heilbronn assumed as a self-evident proposition that the work done was the exact mechanical equivalent of the heat absorbed, and hence calculated the mechanical equivalent of heat. If his data had been accurate, this would have given a good approximation to the true value, but the assumption he made could only be justified by experiment. Joule and Thomson's experiments prove that, to a first rough approximation, the assumption is justified. They also determine approximately its limitations.

Many interesting incidental phenomena are discussed in these papers, as, for example, the effect of fluid friction in drying steam issuing from a high-pressure boiler. Clausius and Rankine (Thomson's colleague at Glasgow) had independently made the discovery that when steam is allowed to expand heat must be added to it if it is to remain dry steam. It is difficult to reconcile this with the fact that when steam escapes from a high-pressure boiler into the open air through a small aperture it remains dry. Thomson explains this by taking into account the heat developed by the fluid friction of the steam rushing through the aperture. The heat communicated to the escaping steam thus keeps it dry. Hence high-pressure steam sometimes produces a much smaller scalding effect than low-pressure steam.

Another question discussed was: Does a mercury thermometer placed in a strong draught of air read the true temperature of the air? The authors found that it will read a little too high. The explanation is that the retardation of the air flowing past the bulb makes it

lose part of the energy due to its motion. This lost energy is converted into heat, and some of it goes to raise the thermometer reading. If the thermometer be sheltered from a gale by being placed near the top of a wall which is at right angles to the direction of the wind, and if the air round it is at rest, then the thermometer will indicate a higher temperature than when placed in the blast. The explanation is that the heating of the air by friction near the top of the wall is on a large scale and affects the thermometer appreciably.

Thomson accepted the theory of the molecular structure of bodies. However homogeneous a substance appears to be, if a portion of it were magnified sufficiently, it would be seen to consist of molecules, and thus it cannot be really homogeneous. At ordinary temperatures, also, it would be seen that the molecules are in motion. The heat in the body is the energy of the motion of its molecules. At the absolute zero of temperature these molecules would be at rest, and the heat contained in the body would be zero. Looked at from this point of view, we see at once why there must be an absolute zero of temperature, and also why the properties of bodies change as we cool them down. For instance, we have now very strong experimental evidence for saying that at the zero of temperature the electrical resistance of a wire would be absolutely zero. Hence millions of horse-power could be transmitted from any part of the world to any other, or even from one planet to another, by an infinitely thin wire, provided its temperature could be maintained absolutely zero.

To enable his students to picture the molecular constitution of solid, liquid, and gaseous bodies respectively, Thomson used to give the following illustration. Imagine a harbour full of small boats, so tightly wedged together that they all kept the same relative places. The waves

would cause the small boats to rub together, and the bigger the waves the more violent this action. In this case the boats would represent the molecules of a solid. At the zero of temperature they would be at rest. At ordinary temperatures every molecule would be vibrating. If the boats were loosely packed together, so that they could drift relatively to one another, but yet always be in contact with many of their neighbours, this would represent the molecules in a liquid. Any given boat might drift from one part of the harbour to another, but it would always be in contact with other boats. Finally, if the number of boats was so few that they drifted about by themselves over considerable distances before they came into collision and bounded off from another boat, they would represent the molecules of a gas.

In a gas, therefore, the path of a molecule would consist of broken straight lines; in a liquid it would be a curve, and in a solid it would merely be a vibration to and fro about a fixed point.

In 1852 Thomson read a paper to the Royal Society of Edinburgh "On a Universal Tendency of Nature to Dissipation of Energy." In this paper he enunciates a very general scientific theorem. It will be easily understood by considering a special case. Let us consider the heat energy of an isolated system. The Conservation of Energy tells us that the total energy must remain constant. The availability of this energy to the inhabitants, however, depends on the heat engines they use. All these engines work by taking in heat at a high temperature and rejecting it at a lower temperature. This rejected heat is of no value to the inhabitants. If, initially, parts of the system are at a high temperature and other parts are at a low temperature, the temperature of the whole is continually being equalised by the conductive flow of heat taking place



in unequally heated solid bodies. In addition, radiation and convective currents of heat in gases help to make the temperature uniform. Hence, when these processes which we see taking place around us—both cooling hot bodies and heating cold bodies—cease, the heat energy will be unavailable to the inhabitants. In the solar system we see this great law of the dissipation of energy always at work, and hence that portion of the energy of Nature which man can utilise is continually getting less and less.

He was careful not to apply his physical generalisations to biology. In his opinion, the real phenomena of life infinitely transcend human science. He gave in 1875 the following illustration of the absurdities to which we would be led if we blindly apply physical laws without considering their limitations. It is a well-known law in dynamics that if at any instant the direction of motion of every molecule of a body were reversed, but the magnitude of the velocity kept exactly the same, the body would move along the path it had come, and at every point of its backward path its speed would be exactly the same as when it passed through that point in the forward direction. It is conceivable therefore, on the materialistic hypothesis, that if at any instant the motion of every particle of matter in the universe were reversed, the course of Nature from that instant would be reversed for ever after. "The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water; the thermal motions would reconcentrate their energy, and throw the mass up the fall in drops, reforming into a close column of ascending water. Heat which had been generated by the friction of solids, and dissipated by conduction and radiation with absorption, would come again to the place of contact, and throw the moving body back against the force

to which it had previously yielded. Boulders would recover from the mud the materials required to rebuild them into their previous jagged forms, and would become reunited to the mountain peak from which they had formerly broken away."

If this materialistic doctrine could be applied to life, living creatures would grow backwards with conscious knowledge of the future, but no knowledge of the past. But this is clearly incredible, and physical speculations of this nature are utterly unprofitable. But it is far otherwise with regard to speculations as to what would happen when the velocity of every molecule of a material system is reversed. In this case we get the full explanation of the theory of the dissipation of energy.

In 1884 Thomson read an interesting paper "On the Efficiency of Clothing for Maintaining Temperature." He showed experiments which prove conclusively that, if bodies are below a certain size, the effect of putting a covering round them is sometimes to cool them. For instance, if we have a bare wire, parts of which are surrounded by transparent substances like glass and mica, and send an electric current through it, if the current be great enough the bare parts of the wire become incandescent, but the parts covered by the transparent substances are quite dark. This proves that the covered portions are the cooler. The reason of this is that the convection currents of air streaming round the conductor carry away more heat from the covered than from the uncovered portions of the wire, owing to their greater diameter. Hence the heat generated in the covered portion of the wire is carried away more quickly, and its temperature is therefore lower. This theorem has important practical applications in connection with the lagging of steam pipes and with electric power transmission along covered wires.

In 1902 Kelvin considered the problem of what the nature of the mechanism is that maintains the human body at about  $98.4^{\circ}$  F. A thermostat is a device which automatically maintains a space at a uniform temperature. The probability is that the mechanism of the human thermostat lies in the small blood-vessels in which the combination of oxygen with the body tissues takes place. It is not easy to see how the instrument acts when the surrounding temperature is above  $98.4$ , and the air is saturated with moisture so that the perspiration cannot evaporate. It seems as if the surplus heat must be carried away by the breath.

Kelvin subsequently came across a paper by Dr. Crawford, published in the *Philosophical Transactions* for 1781, and entitled "Experiments on the Power that Animals, when placed in Certain Circumstances, possess of Producing Cold." By experiments on a living and dead frog, both of which were placed in hot flannel at  $106^{\circ}$  F., he showed that after five minutes the temperature of the living frog was only 78, whilst that of the dead one was 81. The action of the vital power of the living frog was to generate cold. Kelvin suggests that possibly in these circumstances there may be a surplus of oxygen in the breath. More oxygen may be breathed out than taken in. If this be found to be the case, animal cold would be explained by the deoxidation (unburning) of matter within the body. These experiments are very difficult to carry out, but there can be no doubt that, if successful, the results obtained would be of great importance.

## CHAPTER VI

## SUBMARINE TELEGRAPHY AND NAVIGATION

IN the ten years between 1840 and 1850 great improvements were made in systems of land telegraphy. During this period the mystery of the electric method of signalling greatly stirred the popular imagination. Every leading newspaper had a column headed "By Electric Telegraph." People were beginning to realise that the close connection between science and engineering might lead at any moment to still greater marvels. In particular, the possibility of connecting the New World with the Old by means of an electric cable was warmly discussed, but its feasibility as a commercial venture was denied by many capable engineers.

