John William Strutt, third baron Rayleigh, O.M., F.R.S., sometime president of the Royal society and chancellor of the University of Cambridge / by his son Robert John Strutt, fourth baron Rayleigh.

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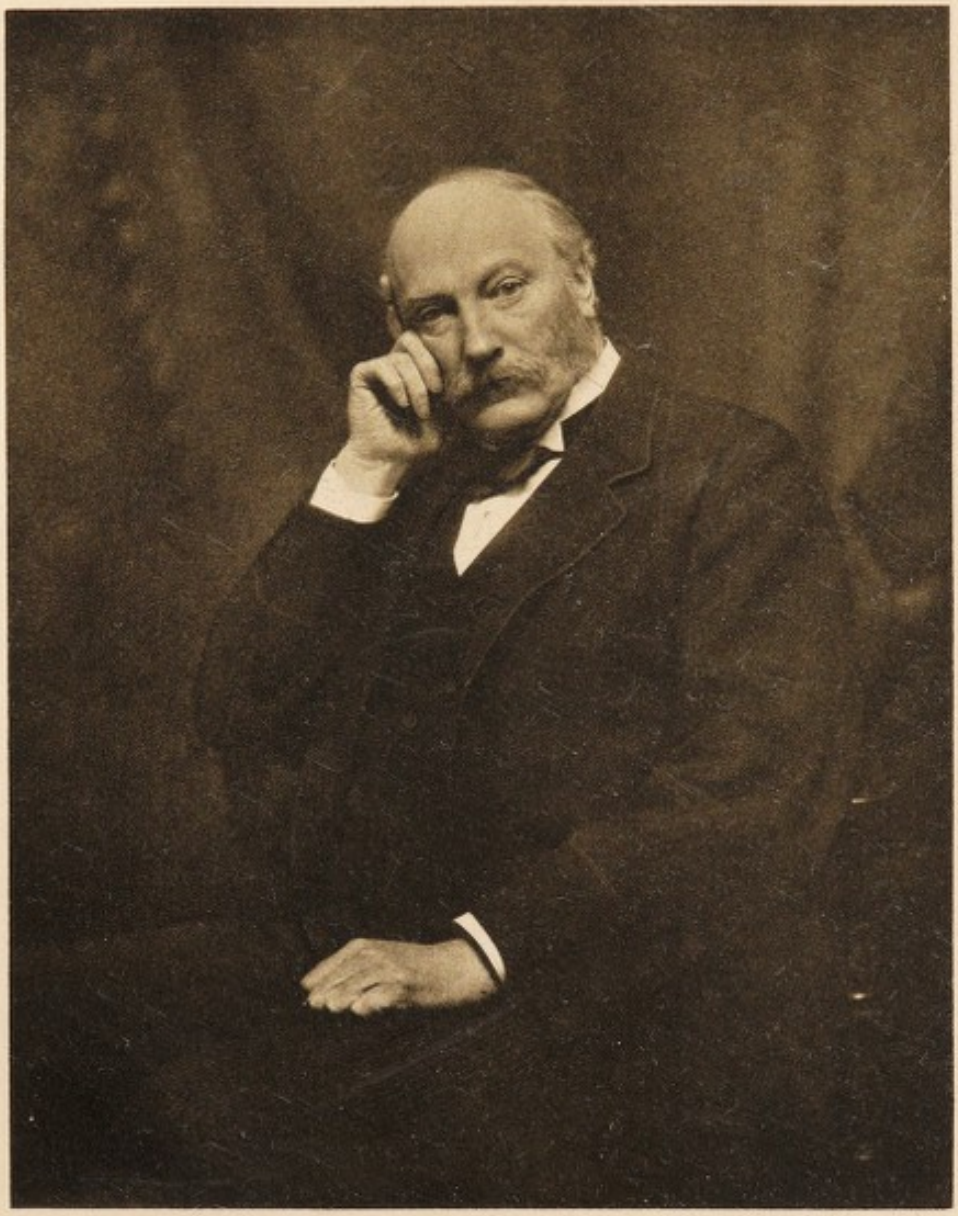
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LIFE OF JOHN WILLIAM STRUTT

Third BARON RAYLEIGH, O.M.



LONDON: EDWARD ARNOLD & Cº

JOHN WILLIAM STRUTT
THIRD BARON RAYLEIGH

O.M., F.R.S.

SOMETIME PRESIDENT OF THE ROYAL SOCIETY AND CHANCELLOR
OF THE UNIVERSITY OF CAMBRIDGE

BY HIS SON

ROBERT JOHN STRUTT

FOURTH BARON RAYLEIGH, F.R.S.

LATE FELLOW OF TRINITY COLLEGE, CAMBRIDGE

WITH ILLUSTRATIONS

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PREFACE

In writing this book it has been my aim not so much to give an account of my Father's scientific work as to depict him as a man. The narrative would, however, be without substance if his scientific career were not made its guiding thread. In the selection of topics it was clearly impossible to refer to more than a small fraction of the 446 papers in the six large volumes of his collected writings. The topics have been chosen for their comparative simplicity, and for their bearing on the external circumstances of his life. Many investigations of epoch-making importance have necessarily been left unnoticed. But it is hoped that some others have been brought within the reach of readers who would be repelled by the severely technical form of the original account. However, this book is concerned rather with the setting of the work than the substance of it, and I have tried to record everything of permanent interest in that connection.

A valuable summary of Rayleigh's work from a more strictly scientific point of view will be found in Sir Arthur Schuster's obituary notice in the *Proceedings of the Royal Society, A*, Vol. 98, 1920.

I have reprinted as an appendix Rayleigh's Presidential Address to the Society for Psychical Research, delivered in 1919 shortly before his death. Another appendix is of a very different kind. It is selected from the collection of humorous stories and anecdotes which he made in his later

years. He collected these not for publication, but for his own amusement, and some of the stories are taken from printed sources. Others were personal experiences, and these latter for the most part have been used in the biographical narrative. The selection in Appendix III is from the remainder.

RAYLEIGH.

TERLING PLACE,
CHELMSFORD.
July, 1924.

CONTENTS

CHAPTER I

PAGE

ANCESTRY. PARENTAGE. BOYHOOD 1

The Strutt Family. Terling. John Strutt. Colonel Strutt. The Peerage. John James Lord Rayleigh. His Marriage. Birth of his Eldest Son, John William. His Sister's Reminiscences of his Childhood. Early Indication of Scientific Tastes. At Eton. School at Wimbledon; Harrow. Torquay. Mr. H. M. Hyndman's Reminiscences. Home Pursuits. Photography.

CHAPTER II

UNDERGRADUATE LIFE AT CAMBRIDGE 25

Trinity College. Mathematics under Dr. Routh. Taste in Books. Coming of Age at Terling. Stokes' Lectures. Senior Wranglership.

CHAPTER III

EARLY MANHOOD. FIRST SCIENTIFIC RESEARCHES . . . 37

Trip to Italy. Difficulty in getting Instruction in Experimental Science. Fellowship. Trip to America. Refuses to stand for the University. Charles Darwin. Early Experimenting. Clerk Maxwell. Researches on Resonance. The Blue Sky.

CHAPTER IV

EARLY MARRIED LIFE 55

Friendship with the Balfours and Cecils. Mr. Gladstone. Marriage to Evelyn Balfour. Rheumatic Fever. Tofts. Egypt and the Nile. Succession to the Peerage. Elected to the Royal Society. Spiritualism. Mrs. Jencken. The Address in the House of Lords. Universities Commission.

CHAPTER V

IN THE 'SEVENTIES. THE THEORY OF SOUND . . . 71

Beauchamp Tower. Hydraulic Experiments. A. Mallock as Assistant. Depression in Agriculture. Charles Richardson. To Madeira. Life at Terling. Book on the Theory of Sound. Reciprocity. Tyndall's Experiments. Review by Holmholtz.

CHAPTER VI

GRATINGS AND THE RESOLVING POWER OF SPECTROSCOPES . 86

Diffraction Gratings. Zone Plates. Photographic Copies of Gratings. Power of the Spectroscope. Vagueness of existing Notions. True Theory and Verification. The Prismatic Spectroscope.

PAGE

CHAPTER VII

THE CAMBRIDGE PROFESSORSHIP AT THE CAVENDISH LABORATORY 99

Pressure on Rayleigh to go to Cambridge. Equipment of the Laboratory. George Gordon. Organization of the Classes. Determination of the Ohm. Spinning Coil. Letter from Joule. Lorenz's Method. Accuracy Attained. Other Researches in the Laboratory. The Ampere.

CHAPTER VIII

LIFE AND LETTERS DURING THE CAMBRIDGE PERIOD. THE BRITISH ASSOCIATION AT MONTREAL 128

Domestic Life. Visits. Irish Land Bill. British Association at Southampton. Rheumatism. Trip to the Continent. Bath. Letters from H. A. Rowland and J. H. Poynting. President of the British Association at Montreal. The Address. Rocky Mountains. American Scientific Friends. Resignation of Cambridge Professorship.

CHAPTER IX

THE LABORATORY AT TERLING 149

Return to Terling. The Laboratory. Rowland's Visit. The Black Room. The Light Room. Workshop. The Schoolroom. The Book Room. The Tunnel. George Gordon. Weighing Hydrogen.

CHAPTER X

THE LATE 'EIGHTIES 166

Wave Theory in the *Encyclopædia Britannica*. Secretary of the Royal Society. J. J. Waterston. Colour Vision. Peculiarity of the Balfours. Royal Society Committee. Colour Vision Tests at Terling. Speech on Electric Lighting Bill. Visit to Balmoral. Joule. Visit to Stornoway Castle. Farming on the Terling Estate.

CHAPTER XI

THE DISCOVERY OF ARGON 187

Weighings of Nitrogen. Discrepancies Encountered. Early Suggestions by Ramsay. Atmospheric Nitrogen heavier than Chemical Nitrogen. Cavendish's Experiment. Paper at the Royal Society. Absorption of Nitrogen. Visit to Crookes. Collaboration. Isolation of New Gas. Ramsay's Work. Partnership Arranged. Announcement at Oxford. Scepticism. Large-scale Operations. Concentration of Argon by Diffusion. Murmurs. Correspondence with Dewar. Discussion at the Chemical Society. Spectrum. Blank Experiments. Collecting the Results. Joint Paper at the Royal Society. Last of the carping Critics. Origin of the Birkeland Process for Fixing Nitrogen. Later Results of the Discovery of Argon.

PAGE

CHAPTER XII

IN THE 'NINETIES 226

The British Association at Oxford. Lord Salisbury's Address. Second Edition of *Theory of Sound*. Eusapia Paladino. Professor at the Royal Institution. Faraday Centenary. Centenary of the Institution. Lecture Experiments.

CHAPTER XIII

FRIENDSHIP WITH LORD KELVIN 239

His Visits to Terling. Scientific Discussions. The Definition of the Unit Alternating Current. Correspondence—Controversy on the Maxwell-Boltzmann Doctrine. Lord Kelvin's Peerage.

CHAPTER XIV

DOMESTIC AND SOCIAL LIFE AT TERLING 254

Daily Habits. Recreations. Mrs. Sidgwick's Visits. Week-end Parties. The Visitors' Book. Joseph Chamberlain's Visits. Scientific Parties. Henry Sidgwick's Last Days.

CHAPTER XV

PUBLIC WORK 267

Lord Lieutenant of Essex. Appointment of Magistrates. Incident arising out of the South African War. Scientific Adviser to the Trinity House. Design of Foghorns. Movement for a National Physical Laboratory. Chairman of the Executive Committee. Difficulties with the Analytical Chemists. Work on Electrical Units at the Laboratory. Finance. Transference of Responsibility to the Research Department.

CHAPTER XVI

RECREATION, AND MORE PUBLIC WORK 286

To Ceylon. India. The Eclipse. Mrs. Steel. End of Indian Tour. Explosives Committee. Chief Gas Examiner. Vibration on the Central London Tube Railway.

CHAPTER XVII

SOME RESEARCHES IN MATURE YEARS 299

The Blue Sky due to Molecular Scattering. Death of George Gordon. J. C. Enock. Direction of Sound. Difference of Intensity and of Phase at the Two Ears. Republication of Scientific Papers. Estimate by Sir J. J. Thomson. Remarks of Sir J. Larmor.

CHAPTER XVIII

PUBLIC REWARDS AND ORNAMENTAL OFFICES 312

Order of Merit. Nobel Prize. Visit to Stockholm. Gift to Cambridge. Question of Presidency of the Royal Society. Ultimately Accepts. Some Extracts from Presidential Addresses. Chancellor of Cambridge University. Installation Ceremony. Attempts to collect Money for the University. Darwin Celebration.

PAGE

CHAPTER XIX

LATER YEARS 329

Trip to South Africa. Illness. Colour of Sea Water. Return Home via Durban, Zanzibar and Palestine. Later Experimental and other Work. War Conditions at Terling. Presidential Address to the Society for Psychical Research. Aeronautics. Early Interest. Soaring Flight of Birds. Advisory Committee for Aeronautics. Application of Dynamical Similarity. Work during the War.

CHAPTER XX

RAYLEIGH'S WORK IN RELATION TO THE RECENT DEVELOPMENTS OF PHYSICS 342

Views on Aberration. Letters from Professors Michelson and Lorentz. Experiments on Rotatory Polarization and Double Refraction. Material for Relativity. Difficulties about the Mechanical Explanation of Spectral Series. Difficulties about Specific Beats. Investigation of Black Body Radiation. Attitude to the Quantum Theory. Conversation on these Subjects.

CHAPTER XXI

GENERAL CHARACTERISTICS. DEATH AND FUNERAL . . 358

Kindly Disposition. Openness of Mind. Evasive Attitude on some Fundamental Questions. Religious Observances and Views. Views on Sexual Morality. Taste in Literature. Humour. Slowness to Decide. Dislike of committing Himself on Imperfect Knowledge. Capable of Drastic Action. Views on Property. Economical Instincts. Dress. Miscellaneous Dicta. Failing Health. Last Illness. Funeral. Memorial in Westminster Abbey.

APPENDIX I

LIST OF HONORARY DISTINCTIONS 375

From Universities; Governments; Learned Societies and Academies

APPENDIX II

PRESIDENTIAL ADDRESS TO THE SOCIETY FOR PSYCHICAL RESEARCH 379

APPENDIX III

COLLECTION OF JESTS AND ANECDOTES 392

INDEX 399

LIST OF PLATES

LORD RAYLEIGH, 1901, *æt.* 59 . . . . . *Frontispiece*

JOHN JAMES, LORD RAYLEIGH, with Charles Richardson, in the conservatory at Terling, *circ.* 1865. From a wet collodion photograph by the subject of this biography . . . . . *To face p.* 10

J. W. STRUTT, 1870, *æt.* 28. From a wet collodion photograph by himself . . . . . „ 44

LORD KELVIN and LORD RAYLEIGH, in the Laboratory at Terling, July, 1900. From a photograph by Prof. A. G. Webster . . . . . „ 250

INSTALLATION OF LORD RAYLEIGH AS CHANCELLOR OF CAMBRIDGE UNIVERSITY, June, 1908. To the right are seen Mr. Asquith, the Duke of Northumberland, Lord Halsbury, and Sir John Fisher (the latter in uniform) . . . . . „ 322

CHAPTER I

ANCESTRY. PARENTAGE. BOYHOOD

The Strutt family can be traced back to about the year 1660, when they were located in Essex, as they have been ever since; they made money by milling corn, and acquired many of the water-driven mills in the neighbourhood of Maldon in Essex. In the days before steam the small water power to be found in the south of England was of course far more valuable and important than now. In this way they gradually improved their financial position and rose in the social scale. About the year 1720 they were in possession of New House, Terling,[1] which still belongs to the family, and in 1761 John Strutt (1727–1816), the first member of the family who had a county position, bought the Manor of Terling Place from Sir Matthew Featherstonehaugh.[2]

Since Terling Place was the scene of much of what I shall have to relate, it will not be amiss to devote a page or two to describing the place.

The original building had been a palace of the Bishops of Norwich. At the Reformation it passed to King Henry VIII, and several of his acts, including amongst others the deed by which Beaulieu Abbey was made over to the ancestors of the present Lord Montagu, are dated from "Terlynge." Henry VIII sold it to Lord Chancellor Audeley. Nothing

[1] Pronounced Tarling.

[2] I do not know anything of Sir Matthew Featherstonehaugh's career, but he seems to have been sympathetic to science, if one may judge from the fact that he presented haunches of venison to the Royal Society Club and in acknowledgment was elected an honorary member on August 27th, 1752. See Sir A. Geikie's *Annals of the Royal Society Club*, 1917, p. 30.

of the original Tudor mansion remains; the greater part of it was pulled down by John Strutt about 1765, and the remainder by Colonel Joseph Holden Strutt about 1820. The brick wall separating the grounds from the churchyard consists, however, of the small bricks characteristic of the Tudor period, and it is probable enough that Henry VIII walked beneath it. Two large oak trees in the garden, 18 and 23 feet respectively in circumference, also very probably date back to his time or earlier.

The old kitchen garden was beneath the wall mentioned, and the wall itself was used for supporting fruit trees. It stands at a higher level than the house, and a steep terrace or embankment was made by Colonel Strutt at an expense perhaps hardly justified by the effect obtained. The terrace is mounted by a steep flight of stone steps, facing the front door at a distance of about 140 yards, and the musical echo from these was found in after-years to give a valuable illustration of the action of a diffraction grating. In the background above the steps were a fine group of cedars of Lebanon, now, alas! almost gone. These, with the steps, formed suitable objects for the practice of pinhole photography, in illustration of the studies of resolving power made in 1891.[1] The church weathercock, which is seen against the sky from the northern windows of the house, was likewise used as a test object in these studies.

The present house is built of white brick, and consists of a central block, built by John Strutt (probably about 1765), and two long wings built out to the east and west which were added about 1820 by Colonel Joseph Holden Strutt, who adopted the idea from Carton, the Irish seat of his father-in-law, the Duke of Leinster. Part of the original Tudor buildings remained, serving as kitchen and servants' quarters of the house, but they were pulled down when the wings were added. The east wing replaced them, the west wing consisted of outbuildings and stables, and was not originally connected with the body of the house, except by a blank wall.

[1] Rayleigh, *Scientific Papers*, Vol. III, p. 439.

Later (1850), when the subject of this biography and his brothers had to be housed, these rooms were converted into living-rooms, and a lean-to conservatory and passage were added along the blank wall that has been mentioned to give access from the body of the house.

When he himself succeeded to the place, they were utilized as quarters for himself and his wife, as well as study and laboratory. The rooms as arranged at that time will be described in a later chapter.

To the south of the house lies the park, bounded on the west by a small stream, known as the Ter.[1] The Ter runs through the garden, part of which lies on the far side of it. In this part lies the "Swan Pond," a small lake fed by two exceptionally abundant springs which afford the village water supply, with a considerable surplus, which runs through the pond, and out down a steep declivity to the river. There is an appreciable amount of water power to be had here, and it is evident that the pond was to a great extent the result of an artificial embankment to impound the water for working a mill-wheel. The mill was pulled down by Colonel Strutt, and no trace of it remains. Later, as will be narrated, advantage was taken of the water power for scientific purposes.

Returning from this digression to the ancestry of the family, John Strutt above mentioned (1727–1816) was M.P. for Maldon, and although he does not appear to have made any particular figure in Parliament, he showed a sturdy independence of character on one occasion. This was in 1779, when a vote of thanks to Admiral Keppel was carried through both Houses of Parliament, and his was the only dissentient voice.[2]

So far as family tradition goes, this adverse vote appears to have been given from public motives. Disapproval of the Admiral's conduct of an action against the French off Ushant had been very general, and public opinion had veered round for reasons which Strutt thought were inadequate. He had

[1] Pronounced Tar.

[2] See Lord Stanhope's *History of England*, Vol. VI, p. 258 (Cabinet edition).

the moral courage to maintain the opinion he had formed. There is no reason to think that his character was perverse or unduly self-opinionated.

He married in 1756 Anne Goodday, and died in 1816. It may be of interest for the history of manners and customs to mention that he is said to have been the last man to wear a pigtail in the House of Commons. There is a portrait of him at Terling by Wilson, and another by Zoffany, which was acquired by the family three generations later.

Joseph Strutt, the author of *Sports and Pastimes of the English People*, was his second cousin.

John Strutt had three sons: the first, John, died young; the second, Joseph Holden, succeeded him at Terling; and the third, William Goodday, had a distinguished military career, which will be referred to later.

Joseph Holden Strutt (1758–1845) was appointed at the age of twenty-two Lieut.-Colonel of the West Essex Militia, and subsequently as Colonel commanded at different times four regiments of Militia. Three of these were formed and drilled (and two raised also) by himself. The fourth was the West Essex Regiment in which he originally served, and of which he became Colonel in 1821.

The other chief activity of his life was parliamentary. He succeeded his father as M.P. for Maldon, and sat in Parliament from 1790 to 1830, giving steady support to the Tory party.

He married, in 1789, Lady Charlotte Fitzgerald, whom he had met at Toulouse, in France. She was one of the numerous children of the 1st Duke of Leinster, and one of her brothers, Lord Edward Fitzgerald, later acquired an unhappy notoriety in connection with the Irish rebellion of 1798. Colonel Strutt had no sympathy with this.

Colonel Strutt's public services were rewarded with an offer of a peerage. He gave an account of this matter, in a posthumous letter to his daughters summarizing his career, as follows:—

"In the midst of my public services, when I was at a levee, Geo. 3rd. in the presence of his own and foreign ministers and about

30 other Nobles and Gentlemen (for in those days Levees were not numerously attended because they were weekly) the King, in walking round the circle, as was usual in those days, when he came to me, He stopped and said 'We must reward you.' Such a peculiar gracious favour, in the presence of the world, being voluntary, proved that his Majesty's mind was full of approbation of my independent conduct, active for the state, and he thus chose to make known his opinion.

"Lord Liverpool in the time of his Majesty George 3rd. wrote to me that He would lay my name before His Majesty for a Peerage and at the Coronation of George IV. the Peerage was granted and I own I had pride that I stood so high in the opinion of the world as to merit such a high distinction, I had through life declined personal honours. And it was granted as I requested, in the person of your Mother Lady C. Strutt. I have lived long enough to repent of this my then determination, I wished honor to my family, but I find it should have been in my own person."

It is difficult in retrospect to understand Colonel Strutt's attitude in this matter, and as he explains he regretted it later. On his wife's death in 1836 the peerage descended to his only son, John James Strutt, during the father's lifetime.

The territorial title was taken from Rayleigh, a small market town in Essex. Colonel Strutt had no special connection with the place, beyond owning some property not far off, and the name seems to have been chosen because it was considered euphonious.

Colonel Strutt's only son, John James Lord Rayleigh (1796–1873), was educated at Winchester and Oriel College, Oxford. He joined the Militia in 1813 and rose to be a Major in the Eastern Battalion of Essex Militia. He resigned from this in 1832. In early life he was (I believe) fond of society. He was a frequenter of Almack's, the fashionable assembly rooms of the day, and one of his sons remembers his saying that it was a proud moment when Beau Brummel asked him to take a glass of wine with him! Later, his mind took a more serious turn, and religion of an evangelical type became the mainspring of his life.

It was contemplated that he should succeed his father and

grandfather as Member for Maldon, but he eventually decided that this was not compatible with his religious life. He gave up hunting, of which he was very fond, for the same reason, when he was about twenty-eight years of age. It does not appear that he thought such pursuits inherently wrong—he judged for himself only, and in after-years enjoyed hearing from his younger sons of their hunting experiences. He thought it unreasonable to expect the farm labourers to continue their work when the hounds were in the neighbourhood. The temptation of such a spectacle was more than anyone could resist.

For a time he was associated with the Irvingite movement. His attitude in this was displeasing to his father, whose religion, though earnest and sincere, was of a more old-fashioned and conventional type, and for a time he was only received at home on giving a pledge that he would not speak to his sisters on such matters. It would seem that later he abandoned the more extreme of the views that he had held, but to the end of his life the religious side of his interest was maintained. His reading was chiefly of a theological kind. It was his custom at family prayers to expound the passages from the Bible that he had read, sometimes at a length that was displeasing to the younger members of the family. At times, for instance, when his sons were leaving for school, he would suggest a prayer together.

As already mentioned, the general tenor of his views was evangelical, and the religious colour of the household may be illustrated by an incident recorded in after-years by his eldest son.

"As a child, I confided to a playmate: Do you know, my Mama once *saw a Roman Catholic*."

Dancing, if not forbidden, was discouraged. Cards were introduced during a temporary absence from his family, and there was some anxiety as to what his attitude would be, but to their relief the innovation was approved.