The first submarine telegraph was laid between Dover and Calais by a steam tug in August 1850. It consisted merely of a copper wire insulated by gutta percha. No protective sheathing or armouring of any kind was used. It was not surprising, therefore, that the anchor of a fishing smack cut it in two a few hours after it was laid. During these few hours electricians had been busy signalling through it. They had noticed that the signals received were extraordinarily sluggish in their action. They were quite different from the clear and sharp signals received on land lines. This was explained by noticing that the line must act like the inner coating of a Leyden jar and store electricity along its length. In 1851 a cable was laid between Dover and Calais, and in 1853 the British and Irish Magnetic Telegraph Company laid a cable between Port Patrick and Donaghadee. These cables were practically success-

ful, and were the forerunners of the ambitious schemes for laying a cable across the Atlantic, in which Professor William Thomson played such a notable part.

In May 1855, when still a young man of thirty, he published the solution of the problem of the transmission of telegraph signals along a cable, and established what is known as the inverse square law. For slow-speed signalling his solutions still apply, but for rapid signalling his solution has to be modified, so as to take into account the electric inertia of the current.

Thomson's conclusions were questioned by Mr. Whitehouse, who was interested in a project for an Atlantic cable. The controversy with him probably led Thomson to take a keener interest in submarine telegraphy than he would otherwise have done. In 1856 the Atlantic Telegraph Company was formed, and Thomson was appointed a director. In a letter to Helmholtz in December 1856, he says that he is sanguine about the success of the project. He points out that, nearly all the way across, the bed of the Atlantic consists of fine sand and microscopic shells. Its depth nowhere exceeds three and a half miles. Unlike the Mediterranean, there are no precipitous mountains or deep ravines along the sea bottom. He also says that "The practical men engaged have all the experience of previous failures." Unfortunately, they had to experience several more failures before success crowned their efforts in 1866.

In 1857 the British battleship *Agamemnon* and the United States frigate *Niagara* started to lay the cable. After 380 miles of the cable had been laid by the *Niagara*, the cable snapped, owing to the inexperience of the man in charge of the brakes. Thomson, who was on board the *Agamemnon*, came back more enthusiastic

than ever and full of ideas for improving the engineering methods for laying cables. In the spring of 1858 he perfected a very sensitive instrument for detecting currents. The instrument is known as the mirror galvanometer. A minute mirror, little bigger than a sixpence, is suspended by a silk fibre, so that it hangs with its plane vertical. Two or three pieces of magnetised watch-spring cemented to its back make its plane point to the Magnetic North and South, and at the same time cause the plane to deflect when a current flows in a coil of wire surrounding the mirror. The deflection is observed by means of a ray of light which, after reflection from the mirror, falls on a graduated scale. If this scale be some distance away from the mirror, it is easily seen that a very minute deflection of the mirror produces quite a large deflection of the spot illuminated by the beam of light falling on the scale.

The same two ships started on another cable-laying expedition in 1858. After experiencing very severe storms and overcoming many practical difficulties, the first Atlantic cable was laid. It connected Ireland with Newfoundland. Mr. Whitehouse, who was in charge of the Irish end of the cable, attempted for some days to use his own signalling apparatus, but with no success. It is highly probable that the high electric pressures which he had to employ with his method of signalling weakened the insulation of the cable. He had finally to fall back on Thomson's mirror galvanometer. It was this instrument that prevented the cable from being an absolute failure. The directors were dissatisfied with Whitehouse's management, and directed Thomson to take full control. Thomson's tests proved that the cable was in a very precarious state, and after a few weeks' troublous existence it broke down completely, and became utterly useless.

During its short life 732 messages had been sent, some of which were of great importance. For instance, by its means the orders for two regiments of English soldiers to leave Canada in order to help to quell the Indian Mutiny were countermanded. This is estimated to have saved the country at least £50,000. It will be seen, therefore, that the results attained by this cable were not merely of theoretical importance.

The experience gained was of the greatest value. No further attempt was made until 1865, when the *Great Eastern* was chartered to lay the cable. In the intervening years Thomson was indefatigable, both in improving theory and methods and in encouraging the shareholders to make a fresh attempt. "What has been done will be done again. The loss of a position gained is an event unknown in the history of man's struggle with inanimate Nature."

The 1865 attempt was a failure. After laying 1200 miles, the cable snapped. Its great weight baffled all the attempts made to grapple it, and so the expedition was unsuccessful. The engineers returned, however, full of hope for the future. In the summer of 1866 not only was a new cable laid, but the old one was recovered and completed.

To Thomson belongs the credit of having done the most to perfect the electrical part of the enterprise. Along with Mr. Canning, the engineer of the company, and Captain Anderson, of the *Great Eastern*, he received the honour of knighthood. Four years later the mirror galvanometer was replaced by the syphon recorder, an instrument which draws a curve on a strip of moving paper when messages are being sent. To this day the recorder, with many improvements made by Thomson himself, remains the standard instrument for submarine telegraphic work.

When the cables were first laid, the speed of working was about eight words per minute. Subsequently the speed was doubled by using condensers at each end of the cable. At the start the tariff was £20 for twenty words and £1 for each additional word. It is now only a shilling a word, and in the North Atlantic alone there are sixteen cables. It was not until 1869 that Thomson got any profits from the Atlantic Cable Company. The first use he made of them was to found scholarships at Glasgow University in experimental physics.

The experience gained when laying the Atlantic cable and his association with engineers affected the tenor of his life. He entered into partnership with Cromwell Varley, a very able electrician, and Fleeming Jenkin, the professor of engineering at Edinburgh University. Varley perfected the method of using condensers at the end of long cables, and Thomson and Jenkin perfected the "curb" method of signalling. In this method each signal current is followed up by a reversed current of shorter duration. This both accelerates the rate at which signals can be sent and makes them sharper.

Thomson made many important contributions to the theory of navigation. The theory of compass deviations was very thoroughly worked out by his friend, Archibald Smith, and the results were incorporated in the *Admiralty Manual of Deviations of the Compass*. A study of Smith's work led up to the invention of an improved compass. The standard ship's compass of 1873 had many defects. When the ship rolled, it was liable to swing through a large angle. Thomson was the first to see that, in order to neutralise the effects of the ship's magnetism, the needle must be short. At the same time the horizontal free swing must be very slow, otherwise it will be unsteady. To prevent sticking also, the friction must be very small. To overcome these difficulties,



he made the moving part of the compass of an aluminium rim, having radial silk threads. Pieces of magnetised knitting-needle were attached to the threads to act as magnets. The whole moving part only weighed 180 grains, but its period of oscillation was longer, and the friction error smaller, than that of the compass in use at that period. The disturbing effect of the ship's permanent magnets was overcome by using three sets of correcting magnets. Two of these sets act horizontally, and one vertically.

At the time of Lord Kelvin's death his compass was practically universally used. In very large battleships, however, owing to the large masses of magnetic material in the immediate neighbourhood of the Kelvin compasses, the difficulties of adjusting them are very considerable, and their liability to get out of order is largely increased. Recently attempts have been made to utilise the principle of the gyroscope, which was first experimentally verified by Foucault in 1852. He proved that a gyroscope which was free to move in two planes only will tend to set itself with its axis parallel to the axis of the earth. Its axis will therefore point to the true geographical north. The instrument, also, will be quite unaffected by the magnetic masses of iron on board ship. This is the principle of the Anschütz gyro compass, which is largely used in the German navy.

As far back as 1884 Thomson had attempted to realise in practice a gyro compass. Not only did he make a gyrostatic model of a magnetic compass which pointed to the geographical north pole, but he made also a gyrostatic model of a dipping needle and a gyrostatic balance for measuring the vertical component of the earth's rotation. The models were not very satisfactory, and so he devoted himself to improving his magnetic compass. In view of recent developments, it is inter-

esting to recall how Thomson had partially explored this field. In the Anschütz compass the wheel makes 20,000 revolutions per minute, and its rotation is maintained electrically.

Thomson's experience in cable-laying probably led him to invent his navigational sounding-machine. The method in use at that time was very laborious. A rope an inch and a half in diameter, with a heavy sinker attached, was employed. In deep water the ship had to be stopped while the line ran out, and while it was being dragged in. Much time was lost, and a large number of sailors had to be employed. Thomson saw that most of the difficulties would be overcome by using steel piano wire, which he knew to have very great tensile strength. He demonstrated in the Bay of Biscay, in 1872, that it was possible to take a sounding in 2700 fathoms with a 30-pound sinker attached to a steel wire of No. 22 gauge.

When the ship is moving, the wire is no longer vertical, and its length, therefore, is not a measure of the depth. If the speed of the ship is known, it is easy to apply the necessary correction. Thomson invented various kinds of gauge, to enable accurate flying soundings to be taken when the speed of the ship is not known. In one of these the sinker contains a long, narrow glass tube, closed at the top. The tube inside is coated with chromate of silver. The deeper it sinks, the further the sea water is forced into the tube. The distance it has been forced up the tube can be seen by the discoloration of the coating, and the depth is read by placing the tube against a suitably graduated scale. In the Kelvin sounding-machine, now extensively used, the length of the cable, which is made up of seven fine steel wires so as to secure great flexibility, is 300 fathoms. As the cable slips out, a sailor presses a light stick against it, and he feels at

once the sudden change in the tension when the sinker hits the bottom. The cable is then wound up by an electric motor. It is customary to use two sounding-machines, which are kept in constant operation near the shore. They are also kept constantly going in foggy weather when the depth is less than a hundred fathoms. The use of these machines not only prevents shipwrecks, but it also enables ships to sail at much higher speeds in certain circumstances than would otherwise be safe. They are therefore a great boon to navigators.

Thomson considerably improved a method of determining the latitude and longitude of a ship at sea which was devised by Captain Sumner, an American navigator. If you measure the angular height above the horizon at any instant of the Sun, the Moon, or a star, then knowing Greenwich time by the chronometer, you can find at what point on the earth's surface this particular astronomical body was vertically overhead at the instant under consideration. In this way it is easy to determine a particular circle on the earth's surface on the circumference of which the ship must lie. A second observation, taken later, determines another circle on which the ship is now lying. These two circles intersect in two points, one of which is near the true position of the ship. In general, there is no doubt as to which of the two points is the correct one, and hence, by calculation, we can determine the true position of the ship. In order to simplify the calculations, Thomson published *Tables for Facilitating Sumner's Method at Sea*. The particular method Thomson advocated has not come into general use, but it was a valuable contribution to the theory of nautical astronomy.

When this method was first published, Sir George Airy, who was then Astronomer Royal, wrote a letter

to *Nature* criticising it. The letter was founded on a misapprehension as to what the method really was. J. A. Ewing (now Sir Alfred Ewing), who had helped to calculate the tables, was indignant, as the criticism was not valid and would do harm. He therefore telegraphed to Thomson asking his permission to answer it. Thomson promptly telegraphed back, "Yes, by all means answer in your own name, but don't hit too hard. Remember he is four times as old as you."

Another invention of Thomson's which might be mentioned in this connection is his tide-predicting machine. By means of the data given by a self-recording tide-gauge, the law governing the tides for a particular port is found out by mathematical analysis. The machine can then be applied to predict all the future tides. The machine is still in daily use at the National Physical Laboratory for predicting tides at various ports.

## CHAPTER VII

### WAVES AND VORTICES

THOMSON made a lifelong study of the motion of fluids. When taking a holiday on the *Lalla Rookh*, he was often busy studying the motion of ripples and waves and finding the laws which govern their motion. In a letter written in 1871 to Froude (the great naval expert, who was at that time studying the motion of ship models in an experimental tank), he describes how he was led to study the action of capillarity in modifying the motion of waves. He relates how once on a calm day off Oban, when the yacht was drifting at about half a mile an hour, he studied the ripples formed by a fishing-line, with a lead sinker attached, hanging over the stern. The

line was preceded by a very fine and numerous set of short waves. Streaming off at a definite angle on each side were the well-known oblique waves, with the larger waves following behind the line. The whole formed a beautiful and symmetrical pattern, the key to which he was fortunate enough to find.

He noticed that although the waves in front and in the rear had different wave-lengths, yet they had the same velocity. As the speed of the yacht slowed down, the waves behind got shorter, and those in front got longer. The speed diminishing still further, one set of waves shorten, and the other lengthen, until they become of the same length, and the angle between the oblique lines of waves opens out until it becomes nearly two right angles. At very slow speeds the pattern disappears altogether. Thomson found that these results were in exact agreement with, and could consequently have been predicted from, his mathematical equations. He calculated that the minimum velocity of the ripples was 23 centimetres (9 inches) per second.

About three weeks later, when becalmed in the Sound of Mull, he, together with his brother James and Helmholtz, actually measured this velocity, and so verified his theory. Thomson suggested that disturbances the wave-length of which was less than 1.7 centimetres (67-hundredths of an inch) should be called ripples. The name waves should be confined to disturbances having wave-lengths greater than this. Adopting this suggestion, we may say that ripples are undulations in which the shorter the length from crest to crest the greater is the velocity of propagation. For waves, on the other hand, the greater this length the greater is the velocity of propagation. The motive force for ripples is mainly the capillary attraction (cohesion), but for waves the motive force is their weight. Capillary attraction is

the motive force which makes a dewdrop vibrate. The effects of capillarity on the velocity of propagation of waves can be neglected when the wave-length is greater than two inches.

The introduction of cohesion into the theory of waves enables us to explain the pattern of standing ripples seen on the surface of water in a finger-glass, made to sound by rubbing a moist finger on the lip. If gravity were the only force, the wave-length for 256 vibrations per second would be one-thousandth of an inch, which could only be seen distinctly with a microscope. Taking cohesion into account, we find that for waves of the same frequency the wave-length would be nearly eighty times as great, which accords much better with ordinary experience.

When the sea is perfectly calm, a slight motion of the air—not exceeding half a mile an hour, or 8·8 inches per second—does not sensibly disturb the smoothness of the reflecting surface. A gentle zephyr destroys the perfection of the reflecting surface for a moment, but when it departs the sea is left as polished as before. When the motion of the air is about one mile per hour, minute corrugations are formed on its surface, the effects produced being like those produced by corrugated glass. The fly-fisher well knows that they help to conceal him from the trout. These ripples cannot propagate themselves, and parts of the surface sheltered from the wind still remain smooth. When the wind attains a velocity of two miles an hour, distinct small waves are formed. A few ripples may, however, still be noticed sheltered in the hollows between the waves. The vertical distance between the crest and hollow of these waves is about an inch. If the wind increase, they become cusped, and rapidly increase in size.

A peculiar phenomenon connected with canal navi-

gation furnished Thomson with a problem after his own heart. In the days when there was a large passenger traffic on the Glasgow and Ardrossan Canal it was discovered that, above a certain critical speed, the resistance the boat offered to motion through the water greatly diminished. The discovery was made accidentally. A spirited horse was once dragging a boat containing one of the proprietors along the canal. Taking fright, it suddenly started off at a gallop. The proprietor noticed that at this speed the foaming stern surge which did such damage to the banks of the canal ceased. The vessel seemed to rest on the summit of a progressive wave. The commercial value of this discovery was apparent. Boats about 60 feet long and 6 feet wide were constructed of thin sheet iron. The two horses which dragged the boat started slowly, and then, at a given signal, they jerked it on to the top of the wave, and set off at a rate of about ten miles an hour. The method was also used on the Forth and Clyde Canal, connecting Glasgow with Edinburgh. The theory given by Thomson is complete and satisfactory. At speeds greater than the critical speed no foaming surge devastating the banks is formed, and therefore the resistance to the motion is very much reduced.

The motion of vortices in liquids was a problem which keenly interested Thomson. In *Crelle's Journal* for 1858 appeared a classical memoir on the subject by Helmholtz. He states with admirable clearness the main laws which govern their motion. Thomson took up the investigation where Helmholtz left it, and developed it much further, with the object of making a complete mechanical theory of the æther based on vortical atoms.

The motion of a whirlpool or a whirlwind is an example of a simple vortical column with two ends. A smoke ring is an example of a circular vortex closed on itself.

If we draw a semicircular plate rapidly through the water for a short distance, we get a semicircular vortex with its ends on the surface. Professor Tait of Edinburgh, who collaborated with Thomson in writing the great treatise to be described in the next chapter, invented a method by means of which large smoke rings, containing a considerable amount of energy, could be produced with ease and certainty. Thomson used this method to illustrate to his class the properties of vortex rings.

When two smoke rings hit one another, they rebound, shaking violently from the effects of the shock, just as if they were made of rubber. A very curious phenomenon is observed when two coaxial vortex rings are moving in the same direction. The leading vortex ring dilates and moves more slowly, but the lagging vortex contracts and moves more rapidly. The two rings apparently attract one another. Hence the lagging ring overtakes, and passes through, the first. The same actions take place again, their rôles being now reversed. These actions can easily be observed with tobacco-smoke rings or with the half-vortex rings made by drawing a semicircular blade rapidly through the water.