Apart from religion, his interests and occupations were those of a country squire. He farmed a part of his estate

in collaboration with his bailiff, Charles Richardson, who, though illiterate, was a man of great natural acuteness and a favourite with his employer. Rayleigh was practically his own estate agent. He was a pioneer in the provision of allotments, and arranged for them to be available to the labourers at Terling as early as the 'thirties of the last century. He acted as Chairman of Quarter Sessions, and of the Witham Board of Guardians for administration of the poor law. A piece of plate was presented to him by the Guardians in 1842 "in token of the high esteem they entertain for him, for his conduct as chairman of the Board since the formation of the union, and to evince their gratitude for the urbanity, kindness and judgement displayed by him on all occasions."

There is no reason whatever to think that either he or his father or grandfather had any aptitude for scientific or mathematical pursuits.

The same may be said of his younger sister, if we may judge by the following extracts (1842):—

"I never see the steam engine without thinking of the Devil, its puffing and snorting always make me imagine it has a life."

And again: "I hope you got the drawing of the sky engine —surely that can never be of use, while man remains on earth his energies may be useful, but if he soars to the sky I think he will not be permitted to succeed."

Lord Rayleigh married in 1842 Clara Elizabeth La Touche Vicars, who lived with her mother, the widow of an officer in the Engineers, at Shenfield, about 20 miles from Terling. Miss Vicars was only seventeen years of age, while the bridegroom was forty-six; and it had been supposed by neighbours that Rayleigh, in his visits to Shenfield, was courting not the daughter but the mother, whose age was more suited to his own.

The Vicars family were of Irish origin, and if my father's scientific gifts can in any degree be traced to his ancestry, it is perhaps through them. His maternal grandfather, Captain Richard Vicars, and his maternal great-uncle, Major-General

Edward Vicars, were both in the Royal Engineers, and the latter was an officer of some distinction.

His maternal grandmother (*née* Williams) was descended from Major-General John Armstrong, Surveyor-General of His Majesty's Ordnance and Chief Engineer of England (d. 1742), and it is perhaps worth while to mention that the first Lady Rayleigh was descended from a brother of the celebrated physicist Robert Boyle.

But to go back to Lord Rayleigh's marriage.

Clara Vicars was one of a family of five. Her brother, Captain Hedley Vicars, who was killed in the Crimea, is well known as an evangelical, from Miss Marsh's biography,[1] where frequent references to his visits to Terling will be found. It may be remarked in passing that there is no reason to think that there was anything in his unregenerate days that would have weighed heavily on the conscience of an average man.

Lord and Lady Rayleigh took a lease of Langford Grove, near Maldon. This was within driving distance of Terling, but Colonel Strutt thought it best that they should not be too near. They took an active part in county society, and frequently visited Terling and Tofts, Little Baddow, where Major-General William Goodday Strutt (1762–1848), Colonel Strutt's younger brother, lived in retirement.[2] General Strutt had seen much service in the French wars. His active military career was brought to an end by the loss of his leg at St. Vincent in the West Indies in 1796, and his services were rewarded in 1800 by the sinecure office of Governor of Quebec—which he never visited. Not much is on record of his relations with his great-nephew except his annoyance at the child not being able to speak intelligibly at the age of three.

It was at Langford that Lord Rayleigh's eldest son, John William Strutt, was born on November 12th, 1842. He was a seven months' child.

[1] *Memorials of Captain Hedley Vicars, Ninety-Seventh Regiment*, by the author of *The Victory Won*.

[2] A notice of him will be found in the *Dictionary of National Biography*.

For his early years, the best information available is from an account written by his sister Clara, afterwards Mrs. John Paley, who was only two years younger than himself,[1] and I propose to quote textually most of the earlier part of this, as any attempt to paraphrase or seriously condense it would be certain to spoil its freshness.

"I have always been told that my brother John was slow in learning to speak. As far as I can remember he was said to be three years old before he could talk, but was a very intelligent child, taking great notice of everything. My mother said when she took him out driving he would point to one object after another saying, 'Ah, Ah, Ah!' and not be satisfied till she said, 'Yes, that is a house' or 'a dog' or even 'a tree.' When first seen by his grandfather Col. Strutt the observation made was 'That child will either be very clever or be an idiot.' It was I imagine the shape of his head that drew forth the remark.

"He nearly met with his death at Langford Grove when about twenty months old, the Nurse left the door open, and he got out and tried to walk downstairs. He rolled from step to step bumping his head on each one, but fortunately was saved from the final bump by a maid who was coming upstairs and caught him. His little head was black and blue afterwards.

"This was the first of many narrow escapes in his boyhood. Once at Southend he fell off a jetty into the water, a rough sea, having over-balanced himself in the excitement of throwing sticks into the waves for a dog to fetch out. Col. Hearn who was talking to my mother on the beach rushed into the water and pulled him out. I can remember thanks being returned in church by 'the parents of John William Strutt' for his preservation on that occasion.

[1] This note was written at my request about 1909 and was found among her papers after her death. It seems to have been left unfinished, but no doubt the gradual and inevitable separation of interests as they grew older made a satisfactory conclusion difficult, and it contains most of what the writer was specially qualified to tell.

"Later, at a picnic at the Rodney[1] there was some firing at a target. Sir Claud de Crespigny[2] was just about to pull the trigger of his gun when he turned faint at the sight of John just in front. It was said he went to look in at the other end of the gun, but I believe it was only carelessness.

"He early showed an enquiring mind, Aunt E. (Miss Strutt) writing to her sister when he was about 4 years old, says, 'Little John is quite conversible he asks a thousand questions how things are made, and what for, that sometimes I hardly know how to answer. Yesterday he asked me what became of the water spilt on the tablecloth after it was dried up.'

"He loved to see the Moon, and could be tempted to the nursery at bed-time by the hope of seeing it out of the window.

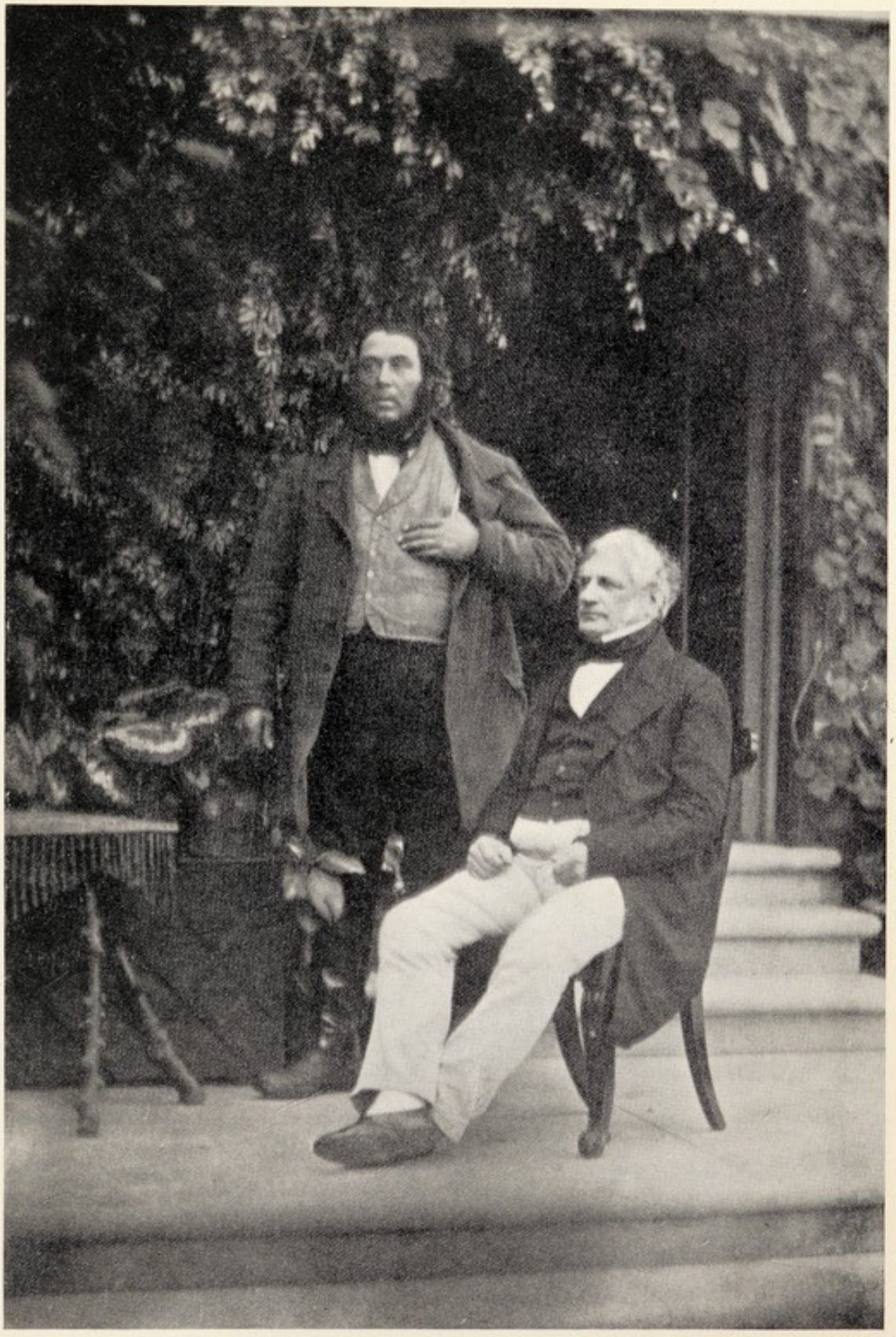
"He was very tender-hearted, shedding tears at the sight of a pet sheep which had lost its companion and was left solitary.

"He showed no aptitude for learning to read and was considered backward, but I think he learnt by heart easily. I remember his repeating the 119th Psalm, and there was a long poem about the stars and their distances from the earth that always impressed my Father greatly when he recited it.

"When he was 8 years old 'Uncle Ned' my Mother's brother gave him lessons [in Latin] and I remember how amusingly he used to describe the process saying that John when in difficulties over his declensions invariably looked up the chimney as if he expected to find them there. At that time he was not a reader and rarely amused himself with a book.

"We had a large box of cubes made I believe by our own carpenter. On one occasion John inadvertently knocked down an edifice I was proudly building. I at once rushed at a very fine bridge he was constructing and deliberately destroyed it, and said in my rage, 'I am glad I have knocked it down.' I can remember his face all working with distress at the destruction of his efforts, but he only said, 'And I am sorry that I knocked yours down.' I remember turning away

[1] A heath at Little Baddow. [2] Third Baronet.



JOHN JAMES, LORD RAYLEIGH, WITH CHARLES RICHARDSON, IN THE CONSERVATORY AT TERLING, CIRC. 1865.

From a wet collodion photograph by the subject of this biography.

overcome with shame and remorse, and it was always an illustration to me of the soft answer which turneth away wrath.

"About 1852 we were taken together for a week's visit to London. There we visited . . . . . and the great globe in Leicester Square. He remarked that it represented the places the wrong way on the inside instead of the out. At the Polytechnic he was much interested with watching glass being spun etc. and would have liked to go down in the diving bell, but that was not allowed in deference to Aunt E's fears. During the visit to London we were taken to the House of Lords. He was taken to the steps of the Throne and stood there [1] while a debate was going on. While he was standing on the steps in came the Duke of Wellington on whom we gazed with awe and admiration. He stopped and looked at the slight little figure solitary there and seemed about to speak to him but John turned shyly away and the Duke passed on. My Mother watching from the Gallery was much vexed that he had thus lost the opportunity of possible notice from the great Duke. The next time we went up to London it was to attend the great Man's funeral. . . .

"There is no one at the present day who in the least holds the position that he held in the public estimation at that time. There was then often talk of a French invasion, but I remember thinking we were perfectly safe as we had the Duke of Wellington. Later on we had to be content with the possession of three great uncles who were Colonels, which we thought much added to our personal security.

"Aunt E.[2] was the most indulgent of Aunts and rarely denied us anything. . . . She took in the *Family Friend* for John's benefit and there he found an account of an experiment with gun-powder and sent to Witham for the ingredients. The chemist when fulfilling the order sent a warning to the authorities of the dangerous nature of the goods he was supplying, and poor John was severely taken to task and the chemicals confiscated. This is the first instance I can remember in

[1] The eldest sons of Peers have this privilege.

[2] The Hon. Emily Anne Strutt.

which his bent was clearly shewn and I am not sure whether it was before he went to school, but I think it was, and I can well remember his shamefaced appearance when his project was discovered."[1]

I must now go back somewhat in time and supplement this narrative from other sources. The following was recorded by himself.

"When a young child my mind turned on mechanical matters and I was much exercised when I saw a train with two engines. I argued that singly one engine would be faster than another, and if so, what was the use of the slower one? The real question of course is whether one engine would be faster *alone* than the other *drawing the train*?"

He consulted his nurse about the difficulty, but failed to obtain any satisfactory reply. He told this incident in an after-dinner speech in his later years. After he had sat down, Bishop Creighton, who was sitting next him, confessed that he was still in the same position!

Other incidents of childhood were narrated as follows:—

"In my very early years I was much puzzled at what made people laugh as when (aged three) I remarked a certain green velvet frock cost 3/6 (I don't remember the figure) a yard.

"Or when, at a brewery, I was given some ale to taste, and probably after a grimace thought it right to say 'It's very nice.'

"As a child I used to come down to dessert (dinners were early) and get some pickings: standing between my father and a lady guest, the latter asked me how old I was, and before I could reply my father said I was *five*; whereas I was really *six*. I was too timid to correct him. I wondered what I had done that it should be said I was five when I was really six. Even if I was in the wrong I did not think it right of him to punish me in this particular way. It was long afterwards that I thought it was perhaps a mistake on his part."

A boy's opportunities of getting any glimpse of science in the country in those days were very limited, if, as in this case,

[1] It seems, however, that the project was still secretly entertained: for one of his brothers remembers a train of gunpowder being laid along the underground tunnel which connects the west wing with the cellars under the body of the house.

none of his elders took any interest in such subjects. There were no motor cars, no electric bells or electric light, no telephones, none of the appliances in fact which now bring the modern developments of science into every household.

One of the first opportunities arose when a lecture was given in the village (I think by a neighbouring clergyman), and the lecturer demonstrated that an egg would float in salt water, though it would sink in fresh water. This was extraordinarily interesting and impressive. He was delighted, and determined to repeat the experiment for himself.

He was sent to school in 1852. A memorandum by his father runs as follows :—

"My dear Firstborn, John William Strutt being ten years old last November 12th, I thought it necessary he should learn classics, which he had not the opportunity of doing at home, I looked out for a preparatory school wishing to send him eventually to Harrow, but the Lord not seeming to me to make the path plain and seemingly making it easy to send him to Eton where provision was made for little boys, with much prayer and anxiety and too little faith I with his mother accompanied him and left him at the Rev. J. W. Hawtrey's April 12th, 1853.

"He was delighted at the prospect and with the reality of going to school."

He only remained at Eton one half, and a good part of the time was spent at the sanatorium, recovering from a mild attack of smallpox. In the holidays he and his sister caught whooping cough, and this prevented his return to Eton. It was thought best that he should remain at home for a time with a tutor.

After a few months he was sent to Mr. George Murray's school at Wimbledon Common. There he received what he afterwards regarded as a satisfactory training in the elements of algebra, and made a beginning on trigonometry and statics. Mr. Murray wrote, after he had left: "It is not often that a tutor has so easy and pleasant a duty to discharge as to instruct such a pupil."

It was there, I think, that a prank of some ingenuity was played. It consisted in blowing air into the gas pipes. This would be done a few minutes before prayer time, in an adjoining room. The air would gradually diffuse back from the branch pipe into the main, with the result that the light would go out during prayers : the beauty of the device was that it provided the mischief-makers with an apparently satisfactory alibi. This was a valuable feature, as Mr. Murray was a severe disciplinarian. I was never told explicitly who was the author of this idea, and I believe it was practised at other schools about that time. However it certainly has a scientific aspect.

The following, undated, must have been written in 1856, when the trial of Palmer, the Rugeley poisoner, took place. The writer was then thirteen years old.

WIMBLEDON, *Saturday.*

It has been a showery day. The fellows all around me are talking about and discussing history. They have got up a history debating society upon such questions as Elizabeth was a good or a bad queen and such like ; I have refused to be a member of it, but I believe the fellows are trying to get up a scientific debating society of which I intend to be a member on such subjects as Astronomy, Electricity, and such as that. Old Neale the master we say our Latin to and who is very old told me (he lives close to Rugely) that when he was lodging at Rugely at the house of the sister-in-law of George Bates (the fellow Palmer is supposed to have poisoned and who was a sort of Jack of all trades to whom W. Palmer owed 20£) she told him that Palmer offered to lend Bates 200£ on condition that he would allow him to insure his life. Bates wrote to his brother in law whether he should except [*sic*] the offer or not who happened to be away from Rugely at the time his brother in law wrote back to him to tell not to do anything till he came back and when he had come back he told him by no means to except it and if he did that he would be dead in two or 3 months which took place. Palmer named 1 of his horses " Golfinder " and another " Strichnine " which seems to imply that strichnine was his goldfinder which he tried to fulfill. We have done with Trigonomitry [*sic*] and have begun statics which treats on forces.

I remain your affectionate son

J. W. STRUTT.

A favourite game at Terling in the winter evenings was "I spy." The house is large, with many cellars and passages in the basement, which afforded mysterious hiding-places. The dim illumination of those days, when candles and lamps burning colza oil were the only illuminants, tended to enhance the effect, and John Strutt was ingenious in devising new and unsuspected hiding-places. The large central saloon had been decorated about 1820 with a plaster reproduction of the frieze of the Parthenon, in consequence, no doubt, of the interest aroused by the recent arrival of the original sculptures, brought from Greece by Lord Elgin. John Strutt would get over the balustrade of the gallery, and lie down on the cornice below the frieze. As the cornice is only 8 inches wide, and is 10 feet 4 inches above the floor, this seems to have been a feat of some daring. In later life he had not a good head for climbing.

It was his father's custom, probably in order to stimulate an interest in agriculture, to allow him to keep pigs at Terling on food provided gratuitously. His pocket money came from the sale of these, and he wrote home from Wimbledon: "My pigs pay very well, so I do not mind being called a pig-jobber." He was already dabbling in chemistry at this time. He speaks in one letter of writing in invisible ink. It was his custom during the holidays to procure supplies from the local druggist at Witham, and he would think nothing of galloping home on his pony with a bottle of strong sulphuric acid in his pocket.

His sister wrote: "During all these years his scientific tastes were developing. He used to have strange bottles in the book-room or in the landing outside where there was a convenient sink. He had magnets and an electric machine, and gave us all shocks with it. On one occasion he burnt himself severely with phosphorus, and I fancy his fingers still bear the marks. He was in the book-room and somebody ran into the schoolroom close by where I was doing my lessons with my governess to tell us. He had run away in great pain eventually finding him with his hands in a bucket of water in the stables." The accident occurred owing to his incautiously holding the stick of phosphorus in his hand while writing on

the wall with it. This incident was mentioned at an after-dinner speech at the Chemical Society, and Lord Kelvin who spoke afterwards, said, "Lord Rayleigh has told us that he burnt his fingers with phosphorus when he was only twelve years old. I burnt mine in the same way at eighty-two!"

But to go back to the time of his boyhood: his friend and contemporary E. R. Bernard [1] wrote: "He had a lively appreciation of anything humorous, and was excitable to a degree which seems to those who recollect it very much in contrast to the composed manner of his later days."

At the age of fourteen he was sent to Harrow, which it was thought might suit his health better than Eton had done. But his stay there was almost as short. A remark which I can call to mind was that he was compelled to do the equations set in algebra for half the boarding house (E. Vaughan's) that he was in. "I thought this was a form of bullying," he said, "and it did not occur to me till afterwards that they could not do them themselves.

"The only people I feel inclined to nourish a grudge against are those who bullied me at school."

But I have no reason to think he was more bullied than the generality of his contemporaries. The few letters from Harrow which survive dwell chiefly on the desire to be exempt from football, which was too much for him, and this was arranged by his parents, who wrote to thank the captain of the house, George Trevelyan (afterwards Sir George Trevelyan, well known in politics and literature), for meeting their wishes.

In the course of his second term at Harrow serious fears of chest trouble arose, and he was taken away. This anxiety lasted for a long time afterwards. There were grave doubts as to whether he would live, and he himself must have realized this, as he made some reference to it at the annual dinner given by his father to the tenants on the estate. "I had not expected to be with you," he said in replying to the toast of his health.

These tenants' dinners at Christmas time were a great

[1] Afterwards Canon of Salisbury.

institution, and there he made his first attempts at public speaking, and it is recorded that as a little boy he joined vociferously in the cheers for himself! His mother used to say that he did this to hide his nervousness, and that had he abstained he would probably have burst into tears.

In the next autumn (1857) he was put under the care of the Rev. G. T. Warner, who took pupils at Highstead, Torquay. Here he remained for nearly four years, and amid much more congenial surroundings than at the schools where he had previously been. Mr. Warner must have held rather severe views of what was decorous. One rule of the establishment was that no unmarried lady of less than sixty years of age was to be mentioned at the dinner-table. As might be expected, restriction of this kind only stimulated curiosity. Thus on one occasion Mr. Warner surprised several of his pupils standing on a table in the window, crowding to get a glimpse of a lady caller. It was difficult to find appropriate words to express his feelings.

The following must have been written in the autumn of 1858, when Donati's Comet attained maximum brilliancy :—

TORQUAY, *Sunday.*

MY DEAR PAPA,—

I am writing with a pencil simply because I have no ink up here. I am getting on much better with mathematics now, classics too. . . . We are doing Thucidides, Euripides, Cicero's epistles, and Horace. I have also begun Greek Iambics. . . . I went again to Hunt's the other night and slept there and came back earlier the next morning. N.B. There may be attractions there which you do not know of. Cricket is over this season. Of course you are looking at that beautiful comet in the south-western heavens which is engaging so much attention. But what gives it especial interest I think is that *we* shall never see it again if anybody ever will, as it does not come back probably for several hundred years. It is now moving at an enormous rate as it is near the sun, but when far away altogether outside the planets it moves so slow as only to get over perhaps a few inches in a day. Adams the great astronomer that discovered Neptune when an undergraduate at Cambridge and who is said to have been as much (above) the 2nd Wrangler as the 2nd was above the last,

thinks it will never come back at all. It attains its maximum on the 6th. The weather here is very cold.

	£	*s.*	*d.*
Ticket	1	8	6
Cab		2	6
	1	11	0
	1		
		11	0

I remain your affectionate son, J. W. STRUTT.

I consider I have written a very decent epistle.

In another letter, probably of about the same date, he writes :—

"Fawcett and S [1] are inventing a Russian or some such grammar, which we are teaching a small boy who firmly believes it to be genuine. He is doing the nouns. I enclose for your edification or otherwise some verses written on the leading members of the place of course ourselves included."

And again :—

"Leeming the mathematical master who had gone away has come back I am glad to say, as —— was about the most consummate ass I ever came across. You see I am not particular as to the wording of my opinions. I send you a copy of some perversions of Watts' hymns which we devised."

His greatest friend at Highstead was A. R. Hunt, whose home in the neighbourhood he sometimes visited, and with whom he kept up a correspondence to the end of his life. Hunt had scientific interests and later wrote papers on problems of petrology which were published by the Geological Society.

Another schoolfellow was F. Grenfell, now Field-Marshal Lord Grenfell. I hope it is not indiscreet to reveal that my father credited him with introducing the practice of betting at Highstead. This, perhaps for want of ready money, was conducted by barter, and my father used throughout his life

[1] Self.

a set of drawing instruments which had been won from Grenfell at Mr. Warner's.

Yet another schoolfellow was the late H. M. Hyndman, afterwards well known as an exponent of Socialism. Mr. Hyndman in 1919 kindly favoured me with some reminiscences, and the following is somewhat abbreviated from what he wrote. Part of it refers to a later period.

"Lord Rayleigh in 1857 undoubtedly was, and looked at that time, by no means strong. Tall, slender and studious in appearance it seemed little likely he would reach the age which he happily attained. He was, I remember, very popular with all of us, though the life we led apart from our studies was of a rather more active kind than he was able or felt disposed to indulge in. About his ability there was no doubt whatever and I was able to form some judgment of it as like himself I was specially studying mathematics.[1] In fact given good health the mathematical tutor at Highstead, Mr. Lewis Hensley, was even then confident that he would be able to take a very high degree at Cambridge. I saw this gentleman, then a very old man, at Falmouth about four years ago, and congratulated him on the accuracy of his forecast as to Strutt's future career at the University. Hensley said to me then that he did not pretend to be a great mathematician himself, but thought he could tell one when he saw his work. It is certain that Lord Rayleigh took this assurance as to what he was capable of with the same modesty that distinguished him when he had achieved all and a good deal more than all that was thus early predicted of him.