When a vortex ring approaches a wall it expands. At the same time its translational velocity slows down, but the rotational velocity of the molecules composing it increases. Thomson explains this by his method of images. We imagine the wall removed and an equal vortex, the image of the first, to be approaching it. The mutual action between them is repulsive; therefore their speed slows down, and it can be shown, from dynamical principle, that they must expand.

If viscosity—that is, fluid friction—could be neglected, a vortex, once started, would exist for ever. This at once suggested to Thomson that the only true atoms of which the universe is made are vortex rings. He considered



that the hard, impenetrable, spherical atom of Democritus and Lucretius was a highly improbable assumption. In years past it was, perhaps, a necessary assumption in order to explain the unalterable distinguishing qualities of different kinds of matter. Now that Helmholtz had proved that the strength of a vortex filament moving in a perfect fluid remains constant, the Lucretian hypothesis can be abandoned. Vortex rings in a perfect fluid would exist for ever. To generate them can only be the action of a creative power.

The results obtained by spectrum analysis prove that the ultimate constituents of simple bodies must be capable of vibrating in one or more different ways. In this respect they must be like a stringed instrument having one or more strings, or a solid consisting of one or more tuning-forks rigidly connected. Thomson pointed out that, on the Lucretian hypothesis, we must suppose that the molecule of sodium, for instance, should consist of a group of atoms with void spaces between them, as a single, infinitely hard, spherical atom could not have one, and most certainly could not have two free periods of vibration. It is difficult to conceive, therefore, that a sodium molecule consisting of several independent spheres could be stable and durable. Hence it loses the one recommendation which inclined philosophers to accept it provisionally.

Thomson proved that the vortex atom has perfectly definite periods of vibration, which depend solely on the motion which constitutes it. He thought it probable that the atoms of substances did not consist merely of simple vortex rings, but consisted of two or more vortex rings linked together like the links of a chain. It is easy to see that a vapour consisting of such atoms would be probably capable of satisfying the very exacting spectrum test.

The theory is a great advance on Newton's corpuscular theory, as it is capable of explaining very abstruse phenomena. Even during Newton's lifetime the many arbitrary assumptions he had to make about his corpuscles in order to explain various phenomena seriously discounted the value of his theory. Thomson at first inclined to adopt the elastic solid view of the æther, but this he abandoned when he saw that the necessary rigidity could be obtained more simply by imagining an æther formed of vortex rings. Modern scientific men are inclined to believe that matter itself is but an æthereal manifestation. Larmor, in the following passage, states this clearly, and points out some of the difficulties in connection with the vortex atom theory. "The fluid vortex atom faithfully represents in many ways the permanence and mobility of the sub-atoms of matter; but it entirely fails to include an electric charge as part of their constitution. According to any æther theory static electric attraction must be conveyed by elastic action across the æther, and an electric field must be a field of strain, which implies elastic quality in the æther instead of complete fluidity: the sub-atom with its attendant electric charge must therefore be, in whole or in part, a nucleus of intrinsic strain in the æther, a place in which the continuity of the æther has been broken and cemented together again (to use a crude but effective image) without accurately fitting the parts, so that there is a residual strain all round the place."

In later life, Kelvin made several interesting speculations about electrons. He suggested, for instance, that a positive electron is an atom which, by attraction, condenses æther into the space occupied by its volume. Similarly, a negative electron rarefies, by repulsion, the æther remaining in the space occupied by its volume.

The stress produced in the æther outside two such atoms by the attractions or repulsions which they exert on the æther within them would cause apparent attraction between a positive and a negative electron and apparent repulsion between two electrons, both positive or both negative. It fails, however, to explain the Newtonian law of the inverse square. No known or hitherto imagined properties of elastic matter can explain this by stress of the æther. By making a certain assumption as to the law according to which portions of the æther act on one another, Kelvin showed that the phenomena of electrical attraction and repulsion might be explained. The great merit of this theory is that it does not necessitate an æther transmitting both electric and magnetic force. This assumption raises a difficulty in the ordinary theory which Kelvin regarded as insuperable.

Making Kelvin's assumptions the main outstanding difficulty is how the enormous attractive force between magnetic poles can be transmitted by the æther. Assuming that the density of æther is only the thousandth-millionth part of the density of water and that the velocity of light is 186,000 miles per second, Kelvin calculated that the rigidity (being density multiplied by the square of the velocity) of the æther was greater than that of steel, and so it would be quite able to transmit magnetic force.

Kelvin appreciated more fully than other men the importance of discovering the mechanisms by means of which electric and magnetic force are transmitted through the æther. When the nature of these mechanisms are discovered there will probably be a gigantic advance made in the practical applications of electricity. The discovery of radio-activity in Kelvin's later years opened up many new and unfamiliar aspects of the problem, and made necessary many modifications of

previous hypotheses. It soon became obvious that extended experimental investigations were desirable before any definite theory was formulated. Kelvin contented himself with pointing out the dangers of deducing general laws from a few obscure phenomena and showing how some of the older theories could be modified to explain the new facts.

## CHAPTER VIII

### THOMSON AND TAIT'S "NATURAL PHILOSOPHY"

P. G. TAIT, of Edinburgh, was Senior Wrangler seven years after Thomson took his degree. Like Thomson, he had gone to St. Peter's College and coached with Hopkins. In 1860, therefore, when he was elected to the Chair of Natural Philosophy at Edinburgh University, there were many points of similarity between his career and Thomson's. Tait was the first to plan the great literary undertaking of writing a complete treatise on Natural Philosophy. He was surprised and delighted when Thomson suggested that they should write the treatise jointly. They started with the intention of discussing in succession the various branches of Natural Philosophy, both experimentally and mathematically.

In 1867 the Clarendon Press published the first volume of the series. It was mainly introductory, and gave a very complete account of the Science of Force (Dynamics) and its action in maintaining rest or producing motion. The treatment of the subject is founded on that given by Newton in his *Principia*, and the book marks the beginning of a new epoch in dynamical science. The high standard they set themselves made it necessary to solve many difficult and abstruse

problems. Owing to the large encroachments this made on their time, they soon saw that it would be impossible for them to complete their great undertaking.

In 1872 Thomson published his *Reprint of Papers on Electricity and Magnetism*. In 1879 the Cambridge University Press published the first part of a new edition of the first volume of their *Natural Philosophy*, revised and greatly enlarged. In 1883 the second part of this volume was published, under the editorship of G. H. Darwin. In the preface the authors state that they have definitely abandoned their intention of writing a complete treatise. They have, however, left in the references to future volumes, as their method of treatment can only be fully justified by taking into account the original design of the work.

The first sentence of Fourier's *Theory of Heat* is printed in the original French at the top of their preface. "Primary causes are unknown to us; but they are subject to simple and constant laws, which may be discovered by observation, and the study of which is the object of natural philosophy." The first two paragraphs of their preface explain very clearly the aim they had in view.

"The term Natural Philosophy was used by Newton, and is still used in British universities, to denote the investigation of laws in the material world and the deduction of results not directly observed. Observation, classification, and description of phenomena necessarily precede Natural Philosophy in every department of natural science. The earlier stage is, in some branches, commonly called Natural History; and it might with equal propriety be so called in all others.

"Our object is twofold: to give a tolerably complete account of what is now known of Natural Philosophy, in language adapted to the non-mathematical reader;

and to furnish to those who have the privilege which high mathematical acquirements confer, a connected outline of the analytical processes by which the greater part of that knowledge has been extended into regions as yet unexplored by experiment."

Before the publication of this treatise many believed that the work done against friction was absolutely lost. Newton believed this. The authors, taking as a physical axiom that "the Perpetual Motion is impossible," prove the great principle of the conservation of energy. Energy, like matter, is indestructible. They point out that every motion which takes place in Nature meets one or other of the following forms of resistance—sliding friction, viscosity or imperfect elasticity, resistance due to induced electric currents, and resistance due to varying magnetisation. Our everyday experience proves that bodies falling freely in the air are impeded by viscosity, and that friction greatly hampers the action of all kinds of mechanisms.