"While at Cambridge I met him not unfrequently and renewed the earlier friendship, though as, fortunately or unfortunately, I was more addicted to pleasure than to study during my three years of university life, our paths lay a good deal apart. He seemed to me to be not at all the worse physically for his steady reading, and he became, if my memory serves me, a very fair tennis player—the fine old court game, not lawn tennis. His coming out as senior Wrangler and first Smith's Prizeman rejoiced all his fellow students of years before, and fully justified Hensley's prediction already referred to.

"Afterwards I saw Lord Rayleigh, as he had then become, several times in London. Once on a (to me) memorable occasion

[1] My father several times mentioned that Hyndman did better than himself at mathematics at Highstead.

I came across him at a well-known house in Grosvenor Place. It was on the evening of the day when I had been acquitted of having caused a riot and fomented the sacking of shops in Piccadilly and South Audley Street a few months before. Socialism was not popular in society then. I doubt if it is even now more than thirty years later. At any rate it struck me people whom I knew looked a bit askance at me. Lord Rayleigh, however, the moment he saw me came straight across the room, shook hands with me and congratulated me warmly on having got handsomely out of such an unpleasant business. This I thought was just what might be expected of him.

"I much regret my old friend's departure from among us. Yet what can any of us desire more than a full useful distinguished career such as his? He 'warmed both hands before the fire of Life.' All felt the loss when that fire sank. His was a fine work well done throughout, and no man ever showed less consciousness of his great genius than he."

John Strutt soon established himself as a favourite pupil with Mr. Warner, and indeed his confidence went so far that he allowed him to choose his own times for work, instead of tying him to the fixed hours of the establishment. Mr. Warner tried to induce him to give up mathematics for classics, which was his own favourite study, but, needless to say, without success.

He competed for a minor scholarship at Trinity, Cambridge, in the autumn of 1860, but did not succeed. On his return Mr. Warner said, "Well, John has failed once, but he will never fail again."

Although he did what was required of him in classical lessons, I do not think his interest was ever aroused by them in the slightest degree. I have never heard him make any allusion to classical literature. On the other hand, I have heard him congratulate himself on a narrow escape when a fellow-guest at a country house—a well-known scientific man, by the way—had proposed to occupy a wet afternoon in reading aloud a much admired English version of a Greek play.

Some remarks he made in taking the chair at a conference "on the neglect of Science" (1916) are worth quoting in this connection:—

"I was myself an average boy, in classical matters anyhow, and I can speak from experience. I was not behind the average; but I know that the long years which I gave to classical work were to a very large extent thrown away, although I have no doubt I got something from it; but any idea of attaining to an appreciation of the language and literature of the Greeks, in my own case, and in the case of most of my friends, was mere moonshine."

A diversion of this period at Mr. Warner's was to send Milton's sonnets as original contributions to the local newspaper, which did not fail duly to publish them as such.

From a letter to his mother (Highstead, Torquay, *Oct.* 1860):—

"There is a fellow just come called Betts whose father is a partner of Sir M. Peto's. He has had the crystalline lens taken out of his eye and has consequently to wear intensely convergent spectacles which magnify his eyes to huge proportions.

"I was asking Leeming [1] the other day about my controversy with Uncle Ned about the propriety of jumping forwards out of a vehicle in motion. He of course at once said the velocity and consequently the violence of concussion was as much increased by jumping forwards, as it was diminished by doing so backwards. There is of course no question about it. . . . Papa is I believe taking measures for getting me entered at Trin: Coll: Cambridge."

The following further extract abbreviated from his sister Clara's notes will give an idea of his holiday occupations during the Torquay period.

"Photography which he began when about 15 was a great interest. My mother used then to help him. The photographs were taken in the conservatory. Bottles and instruments of various kinds increased, there was a high table in the book-room on which was a bagatelle board and this table seemed useful for experiments, and instruments covered the board. Afterwards when I came out and the governess

[1] The mathematical master who succeeded Hensley at Highstead. He was a Cambridge Wrangler, and Mr. Warner credited him with the chief share in my father's mathematical education there. There seems to be some discrepancy with Mr. Hyndman on this point.

departed he got possession of the old schoolroom where he could keep things more undisturbed than in the book-room where family prayers were then held. Slits were made in the shutters to let in solitary rays of light, and he was given undisputed sway. He never had a more regular laboratory in his bachelor days. He did not confine his interests entirely to science, he sometimes rode, and on a few rare occasions followed the hounds, he played cricket and football with the menservants. He was a very good croquet player. I cannot remember his shooting though I think he must have tried it, but he shot with a rifle at a target. He got very proficient in this.[1]

"Edward Bernard, the son of our vicar, just four months older than himself, was his chief companion in these things.

"John was in Ireland the year that the British Association met in Dublin, and I believe attended the meetings. He and my mother were probably staying with my Uncle Ned at Trimleston Lodge, Dundrum.

"He used to be much chaffed for his facility in losing his heart. It began when he was about 13 or 14. The object of his affections was generally a cousin or intimate friend of the family several years older than himself, and a new one arose usually each succeeding holidays and it often happened that the object of the Easter holidays devotion was entirely neglected when the summer holidays began."

The pursuit of photography, referred to by his sister, was a valuable training. In those days of the wet collodion process, when the photographer had to coat and sensitize his plate immediately before use, very much greater demands

[1] He wrote from Torquay (1859 ?): "I think as the spirit of rifle corps (or according to the French Emperor 'epidemic') is ravaging the country, I should like to get a rifle and practice." With a shot-gun he had no success. I remember that when in after-years I began to shoot myself, his old muzzle-loader was assigned to my use. I complained of the tediousness of loading it, but my father had no sympathy whatever with this. His policy was to make things do, and he silenced me by saying that when he was a boy no one had anything else.

were made upon the skill and perseverance of an amateur than at present, and the opportunities of picking up scientific experience were proportionately increased. The process was comparatively new, having been introduced in 1851. The first beginning in photography that he made is probably to be found in a print still extant of a fern leaf on writing paper, sensitized with nitrate of silver and common salt, presented to his mother December 31st, 1857 (*æt.* 15). Wet collodion portraiture followed, and the various dry collodion processes were successively mastered at a somewhat later date. He went on with portrait photography until a short time after his marriage; after that it was abandoned, being probably crowded out by more purely scientific pursuits: but photography in its scientific applications always had a fascination for him, and there will be occasion to refer to this later on. He wrote from Torquay (May, 1860):—"Our holidays begin on the 17th July, though I must try to make [Mr. Warner] let me go a day before, as I wish to photograph the eclipse on the 18th. A great number of English and American astronomers are going to Spain to see it, as it will be from Spain the greatest eclipse of this century."

In spite of his growing knowledge, relatives of the older generation were not prepared to defer to his views on scientific matters. There had been a severe frost, with the usual trouble of burst waterpipes. Some one implied that the thaw had caused the mischief. *John:* It is the frost that does it, not the thaw. *Uncle* ——[1]*:* Come, John, you can't expect us to swallow that; the pipes leak when it thaws, not when it freezes. *John:* Well, you don't expect the solid ice to flow out of the pipes, do you?

Here is a reminiscence of his own.

"At a country house (Ranston) when I was a boy the conversation turned upon wine, and the host (Sir T. Baker) said to an expert present, 'I wish you would give me your opinion upon some I have of uncertain history.' Much ceremony was observed.

[1] A public schoolmaster, perhaps a little inclined to carry his professional attitude into private life.

The expert tasted solemnly, put down his glass, and said nothing. The host: 'Well, what do you make of it?' 'If you ask me, I should say it was the *vin ordinaire* of some damned country.'"

On one occasion probably about this time, a young lady, Miss ——, from the vicarage, was brought in to lunch, when there was a party of visitors in the house. She was veiled and rather shy, apparently having little to say. She was placed next Lord Rayleigh, who was annoyed to observe his wife and children apparently enjoying some private joke of their own, which, he thought, was rather discourteous to the guest. He tried to cover their rudeness by plying her with anxious civilities, but at this their behaviour grew worse. The *dénouement* was reached when after lunch she flung her arms round a rather prim bachelor who was of the party. He cried out, "Take her away, she's mad, she's mad," when it became apparent that the supposed young lady was the eldest son of the house!

He finally left Mr. Warner's in the early part of 1861, and the summer was devoted to mathematical study under Frederick Thompson, a scholar of Trinity, Cambridge, who was recommended to his father by Dr. Lightfoot, and who went to Terling in order to coach him. I think it was not until this time that a beginning was made with the differential calculus. My father was decidedly less advanced in mathematical reading than the best of his contemporaries at the time when he went up to Cambridge in October, 1861. He worked at the subject because it seemed to him the thing best worth knowing, and not at first with any hope of attaining special distinction in it. Thompson recommended him to go to Routh as a mathematical coach, as Todhunter was no longer taking pupils.

CHAPTER II

UNDERGRADUATE LIFE AT CAMBRIDGE

Strutt was entered at Trinity under the tutorship of Lightfoot, afterwards Bishop of Durham, whose "side" was selected at the suggestion of Mr. Warner. Cambridge tutors, however, only exercise a general supervision and I know nothing to suggest that Dr. Lightfoot was an influence in his life. He was entered as a fellow-commoner, as was customary with the eldest sons of peers and young men of fortune. Fellow-commoners paid additional fees: they wore a distinctive academic dress and they dined at the high table with the dons. The system was severely satirized by Thackeray in *The Book of Snobs*, but my father in later years was inclined to think that it might have been better to retain it. He valued the opportunity of social intercourse with the Dons. His rooms were in the great court (staircase H, No. 4, upper floor) opposite the Hall, and he occupied them till Easter, 1865.

His father did not give him a fixed allowance according to the usual custom, but preferred to discharge tradesmen's bills for necessary expenses himself. Strutt was extremely economical, then as in later days, so much so that his younger brothers, though not extravagant, found that their university expenditure was unfavourably contrasted with his. A fellow-commoner, it was thought, might be expected to spend more than the ordinary undergraduate.

His chief recreation was "real" tennis, the original game played in a court, and his lawn-tennis stroke always showed traces of the style thus acquired. He also rowed on the river, though not strenuously. On one occasion he was upset near Clare bridge, and, being unable to swim, had a narrow escape from drowning.

In the long vacation, he and his friends amused themselves with climbing about the roofs of Trinity College. He regarded the "long" as a valuable time for reading without the interruption of lectures. Routh's coaching classes, however, were in session.

At first he evidently found a difficulty in keeping up with the work at Routh's. He wrote home :—

"I went to Routh on Saturday to get his answer as to whether he would be able to read with me. He asked me a great many questions and finally decided that I should read with him Solid Geometry which is a stiff subject. I went last night at 6, but the others (4) had had a day's start, so that I was not up to them. I read like a trooper last night to get up to them, which I shall do in a day or two. He is very *rough*, at least he will grind me pretty hard I think. . . . The Prince [of Wales] was in chapel this morning and looks exactly like his photograph. Hunt is going to read with Thompson. Both the men in this staircase are nice—Jackson will be high in the classical Tripos next, I hear. We all breakfasted with Jackson this morning . . ."

And again, apparently a few days later :—

"After two attempts to find Routh in and disengaged (which are rarely combined), I at last succeeded. As it was clearly impossible to go on reading geometry of three dimensions with the men I was reading with before, I am going to begin dynamics at the end of this week and in the meantime do integral calculus which is required. All together it is a change much for the better. . . . I have to go to Routh this week at 9 o'c. in the evening—a good thing for a cold. A row of the nature of a town and gown was expected last night but it did not come off. On the 5th Lightfoot was hit by a brick, and on an undergraduate knocking the cad down, he said he was much obliged to him for his good intentions but that he would be much more obliged to him if he would go home to college."

As an undergraduate he worked all the morning, took rest and recreation after Hall, which in those days was at four o'clock, and worked again at night after tea, which he always found much more stimulating than wine. He did not smoke, then or afterwards, and always retained the old-fashioned dislike of the practice, particularly in women. His working

day was about seven hours, which was then considered short for a reading man. His friends often found him on the sofa, apparently doing nothing, and said that he was inventing the stories with which he used to amuse them. But in reality he was no doubt grappling with mathematical difficulties.

When one of these was particularly obstinate the remedy was to "write it out for Routh," and it generally melted away in the process of doing so. He put a high value on Dr. Routh's teaching, particularly the weekly problem papers. In after-years, as President of the Royal Society, he had to refer to Dr. Routh's death. "I was indebted to him for mathematical instruction and stimulus at Cambridge, and I can still vividly recall the amazement with which, as a freshman, I observed the extent and precision of his knowledge, and the rapidity with which he could deal with any problem presented to him. His book on Dynamics is well known. In its earlier editions it illustrated perhaps the vices rather than the virtues of the Cambridge school, but it developed later into a work of first-rate importance. I have always been under the impression that Routh's scientific merits were underrated. It was erroneously assumed that so much devotion to tuition could leave scope for little else."

The "vices of the Cambridge school" referred to, were, I think, to regard a display of analytical symbols as an object in itself rather than as a tool for the solution of scientific problems. I have heard my father say that in the current slang of mathematical students, an absurd distinction was made between "the talk," which meant a statement of the problem, and the premises on which it was to be discussed, and "the work," which meant the purely symbolical part of the treatment. In the same spirit is the almost positive hostility to experimental study and illustration which is revealed in one of Todhunter's essays.

On Dr. Routh's retirement in 1888 a portrait of him by Sir Hubert Herkomer was presented to Mrs. Routh by old pupils. My father was in the chair on the occasion of presentation, and he said: "My thoughts often travel back to those three

or four years of undergraduate life, in which, under Dr. Routh's guidance, I first made extensive acquaintance with those wonderful developments of the human intellect which are called Mathematics. In after-life I think one seldom has so often repeated what is perhaps the highest of intellectual gratifications, namely the sense of seeing one's ways through a difficulty. What was at first obscure gradually becomes illuminated as his thoughts play more and more around the difficulty. It was not far from where we are now assembled [Peterhouse Combination Room] that my mathematical education was for the most part undertaken, and it is, I think, perhaps to the credit of the mathematical system of those days, which it is perhaps now rather the fashion to abuse, and to the manner in which it was worked by Dr. Routh, that I have at no time during what I may call my subsequent scientific career had occasion to regret any of the time I spent on the Mathematical course at Cambridge. Looking back upon the time spent in Dr. Routh's rooms I can remember that what most struck us so often was the extraordinary perfection with which Dr. Routh was able to bring his knowledge to bear on every subject with which he was dealing. Many of us felt we could do pretty well with our subject when we had been working at it perhaps a month or two, but when we had mechanics at our disposal we saw with dismay that our analytical geometry was getting behindhand. Dr. Routh seemed to have everything at his fingers' ends. I am not sure that on looking back I do not agree with Sir James Stirling that the most wonderful thing of all was the patience with which Dr. Routh dealt with successive pupils. . . ."

Strutt as a freshman was willing to discuss mathematical questions with his contemporaries, unlike most young men of that age. One of them writes: "I can still remember a discussion I had with him about some question in chances. But I do not think anyone expected him to be senior Wrangler until he came to the higher mathematics, when he wanted to walk in a light peculiar to himself while we meaner intellects were groping in various shades of darkness." My father

attributed his own success as a mathematical student to his being able to see which were the essential points that it would be fatal to pass over without fully understanding them.

It was his practice during these years to keep a list of the books which he read, both in the way of study and amusement. I have thought it worth while to reproduce these lists for the year 1862. The mathematical list was no doubt prescribed for him, and is merely of interest as an illustration of the curriculum. The novels show very much the tastes of his later life.

The third column shows that at this date he was searching for a basis for definite religious convictions, and the books selected were those which represented the best efforts of the day to find an answer to the kind of difficulties which present themselves to a scientific mind. It need scarcely be said however that complete intellectual satisfaction was not attained by reading them.

1862

Mathematics.	Novels.	Science, etc.
Todhunter's *Differential Calculus.*	*The Woman in White.*	Odling's *Chemistry*, Pt. 1.
Todhunter's *Integral Calculus.*	*Shirley.*	Boole's *Laws of Thought.*
Todhunter's *Conic Sections.*	*Jane Eyre.*	Paley's *Evidences.*
Ferrers' *Trilinear Co-ordinates.*	*The Caxtons.*	Babbage's *Bridgewater Treatise.*
Newton, I and II.	*Orley Farm.*	Whewell's *Bridgewater Treatise.*
Todhunter's *Algebra.*	*Framley Parsonage.*	Herschel's *Discourse on the Study of Natural Philosophy.*
Todhunter's *Trigonometry.*	*Verner's Pride.*	Butler's *Three Sermons.*
Geometrical Conics, Drew.	*Lucretia.*	Whewell's *Morality*, I and II. *History of the Inductive Sciences.*
Analytical Statics.		Thucydides, Book IV.
Dynamics of a Particle.		Virgil, *Æneid* XI and XII.
Herschel's *Astronomy.*		
Newton's *Lunar Theory.*		

Mathematics.	Novels.	Science, etc.
Analytical Lunar Theory.		Virgil, *Eclogues* IV to X.
Salmon's *Conic Sections.*		Herodotus, Book VIII.
Rigid Dynamics, Routh.		Tyndal, *Mountaineering.*
		Matter and Ether, Birks.
		Mill's *Dissertations,* Vol. 1.
		Mill's *Logic,* Books I and II.
		Mansel on Miracles in *Aids to Faith.*

The classical books were read, no doubt for the "little-go" or previous examination, which could then only be taken at the end of a year's residence.

This was an appreciable obstacle to him, and (according to his own account) in the *viva voce* examination the examiners, who were aware of his promise as a mathematician, had some difficulty in finding a question that he could answer, so that they could with decency let him pass. Possibly, however, this rather over-states the case.

Among the chief friends of his undergraduate days were Henry Jackson, (Sir) Sidney Colvin, Charles Lyttelton (the late Lord Cobham), and Lord John Hervey.

During his first term he was invited to Madingley to dine with the Prince of Wales. The atmosphere there was rather formidable on entering, but he got on very well after the first five minutes, sitting at dinner next H.R.H., whom he found affable and talkative. About a month later the Prince left Cambridge, owing to his father's death, and never returned.

John Strutt's twenty-first birthday was celebrated at Terling with the customary rejoicings. The village was decked out with flags and the church bells were rung. He laid the foundation stone of some almshouses which were to be erected on the village green at his father's expense. Lord Rayleigh addressed the villagers, explaining his motives in building the

almshouses, and outlining the programme of the day's festivities. He said: "... When my son has laid the stone Mr. Hill will kindly offer a prayer ... and then you will all go to Terling Place where my son will give a present to each of the widows and heads of families, and all those single men who ordinarily work with me. This done each will have either a horn of ale, which Mr. Ellis [1] will tell you ought to be good, as he took care to make it strong with 10 bushels to the hogshead—or a glass of port or sherry, not both. ... Now I wish you to understand that my motive is a thanksgiving to God for his great mercy in rearing my son and heir to man's estate, and especially because in doing so he has given me a son who is also a friend, inasmuch as we have everything in common between us, and from his infancy he has never given me one anxious moment except as regards his bodily health. ... The inscription on (the stone) is 'J. W. S. Nov. 12th, 1863, aged 21'; so that when some of you may, years hence, pass by and read it you will be reminded of a father doing this as a tribute of gratitude to Almighty God and of goodwill towards his eldest son."

Immediately after his coming of age he was appointed a magistrate and a Deputy Lieutenant for the county.

In the summer of 1864 he attended the British Association at Bath, and was introduced to Sir Charles Lyell, the President for the year. "A very affable man," as he wrote home. He met Lyell on several subsequent occasions: I think when staying with Lyell's brother-in-law, Sir Charles Bunbury, at Mildenhall. I once asked for any recollections: he said that Lyell's interests, so far as he remembered, were very much limited to his geology. "I met him accidentally at Cambridge station, and in two minutes he had me into the Miocene Period." The following is a written reminiscence:—

"Sir Charles Lyell once asked me supposing by some miracle that the rainbow had been suppressed would you physicists have missed it? I was obliged to reply I thought it very doubtful."

Strutt also had a glimpse of Sir John Herschel at the

[1] The steward.

Bunburys' house in London—exactly when I do not know. He was encouraged by the host to "go and tackle him" in the drawing-room after dinner, and was kindly received. The conversation turned upon spectrum analysis, which Herschel considered the most important recent development.

He wrote to his mother in the autumn of 1864 :—

"I was sent for by Dr. Whewell [1] to-day who informed me with much ceremony that I had been elected Sheepshanks Exhibitioner (in astronomy), an honour which I scarcely expected. They say I was first in all the papers. My opinion of the examiners is greatly lowered since they were imposed upon by the nonsense I wrote!"

In the autumn of the same year he attended Prof. Stokes' lectures on optics. This was an epoch in his life, as now for the first time he was brought into contact with a leader in physical science, and one whose mode of thought and expression particularly appealed to him. The notes which he took at these lectures are extant, though they are limited to what could not be learnt from the textbooks then available, or indeed, from any source but Stokes' own writings. Above all, he was delighted with the experimental illustrations. "This was what I liked," he said to me towards the close of his life, taking out the notebook and reading as follows :— "A beam of coloured light was passed thro' a dilute alcoholic solution of the green colouring matter of leaves. The transmitted rays were green, but the parts of the liquid traversed by the rays shone with a deep blood red light." The reference is, of course, to the phenomenon which Stokes had himself been the first to investigate with success, and which he had named fluorescence. He saw, too, Stokes' experiments, unpublished as yet, on the effect of a reducing agent on the absorption spectrum of blood.

The Mathematical Tripos was now approaching. Strutt had gradually drawn to the front among his contemporaries at Routh's classes. Dr. Routh never committed himself as to how his pupils were likely to acquit themselves in the ordeal,

[1] The Master of Trinity.

but "Aunt E." met him on some occasion about this time, and in reply to her anxious enquiries Dr. Routh said, "He'll do." This must have been shortly before she died on December 22nd, 1864. She was an important figure in the home life of his boyhood, having nursed him through the smallpox at Eton and in subsequent illnesses. The earliest scientific books he had on astronomy and photography were her gifts. In after years he reproached himself somewhat for making an inadequate response to her loving solicitude. But probably enough this was its own reward. She had been badly disfigured in middle life by the scars she received in saving her aged and helpless father from being burnt alive when his bed had taken fire at Bath.

But to return to Strutt's work at the University. He had taken care to study the idiosyncrasies of the examiners, and guessed with success some of the questions which Todhunter, who was the apostle of mathematical rigour as then understood, would be likely to set. He was rewarded afterwards by hearing that his answers had passed successfully through these formidable hands. He had improved methods of doing some of the book work, and was gratified to hear that one of the examiners had said that "Strutt's answers were better than the books."

The examination was divided into two parts. There were three days of elementary mathematics, in which the methods of analysis were barred. After this a pass list was issued. Then followed, a week later, five days of higher mathematics which mainly determined the order of merit for the better candidates.

He wrote to his mother :—

"The three days are over, though I am sorry to say not at all satisfactorily. There was only one paper of any importance, namely the Problem Paper. Over this paper I entirely collapsed for some unknown cause. I am afraid it will be almost impossible to make up the lost ground. I did the other papers tolerably well, but went to grief with the only paper in which it was possible to do so. I never remember being so annoyed with myself as I was at the time, and I am far from having got over it yet. The worst

of it is that our year is such a bad one to go a mucker in, as the men are so equal that a few marks will correspond to a good many places. . . ."

Naturally his hopes and fears were canvassed in the family circle. When a formidable rival was named one of his schoolboy brothers made the encouraging suggestion, "Well, he might have a carbuncle in his eye when the exam. comes on."

He was always a good sleeper, but during the days of examination he performed what is perhaps the unique feat of taking a twenty minutes nap after lunch, and before the afternoon paper. He arranged with the college porter to wake him in good time. The earlier papers were almost a writing race for the better men, and he used to tell how he had answered one question during the time that the answers were being collected from other candidates. It was an advantage to be low down in alphabetical order!

The result was announced on the morning of January 27th, 1865, and he came out as Senior Wrangler. Alfred Marshall (afterwards professor of economics at Cambridge) was second and H. M. Taylor was third. Two of his younger brothers came up to Cambridge to hear the result read out in the Senate House, and went to announce it to him; but they lost their way and were anticipated by others, one of whom was W. Aldis Wright and others (I think) Henry Jackson, R. C. Jebb, and Arthur Elliot. He was found in bed, and appeared to take it very calmly.

His degree followed the next day. His mother was present and was probably gratified as she took his arm afterwards to hear a cry of "Three cheers for the Senior Wrangler's *sister*!" Such a mistake was not unnatural, as she was only eighteen years older than himself.

The writer of a leading article in one of the morning papers thought it worth while to point out that there was no reason to fear that the honour had been obtained by favouritism to the heir to a peerage. This is strange reading at the present time.

TRINITY COLLEGE, *Friday* (*Feb. 3rd*, 1865).

MY DEAREST MOTHER,—

The Smith's Prize will come out to-day but . . . I believe not till the evening when it would be too late to telegraph. . . . The Smith's Prize papers are very hard and it is difficult to form an estimate as to how one has done. I went to see Walton one of the Tripos Examiners this morning but I did not get much out of him. I was surprised to hear that both he and Todhunter picked me out as likely to be senior after the 3 days in which I was only 5th.

Did you see the letter in Wednesday's *Times* about me from 'a fellow of Trinity' referring to a statement in the *Times* of the day before? . . . It is said to have been written by Sidgwick. The former statement in the *Times* was very uncalled for. I see they are set upon for making it in the *Guardian* of this week. I was amused last night by a request through Routh to send my photograph to the Editor of the *Illustrated Times* in order that a likeness of me (and Marshall) might be published in that periodical. I shall have nothing to do with it. . . .

Ever your affectionate son,

JOHN W. STRUTT.

The letter above referred to ran as follows:—

Times. Wednesday, February 1st, 1865.

THE SENIOR WRANGLER.

To the Editor of *The Times*.

SIR,—In a paragraph in *The Times* of to-day headed "Honours at Cambridge" it is stated that the success of Mr. Strutt (the Senior Wrangler) is attributed "more to his perseverance than to his brilliant talent." This paragraph is, I think, calculated to convey a wrong impression. Mr. Strutt had not, when he came up to the University, received as much special training as usually falls to the lot of Senior Wranglers, and was, indeed, in knowledge behind many of his contemporaries; moreover although his industry has been unremitting, his delicate health has prevented him from attempting any of those feats of intellectual exertion which are frequently undertaken by candidates for the highest honours. I believe no one who has examined him has any doubt about his singular ability.

I am, Sir, your obedient servant,

A FELLOW OF TRINITY COLLEGE,

CAMBRIDGE.

Jan. 30th.

The Smith's Prize results were announced on February 3rd. He was the first Smith's Prizeman and H. M. Taylor the second.

I think that his early successes at Cambridge gave him more pleasure to look back on than anything he achieved in after-life.

The announcement of his degree as Senior Wrangler brought to his father many letters of congratulation from friends and country neighbours. There was a sufficient sprinkling of Cambridge men among them, more particularly among the clergy, to make it generally known that something of no small difficulty had been achieved. Many of these letters foreshadowed a brilliant public career. One of them was so far prophetic that it suggested he might eventually become Chancellor of the University. But it evidently never occurred to any of the writers that his life might be that of a student.

Indeed, in shaping his life in this way, some obstacles had to be overcome. A clerical relative urged on him the claims of "duty," by which he understood the normal public duties of a country squire, and said something to the effect that to neglect these to follow scientific pursuits was a lapse from the straight path. "But I regard that as the duty," was the reply; and the puzzled rejoinder was, "Oh, indeed, is that so?"

His father remarked about this time, "You should have been a younger son," feeling perhaps that he was capable of making his own way, and that paternal anxieties would have been lessened if his intellectual gifts had fallen to one of his younger brothers instead of to him.

CHAPTER III

EARLY MANHOOD. FIRST SCIENTIFIC RESEARCHES

After his degree a trip was taken to Italy with his friends Henry Corry and Sidney Colvin. The latter hardly found him an adequate travelling companion so far as the appreciation of works of art was concerned.

On April 13th, 1865, he made his first public appearance as a lecturer, speaking at Bury St. Edmunds on photography. The lecture was printed, and shows the lucid style of his later writings, but there is nothing in it which even then possessed real novelty. His early attempts at lecturing were by no means successful from an oratorical point of view: he suffered acutely from shyness, and was somewhat inaudible. Later on, however, he became a good lecturer, and such modest political speeches as he had occasion to make in the neighbourhood of his home were not ineffective. He never made the mistake of being too elaborate for the occasion.

He was now comparatively free to pursue his scientific studies in any direction that he fancied. He remained in residence at Cambridge with the fellowship examination in view, and was anxious to make a start in scientific investigation, but he found this extremely difficult. The facilities then existing at Cambridge were very small. In particular, he had had no experimental training beyond his own dabblings in chemistry and electricity at home. He had learnt something from the experiments shown by Stokes, and would gladly have learnt more from this source, but did not find the path open to do so. Professor Stokes was kind and civil in answering questions after the lectures, but his attempts to learn from him where similar apparatus could be procured did not lead to much—

"the man he had bought it from was dead," etc., etc., and he was somewhat disappointed to find that his eagerness did not meet with any response in being invited to assist, even if only in getting out and putting away the apparatus.

Moreover in those days there was no opportunity of learning the orthodox methods of using tools, either at school or university. He never really acquired this knowledge. I remember his showing me some extemporized construction of his early days, and I remarked rather unsympathetically and from the standpoint of my own generation that I thought it would not have been more trouble to make it properly. "I daresay," he replied, "but I did not know how to make it properly, as you call it."

In 1867 he took a course of qualitative chemical analysis (test-tubing, as it is now often called) under Professor Liveing. This was, I think, the only laboratory instruction of any kind which he could get in Cambridge. I have dwelt somewhat in detail on the difficulty he found in getting experimental instruction, because it was a subject which he often spoke of in telling me of his early years of manhood. "It wasted three or four years of my life," he said.

It was not so difficult to get hints as to what to read. At the suggestion of his contemporary James Stuart (afterwards Professor of Mechanism) he read Maxwell's great paper of 1865, *A Dynamical Theory of the Electro-Magnetic Field*. He also read the papers of Maxwell and Helmholtz on colour vision, as well as the chief writings of Stokes and W. Thomson. Also, in a different field, Mill's *Logic*, and the same author's *Political Economy*.

In 1866 he was elected to a Fellowship at Trinity. There were eight vacancies—a very unusual number, so that success was hardly doubtful, unless on the ground of his poor attainments in classics, which then formed a necessary part of the fellowship examination. The present system of dissertations for the fellowship had not then been introduced. He said that after the examination he was ashamed to face the classical

examiners at the High Table, being painfully conscious of the wretched answers he had given.

His father had been rather doubtful about the advisability of his entering for it, and at the time his degree was announced had written :—

"I think [a Fellowship] should not be hastily accepted. The Duke of Devonshire and Lord Lyttelton were not fellows, and however much thought of at Cambridge, would not be thought much of for one in your position in life, and many might think that you meant to confine yourself to university pursuits only, and many that you were taking the place of another to whom it might be all in all. I throw this out for your consideration and you might ask your friend Lyttelton to sound his father about it. . . ." However, before entering, he had taken care to find out the wishes of the college authorities.

Previous to the election of Fellows, the candidates went to the Vice-Chancellor's house to sign what was called the Vice-Chancellor's book. So far as could be observed from the pages exposed to view, this book contained nothing but the signatures of the candidates of previous years. When all had signed, one of them asked the Vice-Chancellor, in apparent innocence, whether any particular significance attached to these signatures. He replied, in some confusion, that he should have explained to them that what they had signed was the declaration that they were members of the Church of England ! The religious tests then in force were not in fact taken at all seriously, and one of the older clerical Fellows of the College was said (truly or not) to describe the Christianizing of Western Europe as "a wave of oriental superstition."

Of his home life at this period there is not much to relate. He insisted on a certain measure of respect from his brother Edward, ten years younger than himself, who had to call him "Uncle John." His relations with his schoolboy brothers were largely composed of friendly chaff. One was given to speaking over-quickly, so that the words were run together into an unintelligible jumble. This would be ingeniously

parodied: "I am sorry I am so late." What's that about a slate? [1]

Another brother would be challenged to play a game of chess, and given the king!

At Cambridge he made friendships among some of those who had come up later than himself. Two of these were with George Darwin and Arthur Elliot. Another, which was to be of prime importance in his life, was with Arthur Balfour, who was a fellow-commoner, and with whom he came into contact at the High Table, and also in the (real) tennis court.

The impression he made on Arthur Balfour at this time formed a picture consistent with later impressions. He was sociably disposed, then as later, and never gave the idea of being in a hurry, or not having time at his disposal.

In August, 1867, he started on a trip across the Atlantic with his friend John Holland, but his letters home do not yield very much that is worth quoting. In a letter dated from Richmond, December 7th (1867), to his mother, he says: "A whole cavalcade of us, including the Amberlys, Morley, Cowper, were presented by Seward [2] to the President,[3] who expressed his views quite freely. He inveighed against the doctrine of a natural right to vote, and said that unless negro suffrage could be revoked in the South it must end in a war of races. The mules in the South he said were just as competent to vote as the negroes. He is an ordinary looking man (among Americans) and does not speak very grammatically. Seward is as 'dried up' an old man as you can imagine and did not impress me favourably. . . ."

He recorded in later years:—

"When in Richmond in 1867 I was taken to the legislature, composed largely of blacks. They were discussing what 'compensation' they should receive, whether 6 dollars or 8 dollars a day. My companion explained that of course they were all really in favour of the 8, but some wanted to vote for 6, could they be sure of being outvoted. The speaker at the moment, Dr. Barr,

[1] The example is explanatory, not historical.

[2] The Secretary of State. [3] Johnson.

a full-blooded negro who had been a slave until recently, was avowedly in favour of the 8, and was drawing a picture of the sacrifices they made, leaving their wives and families, etc., in the service of their country. 'How comes he to be a doctor?' I asked. 'Servant to a dentist before the war,' replied my friend."

He formed at this time a clear idea of the enormous potentialities of development of the United States, which he lived to see abundantly verified. The trip included a short stay in Canada, and the Niagara Falls were visited.

One or two amusing incidents are on record. A lady to whom he had been inaudibly introduced asked him his name, according to the usual and sensible American custom. "Strutt? We usually associate that word with the peacock."

Another he recorded as follows: "During my first journey across the Atlantic (1867) a cabin mate on waking asked for soda water. The steward: 'Won't you have something in it, sir?' 'If you like you may put a little brandy in.' This little pantomime was repeated every morning."

On his return home from America he found that Terling was in the grip of a frightful attack of typhoid fever. Two servants in the house and about fifty of the villagers died of it. The infection was attributed to the drinking water, which was obtained from surface wells, but it seems much more probable that it was really due to the milk, especially as the first case was the daughter of the dairywoman, who had caught the disease when away for a holiday.[1] The methods of bacteriological examination had not of course been developed at that time.

Lord Rayleigh sent his family to Tofts, five miles off, to escape infection, while he remained at Terling to do what he could for the people. Afterwards he installed a village water-supply system from the springs already mentioned (p. 3).

[1] It has always seemed to me very improbable that the surface water could be contaminated *on both sides* of the river Ter, which divides the village, since water cannot cross to the other side of the valley.

It was at this time that the family took to playing whist (p. 6), and it remained a popular evening amusement to the end of Lady Rayleigh's life. It was with difficulty that my father was persuaded to give it up for bridge in later years, but he was forced to surrender by finding that no one else played whist, and that the alternative was no game at all. He was not a good player at any time.

About this time there was more than one tentative proposal that he should stand for Parliament. He wrote from Chicago (October 9th, 1867) :—

"I received to-day a letter from Papa in which political topics are referred to. I do not see what I can do or say at this distance, but I should not wish to press myself forward at all. As far as my personal convenience and comfort are concerned, I think I would rather not be in Parliament : but that would not prevent my standing if I saw clearly that that was the proper course to take.

"The main difficulty is that in the present state of politics I could not pledge myself to take any particular course or to follow any particular leader, and that my vote on ecclesiastical subjects would probably not be quite satisfactory either at home or to a conservative constituency. If it had been possible I should have preferred to stand for a constituency which would otherwise return a radical, for then I should have felt free, if elected, to vote as I please. Perhaps Papa could convey to Majendie a general impression of this.

"It seems to me very surprising that conservatives should like the prospect of this (!) country being governed by men who hold such wild ideas as many trade unionists seem to do. Some American radicals to whom I have talked think their experiment of negro suffrage in the south much less dangerous than ours." [1]

Immediately after his return home there was a suggestion that he should stand for the University of Cambridge, on a vacancy that seemed likely to arise.

NEW UNIVERSITY CLUB,
Feb. 1*st*, 1868.

DEAR COLONEL TAYLOR,—

After much consideration, I have reluctantly come to the conclusion that my opinions on University and Ecclesiastical matters

[1] The reference is to Disraeli's Reform Bill.

are not such as to make me an acceptable candidate to the conservative portion of the constituency. . . . I need not say that personally it is a great disappointment to myself, but I shall always consider it a great compliment to have been thought of at all in connection with the representation of the University.

Yours very truly,
JOHN W. STRUTT.

The above letter crossed that of his correspondent, who was the well-known Tory Whip, and he wrote again on February 2nd :—

". . . After the conversation with Mr. Walpole there was no doubt that my views would not be acceptable, and the only question remaining was whether I could properly waive them, as being on matters of secondary importance. After a good deal of thought I came to the conclusion that I *could not* do this. In order that you may understand exactly my position I may say that I am distinctly in favour of the principle of admitting dissenters to fellowships. On general politics I fancy I should find myself more in harmony with Lord Stanley than with any other prominent statesman of either party.

". . . Much as I regret it I feel that I have taken the only course open to me under the circumstances."

His disinclination for a parliamentary life, which is shown in the first of these letters quoted, was by no means inconsistent with a keen interest in politics. He was, like most other clever young men of his time, a good deal under the influence of J. S. Mill, and was completely in sympathy with the idea of *laisser faire*. His sympathies remained with this idea to the end of his life. If he was inclined to be a tariff reformer it was because free imports by us, combined with foreign tariffs against us, seemed to him a pernicious form of interference with *laisser faire*.

He regarded the control of wages by Trades Unions with suspicion, and would point out that the law of supply and demand worked satisfactorily for the wages of domestic servants without the help of a Trade Union.

Irish home rule he regarded as the limit of folly and the desertion of loyalists.

He later took shelter under the theory that members of the

House of Lords must not interfere in elections, to avoid expending on politics the time he needed for science. But in other ways he was a consistent supporter of the Conservative party, and increasingly so as the Liberal party drifted further and further from its earlier belief in liberty and property. His mother was an ardent supporter of female suffrage, and he gave her an unenthusiastic support.

Strutt had no permanent college rooms at Trinity during this period, but in part resided there, occupying rooms that were casually vacant, and working at the investigations which were published in 1869 and later. He was in correspondence with Tait at this time, and his suggestions led to some corrections in the second edition of Thomson and Tait's *Natural Philosophy*. He had been very much inspired by reading Clerk Maxwell's writings on electricity and on colour vision. He began to correspond with him in May, 1870, and no doubt saw something of him at the British Association at Liverpool in the autumn of that year. Maxwell was president of Section A (mathematics and physics), and Strutt's paper on colour vision followed, as he was proud to notice, on one which Maxwell read on the same subject. "I had the sense to cultivate Maxwell as much as I could," he said in later years, when discussing these early days. Maxwell usually resided on his Scotch estate at Glenlair during the whole interval between Strutt's degree and his marriage in 1871, but he examined in the Mathematical Tripos at Cambridge in the years 1866, 1867, 1869 and 1870, and it was probably on these occasions that the opportunity of intercourse was found.

The following reminiscences may refer to this period :—

"At breakfast at Lubbock's [1] one morning Matthew Arnold passed up a half emptied tea-cup to be refilled. Some one who knew him well said, 'Why don't you drink what you have got ?' 'But my doctor allows me only one cup,' was the reply."

[1] Sir John Lubbock (first Lord Avebury). He was perhaps one of the last to keep up the custom of breakfast parties in London. I have myself been his guest at one of these.



J. W. Strutt, 1870. Aet. 28.

From a wet collodion photograph by himself.

I think that he made Charles Darwin's acquaintance when staying in Kent with Sir John Lubbock, who was a friend and neighbour of Darwin's. He was asked over from there through Darwin's son George, who was his friend at Cambridge. He had read some of Darwin's writings, and put to him an objection or criticism, the same which he wrote to *Nature* in 1874,[1] that we could not rely on the assumption that insects could see the bright colours of flowers as we do.

Darwin said he must think it over, an answer with which his questioner no doubt sympathized. So far as I know the reserved judgment was never delivered.

In after-years my father would sometimes discuss the problems of evolution. I had been reading the *Origin of Species* about 1906, and remember asking him whether, on the whole, he could accept natural selection as a sufficient explanation of evolution. "Well, no," he said, "I don't think I can quite swallow it." This of course did not imply any lack of respect for the scientific character of Darwin himself.

He wrote in later years: "When I stayed with C. Darwin in 1870 he told of a letter from an American with 'You will excuse my remarking that your own remarkable resemblance to an ape must have unduly influenced your views.'"

In 1868, on his return from America, Strutt began experimenting on his own account, though at first rather for his own personal satisfaction than in the expectation of getting anything new.

He then purchased his first outfit of apparatus, if exception is made of a few things he had had as a boy. It comprised a Rhumkorf coil, Grove cells, an astatic needle galvanometer by Ladd, a large electro-magnet, and a little later a high resistance Thomson galvanometer. Most of them are still extant. The first experiments he made were on the permanent deflection of a galvanometer by alternating currents, which he traced to the changeable magnetization of the needle. This led to a galvanometer with soft iron needle for alternating

[1] *Scientific Papers*, Vol. I, p. 222.

currents. He (literally) read a paper [1] on this at the British Association at Norwich in 1868.

Another early experiment was to demonstrate the deflection of a galvanometer by the discharge of an electrophorus.

Strutt's early work on colour vision was in part a repetition of Maxwell's observations with the colour top ; he found that he could make closer matches than Maxwell had done, owing probably to better colour discrimination. He also showed how, by means of a coloured liquid composed of blue litmus and potassium chromate, in suitable concentrations, it was possible to obtain the compound yellow colour. The light transmitted by such a liquid appears yellow to the eye, but when examined by the spectroscope it is seen that the yellow part of the spectrum is absent ; the yellow colour sensation is produced by the mixture of red and green lights which the solution can transmit.

Some early electrical work was also a development of what Maxwell had done, in pointing out the analogies between the self-induction of electric currents and the inertia of moving matter. For instance, it is not ordinarily possible to decompose water by means of a single Daniell's cell. But this he showed could be done by including an electromagnet and a make and break in the circuit.

Similarly a stream of water will not ordinarily rise above the level of the source, but it can be made to do so by using the energy of its motion by the intermittent action of a hydraulic ram, which as Strutt pointed out has a close mechanical analogy with the electrical arrangement which has been mentioned.

Maxwell wrote from Glenlair (May 18th, 1870) :—

" Have you tried whether the sudden starting or stopping of a current in a coil has any the least effect in turning the coil in its own plane as it would be turned if the current were water in a tub ?

" If the coil is hung in a horizontal plane you can easily destroy the earth's horizontal magnetism by means of magnets."

[1] " Reading " a scientific paper usually means giving a brief oral account of it.

GLENLAIR, *Dec. 6th*, 1870.

DEAR STRUTT,—

If this world is a purely dynamical system, and if you accurately reverse the motion of every particle of it at the same instant, then all things will happen backwards to the beginning of things, the raindrops will collect themselves from the ground and fly up to the clouds, etc. etc. and men will see their friends passing from the grave to the cradle till we ourselves become the reverse of born, whatever that is. We shall then speak of the impossibility of knowing about the past except by analogies taken from the future and so on. The possibility of executing this experiment is doubtful, but I do not think it requires such a feat to upset the 2nd law of thermodynamics.

For if there is any truth in the dynamical theory of gases, the different molecules in a gas of uniform temperature are moving with very different velocities. Put such a gas into a vessel with two compartments and make a small hole in the A B wall about the right size to let one molecule through. Provide a lid or stopper for this hole and appoint a doorkeeper very intelligent and exceedingly quick, with microscopic eyes, but still an essentially finite being. Whenever he sees a molecule of great velocity coming against the door from A into B he is to let it through, but if the molecule happens to be going slow, he is to keep the door shut. He is also to let slow molecules pass from B to A but not fast ones. (This may be done if necessary by another doorkeeper and a second door.) Of course he must be quick, for the molecules are continually changing both their courses and their velocities.

In this way the temperature of B may be raised and that of A lowered without any expenditure of work, but only by the intelligent action of a mere guiding agent (like a pointsman on a railway with perfectly acting switches who should send the express along one line and the goods along another). I do not see why even intelligence might not be dispensed with and the thing made self-acting.

Moral. The 2nd law of thermodynamics has the same degree of truth as the statement that if you throw a tumblerful of water into the sea, you cannot get the same tumblerful of water out again.

Many thanks for your two papers; the electromagnetic one has just come in time for me as I am at that part of the subject. Have you seen Helmholtz on the Equations of Motion of Electricity in conductors at rest? It is a very powerful paper.

"I have been doing Weber's theories of magnetic and dia-

magnetic induction. There are some mistakes in integration but the theory of moveable magnetic molecules is of great use in explaining phenomena, especially all about magnetization, demagnetization and remagnetization.

Yours truly,
J. Clerk Maxwell.

I have improved my book by means of three of your suggestions:—

1. Wrong sign in an equation about M.
2. Discussion of terms in kinetic energy involving products of currents and ordinary velocities.
3. Magnetization as a test of maximum current and as a cause of anomalies in galvanometry.

If therefore any more occur to you and you send me them I shall be thankful.

The conception of the "sorting demon" described in this letter, was afterwards published by Maxwell in his *Theory of Heat* (1871) and is now known to every student of physics.

Early in 1871 the appointment to the newly created Cavendish chair of Physics at Cambridge was to be filled up, and Strutt wrote to Clerk Maxwell as follows:—

Trinity College, *Feb.* 14*th* (1871).

Dear Maxwell,—

Thanks for your hints about colour boxes, which will be found very useful. I suppose you think that ordinary 60° prisms would not be suitable for a reflecting box. I had myself experienced the inconvenience of bisulphide of carbon. I believe the new limit you give for the resistance of a conductor of revolution is different from any in my paper and closer, but it is nearly a year since my paper was written, so that I hardly remember.

Do you refer to resonators communicating with external air by holes or necks? I could calculate the pitch if the *inertia* of the elastic case could be neglected, so that a simple relation would hold between the volume and pressure inside. A moderator globe makes a good resonator. A piece of rubber tubing (French is best, and if of suitable diameter will stay in the ear without being held) forms communication between ear and interior. Covering one hole lowers pitch about a fifth. I can determine by resonance the note of almost anything down to a jam pot to about $\frac{1}{4}$ of a semitone, but that requires practice.

When I came here last Friday I found every one talking about the new professorship and hoping that you would come. Thomson

it seems has definitely declined and there is a danger that some resident may get promises unless a proper candidate is soon in the field. There is no one here in the least fit for the post. What is wanted by most who know anything about it is not so much a lecturer as a mathematician who has actual experience in experimenting, and who might direct the energies of the younger fellows and batchelors [of arts] into a proper channel. There must be many who would be willing to work under a competent man, and who, while learning themselves, would materially assist him. There would I am told be every disposition on the part of authorities to help the new Professor. I hope you may be induced to come; if not I cannot imagine who it is to be. Do not trouble yourself to answer me about this, as I believe others have written to you about it.

Yours very truly,
JOHN W. STRUTT.

The latter part of this letter was published in Maxwell's biography, somewhat indiscreetly as my father thought. Shortly afterwards he wrote home from Cambridge:—

"I have been lingering on mainly to hear Maxwell's decision about standing for the Professorship of experimental physics here. Some people thought that if he would not, I was the proper person. It is now I believe nearly certain that he will come, and so I am relieved of having to make a difficult decision. Maxwell has reported favourably on my paper on Resonance for the *Phil. Trans.* which I shall accordingly have to review. . . "

He first heard of the acceptance of this paper on Resonance (of flasks and bottles) from Lord Salisbury, who characteristically remarked, "We had your broken bottles at the Royal Society Council the other day."

Maxwell wrote (March 15th, 1871):—

"Many thanks for your good wishes with respect to the new professorship. I always looked forward to it with much interest tempered with some anxiety when it was merely to be erected in the University. I now take your good wishes as personal to myself and my anxiety has developed into responsibility.

"I hope you will be in Cambridge occasionally yourself for it will need a good deal of effort to make Exp. Physics bite into our university system which is so continuous and complete without it.

"To wrench the mind from symbols and even from experiments on paper to concrete apparatus is very trying at first, though it is quite possible to get fascinated with a course of observation as soon as we have forgotten all about the scientific part of it.

"If we succeed too well, and corrupt the minds of youth till they observe vibrations and deflexions, and become senior Ops instead of wranglers, we may bring the whole university and all the parents about our ears."

Sir William Thomson wrote to Helmholtz (October 29th, 1871):—

"Did you meet Strutt when you visited his family [1] in England? I hear he would have been the new professor in Cambridge if Maxwell had not accepted."