The analogies of Nature lead us to believe that every star and every body of any kind has its relative motion hindered by the medium in which it moves. A curious result is that this frictional resistance tends to shorten the year, for it makes the earth move closer to the sun, and so the attraction between them is increased and the year thus shortened. Astronomical data prove that this shortening is appreciable, although very minute. A fraction of the year, therefore, cannot be used as our unit of time. Neither is the time of rotation of the earth round its axis a constant. Newton explained clearly the action of the sun and moon in producing tides. Kant was the first to point out that the frictional resistance to the motion of the tidal waters in oceans, lakes, and rivers acts as a brake on the earth's motion of rotation. It thus tends to equalise the lunar

month—that is, the period of the moon’s rotation round the earth with the sidereal day—that is, the period of the earth’s rotation round its axis. The authors make interesting speculations as to what will happen in the future, taking into account also the effect of the solar tides. They conclude that the moon is moving gradually further from the earth, and that the lunar month is lengthening. After attaining a maximum distance from the earth, it will then gradually return to the earth in a spiral path, the month shortening, and it will finally fall on the earth. Sir G. H. Darwin has carried this investigation much further, making use of it even to calculate the age of the earth. Starting with a planet in a partly solid and partly fluid condition and rotating round its axis in from two to four hours, he shows that the motion is unstable. It is, therefore, highly probable that it split into two, the larger portion being the earth and the smaller the moon. He then traces the relative motion of the earth and moon up to the present day, and computes that the minimum time required for the moon to have attained its present distance is fifty-four million years.

The data for making these calculations are somewhat uncertain, but there can be no doubt as to the ultimate result. All the bodies of the solar system must ultimately fall together into one mass, which, although rotating for a time, must finally come to rest relatively to the surrounding medium.

Since neither the period of the earth’s rotation round the sun nor round its own axis can be considered constant, the authors consider the question of the best standard of accurate chronometry. Their first suggestion was to make a metallic spring and hermetically seal it in an exhausted glass vessel. In their second edition they mention Clerk Maxwell’s suggestion that a period of

vibration of an atom of hydrogen or sodium would be an excellent natural standard. These atoms are all ready made; there is an infinite number of them all exactly alike in every physical property, and the time of vibration of an atom as determined by spectrum analysis is absolutely independent of its position in the universe. Clerk Maxwell also suggested to the authors that the time of revolution of an infinitesimal satellite close to the surface of a globe of water could be advantageously taken as the unit of time, as it is quite independent of the size of the globe.

The authors lay great stress on the importance of experiments. They point out that, without experimenting, terrestrial magnetism would never have been discovered. The same is true of the connection between a lightning-flash and the phenomena exhibited by rubbed amber.

They give rules for the conduct of experiments, and, judging from their own very extensive experience, they say that endless patience and perseverance in designing and trying different methods for investigation are necessary for the advancement of science. The experimenter who is likely to succeed is the one who does not allow himself to be disheartened by the failure of an experiment, but immediately sets about to vary his method so as to interrogate Nature in every conceivable way.

They follow Herschel in pointing out the importance of residual phenomena. If, after making allowance for all known causes of error, there still remains an appreciable discrepancy, it is of the greatest importance to investigate the reason for this. Adams and Le Verrier were led to discover a new planet by noticing very slight anomalies in the motion of Uranus. Schönbein was led to discover ozone—a gas of great chemical



activity—by noticing that a frictional electrical machine produced a distinct smell when working.

On the other hand, when the agreement between our results is closer than we have a right to expect, it is probable that our apparatus is not trustworthy. For example, a very good achromatic telescope makes every star appear to have a sensible disc. But it would be rash to draw any conclusion as to the size of the star from this. Further investigation proves that it is merely a phenomenon of the diffraction of light.

Until we know thoroughly the nature of matter and the forces which produce its motion, it is impossible to get rigorously exact solutions of physical problems. In order to obtain solutions, we introduce limitations into the problem. The authors, for example, take the case of a crowbar used to move a heavy mass. We first of all imagine that the bar is perfectly rigid, and obtain an approximate solution. We next suppose that it bends slightly, and so obtain a more accurate solution. Next, supposing that the mass is perfectly homogeneous—which, owing to its atomic constitution, it never can be—and that the forces consequent on dilatation, compression, and distortion are in exact proportion to these deformations, we can get a still more accurate solution. The complete discussion would involve the discussion of the deformations which take place in every part of the bar, fulcrum and mass, and we should also have to take into account the heat generated and its conduction throughout the mass. Our ignorance of the nature of matter makes any such discussion impossible. But in many cases solutions of great importance in practical work can be obtained by limiting the generality of the problems in the ways shown by experiment to be permissible.

It is interesting to notice that the proof the authors

give that a sphere attracts an external particle as if all its mass were collected at its centre is purely geometrical, and is practically identical with that given by Newton. Several terrestrial applications of Newton's law of gravitation are given. The attraction of a hemisphere on a particle at its edge is found. The result is used to compute the deflection produced by a hemispherical hill on a plumb-line at its base. Similarly, near the edge of a hemispherical cavity an opposite effect would be produced on the plumb-line.

The more important theorems discovered by Thomson in the theory of elastic solids are incorporated in the second part of the treatise. Many of these problems are of very great difficulty, and the results arrived at by very powerful and neat analytical methods are of great importance. It is an excellent introduction to the advanced theory of the subject.

One of the problems solved is to find the deformation of the solid earth, supposed to be a homogeneous sphere, by the tides produced by the moon and sun. Thomson was the first to point out that in any complete theory of the tides this deformation has to be taken into account. All previous dynamical investigations of tidal phenomena and of precession and of nutation proceeded on the assumption that the outer surface of the solid earth is absolutely unyielding. In Thomson's opinion, the mere fact of the existence of tides disproves the hypothesis formerly commonly made that we live on a mere thin shell of solid substance enclosing a fluid mass of melted rocks and metals.

If the earth were a thin shell covered with a thin layer of lighter liquid, the liquid would have practically the same depth all round. Under tidal influences, it would simply rise and fall with the shell, and so the tides would be infinitesimal, land and sea rising and

falling together. He calculates that a solid steel sphere of the size of the earth would yield one-third as much as a perfectly fluid globe. This yielding would reduce the height of the tides to two-thirds what it would be if the rigidity were infinite. Sir George Darwin has pursued this investigation further, and has obtained interesting results.

The case when the liquid surrounding the globe has a greater density than the globe itself is interesting, as it leads to unexpected results. The discussion shows that the equilibrium in this case is unstable, but the complete investigation still baffles the skill of mathematicians.

Thomson considered the augmentation of the tides due to the mutual gravitation of the water on itself. The results show that it is appreciable. Robison had pointed out previously that the great tides in the Bay of Fundy would produce a very sensible deflection of a plumb-line in the neighbourhood. As this is due indirectly to the attraction of the moon, it is not correct to say that the attraction of the moon has no appreciable effect on the deflection of a plummet. Even ordinary tides must produce at places near the sea an effect on the plummet considerably transcending the direct effect of the moon. The suggestion is made that observation of this effect might be used to determine the earth's mean density. In order to show Thomson's keen insight into physical phenomena, his discovery of a thermodynamic acceleration of the earth's rotation may be mentioned. It is well known that the barometer indicates variations of pressure during the day and night. The semi-diurnal constituent has its maximum values about 10 A.M. and 10 P.M. respectively. The crest of the nearer tidal protuberance is thus directed to a point of the heavens westward of the sun, and the solar attraction on these protuberances causes a couple about

the earth's axis, which accelerates its rotation. As the barometric oscillations are due to solar radiation, it follows that the earth and the sun form a kind of heat engine. Thomson calculates that the earth gains about 2·7 seconds per century on a perfect clock. On the one hand we have this effect and the shrinkage of the earth tending to make the earth rotate more quickly, and on the other we have the tides and the fall of meteoric dust tending to lengthen it.

Looking back on the work done by Thomson, there can be little doubt that, from the point of view of scientific and engineering progress, the authors did well to abandon the major portion of their undertaking. Much of the work they originally planned has been excellently done in special treatises. We need only mention Maxwell's *Electricity and Magnetism*, Rayleigh's *Sound*, and Lamb's *Hydrodynamics*. Had they proceeded with their undertaking, there would have been considerable overlapping with these works. The demands that would have been made on Thomson's time would have seriously crippled his work, and there is little of this that could be spared, without a loss which would probably outweigh the advantages that would accrue from having a complete treatise.

## CHAPTER IX

### THE AGE OF THE EARTH AND THE COOLING OF THE SUN

IN 1862 Thomson published two epoch-making papers. The paper on the secular cooling of the earth was published in the *Transactions* of the Edinburgh Royal Society, and the paper on the age of the sun's heat

was published in *Macmillan's Magazine*. Both of these papers were considered so important that they were published as appendices to Thomson and Tait's *Natural Philosophy*. They were read with delight by physicists, but geologists and biologists regarded them as unconvincing. Huxley was the great advocate of the latter class, and his attack elicited a brilliant and spirited rejoinder from Thomson.

Hutton, Playfair, and Lyell taught what Thomson called the doctrine of eternity and uniformity in geology as opposed to the cataclysmal doctrine, which predicted great disturbances and tremendous differences of climate in past ages. Playfair, the brilliant advocate of Hutton's theory, says that in the planetary motions we discover no mark either of the commencement or the termination of the present order of things. Thomson totally disagreed with this. He points out that Newton in his *Principia* says that planets and comets keep their motions a long time because the space in which they move offers little resistance. The motion of comets proves that it offers some resistance, and hence changes are continually going on in the solar system. Laplace's nebular hypothesis and other astronomical theories have a cataclysmal basis. This is common knowledge. Pope, for instance, says :

"Who sees with equal eyes as God of all,  
A hero perish, or a sparrow fall,  
Atoms or systems into ruin hurl'd,  
And now a bubble burst and now a world."

Even the great Charles Darwin demands hundreds of millions of years in his geological periods. It is of great importance, therefore, to study Thomson's reasoning, and see how far he has been successful in putting limitations to the periods of time demanded by geologists.

It is universally admitted that at one period of time the earth must have been a rotating, molten mass. If we assume that this mass is practically homogeneous, it is not difficult to calculate the shape that it would assume. The shape would be approximately spherical, the poles being flattened and the equator protuberant. Now, when the earth solidified, it would probably retain the same shape as it had when molten. But this shape depends on the velocity of rotation, and hence geodesic measurements enable us to compute this velocity. The polar diameter is known to be 26·7 miles less than the equatorial diameter. On the given assumptions it can be shown that this is the shape that a liquid mass the size of the earth would have if rotating at its present rate. If the liquid mass were rotating at twice this rate, it would be distorted from the shape of a sphere four times as much. In addition, if it cooled when rotating at this rate, it is highly probable that all the continents would have formed a dry belt round the equator, and that the poles would be the central points of the polar oceans. The mere fact that we have no dry equatorial belt limits the velocity of rotation when the earth was solidifying.

Kant was the first to show that the tides must act as a brake on the earth's rotation. Adams and also Thomson and Tait calculate that the time of the earth's rotation increases by twenty-two seconds every century. Taking this figure, we see that 7200 million years ago it would have been rotating twice as fast. Its shape therefore proves that it could not have solidified at this period.

Taking into account all the uncertainties in the figures and calculations, Thomson concludes that 5000 million years ago the earth was certainly not solid, and that it was probably not solid even 1000 million years ago.

It will be seen that the reasoning is cogent, and so the conclusions cannot be lightly disregarded.

Another method of determining the age of the earth is by finding the rate at which it is cooling, and then by Fourier's solutions calculate backwards to the time when the earth was molten. Thomson was particularly attracted by this method. From a survey of underground temperatures in different parts of the world, it is found that on the average the temperature of the earth increases one degree Fahrenheit for every fifty feet of descent. Heat, therefore, must be continually flowing from the earth's interior to its surface, and, since the earth's surface does not get hotter, there must be a continual loss of heat from year to year from the surface of the earth. The earth, therefore, must be either getting cooler from age to age, or some temporary dynamical action inside the earth must be keeping up the heat. Thomson considered it proved that there was less volcanic action in the earth now than there was a thousand years ago—just as a battleship has less ammunition on board after it has been discharging shot and shell for several hours.

The chemical hypothesis to account for underground heat would be probable if the rise of temperature as we go downwards occurred only in isolated localities. The suggestion that there may be some slow uniform combustion going on at a great depth under the surface he thinks highly improbable. Poisson's hypothesis that the present underground heat is due to a passage at some former period of the solar system through hotter stellar regions is only tenable if there was a well-marked period of discontinuity in palæontology. Thomson calculates that if this passage took place between 1250 and 5000 years ago, then, in order to account for the present underground temperature gradient, the

temperature of the supposed stellar region would have to be from  $25^{\circ}$  to  $50^{\circ}$  F. hotter than the present mean temperature. Hence there would have been plenty of evidence available of such a phenomenon. The further back we place this passage of the earth, the hotter the stellar region would have to be. If the passage took place more than 20,000 years ago, the excess of temperature would have been greater than  $100^{\circ}$  F., and so animal life would have been destroyed. As no evidence has ever been deduced to support this, the hypothesis is untenable.

Following Leibnitz, he assumes that the earth was once an incandescent liquid sphere. Assuming that the heat constants of this mass are the same as the constants he and Forbes found by experiments on rocks from a quarry near Edinburgh, and making allowances for the uncertainty as to his data, he concludes that the period of time since the earth solidified lay between 20 and 200 million years.

After the beginning of the crusting over the terrestrial heat would have little direct influence on climate. After 40,000 years the rise of temperature as we bore downwards would be about  $1^{\circ}$  per foot, after 160,000 years it would be half a degree per foot, and after 100 million years it would be the fiftieth part of a degree per foot, which is the temperature gradient at the present day. "Is not this, on the whole, in harmony with geological evidence rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain ranges, the violent distortions and fractures of strata, the great prevalence of metamorphic action (which must have taken place at depths of not many miles, if so much) all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake



shocks, and every kind of so-called Plutonic action have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age ? ”

Thomson imagined that the interior of the earth was like a honeycombed solid, the liquid always tending to work its way up, owing to its lower specific gravity. The actions that would take place in such a mass would be sufficient to account for geological phenomena like earthquakes, subsidences and upheavals and eruptions of melted rock. The oceans on the surface of an earth built up in this way would exhibit the phenomena of tides.

In 1899 Kelvin, having the advantage of more accurate data, stated that he agreed with Clarence King in thinking that the time since the earth was molten was about twenty-four million years. This estimate, however, is generally considered to be too small. Perry has pointed out that if the conducting power of the material near the centre was greater than that of the material near the surface, Thomson's estimate would have to be raised very considerably.

The discovery of radio-activity throws grave doubts on the possibility of determining the age of the earth from the known temperature gradient of its crust. Curie has calculated that the heat emitted per hour from one pound of radium would raise a pound of water from the freezing to the boiling point. R. J. Strutt has detected radium in many rocks of the earth's crust in sufficient quantity to account for the temperature gradient without the necessity of making any hypothesis about heat being conducted from the interior. In fact, if the crust were more than forty-five miles thick, the outflow of heat would be greater than that actually observed. We have even to suppose that inside the crust

there is no radium. But, nevertheless, Thomson's deductions mark an epoch in our knowledge of the world's scientific history, and have stimulated and encouraged many physicists to follow up his investigations.

Thomson's paper on the age of the sun's heat is divided into three parts. In the first part he discusses the cooling of the sun, in the second part, the present temperature of the sun, and in the third, the origin and total amount of the sun's heat. He begins by pointing out that we cannot be certain that the sun is losing heat at all. It is quite conceivable that the heat generated by the influx of meteoric matter may compensate for the loss of heat by radiation. It is also conceivable that it is an incandescent liquid mass, the heat of which is due to the influx of meteors in bygone ages. The heat generated by the influx of meteoric matter at the present time may be negligibly small compared with the heat radiated. Spectrum analysis proves that the sun's substance is very like the earth's. From Herschel's and Pouillet's investigations, he concludes that the rate at which heat is radiated from a square foot of the sun's surface is 7000 horse-power. It would be incredible to suppose that the sun had existed for countless ages radiating heat at this rate. He supposes that the sun was formed by the falling together of a large number of smaller bodies by mutual gravitation. The energy of the motion lost by the collision would all be converted into heat.

Making certain assumptions about the specific heat of the mass of the sun, he calculates that probably it has not illuminated the earth for 100 million years, and almost certainly that it has not illuminated it for 500 million. He concludes that the inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer,

“unless sources now unknown to us are prepared in the great storehouse of creation.”

The discovery of radium makes it necessary to modify this calculation also. It is true that an examination of the sun's spectrum has not, so far, revealed any radium lines, but it is well known that helium, a transformation product of radium, is present. It is probable, therefore, that there is radio-active matter in the sun. It is possible that Thomson's estimate may be a hundred times too small.

## CHAPTER X

### ELECTRICAL ENGINEER

THE industry of electric lighting and power distribution is immensely indebted to Lord Kelvin. In the early days he advocated enthusiastically the use of the electric light, emphasising its many advantages. He also used all his influence to remove the legislative restrictions which at that time seriously hampered the industry. As far back as 1874, Sir William Thomson was President of the Society of Telegraph Engineers. When the title of the Society was changed to that of the Institution of Electrical Engineers, he was chosen as the first president. He was president for the third time in 1907—the year of his death.