The paper on Resonance above mentioned marks the opening of Strutt's original work on Sound. The circumstances which led his thoughts in this direction are worth recording. He went to Eton to examine for the Tomline prize in mathematics, and there he met W. F. Donkin, Savilian Professor of Astronomy at Oxford, who must I suppose have been his co-examiner. Donkin asked him whether he could read German, and said that he ought to learn to do so. On returning he asked the advice of Coutts Trotter, who recommended him to read Helmholtz's *Lehre von den Tonempfindung*. This led to his studies on resonance.

The paper treats chiefly on how to calculate from the known properties of air the fundamental musical note to which a flask or bottle of known dimensions will resound. The conclusions are then verified experimentally. The essence of the treatment is that the changes of pressure may be virtually regarded as propagated instantaneously through the interior of the vessel, so that the pressure within at any given moment is uniform. It is the alternating movement of air in the neck determined by the excess or defect of pressure within the vessel that governs the phenomena. In the neck it is the velocity of the air which matters; inside the vessel, its pressure. If we compare the resonator with a mass vibrating

[1] This reference is unintelligible to me.

up and down on a spiral spring, the excess pressure of air within the vessel corresponds to the force of the spring, and the inertia of the air in the neck to the vibrating mass.

Thus the behaviour of a bottle resonator can be treated on different, and in many respects simpler, lines than an organ pipe, when both the inertia and the pressure differences in the vessel have everywhere to be taken into account, or in other words when the time which sound takes to travel through the resonating vessel has to be reckoned with.

The theoretically calculated results were found to agree very satisfactorily with experiments for bottles and flasks of the proper shape. The resonance was determined by holding the bottle over an appropriate wire of the pianoforte. These experiments were carried out at Terling on an old grand piano which had been discarded from one of the reception rooms of the house and moved into the book-room (see p. 21) for his sister Clara's musical education. It was often used in these early days as a laboratory table for electrical and other experiments, as well as for acoustical determinations. The room was used for family prayers, and everything was cleared away after working. It was lighted on winter evenings by two candles only. In this, as in his other early experimenting, the object was chiefly to verify the results of calculation. His special pleasure was in anything paradoxical and unexpected by experimenters, which he was able to verify.

The experiments on colour vision already mentioned were, I think, the origin of a much more important piece of work, and one of those which has been most widely appreciated. He had noticed that the colour matches made with Maxwell's colour top were appreciably affected by the blueness or otherwise of the sky at the time of observation, and this led him to make comparisons of the colour of the blue sky with that of direct sunlight.

It was perhaps from this experimental standpoint as well as from the stimulus of Tyndall's writings, that he was led to consider the question of why the light of the sky is blue,

and at the same time the closely related question of why it shows the peculiar phenomena of polarization which had been examined early in the century by Arago, Biot, and Brewster.

Needless to say, these questions had been considered before. Some earlier writers, notably Clausius, had supposed the light to be reflected from thin plates, and the colour to be the blue of the first order in Newton's scale.

Clausius assumed the existence of bubbles floating in the upper atmosphere of the appropriate thickness to give this effect. But this theory, besides requiring a rather forced *ad hoc* supposition, indicates a blue colour less rich than that which actually prevails.

A better theory was advocated by Tyndall, whose beautiful experiments showed that the blue sky could be imitated by passing the beam of the electric arc through certain organic vapours. These gave clouds of extremely firm particles which scattered laterally light of a blue colour. A more familiar though less perfect illustration is found in the blue light scattered from tobacco smoke. The light scattered at right-angles in this way is almost completely polarized, and in this respect also corresponds approximately with the light emitted from the sky in a direction at right-angles to the sun.

Tyndall considered it very strange that, as he expressed it, the polarizing angle for matter in this condition should be 45°, apparently considering that when light was diverted through a right-angle by a cloud of particles the case had some analogy with reflection from a glass surface placed at 45°.

Strutt pointed out that according to the principles of the wave theory such a comparison was wholly illusory. He then went on to consider the question from the standpoint of the theory which assimilates the vibrations of light to those of an elastic solid. This theory in its literal form is of course obsolete, but for many purposes it remains even now a perfectly valid instrument of research. Its limitations were recognized clearly at the time, as the following quotation shows: "The fact that the theory of elastic solids led Green to

Fresnal's formulæ for the reflection and refraction of polarized light seems amply sufficient to warrant its employment here, while the question of whether the analogy is more than formal is still left open."

The point of view taken was that the ether was locally loaded by the small particles, which were supposed to be so small that the phase of the vibration was the same at all parts of the particle—like a cork on the ocean waves, not like a ship which may have its stern on the crest of a wave, while the bow is in the trough of the wave.

By supposing suitable forces to act on these places of increased local density, it was possible to annul their effect on the general motion of the medium, and consequently the effect of the loading would be exactly the same as that of forces the opposite of those provisionally introduced in this way for the sake of calculation.

When this step had been taken, simple considerations of symmetry were enough to show why the polarization phenomena were as observed, and an almost equally simple argument by the method of dimensions showed that the intensity of the scattered, in terms of the incident light, is inversely proportional to the fourth power of the wave length. The great preponderance which this law gives to short wave lengths accounted much better than the thin plate theory with the observed intensity of sky blue.

In the second paper a more generalized discussion of the problem by analytical methods was given, and a new method of illustrating it experimentally, by using the particles of sulphur which are precipitated when a solution of "hypo" is acidified. This method is easier than Tyndall's, and lends itself well to the study of the effects produced by a growth of the particles to appreciable dimensions. The nature of the postulated small particles in the atmosphere was in the main left vague at this time.

These papers, though in parts somewhat abstruse, were appreciated at the time by those qualified to judge. Maxwell wrote to a friend (March 15th, 1871):—

"I think Strutt on sky blue is very good. It settles Clausius' vesicular theory.

> 'For putting all his words together,
> 'Tis three blue beams in one blue bladder.'"
> (*Mat. Prior.*).

ROYAL INSTITUTION, *Feb. 3rd.*

MY DEAR SIR,—

Though I did not expect you to be so sharp upon me I am truly delighted to see you taking up this subject of the scattering of light by small particles. It will certainly repay you and instruct us all.

I send you my last memoirs on the action of light on vapours which I hope you will accept from me.

Wishing you all success,

I am, yours faithfully,
JOHN TYNDALL.

The Hon. J. W. Strutt.

It may in fact be admitted that his criticisms of Tyndall were not quite so gently expressed as they would have been at a later date. He was, however, always entirely opposed to the belittling of Tyndall's scientific work which was attempted by some writers of the day.

CHAPTER IV

EARLY MARRIED LIFE

Lord Eustace Cecil, younger brother of the third Lord Salisbury, was at this time M.P. for West Essex, and as such had been a guest at Terling. Strutt dined at his house in London in 1869, and there met Eleanor and Evelyn Balfour, the elder sisters of his Cambridge friend Arthur Balfour, and nieces of the host. Eleanor Balfour, who was placed next him, had been very much impressed by hearing that he was a Senior Wrangler, but did not find him so formidable as she had expected.

That summer he went to stay with Lord and Lady Salisbury at Hatfield and there he met Arthur Balfour, who had probably suggested that he should be invited, and his two sisters. He was invited to come back to their home, Whittingehame, in East Lothian. Their father James Maitland Balfour, was dead, and their mother, Lady Blanche Balfour, was an invalid, practically confined to her room.

There were other friends and relations there, and the time was spent in large riding parties, in picnics, and in playing croquet. The next summer (1870) the visit to Whittingehame was repeated.

Evelyn Balfour was fond of music. He lent her Helmholtz's *Sensations of Tone*, which discusses the subject from a scientific standpoint, and which, though profound, is not a difficult book. This was one fruitful topic of common interest.

From Whittingehame he went with Arthur Balfour to the British Association at Liverpool.

Arthur Balfour was amused to watch his encounter with a circle-squarer, one of those curious camp-followers of science

who are met with at such meetings. This gentleman's demonstration was followed with agreement to a certain point, up to a step in the argument which was disallowed. He offered an alternative reasoning, but his listener had had enough.

After the B.A. was over they went back to Whittingehame, which, my father concluded, would be the most convenient route home to Essex.

TRINITY COLLEGE, *Nov.* 1*st* (1870).

MY DEAREST MOTHER,—

I came here this morning from Hatfield. We had at first a very learned party but as usual I outraged most of them. Miss M. Alderson was there but not her younger sister, who was still in Scotland. Mrs. Gladstone has asked me to go there on Dec. 5th, and I think I shall go though in some ways it is rather inconvenient. A.B. [Arthur Balfour] is to be there.

Lord S. [Salisbury] does not think we shall have war with Russia. He took me into his laboratory which also serves as a dressing room and showed me some magnetic experiments which I am supposed to explain! He is too awkward to succeed well as an experimenter I think.

Your affec: son,
JOHN W. STRUTT.

The following letter from Lord Salisbury shows that the required explanation was forthcoming, and in part at least satisfied that sceptical mind :—

HATFIELD, *Dec.* 5*th*, 1870.

MY DEAR STRUTT,—

Many thanks for your letter. Your explanation is very clear and I have no doubt in the main satisfies the facts. But there are two experiments which I should like some day to show you, which hardly seem to square with it. My difficulty arises exclusively in those cases where the mass of iron added increases the induction. The other set of cases when it diminishes the induction I will set aside as explained. . . ."

The visit to the Gladstones at Hawarden was duly paid. He was deeply interested in Mr. Gladstone's personality. He admired his originality and power, and was fond of contrasting it with his curious incapacity for scientific thinking. The following anecdotes are on record.

"Staying at Hawarden after my first American visit, I

told some stories picked up there. Knowing that Mr. Gladstone was rather impervious I explained beforehand that one form of American humour consisted in leaving out the middle of a story. 'At Smithville the other day two little boys went out to play William Tell. The funeral sermon was three-quarters of an hour long.' Mr. G. could not see it, and Mrs. Drew had to explain across the table, 'You know, father, one little boy killed the other.' 'Oh! oh!' said Mr. G.

"Perhaps it was at this time that Mr. Gladstone propounded his theory that snow had a greater power than water of penetrating leather. I suggested that snow had a trick of lodging on the upper part of one's boots, and that in any case it penetrated as water and not as snow. 'Oh no,' says Mr. G. 'Snow has a *peculiar power of penetration*.'

"When I was at Hawarden in 1870, Gladstone raised the question of who were the four greatest men of Christian times. I have never known anyone guess all four, except Beresford Hope. They were Dante, Charlemagne, Innocent III and St. Thomas Aquinas. I used to enjoy putting this before Mr. G.'s nonconformist admirers."

Strutt saw more of the Balfour sisters in London, in the spring of 1871, and used to go with them and their brother to Tyndall's lectures at the Royal Institution, when it was noticed that he generally found a seat next Evelyn Balfour. He used also to ride with them in the Park in the evening, which was then a favourite amusement.

In May, 1871, he was engaged to Evelyn Balfour, and they were married at St. George's, Hanover Square, on July 19th. By marriage, he vacated his fellowship at Trinity according to the statutes then in force, which embodied a curious survival of the monastic idea. Shortly before the event he wrote: "To me, the breaking of my connection with Cambridge is the dark side of the approaching change."

The first two or three days of the honeymoon were spent at Hatfield, lent by Lord Salisbury for the occasion, and the remainder at the Cumberland lakes. After that, they went to Whittingehame for the British Association at Edinburgh.

Strutt went in regularly for the meetings, the other members of the party occasionally. It was then (I believe) that he first made the acquaintance of Sir William Thomson, who was President of the meeting. This acquaintance ripened into a life-long friendship. He also met for the first time P. G. Tait, who was president of Section A.

Strutt returned to Terling with his wife for the marriage of his only sister, Clara, who had been the chief home companion of his youth, to John Paley (afterwards of Ampton Hall, Suffolk). They spent the autumn and part of the winter at Terling, while Tofts, the house at Little Baddow, already mentioned, was being prepared for their reception. They drove or rode over two or three times a week to superintend progress.

In January, 1872, they went on a visit to Bedgebury in Kent, the home of A. J. Beresford Hope and Lady Mildred Beresford Hope, Mrs. Strutt's maternal aunt. Strutt had not been well for a week or two, and at Bedgebury he was laid up with a very serious attack of rheumatic fever. For weeks he was attended by two nurses, often in great pain, and sometimes light-headed. There was no limit to the kindness of the Beresford Hopes. They put off their annual visit to London. His mother, and later on his sister Clara and his brother Dick, came to stay.

His temperature rose to 105° F. Sir William Jenner was sent for from London, and apparently took a somewhat gloomy view of the case. Arthur Balfour saw Jenner in London, and was so much alarmed that he went down to Bedgebury to learn what he could on the spot.

Lord Rayleigh, then an old man, wrote to Lady Rayleigh:

"I don't offer to come for I feel quite unable physically and I know mentally I should only add to the grief. I can in my own room say 'Thy will, O God, be done,' but if I saw him perhaps I could not say so. You even do not know how I love him and how proud I have been of him. May our wills be conformed to God's will and this affliction sanctified to us all.

"My blessing on him."

His silent misery was piteous to behold. He would sit over the fire for hours doing nothing.

The disease attacked my father's lungs as well as his joints, and when at last it left him he was a changed man in appearance and activity. He had been very slight; he got up with a middle-aged figure, and remained always easily put out of breath. He drove with his wife to St. Leonards to recruit, where they remained until June. They then went to London for a short season.

While they were there, Lady Blanche Balfour, who had long been an invalid, was found dead in bed, from heart disease. Immediately afterwards, chiefly for the sake of mental distraction, several members of the Balfour family came to stay at Tofts, and helped in black-washing the walls of a room which was destined for the optical laboratory.

The scheme of fitting up a laboratory at Tofts had been contemplated before Strutt's marriage took place, and he wrote to Maxwell asking for hints. Part of the reply may be quoted.

"I have received your letter dated 'Fourth of July Day,' I wish you all joy in the new state of things, and I hope that after the 19th the number of individuals existing will be so far diminished and the number of dualities increased.

"With regard to the laboratory the word denotes a place to work at experiments and connotes a place full of articles not wanted at present and liable to noxious fumes. Hence, especially if you use nitric acid, a corner should be devoted to it and a pipe or flue constructed to carry the fumes up the nearest chimney. . . .

"To support anything so as to be free only to move in one direction parallel to itself place it on a stand (a T square) having 3 legs with hemispherical (not conical) feet. Two of these feet slide in a groove. The other leg is a little shorter, and slides on a flat board. This is Thomson's (and a good) plan for securing one degree of freedom. No carpenter will believe this till he is converted. . . .

"For a table I prefer a few trestles (mason's horses) and a plank or two pretty thick. . . . Have the frames of your lenses, etc. with wooden bottoms with holes to screw them on the planks. . . . I am glad to see Clifford going in for a chair in London instead of

poking in rotten trees for South American beetles and fevers. I have drawn the lines of force for a single and double tangent galvanometer."

Strutt and his wife lived at Tofts till November, but never returned there, so that the laboratory there was not in use for more than four months. It was arranged with two adjoining rooms, one light and one dark, with the potentiality of using both for long-range optical experiments. The work done there was entirely on the photographic reproduction of diffraction gratings.

Subsequent occupants of the house turned the laboratory into a billiard room.

Strutt had been advised that he ought not to spend the first winter after his illness in England, and accordingly it was resolved to make an expedition up the Nile. They sailed in November in the P. & O. liner *Pekin*. Eleanor Balfour accompanied them. After touching at Alexandria they went on to Suez, to see the Canal, which was then still rather a novelty. Here Strutt was interested to find himself completely deceived by the mirage, though well informed of what to expect.

From Suez they went to Cairo.

The ladies were introduced to two Royal harems. They first visited that of the Khedive Ismail Pasha, where they saw two or three of his wives in Parisian fashions, and Mrs. Strutt had to make the disappointing admission that she had no children.

The next visit was to the widow of Mahomet Ali, who, like her ladies, was in Eastern dress, and gave them long pipes to smoke with the bowls resting on the floor, and set with large diamonds. The visitors were, however, quite incapable of drawing them.

They expressed, through the medium of the English lady who accompanied them as interpreter, a wish to see dancing girls. The hostess explained that this was impossible just that day. But before there was time to express disappointment the dancing girls trooped in and performed a dance,

which no doubt was suggestive, but they did not quite know of what, and failed to appreciate its beauty.

Mrs. Strutt wrote home from Nile boat, *Nigma*, November, 1872 :—

" The front half of the boat is devoted to the crew and contains the cooking apparatus. The back half contains the saloon, which I should guess to be 12 feet by 14, occupying the whole width of the boat, also six small cabins of which one is a bath room, and a comparatively large one at the very end of the boat, occupied by John and me. There is a quarter deck with sofas and armchairs on the top of the saloon and cabins. . . .

" We hoped we had got a really clean boat, but three bugs have already been caught and we mean to have a regular hunt to-morrow and stop up all chinks with carbolic acid. Meanwhile we are so bitten by fleas and mosquitoes that we do not feel that a few bug bites in addition will make any appreciable difference. . . . We found climbing up the Great Pyramid a much less exertion than we had been led to expect. It would have been still less had we been allowed to go up by ourselves instead of being dragged quickly up by the Arabs. You have one holding each hand pulling and one pushing behind—and they keep bothering you for ' backsheesh ' till you are nearly worried to death. After the ascent we sat in the shade and lunched, and after lunch John gave the Arabs the slip and began a second ascent of the pyramid to see what it was like without Arabs. Before he was half way up, however, they saw him and gave chase, and he finally had to give them a franc to allow him to mount without assistance though they would not leave him.

" After looking at some tombs in the afternoon we went inside the pyramids, and terribly hot and stuffy it was. I do not know how the others managed, but I was seized round the waist by two Arabs, and hurried up a slippery inclined plane by the light of a candle one of them carried, and entertained the while with their expectations of backsheesh when we got out again. It was a great relief to all when that happy event occurred."

My father wrote home from Cairo (December 6th, 1872) :—

" There was a reception the other day. Mrs. S. and I went up to the citadel at 9 a.m. in evening dress and were presented by Colonel Stanton to the Viceroy.

" He spoke a few sentences to us in French which were rather feebly responded to on my part. The Ladies have visited several

harems including the Royal. We consider ourselves connected with the court, as one of our boats belongs to the chief Eunuch of the Viceroy's Mother, one of the greatest men in the country. To-day we went to a performance of whirling dervishes which was rather curious."

As will have been gathered from the letter quoted, the voyage up the Nile was made in a *dahabeah*—a kind of sailing houseboat now, I believe, almost extinct. It was chartered for the trip. The catering was done by a black dragoman, with a beautiful crimson turban, who worked for another boat as well as theirs. His designation for my father was "the honourable Starch." There was an Arab cook and a crew of about thirteen fellaheen. The crew slept on deck in their ordinary clothes. A goat with a kid was carried to provide milk.

They started on the Nile voyage about December 12th, 1872. It was a monotonous life, but it suited them. My father began his well-known treatise on the Theory of Sound, and worked at it all the morning in the cabin. He was able to write much of the earlier part of the book without access to a large library. It was found necessary to close the entrance and kill every fly in the cabin before any progress could be made. His sister-in-law, who was studying mathematics under his direction, sat opposite him at the cabin table. In the mornings it was difficult to persuade him to land, even to see the most enchanting temple. In the afternoon the party sat on deck, sipping Egyptian coffee and attempting to improve their minds on the subject of Thothmes III and Rameses II, but with only limited success.

The wind was usually from the North, and when it overbore the current they sailed along merrily. When it fell quite calm the crew walked along the shore towing the boat.

They stopped occasionally to visit temples, and at Thebes they were entertained by the British Consul, a Turk, who presented them with what he called genuine antichas, little blue imitation china figures, no doubt of Birmingham origin, which crumbled away into broken plaster on the smallest blow. On landing the men of course pestered in the usual

way with offers of antichas. Strutt raised a laugh by picking up pebbles and offering them in return as antichas. Baksheesh would be demanded even from a boat in mid-stream, when it would have been impossible to give it. A number of sketches made by Mrs. Strutt from the deck of the *dahabeah* are extant.

The most thrilling events of the voyage were passing up and down the First Cataract—now of course obliterated by the Assouan dam. Going up, the boat was towed by two or three hundred men, and on the return journey they shot the rapids through another channel. This was supposed to be a process of some slight danger, for their dragoman, Ali, was tremendously excited, even going so far as to remove his turban. It was necessary to row hard to get enough way on in the smooth water to enable the boat to be steered successfully round a curve in the broken water.

At Wadi Halfa they saw a caravan bringing slaves from the South. In Nubia above the First Cataract they were continually haunted by the squeaking of the *saggias*, or bullock-driven irrigation machines.

Coming down the Nile they usually drifted with the current, and the crew rowed, singing the while.

They got back to Cairo about the middle of March, 1873, where they were joined by Arthur Balfour and Spencer Lyttelton. After a tour in Greece, where their movements were considerably hampered by the official precautions against brigands, they returned home via Brindisi and Venice. They arrived in London early in May, 1873.

On returning to London, Strutt and his wife went straight to No. 4 Carlton Gardens, a large house of which Arthur Balfour had recently bought the lease. This was to be their London home until my father's death, and they only once missed passing some weeks or months there every year, first as the guests of Arthur and Eleanor Balfour, and, after her marriage in 1877, as the guests of Arthur and Alice Balfour. This pleasant arrangement prevented their ever taking a house in London.

Lord Rayleigh had taken a house in Portland Place for the season of 1873. He looked very ill when they got back from abroad, and on June 14th, 1873, he died. Not only was this a great sorrow to my father, but it distressed him to take up the responsibility of administering the property, which he greatly feared would interfere with his scientific work.

The funeral was at Terling.

In the autumn they went to Arthur Balfour's shooting lodge at Strathconnan, in Inverness-shire, while the Dowager Lady Rayleigh was removing her effects from Terling, and arranging to live at Tofts. Generally speaking Strutt, now become Lord Rayleigh, took very little interest in sport, and, indeed, had scarcely the walking powers required for deer stalking, but on the occasion of this visit he was persuaded to go out. Two stags had come within easy reach of the road up the glen, and he got them both in as many shots. But this did not encourage him to go on, and when pressed to do so he said that he was not going to risk the reputation he had won ! The real reason was more probably a feeling that the pursuit was artificial, when it had to be carried out with the assistance of a stalker, and I think also a dislike of disturbing wild creatures in their natural haunts. For the same reason he would in later years try to give the slip to my brother's fox terrier when going out for an afternoon walk, though often without success.

In the autumn of 1873 they settled in at Terling, and Rayleigh made a few improvements. A private gasworks was installed for lighting the house (which had previously been dependent upon oil lamps), and for Bunsen burners, blowpipe work, etc., in the laboratory.

At the same time an organ which had originally been given by the Duke of Leinster to Rayleigh's father was moved from the saloon into the library, which, in Rayleigh's time, was the sitting-room in everyday use. Lady Rayleigh, at his suggestion, learnt to play it, He liked listening to music when he could do so at home under comfortable conditions, seated

in his arm-chair, but would not as a rule take time or trouble to go out in search of it.

At this time the laboratory was fitted up, but I shall defer the description of it till a later chapter.

The few improvements which have been mentioned were the only ones in the house in which Rayleigh in any way took the initiative. Other changes were carried out in the house during his regime, but he only assented to them after a good deal of pressure from his family. In particular it was very difficult to get his consent to the removal of any piece of furniture which was associated with his boyhood, however obsolete or inconvenient it might be.

In this year (1873) Rayleigh was elected a Fellow of the Royal Society, chiefly on the initiative of William Spottiswoode, who was afterwards President. Spottiswoode had communicated his early papers to the Society, and had entertained him at dinner to meet other scientific men. In their early married life my father and mother frequently dined with him before the Friday evening lecture at the Royal Institution. How the acquaintance originally began I do not know.

From 1874 to 1879 they lived quietly at Terling, with yearly visits to 4 Carlton Gardens, usually before Easter, and visits to Whittingehame, Hatfield, and other country houses.

It was during the summer of 1874 that Rayleigh attempted the investigation of " spiritualistic " phenomena. His interest had been primarily aroused by the investigations of Crookes, whose scientific work in other fields he knew and appreciated. He was also in communication with Henry Sidgwick, Edmund Gurney, and F. W. H. Myers, all of whom were Fellows of his old college, Trinity. Sidgwick in particular, who originated the movement for investigation, was senior to Rayleigh, and they were well acquainted when my father was in residence after his degree at Cambridge, if not earlier.

4 Carlton Gardens, *May 3rd*, 1874.

My dearest Mother,—

You will be interested to know the result of my spiritualistic

enquiries. I had an hour and a half's conversation with Crookes [1] and saw no reason to doubt his trustworthiness. He said that Miss Showers' ghost Florence was as to bodily form Miss Showers herself, as he had proved in various ways, for instance by making Florence dip her hands into a bowl of dye, with the result that Miss S.'s hands were afterwards dyed. She made no difficulties about submitting to tests, but they all failed. Crookes thinks that she may be got hold of by spirits who perhaps supply the dress, she all the while being unconscious. The same tests which fail with Miss Showers prove that Katie is not Miss Cook. Miss Cook is going to be married shortly, and Crookes is making the most of the interval.