In 1877, when a juror at the exhibition in Philadelphia, he wrote an appreciative report on the exhibit of Gramme Dynamos. The method invented by Werner von Siemens ten years previously of using electro-magnets, which were excited automatically when the machines were running, was employed. Thomson saw at once that the abolition of permanent magnets was a great

step in advance, and he started formulating the mathematical theory of their working. This theory was generalised later in the classical paper by J. and E. Hopkinson, published in the *Transactions of the Royal Society* in 1886.

In a discussion on a paper read to the Institution of Civil Engineers in 1878, Thomson pointed out the feasibility of conveying electric energy to a distance of several hundred miles. The "economical and engineering moral" of the theory was that the towns of the future would be illuminated by electricity generated at electric stations near the pit's mouth, where the coal dross, most of which was at that time wasted, could be used for working engines of the most economical kind. Electrical engineers will recognise how closely this prediction has been fulfilled. There are now many power stations all over the world, where electricity is generated in "bulk" and transmitted considerable distances for lighting purposes.

He did not fail to foresee that waterfalls would soon be harnessed to provide power for industrial purposes. Werner von Siemens had mentioned to him in conversation that the power of the Falls of Niagara might be transmitted electrically to a distance. The idea might well strike them as fantastical. Only a year before the telephone and the phonograph would have been classed as equally chimerical. There could be no question about the "vast economy" effected by harnessing Niagara.

In 1879, one year after the lighting of the London embankment by Jablochkoff candles—the current for which was furnished by Gramme dynamos—Thomson gave evidence before a Parliamentary Select Committee, which had been appointed to consider the question of electric lighting. The evidence he gave proves the

thorough grasp he had of the whole subject, both from the scientific and the commercial point of view. He mentioned that the experiments made by Messrs. Siemens and at Edinburgh University proved that one horse-power would produce 1200 candles of visible electric light. The electric light, therefore, would soon be used even in the passages and staircases of private dwellings. It was much more economical than gas.

Arc lamps might be put on iron poles 60 feet high, or the old French plan of span wires might be used. There was no need to employ globes of opal glass absorbing half the light generated. From the hygienic point of view, the electric light was much preferable to gas. As a mathematician, he saw nothing impossible in subdividing the electric light. As a motive power, he could see no limit to the applications of electricity. It could do all the work now done by steam engines, no matter how powerful they were. The duty of legislators was to encourage inventors to the utmost.

The abstract of this evidence, which appeared in *Nature* for May 29, 1879, concludes with the sentence, "This may be called the fanatical view of electric light." At the present day it reads merely like a plain record of the facts. As a matter of history, this evidence was instrumental in turning the attention of certain financiers and young engineers to developing the practical applications of electricity.

The invention by Faure, in 1881, of a battery (accumulator) by means of which electricity could be economically stored, again roused Thomson by its many possibilities of usefulness. In letters to the *Times* and to *Nature*, he points out that this discovery is a great step in advance. It will now be possible to use "Swan or Edison" lamps both for mast-head lights and for the red and green side-lamps. He also makes the important

statement that accumulators could be used for traction purposes. To drive a car by electric motors would be more economical than to employ horses. During this year at the British Association Meeting he explained how accumulators would prove to be an invaluable adjunct in country-house lighting. He also enunciated his law for determining the size of the mains to be used for electric lighting, so that the distribution should be made with the maximum economy.

At the same meeting he proved the law governing the efficiency of a dynamo, and gave a simple formula by means of which the load at which the efficiency is a maximum can be found. This law is well known to dynamo designers. In conjunction also with his nephew and colleague, J. T. Bottomley, he gave the results of tests which they had made on the illuminating power of glow-lamps. They measured the rate at which electric energy was supplied to the lamps and the illumination produced. They were thus able to find the light produced by a given amount of energy, and hence compare the efficiencies of the lamps. The principle of the method is the same as that now employed in every lamp factory in the world.

In 1881 Thomson patented a special winding for an alternating current dynamo. Ferranti independently invented an improvement on this design. They therefore entered into a working agreement, and this association was influential in turning Thomson's attention to many of the important problems then occupying the minds of electrical engineers.

In connection with the practical difficulties experienced at the start of the London Electric Supply Corporation, Ferranti often consulted him. This company transmits power from Deptford to Trafalgar Square, London, at a pressure of 10,000 volts, and the experience

gained by the working of this pioneer company has been of great value to the electrical industry. One of the problems Thomson solved was the distribution of the current over the cross section of a conductor carrying a rapidly alternating current. His results show that in some cases the current does not penetrate far into the conductor. It is confined merely to a thin skin of the conductor. So far as current-carrying is concerned, the great bulk of the copper is ineffective. It would be economical and quite as efficient, therefore, to use a thin tube of copper. This result had been previously obtained by Maxwell, Rayleigh, and Heaviside. The latter especially has done excellent mathematical work on this problem, and his solutions are much the most complete. Thomson, however, popularised the theory, and, in his presidential address to the Institution of Electrical Engineers in 1889, he gave it in a form which could be readily grasped by engineers. This address, which was entitled "Ether, Electricity, and Ponderable Matter," discussed problems to which he had given much thought, and it excited much interest.

In 1884 Thomson read to the Glasgow Philosophical Society a paper on galvanometers for measuring potential differences and currents. He also describes a regulator he had devised for maintaining constant the electric pressure applied to the lamps in his own house. This device was the forerunner of many similar ones for effecting the same object.

A little later, in 1887, Thomson describes a "Double Chain of Electrical Measuring Instruments, to measure currents from the thousandth of an ampere up to 1000 amperes, and to measure pressures up to 40,000 volts." In this paper he begins a description of the marvellous series of voltmeters and ampere balances, with which

his name is inseparably associated. They are practically in universal use as standard instruments.

Thomson's earlier instruments were all made by Mr. James White of Glasgow. He personally supervised their construction, however, and was continually perfecting details in their design. He wanted his instruments to be as useful in the test-room as in the laboratory. He said that his ambition was that boxes leaving the factory should be labelled "Glass—without care; any side up." The making of Thomson's instruments soon developed into quite a large industry. The business is now owned by a private company, under the title of Messrs. Kelvin & James White, Ltd. They have large works in Cambridge Street, Glasgow.

In 1889, with the help of some of his students, he made a determination of the number of electrostatic units of potential in the electro-magnetic unit of potential, and obtained a result the inaccuracy of which was less than the half of one per cent. He also perfected methods of standardising ampere balances by means of a copper voltameter. One of his old pupils relates that Professor Ayrton retested in London one of the balances standardised in Glasgow. He found that the error of the reading was about the tenth part of one per cent. high. Thomson's jubilation was natural and justified when he found that practically the whole error could be attributed to the difference between the force of gravity in Glasgow and London.

In 1891 Thomson gave an interesting and suggestive lecture to the Royal Institution on "Electric and Magnetic Screening." The importance of electrostatic screening in electric theory is not sufficiently recognised. Green was the first to give a rigorous proof that a continuous metallic surface acts as a perfect screen against all electrostatic influence. Faraday gave an



elaborate experimental demonstration of the truth of this. The lecturer showed many experiments illustrating partial screening by sheets of metal, and showed how the results might have been predicted by the method of images. A screen of imperfectly conducting material acts as efficiently as a screen of metal, if sufficient time be allowed. But when the electrostatic force varies rapidly, the efficiency of the screening is much less. On a damp day a sheet of paper will sometimes act almost like a perfect screen. The screening effect is increased by blackening the paper with ink on both sides.

To screen off magnetic action is very much more difficult than to screen off electric action. Even with the best magnetic iron only partial screening can be obtained. The best screen is obtained by surrounding the space by a thick shell of iron. The conning-tower of a battleship screens off a large fraction of the earth's magnetic field from a compass placed inside it. Measurements prove that a conning-tower, having a belt of iron one foot thick, five feet high, and ten feet in internal diameter, screens off 80 per cent. of terrestrial magnetism from a compass placed at the centre of the belt.

Kelvin was a strong advocate of the continuous current system of electric lighting. A few months before his death, he spoke in the discussion on a paper on the Thury high-tension continuous current system, read by Mr. Highfield to the Institution of Electrical Engineers. Notwithstanding the improvements which have been made in polyphase methods of distributing power—improvements which no one appreciated more than himself—he said, “I have never swerved from the opinion that the right system for long-distance transmission of power by electricity is the direct current system.”