The other night we had here to dinner Mr. and Mrs. Jencken (who was Kate Fox) with the Salisburys. We did not get anything quite conclusive but some raps, which could hardly have been made by the medium. The answers to questions were all wrong. Arthur (Balfour) and I are going to see them at their own house. My mind is still in suspense, but I rather expect to be converted.

Ever your affec: son,

RAYLEIGH.

In order to pursue the matter further, Mrs. Jencken was invited to Terling in August, 1874, and again in September, 1874. Henry Sidgwick took part in some of the séances which were held.

Rayleigh wrote to him (June 7th, 1874) from 4 Carlton Gardens:

"I have now seen a good deal of spiritualism with Mrs. Jencken and a little with Mrs. Guppy, but hitherto nothing to my mind absolutely demonstrative. At the same time my impression (which is pretty strong tho' very variable!) is in favour of the genuineness of the phenomena. I have come to the conclusion that unless by a lucky chance I am unlikely to get absolute demonstration by casual sittings in London, whether at the medium's house or here, and have therefore been contemplating getting Mrs. Jencken down to Terling, when it might be hoped Mr. J.'s business would frequently require his absence. Mrs. Guppy I don't think I could stand, even in the cause of science and philosophy!

"If you could come to Terling, I think we could come to a conclusion satisfactory to our own minds, but if that conclusion were in favour of the spirits, I doubt if we should move the outer world.

[1] Later Sir William Crookes, O.M., P.R.S.

In my laboratory I could invent tests and help the manifestation by using only red light, etc. better than anywhere else. . . .

"I am quite amazed at the little interest most people take in the question. A decision of the existence of mind independent of ordinary matter must be far more important than any scientific discovery could be, or rather would be the most important possible scientific discovery.

"Mrs. Jencken seems to me to be rather a fool, and if this be so the phenomena must be genuine, as no fool could do them as tricks. As one inquires among one's friends stories multiply, and many of the most weighty come from unbelievers."

The result of the sittings at Terling is told in Rayleigh's Presidential Address to the Society for Psychical Research, reprinted in Appendix II. This address was delivered forty-five years later, and if the conclusions had been definitely positive, it would have been unsatisfactory, to say the least, to rely on an account given so long after the events. There is, however, a contemporary note among Rayleigh's papers, dated September 21st, 1874, and recording most of the same phenomena. It has reference to one sitting only.

Lady Rayleigh to the Dowager Lady Rayleigh. Terling (*August*, 1874).

"We have had Mrs. Jencken here for ten days and find that we have made a good deal of progress towards believing, but alas! the box trick [1] has not come off. The last night we heard a pencil scratching on it when we were holding hands, but when we examined it we found the writing was outside. 'This is difficult but we will succeed' or words to that effect signed 'Richard' and 'James.' I suppose Dick [2] did not go into a trance or walk in his sleep that night? We saw lights on three nights which John's science is inadequate to explain. The best proof we had was raps on the box while we held Mrs. J.'s hands and her feet were on mine. My bonnet was found under the table one night, but she may have brought it. A large paper cutter was worked about under the table another night, when we had possession of her hands."

[1] I.e. the experiment of sealing up pencil and paper in a box with a glass lid, for the spirits to write with. See Appendix II. A glass retort hermetically sealed was used later as an improvement on the box.

[2] Rayleigh's eldest brother.

Many of the séances proved intolerably tedious, and on one occasion Rayleigh yawned more conspicuously than was altogether consistent with politeness. Mrs. Jencken remarked, "Thanks, I'll stay outside," and this was the only remark she made during her visits which betrayed a sign of vivacity or intelligence. However, Rayleigh learned afterwards that it was not original.

In February, 1875, Rayleigh visited Sir William Crookes' house (20 Mornington Road, N.W.), where some tests were carried out with Mrs. Fay.

On other occasions Rayleigh had sittings with Slade, a medium whose name is well known in the history of this subject. His impression of Slade was decidedly unfavourable. He noticed that the medium was always trying to divert attention, saying, for instance, "There's a light on your arm." Rayleigh made a point of ignoring these attempts, and concentrating his attention on the medium's hands; and this seemed to have a very damping effect.

The subject of spiritualism was one to which Rayleigh very frequently recurred in conversation in later years. Many visitors at Terling were anxious to hear his views on such subjects, and he was always ready to discuss his experiences. Perhaps he found it a useful meeting ground. His non-scientific friends not unnaturally expected him to tell them something of his own pursuits and interests, and he was willing to gratify them, but the greater part of his work was hardly within range of popular explanation in a few words.

At the beginning of these psychical investigations he did not doubt that they would soon lead him to a positive or a definitely negative conclusion, and (as in the letter quoted) he inclined to expect a positive one. From remarks that he made later, I believe that in that event he contemplated throwing the greater part of his energies into the investigation of these phenomena, which, if genuine, he thought would be of supreme importance. As we have seen, his expectations of a definite result were not fulfilled, and he returned to his orthodox scientific studies.

Rayleigh was asked to second the Address in the House of Lords in 1875 (February 5th). The suggestion that he should do so came from the Duke of Richmond, through Lord Salisbury, who wrote : " There is nothing in the measures which we shall propose to which you can possibly take objection."

His speech was short, and not specially effective. He complained that when he received his instructions, every subject was barred in turn—" You had better say nothing about that "—till there was nothing left for him to say about anything. A part of the speech may however be quoted, as characteristic of his way of thinking :—

" Among the subjects which will engage the attention of Parliament is the consolidation of the various sanitary acts. Those of your Lordships who have turned your attention to the question will admit the necessity of some legislation. At present there is great confusion, and the various local bodies on whom the administration largely devolves are scarcely equal to the task of interpreting intricate and sometimes almost conflicting acts of parliament.

" Of the importance of these matters there is no doubt at all ; but our knowledge of the laws of health is still very scanty, and there is danger I think lest enthusiasts, led away by the fashion of the moment, may encourage an excessive expenditure which may not only fail to attain its object, but excite a disgust that may prove a serious impediment in the way of future attempts at improvement."

There was some idea that seconding the Address would be the prelude to a political career, and it was doubtless with that in view that he was asked to do it. But Rayleigh himself was fully determined not to allow politics to distract him from science, and there can be little doubt that he decided wisely. He was interested in politics, but conscious of not being qualified to take a leading part in it.

In 1876 Rayleigh intervened to propose an amendment in the Cruelty to Animals Bill, in the interests of physiological research, which, he thought, was over strictly regulated. Some relaxation was conceded, though less than he wished for. In this matter his action was instigated by his brother-in-law, F. M. Balfour, who, as a biological investigator, was directly interested.

In 1872 my father had been asked by Mr. Gladstone to serve on the Royal Commission to report on the financial resources of the universities and colleges of Oxford and Cambridge, and his name appears as one of the signatories of the report in 1874, but owing to his illness and absence abroad he cannot have given much attention to it until the summer of 1873. The Duke of Cleveland was the chairman, and this was in part the origin of a friendship which took my father and mother on a visit to Raby Castle in 1873, and in later years to Battle Abbey in Sussex, as guests of the Duke, and after his time of the Duchess. A Bill was passed in 1877 appointing commissioners to make new statutes for the universities and colleges, and Rayleigh was one of these. Sir Alexander Cockburn was the chairman, and the other commissioners were the Bishop of Worcester, E. P. Bouverie, J. B. Lightfoot, G. G. Stokes, and G. W. Hemming. The commissioners' work was mainly directed to taxing the colleges in one form or another for the benefit of the university. How far Rayleigh took a prominent part in the deliberations I have no means of knowing. He was amused to observe that his old teacher Stokes would come over specially from Ireland to attend meetings, without uttering a single word when he got there. The work of the commissioners lasted till 1882, into the period of Rayleigh's professorship at Cambridge. The present Master of Trinity remembers, on putting some scientific difficulty to him, that he said he would have time to think it over during the meeting of the commissioners that afternoon!

He was summoned home by telegram from the British Association at Bristol (August 28th, 1875) and was met at the station by his brother Charlie, who told him that a son and heir (the present writer) had arrived half an hour previously. The event was one which he had much desired and he said in after years that he often wished that his father had lived to see it. There was a family party in the house for the christening ceremony, in which Lord and Lady Salisbury and most of their children were included.

CHAPTER V

IN THE 'SEVENTIES. THE "THEORY OF SOUND"

During a visit to Weald Hall, Brentwood, Rayleigh first met Beauchamp Tower, brother of the hostess Mrs. Christopher Tower. This was probably in the summer of 1875. Tower had been trained as an engineer under William Froude, younger brother of J. A. Froude, the historian. Froude was a great authority on questions connected with fluid motion and the resistance of ships, and had imparted knowledge and enthusiasm about these matters to his pupil. Afterwards, Tower had been employed at the Elswick works, but to the regret of the firm he had given up his position there, in order to work out a scheme for propelling ships by wave power, which was to enable them to get in and out of harbour, or perhaps across "the doldrums" without the assistance of the wind. Rayleigh and he took to one another at once, and Rayleigh wrote a little later asking him to come and assist in the laboratory at Terling. This he agreed to do on the understanding that his professional prospects would not allow of more than a short stay, and he was there for some months in the winter of '75–'76. He stayed for a time in the house and afterwards at the "Rayleigh Arms" Inn in the village.

Tower was a good mechanic, and made various more or less elaborate pieces of apparatus during the time he was at Terling. A foot lathe was installed for the first time at his suggestion, and Rayleigh probably learned from him most of what he ever knew of practical metal work, though this did not go beyond the simplest operations.

During his stay a "hydraulic laboratory" was installed

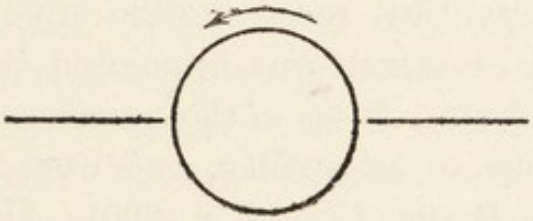
in the grounds. Advantage was taken of the water power available at the "Swan Pond" already mentioned (p. 3) to experiment on fluid motion on a considerable scale. A wooden hut was built below the pond and the water conducted to it with a good-size cast-iron pipe provided with a full-bore stopcock. Various shaped apertures could be placed over the terminal flange of the pipe, and the water squirted out from them in a trough and fell to the tiled floor of the hut, from which it could flow away. The investigation contemplated was to determine the static pressure within the aperture by means of a very fine tube with a side opening introduced into the jet, and connected with a pressure gauge. As may be imagined, it was wet work. The experiments, which were carried to a final conclusion by Mr. Arnulph Mallock, verified closely a theoretical calculation which Rayleigh had made, indicating that in a slit-shaped aperture the pressure in the middle of the plane of the aperture would be ·58 of the head of water available.

Tower carried on his model experiments on his wave-power scheme while at Terling. Rayleigh had no confidence in this as a practical thing, and the event unfortunately proved him right. At all events the idea never came to a large-scale trial.

He remained a warm friend and frequent visitor at Terling after he had left, until the time of his death thirty years later. His later life was clouded to a considerable extent by an unfortunate misunderstanding with the Admiralty amounting in his view to a breach of faith. He devoted many years to the perfection of a "steady platform" for keeping a gun pointing steadily in a fixed direction, notwithstanding the rolling of the ship. The Admiralty made a grant towards the expenses of developing this invention, and it was apparently not denied that it achieved all that he had claimed at the time the grant was made. But after a successful trial it was not adopted, and all the labour and ingenuity he had spent went without reward.

During the winter of '76–'77 Rayleigh had the assistance of

Mr. Arnulph Mallock, who was a relative of William Froude and, like Tower, had worked with him. It was on Tower's suggestion that he was asked to come, and he remained about four months. As already mentioned, he carried out the measurement of pressure in the water jet. Another experiment in the hydraulic laboratory was made on the flow of water in a long trough past a cylindrical obstacle, with its axis at right angles to the flow. It is known that in such a case the condition of stream-line flow is obeyed on upstream side of the obstacle, but that the lines of flow refuse to close in again and follow the outlines of the obstacle on the down-stream side. The idea was to see if the stagnant water, bounded by eddies, on the down-stream side could be got rid of by arranging a fixed longitudinal partition, and keeping the cylinder in rotation thus :



The direction of rotation and of flow of the water is indicated by the arrow. No attention would be paid to what went on on the other side, where the cylinder moved against the flow.

The experiment gave an entirely negative result. It is curious that Rayleigh quite forgot having carried it out with Mallock and that he proposed a practically equivalent experiment forty-one years later (*Collected Scientific Papers*, VI, p. 552).

Mr. Mallock's subsequent scientific career is well known. In addition to work as a consulting engineer, he has published many interesting papers on fluid motions and other physical subjects. He, like Tower, remained a close friend and constant visitor.

Rayleigh found, however, that men of the stamp of Tower and Mallock were not exactly the assistants that he needed. "I felt," he said, "that Mallock might as well have been doing the work by himself." It was more in the purely

constructional and preparatory work that he needed help, to economize his own time, and to supply workshop knowledge. After Mallock had left, he worked on without an assistant until he went to Cambridge in 1880; but he afterwards regretted having done without one.

Rayleigh accepted the position of additional examiner in the Cambridge Mathematical Tripos of 1876. The invitation came in a letter from Clerk Maxwell, who was chairman of the board of mathematical studies, and probably suggested his appointment. He invited the other examiners (C. Smith, J. B. Lock, H. M. Taylor, R. T. Wright) to Terling to discuss the proposed questions. The date (about the middle of December, 1875) which had been fixed for this meeting was somewhat unfortunate as it prevented Rayleigh and his wife from accepting an invitation to Hatfield for the interesting occasion of Lord Salisbury's political reconciliation with Disraeli.[1]

The additional examiner was appointed to deal with the more physical subjects. Some of the questions which Rayleigh set possessed points of originality, and were reprinted in his *Collected Scientific Papers* (Vol. I, p. 280). Usually speaking, they are directed to essential points, rather than to merely elegant academic exercises. Rayleigh's principle as an examiner was to set fairly easy questions, and to insist on their being properly done, or, at least, not to give the marks unless they were.

When Rayleigh came into the property in 1873, the condition of agriculture was still prosperous. The boom in trade of 1870 had increased the consuming power of the population, with the result that agricultural prices were high, and the competition for farms very keen. Thus the rents for his farms (most of which were let) stood at a high figure. The natural caution of Rayleigh's disposition prevented his presuming on the continuance of this state of things, or spending money freely on improvements, as many men would have done under like circumstances: and it was well that he did not, for the tide began to turn very soon afterwards.

[1] See Buckle's *Life of Disraeli*, Vol. V, p. 455.

In 1874 there was a serious collapse in trade generally, and this reacted in more than one way. It diminished the purchasing power of the industrial populations, and, what was even worse from the standpoint of the English farmer, it drove many of the town workers in the United States from their previous occupations, to till the virgin soil of the West. Added to this was the effect of a series of very unfavourable seasons in Essex which were not relieved as of old by scarcity prices. The position of the tenant farmers became very difficult, and they naturally fell into arrears and pressed for a reduction of rent. Rayleigh has told me that he did not sufficiently appreciate the gravity of their difficulties. He thought that they were trying to take advantage of his inexperience and not unnaturally refused at first to do what they wished. One tenant, for instance, complained that he was "eaten up with rent," but it appeared on investigation that he had not paid a penny for several years !

However, things went from bad to worse, and in 1879 and the following years, though considerable remissions of rent had been made, many of the tenants elected to leave, and it was impossible satisfactorily to replace them. There was no alternative but to take the farms in hand and farm them with his own capital.

These conditions made Rayleigh's financial position somewhat difficult, and this was one of the reasons which influenced him in deciding to go to Cambridge as Professor.

When he succeeded in 1873, it was necessary, though very uncongenial, to turn part of his attention from scientific pursuits to the management of the property. This had been his father's chief interest and occupation in his later years, and Thomas Isted, the confidential clerk whom he had trained to do the estate accounts, continued to take charge of them. His office was in the house, being in fact the room which my grandfather had used as a study ; thus he was within easy reach, and was often consulted with great advantage as to the currents of feeling in the village.

The collection of rents, and the necessary business com-

munications with the tenants, were in the hands of Mr. W. B. Blood, the local solicitor, and the farming of the home farms, amounting to about 1,000 acres, was supervised by Charles Richardson, the bailiff, already mentioned (p. 7).

Rayleigh's personal part was chiefly to act as a court of final appeal to decide whether the wishes of the tenants with regard to repairs and improvements should be met or not. He would interview them with Mr. Blood on these matters. He also exercised a general supervision over the farming as conducted by Richardson.

When he first succeeded, Richardson remarked to him that there were some repairs that were very urgent, others that were less urgent, but that there was one thing which admitted of no delay at all, and that was the pulling down and replacement of certain farm buildings at Terling Hall Farm. But Rayleigh, after inspecting them, came to a different conclusion, and said that in his opinion they might go on for a time yet. They did, in fact, go on for fifteen or twenty years, and I think that these tumble-down buildings with the history attaching to them gave him much more satisfaction when he passed by than costly replacements fair to the eye would have done.

In another case, he allowed Richardson's views to prevail. The river frontage of a public house which he owned at Maldon was threatened with collapse, and to prevent this a wharf was constructed which eventually cost much more than the entire annual value of the property would justify.

When he said something to Richardson to the effect that the whole enterprise had been a hopeless failure financially, the latter replied, " But look how your property is improved." To his employer this was no consolation. A costly structure that was not economically justified was to him the worst kind of eyesore.

Rayleigh's knowledge of agriculture was considerable; at all events in later years he could satisfy me on any question that I wished to ask, though my questions may not have been of a very searching kind. Occasionally he would bring his scientific knowledge to bear, making a determination of the

amount of chalk in the boulder clay, or trying the effect of preservatives on cream; but upon the whole not very much came of this.

He was interested in the subject of artificial manures, and made experiments in applying the ammoniacal liquor from the private gas works directly to plots of grass in the park. He insisted, in the face of considerable opposition from Richardson, on the use of nitrate of soda on the land. At that time it was a novelty, and old-fashioned farmers were by no means disposed to view it with favour. Indeed it was said that shiploads of it lay for a considerable time at the London Docks without an offer being made. Richardson was forced to admit the immediate benefit; but he fell back on prophecies of ill for the future. According to him artificial manures were as harmful to the land as drugs like morphia or cocaine to the individual. The immediate effect might be stimulating, but it had to be paid for afterwards!

Richardson's comments on men and events were often worth recording. Thus, to illustrate the disreputable character of one of the farm tenants, "He don't go to church nor meeting nor tenant's dinner nor nothing."

Rayleigh's younger brother, Edward Strutt, who had left the University in 1874, was trained in farm and estate management immediately afterwards, and in 1876, Rayleigh handed over to him the management of the property and of the farms which were in hand. He was only twenty-two years of age, and had of course to gain his experience. But after 1876 Rayleigh did not take any further share in managing the estate, leaving it entirely to his brother.

In March, 1877, Rayleigh and his wife made a trip to Madeira, which she had known in childhood. Her father, J. M. Balfour, who was consumptive, had gone there with his family, hoping for benefit from the mild winter, and had died there.

Eustace and Alice Balfour accompanied them. Sir Bartle Frere and his family were fellow-travellers. They were on their way to South Africa, where he was about to take up his

appointment as High Commissioner. They were already friends, and had stayed at Terling two months before. The friendship was kept up after his return from an eventful tenure of office culminating in the Zulu War, and he paid further visits to the Rayleighs at Cambridge and at Terling. He was keen to make acquaintance with academic society. One of my early recollections is seeing him come down to dinner at Terling wearing the red ribbon of a G.C.B. It would seem strange at present to see anyone so adorned during a quiet country visit, but he was one of the last to adhere to a vanishing custom.

THE UNIVERSITY, GLASGOW, *March* 8, 1877.

DEAR LORD RAYLEIGH,—

I see by this morning's paper that you and Lady Rayleigh are to be passengers in the *Balmoral Castle* to-morrow from Dartmouth for the Cape. There are three of my compasses on board—an azimuth compass and two steering compasses in the wheel house. I should be very glad if you would look at them a little and see how they behave, and much obliged if you would write me a line from Madeira or the Cape telling me about them.

The enclosed private papers about the compass card (and a new sounding machine which has made a good beginning already in several ships) will tell you all that you may care to know about my system of adjustment.

My wife joins in wishing you and Lady Rayleigh a pleasant passage, and I remain,

Yours very truly,

WILLIAM THOMSON.

The following passage from his lecture on the Mechanical Principle of Flight (1900) refers to this expedition :—

"Some years ago I visited the north side of Madeira, where cliffs nearly 2,000 feet high rise perpendicularly from the sea. Being on the top of the cliff we had difficulty in finding a sheltered spot until we noticed that *close to the edge* there was almost complete calm. Lying upon the ground and moving only one's arms it was possible to hold a handkerchief by the corner so that a little behind the plane of the cliff it hung downwards as in still air, and a little in front of the cliff was carried upwards in the vertically rising stream. A ball of crumpled paper thrown outwards was

carried up high over our heads. Of course gulls and other birds found no difficulty in rising up the face of the cliff without working their wings."

On their return they landed at Plymouth and paid a visit to Rayleigh's old schoolfellow A. R. Hunt at Torquay. While there they visited Mr. Froude's experimental establishment for testing the resistance of model ships. Rayleigh had the greatest respect for Mr. Froude's gifts, and was charmed with him personally. From a limited acquaintance with Mr. Froude's writings I should say that his turn of mind was very similar to my father's, both going straight to the essential point, with the simplest reasoning that the case would admit of.

On October 2nd, 1878, Rayleigh's second son Arthur (now Captain R.N.) was born.

He wrote from Terling (July 24th, 1879) :—

"Good news from S. Africa. I am glad Chelmsford has had the opportunity of retrieving his position. The whole thing seems to have been very creditable.

"Bright has been making one of his wild speeches about India. It will be lucky if it does no harm. I cannot understand a man who has held responsible positions going on as he does.

"I have been trying my modification of Mayer's experiment about intermittent sounds. A 128 tuning fork interrupter excites another of nearly similar pitch. If a wire be taken across the poles of the 2nd electro-magnet, the current is diverted. This short circuit is periodically open and closed by another interrupter making 4 vibrations per second. If the 2nd fork be tuned to 124 or 132 it will go when the 2nd interrupter works but not when the shunt is permanently broken. If tuned to 123 it will go anyhow."

A. M. Mayer, referred to in the above letter, was an American experimentalist of some note, and he was a guest at Terling on more than one occasion. On the first of these he met W. K. Clifford, the well-known mathematician and philosopher. Clifford was a man outside all ordinary rules, and he took advantage of his position in this respect to tell Mayer American

stories introducing an imitation of the nasal twang supposed to be characteristic of his hearer's nationality!

During the years from 1873 to 1878 annual shooting parties were held at Terling in the winter and occasionally garden parties and dinner parties were given to the neighbours, but on the whole life there was quiet. A ball was given in 1878. Rayleigh's brothers were constantly coming and going. They were then all unmarried, and in the course of settling down into professional occupations. He gave a course of four afternoon lectures at the Royal Institution on Colour in May, 1878, but it seems that no record of the subject matter has survived. In 1878 and 1879 he served on the Council of the Royal Society.

As we have already seen, Rayleigh had begun his book on the Theory of Sound during the Nile trip in 1873. The treatment adopted is by no means popular, and in fact the greater part of the book is unintelligible to anyone without considerable knowledge of mathematics, though, according to his accustomed method, common-sense methods are used as far as they will go, and mathematical analysis is introduced as a tool, rather than as an object in itself. There was (and is) no other book covering anything like the same ground.

Maxwell wrote from 11 Scrope Terrace, Cambridge :—

20th May, 1873.

MY DEAR STRUTT,—

I am glad you are writing a book on Acoustics. . . . You speak modestly of a want of Sound books in English. In what language are there such, except Helmholtz, who is sound not because he is German but because he is Helmholtz. The next best book is Herschel whom you may regard as of German organic descent. Observe Deschanel's theory that the sound of an organ pipe is excited by the vibrations of the thick wooden lip of the mouthpiece. "This lip is of itself capable of vibrating in unison with any note lying within a wide range and the note which is actually emitted is determined by the resonance of the column of air in the pipe." This is what organists desirous of scientific knowledge have to receive and believe.

Another doctrine delivered to such persons is that anyone with

a mere smattering of science ought to understand completely the motion of the air at the mouth-hole say of a flute.

Many thanks for your paper on Bessel's functions.