He related how, many years ago, Lord Rayleigh had said to him that he was glad that alternating currents were being used in practice, as people will now learn the subtleties of electrical science, a knowledge of which was unnecessary so long as they use continuous current. Rayleigh also prophesied that engineers would ultimately come back to the use of the latter. The first part of the prophecy has been fulfilled. For twenty years technical schools have given youthful engineers a thorough grounding in the "subtleties" of electrical science. It seems probable that Mr. Highfield's paper foreshadows the return to continuous current.

## CHAPTER XI

### CONCLUSION

LORD KELVIN was twice married. In 1852 he married Miss Crum, daughter of Walter Crum, F.R.S., who was a first cousin of Thomson's father. She died in 1870. During a visit to Madeira in 1873, when acting as consulting engineer for the Western and Brazilian Cable Company, he met Miss Blandy, daughter of C. R. Blandy, of Madeira, whom he married in 1874.

In 1860 Thomson had a serious accident when curling. He slipped on the ice, and, falling heavily, had the great misfortune to fracture the neck of his thigh-bone. This rendered him permanently lame. During the months when the limb was in splints, although he sometimes suffered great pain, yet his mind was always occupied with physical problems. His pencil and his note-book were his inseparable companions.

During the last years of his life his general health was good, but he was afflicted with a very severe form of

facial neuralgia, which proved very intractable to treatment. He endured the paroxysms with great fortitude and patience, and immediately they were over he recommenced his work.

During the autumn of 1907 Lady Kelvin had a serious illness. On November 23 Lord Kelvin caught a chill. This, superposed on his natural anxiety about Lady Kelvin, confined him to his bed. He still for a week or two struggled on doing some work, but gradually he became weaker, and died on December 18.

On December 23 he was buried in Westminster Abbey with all the honours due to a prince of science. The service was simple and impressive, and the prominent note was that of thanksgiving. The mourners, whilst lamenting the loss of their beloved and revered master, yet rejoiced that it had been given to him to set for all time an example of a noble, unsullied, and strenuous life spent in the arts of peace, and crowned with such a noble record of work done for the benefit of humanity.

It is satisfactory to remember that many tributes in appreciation of his labours were paid to him in his lifetime. When President of the Royal Society, in 1892, Queen Victoria raised him to the Peerage, with the title of Baron Kelvin of Netherhall, Largs. Later on King Edward elected him into the Order of Merit, when that order was first instituted. Both Germany and France bestowed the highest dignities on him. A list of the honorary degrees conferred on him by universities and of the honorary memberships of scientific societies to which he was elected would fill several pages.

In the bibliography published in the *Life of Lord Kelvin*, by Silvanus Thompson, the titles of 661 of his papers are given. His *Reprint of Papers in Electricity and Magnetism*, published in 1872, is still one of the classical treatises on the subject. The influence of this

work can be perceived in every modern book on electricity. His articles in *Nichols' Encyclopædia*, in the *Encyclopædia Britannica*, and in various magazines are well known, and have often been republished. His Popular Lectures and Addresses are published in three volumes. The first is on the *Constitution of Matter*, the second is on *Geology and General Physics*, and the third is on *Navigation*. His *Mathematical and Physical Papers* have been published in six volumes, the last three of which have been ably edited by Sir Joseph Larmor. During the concluding years of his life he derived much pleasure from rewriting and carefully editing the Baltimore Lectures, which he delivered to the Johns Hopkins University in 1884.

The above brief sketch of the life of a man of genius shows what a quantity of glorious work can be crowded into one man's lifetime. The childhood in Ireland; the busy and happy years as a boy student at Glasgow University, coloured by dreams of the discoveries he would make in unravelling Nature's mysteries once he had acquired the requisite skill with the mathematical tools. The active and strenuous life at Cambridge when he was preparing himself for life's work, and where he first proved that he could wield the philosopher's tools with masterly skill. The full and varied life at Glasgow University, brightened with the discoveries that were then opening up a new page on the scroll of science. His enthusiasm for the submarine cable, which was to link the nations together in the cause of peace. The dawning of an electrical age, which would render antiquated the "marvellous scientific inventions" of his boyhood. The busy life of a consulting engineer, with its struggles and successes. The Presidency of the Royal Society, with the multifarious duties which it entails. His professorial jubilee, when

delegates came from every part of the world—from kings, from learned societies, and from colleges—all vying to do him honour. The enthusiasm with which he was claimed by scientists as their leader marks an epoch in the history of the world.

Although he derived substantial material benefits from his labours, yet where science was concerned he gave both time and money freely. The making of his electrical instruments and of his compass and sounding-machine created a small but flourishing industry. But the perfecting of apparatus for demonstration or research purposes, the making of tide-predicting machines and of machines for solving equations were labours of love. The following story throws a light on his manner of attacking problems. When carrying out experiments on the spectra of liquids at Glasgow, the writer once heard his assistant, M'Farlane, tell him that the hollow prism which had come from White's was not properly made. Thomson at once told him to send it back to be remade. "If they cannot do it, send it to London, and if no one there can do it, send it to Paris. I must have a proper prism." Amongst the large number of scientific men who developed some of Kelvin's ideas, and delighted to acknowledge their indebtedness to him, the following may be specially mentioned: Helmholtz, Clerk Maxwell, Stokes, Mascart, Sir George Darwin, Sir Charles Niven, Sir Alfred Ewing, and Professor Chrystal. Among his pupils, Professors Jack, Ayrton, Perry, Andrew Gray, Gibson, and Carslaw may be mentioned. Professor A. Gray, Kelvin's successor at Glasgow University, has written an excellent biography of him. Professor Carslaw has written a standard work on Fourier's Series. Amongst the younger generation there are several electrical engineers, as, for example, Professors Cormack, W. Buchanan, and J. B. Hender-

son. But only a small fraction of Kelvin's students followed on his own lines. The great bulk of them were studying for professional or commercial careers. Some of them have become judges, bishops, generals, consulting physicians, and engineers. One is Sir William Ramsay, the great chemist, and another is the Archbishop of York. We must also specially mention the large number of Japanese students who studied under him, and now hold leading posts in their own country. All old students look back on their attendance at Kelvin's lectures as something never to be forgotten and to be treasured deep in their hearts.

The advice Kelvin gave to his old students when starting on their careers was of great value to them. He also often gave them letters of introduction, which they found of the greatest service. The letters were generally typewritten, but occasionally he sent holograph letters. At the time of Lady Kelvin's illness, and only a fortnight before he himself was incapacitated by his last illness, Lord Kelvin wrote the author the letter shown on the opposite page, regretting his inability to be present at the reading of the author's paper at the Institution of Electrical Engineers.

Whilst speaking of his own work, Lord Kelvin, like Sir Isaac Newton, was impressed with the smallness of that which had been actually achieved in comparison with what had been attempted. For instance, in his speech at his jubilee he said that, after fifty-five years of constant study, he knew little more of electricity and magnetism than he did at the beginning of his career. He thus pointed out the boundless, unexplored fields which still stretch in endless vista before the scientific man. Macaulay said much the same when discussing the Baconian method. "These are but a part of its fruits and of its first-fruits. For it is a philosophy

which never rests, which has never attained, which is never perfect. Its law is progress. A point which yesterday was invisible is its goal to-day, and will be its starting-post to-morrow."

No 4, 1907

NETHERHALL,  
LARGS,  
AYRSHIRE.

Dear W. Russell  
Many thanks for  
your paper on Dielectric  
Strengths. I shall  
read it with much  
interest.

Yours very truly

Kelvin

Kelvin had a powerful imagination, a strong inductive faculty, and a power of realising his conceptions in practice, which has only been equalled by the greatest inventors. The possession of three such faculties by the same man is probably unique. His deductions were not of such world-wide importance as

those of Isaac Newton. It is very difficult to make a comparison between these two intellectual giants, as the ages in which they lived are so far apart. Newton was more painstaking and thorough. He published little that was open to criticism. Kelvin, on the other hand, could hardly wait until his experiments were finished. There was so much to do and such a short time to do it in. Whatever his hand found to do he did with all his might, and the world knew the result. If it was not satisfactory, then he would reshape and re-polish it. He welcomed co-operation in his work. In his senior class he often attacked original problems on the board, and invited his students to help him in working out details.

His work lives, and will continue to live. To him it has been given to make history, which will live so long as intelligent man survives on this earth. As the years roll on, our indebtedness to him increases. May his memory long be kept green in the land he loved so well.



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