Yours very truly,

J. Clerk Maxwell.

Maxwell wrote again from Cambridge, November 22nd, 1873 :—

"Your theorems on vibrations are first-rate. Will you send me a separate copy if you can spare one (I have that in the Math. Society's *Trans.*).

"I made use of your dissipation function in lecture to-day in proving the existence of a system of conjugate harmonic solutions in every problem in the conduction of heat (or of electricity when electro-magnetic induction may be neglected) and that in any given initial state of the system may be expressed as a sum of a set of harmonic solutions. . . . When is your book of Acoustics coming out? I am afraid that every effort you make towards finishing it will only turn up fresh material which it would be wrong not to insert. . . ."

As the writing of the book went on, a number of theoretical investigations were made on points which presented themselves in the course of writing it, and they were published in various periodicals without waiting for the completion of the book.

It is difficult to convey any notion of these investigations in their general form without technical language, but some of the particular conclusions may be illustrated from commonplace experiences. It is shown, for instance, that if A and B are two points of a uniform stretched string, then plucking at A will produce the same vibration at B as would have ensued at A had the same plucking been applied at B. A and B are, in fact, interchangeable.

The same will apply to a simple source of sound in air, i.e. one which in the open would sound equally in all directions. For such the source and the listener may have their positions interchanged, even though there may be obstacles, and even obstacles which can share the vibration, between.

Some experiments made by Tyndall at the Royal Institution seemed to indicate that this law was violated, and Rayleigh

was a good deal perturbed at the discrepancy. This experiment, like so many of those made by that brilliant experimenter, depended on the use of a sensitive gas flame, which flared when exposed to a sound of high pitch, for instance that produced by a reed. Tyndall found that a glass screen placed between the reed and the sensitive flame reduced the response more when it was near the reed than when it was equally near the screen. This appears at first sight to violate the "principle of reciprocity."

Rayleigh was anxious, as anyone should be in such a case, to see the experiment for himself. It is not altogether easy to explain why this is so great an advantage. Sometimes of course there may be complete scepticism as to whether the facts have been truly stated—"seeing is believing"—but this by no means exhausts the subject. In many cases the advantage is perhaps of the same kind as is got by talking over a difficulty with a friend, or even writing out an account of it for one's self. The subject is presented from a new angle, and the chance of some new light being thrown upon it is not easily over-rated. No one appreciated this more thoroughly than Rayleigh.

ROYAL INSTITUTION, *May 12th* (1876).

DEAR RAYLEIGH,—

I have just had Cottrell [1] up with me, and he craves a day's respite to arrange the experiments.

To-morrow at 3 he would be ready. Monday at 3 would answer still better, for the matter requires some little preparation.

I should really like you to see the effect in the hope that you might conquer the problem of diffraction involved in the experiment.

Yours faithfully,
JOHN TYNDALL.

The visit to the Royal Institution quite convinced Rayleigh of the correctness of Tyndall's observations, but another experiment which was incidentally shown to him by Mr. Cottrell provided a clue to the mystery.

[1] Tyndall's laboratory assistant.

The theory of reciprocity requires that the sound should proceed from a source which emits sound alike in all directions, and it had been tacitly taken for granted that the reed pipe would nearly enough fulfil the condition. It was shown, however, that such was by no means the case. The emission from the reed was highly directive. When the tube containing it was pointed at the sensitive flame, there was much more response than when it was pointed a little away. Taking this into consideration all was clear.

The book was written on the backs of the papers which had been handed in by the candidates for the Mathematical Tripos in 1876. This kind of economy was very characteristic of Rayleigh, who (I feel sure) was not at all annoyed by the untidy appearance which it inevitably gave to his manuscript.

He asked his friend and former rival in the Mathematical Tripos, H. M. Taylor, if he could suggest the name of a young Cambridge Fellow who would be willing to read the proof sheets of his book as it went through the press. Taylor suggested doing the work himself, an offer which Rayleigh gratefully accepted. Indeed, in the case of a book like this, it was no light task. Taylor however found the work congenial, and executed it with great care, witness the following: "I well remember," he wrote, "that I tried unsuccessfully to get Rayleigh to omit St. in St. Petersburg, which occurred very frequently in the references to authorities."

The book was published in two volumes by Messrs. Macmillan in 1877, and the sale was not wholly unprofitable, though, it need scarcely be said, the writer of an abstruse book of this kind which appeals only to the few cannot expect a great monetary reward for his labour.

Dr. Routh wrote (June 11th, 1877):—

"The much wanted book on Sound reached me just before I left Cambridge . . . it is needless to say that it seems to be just what we want. This next long (vacation) I shall of course use it as a text-book and I hope to learn much from it. I suppose we shall go much further into the subject now than we ever did before."

FLAMSTEED HOUSE,
GREENWICH PARK, LONDON, S.E.
1877, *May* 30.

MY LORD,

I am much obliged by the present of a copy of your Lordship's work on "Sound" which has reached me through Messrs. Macmillan.

The work evidently contains not only most profound discussions on sound proper, but also much on non-soniferous vibrations, and all worked out with a depth of mathematics applicable to even more complicated subjects. It almost merits a title of wider meaning.

I am, my Lord,
Yours very faithfully,
G. B. AIREY.

The book was reviewed in *Nature* by Helmholtz, the great master of the subject in Germany. He emphasized the advantage that resulted to the higher study of acoustics from having the subject presented in a coherent and accessible form. The book (he said) was comparable as to style and method to Thomson and Tait's celebrated *Treatise on Natural Philosophy*, which, as is well known, was never finished. Indeed, Thomson (Lord Kelvin) stated in after years that one of the reasons for not proceeding with the later volumes was that the ground had been covered to a great extent by Rayleigh's book, and by Maxwell's *Electricity and Magnetism*.

After analysis of the contents of the book with some incidental remarks of his own, Helmholtz concluded by hoping that a third volume might eventually appear, dealing with the theory of reed pipes, including the human voice. "For the former at least," he said, "the principles of their mechanics can already be given, and the methods the author employs seem to me to be particularly well adapted to further progress in these domains."

He also expressed a wish for treating of singing flames, action of the violin bow, and other cases of the maintenance of oscillations. Rayleigh at one time entertained the notion of such a further volume, and the publishers urged it on him, but it was never written.

The reason is perhaps to be found in this—the writing of

a scientific treatise which aims at completeness makes far greater demands on the author than the treatment of many of the same topics in separate memoirs. In the latter, there is no obligation to treat matters on which the writer has found it difficult or uncongenial to arrive at definite views. Some of the topics mentioned were, however, afterwards developed in the second edition.

At the suggestion of Helmholtz, the book was translated into German by Dr. F. Neesen. The translation appeared in 1878. Rayleigh was no doubt pleased with this mark of appreciation; but I have heard him express doubts whether translation of a book of the kind is really called for. Mathematics as a language, if not universally understood, is at any rate international.

CHAPTER VI

GRATINGS, AND THE RESOLVING POWER OF SPECTROSCOPES[1]

This work spread over a considerable part of the early epoch of Rayleigh's scientific activity (1871–1879). I shall try to show the sequence of his ideas on the subject, so far as it can be reconstructed from the surviving notebooks, and from what he told me in his later years. In the original papers it was his habit to present the conclusions to which he had come in the clearest and most logical way. With all its advantages, this method involves some sacrifice of human interest, and sometimes the suppression of what should form a part of scientific history.

The investigations in question began with the idea of producing diffraction gratings by photography, which had been in his mind for some years. Diffraction gratings, the reader may be reminded, are flats of glass or metal ruled with a great number of fine parallel lines, which if they are to be useful must be spaced with very great accuracy. If such a grating is placed in front of the eye and a distant narrow source of light is examined, a series of spectra of the source will be seen, on either side of the source itself.

With a fine grating, even the innermost of these spectra is much longer than a 60-degree prism would give under the circumstances, and the outer ones much longer still. Moreover, the red part of the spectrum is seen to better advantage than the violet. In this respect the grating is complementary to the prism.

It is therefore essential for the minute study of e.g. the sun's

[1] The general reader may prefer to omit this chapter.

spectrum to be provided with a diffraction grating of great accuracy. The naked eye can only utilize a part of the grating of the same size as the pupil of the eye, $\frac{1}{5}$ inch diameter at the most; thus to use the grating to advantage it is necessary to artificially increase the size of the pupil, and this may virtually be done by means of a telescope, when the object glass takes the place of the observer's eye, and may be made as large as the grating.

Gratings ruled on glass were first constructed by Fraunhofer, about the year 1820, but very few of his construction were sold, and the first on the market were made by Nobert, twenty or thirty years later. These were the best, and indeed the only ones to be obtained at the time Rayleigh began his experiments. Some of them were good, and the classical map of the solar spectrum published by A. J. Angström in 1868, the first in which the wave-lengths of a large number of lines were given, was made by means of one of these gratings.

The construction of a grating by mechanical means is not an easy matter. Nobert's gratings, for instance, had from 3,000 to 6,000 lines to the inch, so that a fairly high power on the microscope was necessary to see them at all; further it is necessary that the spacing should have a high degree of uniformity. Any part of the grating which does not conform with this requirement is worse than useless, if the object is to resolve minute details in the spectrum.

Rayleigh had the idea of making them by reducing photographically a large-scale grating, such as it seemed at first sight would be fairly easy to make. This idea would present itself very naturally to anyone engaged as he was in the practice of photography. Preliminary attempts were made in this way by photographing a piece of striped stuff, 200 lines being got into $\frac{1}{4}$ inch. A spectrum was obtained, but the results were not good enough to be very encouraging. "I am now rather inclined to think," he wrote, "that nothing would be gained by this course, that the construction of a grating of a given number of lines and with a given accuracy would not be greatly facilitated by increasing the scale, and that it is doubtful

whether photographic or other lenses are capable of the work that would be required of them."

These experiments led, however, to results of interest in another direction. A peculiar kind of circular grating can be made, having concentric circles with their radii proportioned to the square roots of the series of natural numbers, and with the alternate zones thus defined, blocked out. Such gratings were described by Soret in 1875, and by R. W. Wood in 1898. Soret's were made by photographic reduction of a pattern drawn out on cardboard. Such a grating has the property of focussing a luminous point on its axis after the manner of a lens. It acts in accordance with Hughens' principle by suppressing those parts of the aperture which would give rise to a disturbance in one phase, leaving those which give rise to a disturbance in the opposite phase. The latter can therefore add their effects, without the former as an offset, and a greatly enhanced illumination results at the focal point.

What is now made public for the first time is that Rayleigh anticipated Soret by four years in the construction of these "zone plates." It is hard to understand why he did not publish the observations. He never even mentioned them to me. The explanation may be that he did not think it added sufficiently to the experiment of Poisson and Arago, showing the bright point at the centre of the shadow of a circular disc, which had been made long before. The relevant entry in his notebook is as follows (April 11th, 1871) :—

"The experiment of blocking out the odd Hughens' zones so as to increase light at centre succeeded very well and could be shown in quite a short space. The negatives should not be varnished. I have little doubt that the number of zones blocked might be advantageously increased much beyond what I used (15). No great accuracy is required in filling in the odd zones with black."

Going back, however, to the main problem:

His next attempts were directed to the copying of one of Nobert's glass gratings by contact printing, in the same way that a print is taken from an ordinary photographic negative,

and here immediate success was obtained. The entry in the notebook is dated thus, October 14th ! ! (1871), which suggests that he was surprised and delighted at the result.

It should in fact be emphasized that the feasibility of such copying is by no means obvious at first sight. The lines on the original are very fine, and it was doubtful whether the photographic plate could be brought close enough to avoid making the shadows of the lines too diffuse. Some advantage was gained in this direction by using a small source of light, which gave sharp shadows. An image of the sun formed by a short-focus lens served the purpose.

It is to be remarked also that gratings cannot be successfully copied by means of modern gelatine plates. The structure or grain of these plates is far too coarse for the purpose, being on a larger scale than the grating to be copied.[1]

Gelatine emulsion plates were of course unknown at that date, and some of the processes in use such as the tannin, albumen, and collodiochloride processes were of incomparably finer grain, so as to be well adapted to the purpose. Many of the gratings were made with these processes. But some of the most remarkable results were attained by the use of bichromatized gelatine, which has of course nothing in common with the use of silver-gelatine emulsion. Gelatine mixed with potassium bichromate becomes insoluble in water after exposures to light, and some of the gratings made by this method presented the remarkable peculiarity that the first spectra of a soda flame were actually brighter than the central image. Such a result Rayleigh saw could not be produced by the mere action of opaque bars in stopping out a part of the wave front, and he showed that it necessarily implied that there must be

[1] After writing the above, I thought it worth while to test the point directly, and attempted to copy my father's Nobert grating of 3,000 lines to the inch by contact printing on a lantern plate. Rather to my surprise diffraction spectra of the first and second order were obtained from the copy, though doubtless of poor definition. Under the microscope the grating lines were quite unrecognizable, being lost in the grain of the plate. But the diffraction phenomena prove that they must have been present in some statistical sense.

a reversal of phase produced by the transparent bars. The idea is now familiar, and has its highest development in Michelson's Echelon grating, but this was the first time that it had been suggested.

Of late years the photographic copying of gratings has fallen out of use, because all the modern *original* gratings have been reflexion gratings ruled on speculum metal instead of on glass, and these do not lend themselves well to the process.

It is possible, as Rayleigh showed, to copy them photographically, but the light has to traverse the photographic film before arriving at the grating, and it is only on its return passage that a useful impression is made.

In later years superb speculum gratings have been ruled in America by Rowland, Michelson, and Anderson, which have ten or more times the total number of lines of Nobert's gratings. These have been copied by Thorpe and Wallace, whose method consists in taking casts in celluloid, and copies thus made have been placed on the market. It is worth while to note that Rayleigh used this method also. The following are extracts from his notes :—

"Aug. 11th, 1873. I have taken a collodion cast of the 3,000 grating, which showed pretty well on a candle, but I don't see how to secure these against distortion."

"Aug. 31st, 1873. A gelatine cast of the 3,000 grating gave spectra in very much the style of the original. If the manipulatory difficulties can be overcome these may turn out well yet."

Rayleigh distributed a large number of his copies among scientific friends, and some use was made of them, particularly by H. C. Sorby, the pioneer of microscopic petrography, in the course of his classical studies on the absorption spectra of animal colouring matters. But the chief importance of what Rayleigh did was that it led his thoughts to the question of the resolving power of spectroscopes.

The yellow line of sodium, as every student of physics knows, is really double, and it is a common and convenient test of the powers of a small spectroscope to try whether it

will or will not show the components separate: whether, in the usual phrase, it will resolve the sodium lines.

In testing his grating copies, and comparing their performance with that of the originals, Rayleigh necessarily had his attention closely drawn to this matter of the performance of spectroscopes, and the question presented itself how many lines must a grating have, and how closely must they be spaced in order that, e.g., the sodium lines in the spectrum may be resolved? I believe that this question had never been propounded before he took it up. At all events, no clear answer to it could be found in the published literature.

Some readers may perhaps be inclined to think that such questions are not of much importance, and may even be inclined to class them as technical details. In one sense no doubt they are so. It is a technical detail but an important one whether an army is armed with rifles or with smooth-bore muskets. The modern triumphs of astronomy, nearly all that we have learnt in recent years about the chemical composition and the motions of the sidereal universe, depend on minute spectroscopic study, which requires the instrumental weapons to be sharpened to the uttermost, and the same applies even more to the wonderful and stupendous phenomena of the sun's surface. I do not think it is an exaggeration to say that my father's investigations on the theory of the spectroscope have been the bedrock on which astronomers have built in developing their instruments.[1]

It is difficult at the present time to realize how confused were the notions entertained on such matters. It is perhaps somewhat invidious to dig out the forgotten errors of eminent scientific workers, but it is necessary to do so if the state of contemporary opinion is to be illustrated.

Thus there was a discussion at the Physical Society about this time on the technique of spectroscopy, and in particular on the efficiency of diffraction gratings, in which some leading spectroscopists, such as Norman Lockyer, L. M. Rutherford and others took part. One speaker (I am not sure who) urged

[1] See, for instance, J. E. Keeler, *Sidereal Messenger*, No. 99.

that the limit to the performance of a grating depended on diffraction by the edges of its aperture, and that a great improvement would result if these edges could in some way be eased off. No one present seems to have been able, or at all events willing, to challenge this position.

Again the following quotation may be given from *Spectroscopic Notes* by C. A. Young,[1] well known for his researches on solar spectroscopy at Princeton University :—

"If we know that the D lines are separated 1 degree, or, what comes to the same thing, appear to be $\frac{1}{6}$ of an inch apart, we have a definite idea of the power of the instrument."

This, however, according to Rayleigh's teaching, is exactly what we have not got. We can always separate the centres of the D lines by 1°, or by any larger number of degrees, by putting an eyepiece of sufficient power on to the observing telescope, and this without adding to the cost or complexity of the instrument. The practically important question is whether we can do this *with advantage*. This depends on whether the images of the individual component D lines are sharp enough to bear the magnification. If they are not, the only result of magnifying them is to reveal their diffuseness, not to show more genuine detail. If it be asked what detail is to be expected, we may instance a "reversal" of each line by a dark strip down the middle of it, such as may be seen in a heavily salted flame, or the more complex structure which should appear when the flame is placed between the poles of a powerful magnet (Zeeman effect).

Again there is the question fundamental in astronomy, of whether, and to what extent, the spectrum lines in a star are displaced relative to those given by a stationary terrestrial source. This is the criterion of motion in the line of sight. The displacement to be measured is never (for a star at least) so great as the distance between the soda lines : and hence the fundamental importance of this question of what instrumental means are necessary for detecting these minute displacements. Questions of even greater nicety arise in studying

[1] *Nature*, Vol. V, p. 85–88, 1871.

the sun's rotation by the spectroscope, and in searching for the shift of spectrum lines in the sun's atmosphere relative to a terrestrial source, which is one of the predictions of Einstein's generalized theory of Relativity.

Rayleigh's method of dealing with the resolving power of a grating is somewhat as follows: Suppose first of all that the aperture of the observing telescope 1 inch square is left blank, instead of being filled with lines. If we examine with sufficient magnification the image of a distant and narrow slit, we shall find that it consists of a central image, bordered on either side by subordinate and much fainter bars, with intervening minima of illumination. With yellow light the angular distance of the first minimum from the centre of the system is 5 seconds of arc, and thus the extreme breadth of the central image is 10 seconds. With a larger aperture it would be less.

These effects may be explained (as Fresnel explained them) on the principle of dividing up the square aperture in imagination into narrow vertical strips each of which is regarded as the origin of a spreading wave. These waves all start in the same phase from the plane of the aperture and the action of the lens causes them all to arrive in the same phase at the centre of the field of view, in the case supposed. Thus they all co-operate, and the maximum of brightness is produced. If we go a little away from the central position, some of the secondary waves have a longer effective distance to go than before, and others a shorter effective distance. Thus there is a discrepancy of phase, so that the crests of the waves from some of the apertures tend to fill up the troughs of the waves from others. This results in a diminished aggregate effect, and this continues until we get away by an angular distance of 5 seconds, when the phases of the secondary waves range over a complete period: that is to say, we have elementary strips of the aperture contributing vibrations in every possible phase; this explains the first minimum. Going still further, we begin to get a certain measure of agreement again, but it is not complete, and therefore the secondary maxima are

less bright than the primary ones. Detailed calculation on these principles shows that the second maxima have about $\frac{1}{20}$ the brightness of the central one.

Suppose now that the area of aperture 1 inch square is ruled with opaque lines or bars, 3,000 in number, uniformly spaced. We shall now have a grating similar to that used by Rayleigh in his experimental verifications. It will be convenient to suppose (as indeed we might have supposed from the first) that the light behind the slit is a homogeneous yellow, corresponding to one of the component soda lines.

The presence of the grating bars will not fundamentally affect the principle upon which the brightness at any point of the field is to be determined. At the centre of the field we shall have agreement of phase, resulting in the principal maximum bordered by the subordinate maxima, as before. But there will now be other principal maxima as well.

For example, if we go 4 degrees to the right or left of the centre, we shall come to a place where the wave from the centre of each aperture is, not indeed strictly in the identical phase as that from the other apertures, but what in some respects is the same thing, differs from its immediate neighbour only by one exact period. Here, too, we get a maximum equal in breadth with the central one and, like it, fringed on either side by its subordinate maxima. We may call this maximum the first diffracted image or the first spectrum, and there is a second diffracted image at 8 degrees and a third at 12 degrees.

Suppose now that instead of the sodium line of shorter wave length, the light examined belongs to the other sodium line of longer wave length. The whole pattern will be on a slightly enlarged scale. The increase of scale will be about $\frac{1}{1,000}$ part, and will not, of course, lead to any displacement of the centre of the pattern. The question is whether the amount of stretching mentioned will be enough to bring the first diffracted image clear of the corresponding image in the former pattern.

Here, evidently, further definition is required. What

exactly is to be reckoned "clear"? It is desirable to adopt some standard that will represent the least that can be distinctly seen, but this must to some extent be a matter of opinion, or rather of subjective impression. If the first minimum on the right of one image coincided with the first minimum on the left of the other, we should have what might be reckoned complete separation, with an absolutely dark interval between the two. But it is by no means necessary to have this in order to recognize definitely that the two patterns are not coincident.

Rayleigh adopted as his standard of resolution the condition that the maximum of one image should coincide with the first minimum of the other. This is theoretically simple, and agrees pretty well with experience. As we have seen, it involves an angular separation of 5 seconds of arc. Now, since the scale of the pattern differs by $\frac{1}{1,000}$ part according to which sodium line is used, the distance of the first maximum from the centre will have been increased by $\frac{1}{1,000}$ part of 4 degrees, or about 15 seconds of arc. This is three times as much as the 5 seconds required for distinct separation. We conclude that the grating will do three times what is required to separate the sodium lines. If the gratings with the same fineness of ruling were narrower, the whole aperture would be smaller, and the central image would be proportionately broader, as in the case of a telescope without any rulings. The same applies to the diffracted images, and there would come a point when the separation of 15 seconds of arc would only be just enough. This would obviously be reached if the grating was reduced to $\frac{1}{3}$ of its original breadth, or $\frac{1}{3}$ inch, containing altogether 1,000 lines.

In this instance, then, we have come to the conclusion that 1,000 lines on the grating would be required to separate the sodium lines, of which one has a wave length $\frac{1}{1,000}$ part greater than the other. Consideration of the principles on which we have proceeded will show that it does not matter whether these 1,000 lines are spread over a wide or a narrow aperture. Suppose, for instance, that the aperture were

doubled. The angular separation between the diffracted images of the two components would be halved: but as against this, the images themselves would be of only half the breadth, and therefore the grating would still remain just adequate to the work.[1]

The argument as given applies only to the first diffracted image or spectrum. It will, however, easily be seen that the resolving power in the second spectrum will be twice, and in the third spectrum three times, as great.

These latter images are as narrow as the first ones, and the angular separation between the component soda lines is 30 seconds of arc for the second spectrum, and 45 seconds of arc for the third spectrum. Here is the entry in Rayleigh's notebook in which these conclusions were first put to the test.

"*Sept. 8th*, 1873.

"Made a rough estimate by observation of the number of lines required to resolve the soda lines in the various spectra. The 3,000 original was used, but did not seem to define better than a copy, though it required a different inclination of the object lens. The widths of slits required were put at 25, 42, 79 200ths of an inch, which should be in the ratio 2 : 3 : 6. If we take 8 200ths as required in the 1st spectrum, we find for the number of lines

$$\frac{80}{200} \times 3{,}000 = 1{,}200.$$

"I had supposed from theory that 1,500 or 1,000 might be required: but 'resolution' is of course a vague thing."

It was not until five years later that he completed the subject by investigating the more ordinary form of spectroscope in which a prism is used to form the spectrum instead of a grating. He wrote:—

"At the time the above paragraph [on resolving power of gratings] was written, I was under the impression that the dispersion in a prismatic instrument depended on so many variable elements that no simple theory of its resolving power was to be expected. Last autumn, when engaged upon some experiments with prisms,

[1] The observing telescope would, of course, have to be made larger to cover the larger grating. Hence a close ruling of the grating is generally advantageous in practice.

I was much struck with the inferiority of their spectra in comparison with those I was in the habit of obtaining from gratings, and was led to calculate their resolving power. I then found that the theory of resolving power of prisms is almost as simple as that of gratings."

The general principle of this investigation is easily understood. Suppose for the moment that we limit our consideration to equilateral prisms of ordinary dense flint glass, and of 60 per cent. angle, used in the position of minimum deviation in the ordinary way, the breadth of the beam which such a prism will admit within it depends of course on the length of its refracting sides, or, what is the same thing, the length of the base, which we will suppose to be one centimetre (a little less than ½ inch). Owing to the deviation of the prism, this breadth is considerably foreshortened in the emergent beam, which is, in fact, ·57 cm. broad. We apply a telescope to this beam, and exactly as in the case of the grating we shall find that the image of each soda line has a small but definite breadth, being bordered on both sides with minima, alternating with subordinate maxima. The angular distance from the centre to the first minimum is smaller the greater the base of the prism; in the particular case we have taken it is 12·5 seconds of arc. The two component lines are differently deviated by the prism [1] and the angle between the two emergent beams is 12·3 seconds. This is just about enough to separate the two patterns to the extent required for resolution.

Now suppose that the prism was enlarged, say, to double its dimensions, without change of shape. The emergent beam would be twice as broad, and the central maximum of the pattern would be narrowed to half its previous dimensions, so that the separation would be twice that required for resolution.

Instead of using the larger prism, we might have used two prisms of the original size in succession. This would have

[1] If they come from the same direction they cannot both be exactly in the position of minimum deviation, but this makes no important difference.

left the breadth of the central maximum of each pattern the same, but have doubled the angular separation for the two, so that this is an alternative way in which the resolving power might thus be doubled, and it will be noted that in either case the resolving power is proportional to the length of glass traversed at the thick end of the prisms; assuming, of course, that the whole breadth of each prism is utilized, right up to the refracting edge. This relation can be generalized, no matter what the angles of the prisms themselves, or the angle at which they are presented to the incident beam. The only thing that affects the resolving power is the total thickness of glass traversed. We may get this by one big prism, or by a number of small prisms, as convenience may dictate. One centimetre of flint glass will resolve the soda lines, differing in wave lengths by $\frac{1}{1,000}$ part. Ten centimetres of flint glass would just separate two lines differing by $\frac{1}{10,000}$ part, supposing that the two were in the yellow part of the spectrum, like the soda lines.

It will perhaps seem strange that considerations so simple as those which have been described should have been fundamentally novel at so comparatively recent a date. It seemed so to Rayleigh at the time.

He wrote from Terling (August 5th, 1879): "I have finished writing the first part of my optical work, and shall send it off to the *Phil. Mag.*, in a day or two. I wonder how it will strike others. To me it now seems too obvious."

Things very often are obvious when they have been done.

CHAPTER VII

THE CAMBRIDGE PROFESSORSHIP AT THE CAVENDISH LABORATORY

On November 5th, 1879, Clerk Maxwell, who had been in failing health for some time, died at the early age of forty-eight, a loss to science that could hardly be overrated. The question of his successor, which must in all probability have been in men's minds at Cambridge for some months, immediately arose, and the general feeling was that if Sir William Thomson could not be induced to reconsider his refusal of 1871, Rayleigh would be the right man. Indeed, I have not been able to learn of any other name being mentioned.

The letters which follow tell the story of what ensued. It will be seen from these that one important determining consideration with Rayleigh was the loss of income which had resulted from the agricultural depression of 1879, and which made it difficult for him to continue to live at Terling as before. On first succeeding to the property, a much larger income was reasonably anticipated, and he had in view the possibility of maintaining at Terling a research establishment on a considerable scale, with several salaried workers. This scheme, which probably never took very definite shape, was necessarily abandoned with the depression of agriculture, and he may perhaps have felt that the Professorship at Cambridge, which would give others the opportunity of working under his advice, would be a substitute.

Sir William Thomson wrote (November 17th, 1879) :—

" Your letter was forwarded from Glasgow and reached me here this morning. I feel that my destiny is fixed for Glasgow for the rest of my life. I felt strongly attracted to Cambridge at the time

the new chair of Experimental Physics was founded, but resolved then to remain in Glasgow.

"If you could see your way to take the Chair it would I am sure be much for the benefit of the university, and of science too, as the Cavendish Laboratory would give you means of experimenting and zealous and duly instructed assistants and volunteers and would naturally lead you to more of experimental research than might be your lot, even with all your zeal and capacity for investigation, if you remain independent. If, however, you feel that in taking the professorship you would be taking a burden on you to any degree uncongenial or inconvenient or tending to occupy time which you might more advantageously spend in writing or in independent research, and that you would only hold it till some of the younger men might show suitable qualifications, I would not advise you to take it. . . ."

Rayleigh wrote from London (November 12th, 1879):—

"Stokes had a good opportunity last night at the Athenæum of sounding me about the Professorship, and as he did not take it I rather conclude that I am not so necessary to the welfare of the university as Frank (Balfour) supposes."

In the light of later events this may perhaps be put down rather to Stokes' well-known habit of silence than to the reason suggested.

He wrote to his mother from Terling (November 17th, 1879):—

"Did you see in the paper that they are getting up a memorial to me at Cambridge to offer myself for the Professorship of Experimental Physics vacant by poor Maxwell's death? Neither of us much likes the idea of living at Cambridge but perhaps I ought to take it for 3 or 4 years, if they can get no one else fit for the post. It would fit pretty well with the agricultural depression."

CHATSWORTH, CHESTERFIELD, *Nov.* 15*th*, 1879.

MY DEAR LORD,—

I understand that there is a strong wish among the Cambridge residents originating with those who take a special interest in the experimental along with the mathematical treatment of Physics that your Lordship would consent to accept the chair of Experimental Physics, which has become vacant by the death of the much lamented Professor Clerk Maxwell.

Though it is perhaps somewhat unreasonable to ask you to

undertake duties the discharge of which would involve heavy demands on your time, and might very probably be attended with no small personal inconvenience, I feel so strongly the advantage the university would derive from your acceptance of the office, that I hope you will allow me as Chancellor of the university, and also as taking a special interest in this Professorship, to support the appeal which I am told is about to be made to you, and to express a hope that you will consent to take the proposal into your favourable consideration.

I remain,
My dear Lord,
Yours faithfully,
DEVONSHIRE.

The Lord Rayleigh.

The memorial ran as follows :—

"WE the undersigned members of the Senate of the University of Cambridge, whose names are on the Electoral Roll, are of opinion that in the probable event of the establishment of the Professorship of Experimental Physics which has been terminated by the lamented death of Professor Clerk Maxwell, it would tend greatly to the advance of Physical Science and the advantage of the University that Lord Rayleigh should occupy the Chair."

Among the signatories were :—J. C. Adams, W. H. Besant, A. Cayley, J. Challis, G. H. Darwin, J. Dewar, N. M. Ferrers, J. W. L. Glaisher, R. T. Glazebrook, Wm. Hicks, W. D. Niven, E. J. Routh, W. N. Shaw, G. G. Stokes, H. M. Taylor, I. Todhunter, with many others.

To Lady Rayleigh.

ATHENÆUM CLUB, *Nov. 26th*, 1879.

We had a tedious day's work with —— College.[1] The Master is the most timid person I ever came across, and must think me a vile radical.

I dine to-night with Mr. Bouverie.

Yesterday I had some talk with Hemming and Stokes about the Professorship. No one seems to think it objectionable that I

[1] Referring to the work of the Commissioners in revising the statutes of one of the Cambridge Colleges (see p. 70).

should take it for a short time. From what our Secretary [1] Brown says I think there may be a difficulty about a suitable house. I saw Arthur (Balfour) last night and he still takes the same view.[2] . . . Lord S(alisbury)'s idea seems to be that if I failed with the Professorship I should lose my scientific reputation with the world.

TERLING, *Dec. 1st*, 1879.

MY DEAREST MOTHER,—

After much consideration I have made up my mind to stand for the Professorship. I quite feel that there is a good deal to be said against it, but the arguments in favour seem to me to have the preponderance, and if I refused merely on grounds of convenience I should be ashamed of myself afterwards. It is open to question whether in any case I could have gone on living here as hitherto, as my financial difficulties seem rather to increase.

Ever your affectionate son,

RAYLEIGH.

I wish, though I hardly expected, that the idea had commended itself to you more.

On December 12th, 1879, he was elected to the Chair.

On going into residence at Cambridge and looking into the state of things at the laboratory, Rayleigh found the stock of apparatus totally inadequate. As it was very generally supposed that the Duke of Devonshire had presented all that was required, along with the building itself, this surprised him, and he looked up exactly what Maxwell had said in his report to the University.

It appeared that his words were carefully guarded, and that he had never said that the stock of apparatus was adequate. The words were ; "The Chancellor has completed his gift to the University by furnishing the Cavendish Laboratory with apparatus suited to the present state of Science."

It is perhaps not difficult to understand how this had happened. As every experimentalist will realize, it is impossible to foresee requirements very far ahead. To buy everything that *might* be wanted in the way of standard articles would be very extravagant, and would not improbably

[1] Of the Commission.

[2] In an earlier letter he writes : "Arthur seems very much pleased with the idea."

result in buying many things that would never be used before they became obsolete. Moreover, standard articles only form a part of the requirements of research. Specially designed apparatus is often the more important part.

Maxwell could not, however, very well continue sending in bills to the Duke of Devonshire indefinitely, and doubtless this was the reason why he wound up the matter in the somewhat cryptic words that have been quoted. Current requirements were met out of his own pocket, and this naturally made workers in the laboratory very reluctant to ask for anything, however urgently needed.

To meet the deficiency Rayleigh got up a fund to which he himself gave £500, and the Duke of Devonshire another £500. Other contributions made it up to about £1,500, but resident members of the university were not asked to contribute from their (too often) slender resources.

The fees paid by students, which in those days were the Professor's perquisite, were also devoted to this purpose, and to the general expenses of the laboratory. As the numbers increased, this became more of a resource than at first.

It was Rayleigh's intention to develop general elementary instruction on a much larger scale than had previously been attempted, and for this purpose considerable numbers of galvanometers and other instruments were required of a simple pattern, allowing of cheap construction. At that time there was no commercial demand for class apparatus, and nothing of the kind was on the market. Professor James Stuart had recently established the mechanical workshops which were to be the germ of the engineering laboratory, and he offered help in this matter. Some of the necessary instruments were constructed in his department.

There was only one laboratory assistant inherited from Maxwell's regime. He was capable enough in any case when it was necessary to make himself unpleasant, as, for instance, in collecting students' fees which were in arrears ; or in dealing with railway employés who made a difficulty about accepting delicate apparatus for transport : but in all other respects

he was hopelessly incompetent. This was a source of considerable worry to Rayleigh, but it appeared later that the cause was partly to be found in failing health. After a few months he died.

A new assistant was advertised for, and George Gordon was selected out of a large number of applicants. He had been in business as a shipwright in Liverpool. His trade had given him a command of carpentry, wood turning, smith's work, and a number of other mechanical arts, and his enthusiasm as a scientific amateur had led him to spend much of his leisure in constructing an induction coil, mounting objects for the microscope, and other similar occupations. In this way he had acquired a varied knowledge which made him most useful in his new post. Rayleigh wrote, October 10th, 1880 :—

" Gordon has turned up and promises well, but is rather pompous in his language. I shall talk as much slang to him as possible. The Laboratory looks a good deal cleaner than I have ever seen it do before, and I hope now we shall be able to get things into shape."

Gordon would scarcely have passed muster as a skilled instrument maker, as the term would be understood in the trade, but this was no material disadvantage. Lacquering and French-polishing were not what Rayleigh wanted. He wanted something that would *work* without unnecessary delay. Indeed, if Gordon showed any signs of putting " finish " on to his constructions, or what he called " making a good job of it," Rayleigh usually took it away and put it into use before the process had got very far. But Gordon soon fell into his ways, and intervention of this kind was not often necessary.

Gordon aspired to a university degree and matriculated as a non-collegiate student ; but he found that with the limited time at his disposal qualification in the classical languages formed an insuperable obstacle. He remained with Rayleigh to the end of his (Gordon's) working life, and I shall have more to say about him in a later chapter.

Maxwell had been ill, and unable to attend much at the

laboratory during the last year or two of his life, and William Garnett, his demonstrator, had acted as his deputy. This had naturally led to Garnett devoting his attention to lecturing. The new Professor was required by the ordinances of the University to lecture himself, and he wished specially that the demonstrator should develop that experimental teaching of which he had stood in so much need in his own youth. Garnett, who had other work in the University unconnected with the laboratory, preferred to leave this to others, and he resigned his appointment as demonstrator. Those who had been trained in the laboratory under Maxwell were not numerous, and two of them were clearly marked out for the work. These were R. T. Glazebrook [1] of Trinity and W. N. Shaw [2] of Emmanuel, and they accepted the appointment, which they retained beyond the period of Rayleigh's tenure of the Chair.

Previous to his arrival the opportunities of practical instruction for the average undergraduate had not been adequate, and had by no means reached the standard set in Germany by Helmholtz. Lack of system was the deficiency. An undergraduate who entered for the laboratory course came when he liked, instead of at set hours, and his chance of getting promptly set to work depended on whether the demonstrator was disengaged, and whether suitable apparatus happened to be available for his use. The delays and disappointments of such a system or want of system soon took the heart out of an ordinary beginner, and those who attended at the beginning of a term would often tail off to a very small fraction of their original number.

It was necessary to be much more systematic. Each experiment was set out permanently on a table to itself, and written directions were provided. The classes were at regular hours, and a demonstrator was in attendance, who assigned the experiment, and gave help in any difficulty, finally approving or disapproving the numerical result.

[1] Now Sir Richard Glazebrook, K.C.B.
[2] Now Sir Napier Shaw.

This organization of laboratory instruction was chiefly the work of Glazebrook and Shaw, and an account by the former of what they did will be found in *A History of the Cavendish Laboratory* (Longmans, 1910). The course was planned out in consultation with the Professor. He rather leaned towards the inclusion of experiments requiring some degree of skill and persistence, as he had been painfully struck with the awkwardness that was only too common. "Anyone who could handle a thing without knocking it off the table was an acquisition," he said to me in later years. He suggested including in the course the accurate adjustment of a metal weight to agreement with a standard. "They'd never take the trouble to do that," said Glazebrook. "Well," said Rayleigh, "I think I know how to remedy that," and he set the problem in the examination for the Natural Science Tripos.

The course as it eventually shaped itself was published in book form as Glazebrook and Shaw's *Practical Physics.* Several of Rayleigh's own early experiments are included among those described, for instance his method of measuring the wave length of high-pitched sounds by exploring the nodes and loops of stationary vibrations with the sensitive flame.

The school of natural science at Cambridge was in those days still in its childhood, if no longer in its infancy, and naturally the numbers attending at the Cavendish Laboratory were not great. Rayleigh lectured first "On the Use of Physical Apparatus" and then on Galvanic Electricity and Electro-magnetism.[1]

The number attending was only sixteen, and remained about the same throughout his tenure of the Chair. They included one or two senior members of the University, and these did not hesitate to break in upon the lecture with ques-

[1] The phrase "galvanic electricity" sounds strangely old-fashioned at the present day. It is reminiscent of the time when the "identity of the various electricities" was still an open question. The epoch of the lectures so entitled was, after all, merely half the way back to that time.

tions; this, no doubt, was good within limits, and added an interest, but it could hardly be encouraged with the much larger classes of to-day.

An early recollection of my own is of being taken in by my mother for a few minutes while the lecture was going on. We sat at the back (top) bench of the lecture room, so as not to create a distraction. Rayleigh was showing an experiment, involving a jet of water. I think it must have been the breaking up of such a jet under the influence of electrification. The notes show that he treated this subject in considerable detail. I remember being much pleased to see how the flow could be controlled by nipping a rubber tube with a pinch-cock. The notes also show that he did not trust to working out calculations correctly during the lecture, but preferred to have them written out ready to copy upon the blackboard. There are notes of elementary courses on electrostatics and magnetism, current electricity, and sound, and advanced courses on electrical measurement. He does not seem to have lectured on Optics or on Heat.

The following appreciation of Rayleigh's work in organizing the laboratory is by Mrs. Sidgwick :—

"I think the way he affected other people and his success in inspiring work and in getting others to work with and for him was largely due to his gentleness and his sympathetic interest in what others did. He hardly ever betrayed any irritation, or hustled or worried people, and he never put himself forward unduly or allowed personal ambition to prevail. His desire was manifestly for the general good and the advancement of knowledge in whatever way was best.

"I think he must have enjoyed his time at Cambridge. I always had the impression that he did. He was an excellent teacher and lecturer and knew well I think how much he had succeeded in developing the Cavendish laboratory in the direction of his own idea of what a university laboratory should be.

"It was characteristic that he looked at things in a broad all-round way—never lost sight of the wood for the trees. If

things went wrong, and there was no obvious reason to suspect a particular mistake, he began at the big end, thinking of the general principle of the experiment and studying from that end whence the error might have come in—not first at the small end—the calculations or workmanship or reading of the apparatus.

"The same characteristic appeared also in his early reflections on a theoretical piece of work,—the kind of thing he could explain to one before he had tried to work it out methodically. I think too it underlay his love of home-made apparatus. He liked it reduced as far as possible to its bare bones."

Going back somewhat in time, it will be remembered that my father had met Eleanor Balfour at the same time as her sister. He had guided her mathematical reading on the Nile trip of 1872. At the end of 1875 she became engaged to Henry Sidgwick, whose friendship with my father and with Arthur Balfour had dated from their undergraduate days at Trinity, when he was assistant tutor in Moral Science. They had also been drawn together over experiments in spiritualism, and as we have seen he helped at Terling in some of the séances with Mrs. Jencken. During the early part of 1876, when they were engaged, Arthur Balfour (for whom ordinarily she kept house) was abroad, and she lived at Terling, while Sidgwick frequently came over from Cambridge for week-ends. After the marriage they set up house at Cambridge, at Hillside, in Chesterton Road, and she found a congenial occupation in assisting Rayleigh at the Cavendish Laboratory during the whole of his tenure there. Her patient accuracy and neatness of hand were of great value to him. She kept the notebooks on the later electrical measurements, and also made and checked most of the long arithmetical computations involved, as well as taking part in the observations themselves. Her name appeared as joint author of several of the more important papers.

When Rayleigh first went to Cambridge as Professor, he had not formed any definite scheme of research work for himself or others and the first experiments made were a continu-

ance of those which he had already begun at Terling on the action of electricity on water jets. They were carried out with Mrs. Sidgwick's help in the small room on the first floor between the class room and the apparatus room. It was then known as the Professor's room, but has since been used as an overflow from the class-room next door. These experiments were begun in April, 1880, three months after he had come into residence.

In the meantime a larger scheme was maturing in his mind. I quote the following from Sir Arthur Schuster [1] :—

"One idea to which he attached importance and which was entirely his own, was to identify the laboratory with some research planned on an extensive scale so that a common interest might unite a number of men sharing in the work. As a suitable subject he selected the redetermination of the electrical standards, and tried to obtain voluntary workers to take part in it. In this he was not altogether successful, partly because the number of sufficiently advanced students was not great, and perhaps also on account of the natural wish of the beginner to try his hand at a problem in which he could show his individual powers. Mr. Horace Darwin, who assisted in the work in its preliminary stages, was prevented from continuing by other occupations. I was then engaged in spectroscopic investigations, but had sufficient faith in the advantage of the general scheme and the importance of the special problem to abandon for a time my own work."

To give some idea of what is meant by a "redetermination of the electrical standards" a disgression will be required. I shall not, however, go into the matter fully. Rayleigh was not concerned in the original ideas, which were elaborated while he was still a youth, mainly by H. Weber and Sir William Thomson (Lord Kelvin). They are now explained in all but the most elementary electrical textbooks, and the reader must be referred to one of these for details.

The expressions ohm, ampere, volt, are probably familiar to the reader. How do we know that a given conductor has a resistance of 1 ohm ? Practically, by comparing it with a standard ohm, i.e. by showing that it conducts electricity

[1] Obituary notice in the Royal Society's *Proceedings*, A, Vol. 98.

as well and no better, when it is substituted for the standard.

So far the case appears analogous to the measurement of length, when we conclude that a bar is 1 metre long if it agrees end for end with the international standard metre bar. If it is asked how we know that the standard metre bar is really exactly a metre [1] the reply is that the length of that particular bar is the *definition* of a metre, and that the word has no other meaning. But if the same question is asked about the standard ohm, no analogous answer can be given. The ohm is *not* defined as the resistance of any particular existing coil. The metre (or the centimetre) is a *fundamental* unit, the ohm is a *derived* unit. To explain this distinction, let us take a simpler case than the ohm, namely the cubic centimetre or the litre (1,000 cubic centimetres) which are the continental units of volume. A litre is defined as the volume of a cube of 10 centimetres side. If we wanted to test whether a particular measuring vessel held 1 litre we should in practice see whether it had the same content as a standard vessel. But if the correctness of the standard vessel were itself in question, it would be necessary to determine whether its contents would exactly fill a cube with measured sides of 10 centimetres each, or to do something equivalent to this: so that the ultimate reference would be to the standard of length. We might call this procedure "determining the litre." Determining the ohm, which Rayleigh undertook, is in principle analogous.

A resistance coil is prepared, and its resistance is determined in terms of the ohm, as theoretically defined. This is an elaborate and difficult task; but when once it is done, we have an intermediate standard of resistance which may be copied at pleasure, and with which any resistance we may meet with in electrical practice may readily be compared. The litre depends on the unit of length only, but the ohm is defined by reference to two fundamental units, namely length (the centimetre) and time (the second), and any method of determining it must necessarily involve both of these. The best known of these methods is what is called the British Association method,

[1] Abstraction is made of the question of temperature variations.

and was devised originally by Professor William Thomson (Lord Kelvin). A circular coil is caused to rotate uniformly about a vertical axis and a magnetic needle is suspended at the centre. The coil rotating in the earth's magnetic field has an electric current induced in it, and this produces a magnetic effect on the needle, tending to deflect it out of the magnetic meridian (approximately the N. and S. line) in which it would otherwise lie. This deflecting force is not uniform, it is true. The current is (on a rough view of the matter) reversed in direction in the coil each time the latter passes through the magnetic meridian, and if this were all the couple on the needle would alternate in direction, and there would be no steady deflection. But it must be noticed that the coil is reversed twice in a revolution relative to the needle, and in this way the effect of the other kind of reversal is neutralized. Thus there is a deflecting force which, if the coil spins fast enough, produces a practically steady angular deflection of the needle, which can be measured.[1]

Now what can this angular deflection depend upon? It would be tempting to reply that it depends in part on the strength of the earth's magnetic field, which determines the current in the coil. But on consideration it will be seen that this is not so. If the strength of the earth's field were doubled, the induced current and the deflecting force which results from it would be doubled in consequence; but the controlling force which tends to hold the needle in the magnetic meridian would be doubled also. Hence the direction of the needle, dependent on the relative values of these forces, is unaffected. The angle of deflection then does not depend on whether the earth's magnetic force is weak or strong. It depends only on the diameter of the coil, the rate at which it spins, and the ohmic resistance of the coil itself which, of course, determines how large a current can flow. I do not enter upon the algebraical expression of this relation. It is enough to notice that the diameter of the coil requires reference to the fundamental

[1] An angle which may be reckoned as a fraction of a complete revolution requires no reference to the unit of length.

unit of length, and the number of turns per second to the fundamental unit of time. If these and the angular deflection are determined with precision, we can calculate the resistance of the coil in ohms.

This experiment as described had been carried out in 1863–4 under the auspices of a committee of the British Association, by Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin. The work was done at King's College, London, where Maxwell was then Professor. The committee as the result of their experiments issued a standard resistance coil, intended to represent 1 ohm, which became known afterwards as the British Association (B.A.) unit. In the sixteen years that had elapsed since their work was completed, serious doubts had arisen as to its correctness. The ohm had been determined by other methods. Kohlrausch (1874) had found it 2 per cent. too great, and Rowland (1878) nearly 1 per cent. too small. On the other hand H. Weber (1878) had substantially confirmed it. The differences between these capable experimentalists are eloquent of the difficulty of this kind of work. It may be suggested that after all a difference of 1 or 2 per cent. was not of substantial importance, but this is a totally mistaken idea. On the scientific side, the law of the conservation of energy and the electro-magnetic theory of light may be considered as two of the most important conceptions we have. As long as the value of the ohm is uncertain, the experimental verification of these principles remains equally uncertain. On the commercial side, a knowledge of the ohm is essential to an estimate of the mechanical efficiency of dynamos and motors, and it is hardly necessary to emphasize the enormous sums of money that an error of 1 or 2 per cent. on these estimates would now involve. It is true that at the time the prospective importance of this latter consideration could hardly have been foreseen.

In deciding to attack the question Rayleigh was partly influenced by the fact that the original apparatus and the standard coils themselves were in the Cavendish Laboratory, although the apparatus was of smaller size than was desirable.

He characteristically decided to do what he could with the means which were at hand before embarking on anything more ambitious.

The committee had determined the rate of revolution of the coil by the simple method of timing it over a large number of revolutions. It is however obviously much better to have something which shows at a glance and with full accuracy whether or no a certain standard speed is being maintained. In this way the time taken in getting a simultaneous value for the rate of revolution, and the deflection of the needle, is much diminished, and it becomes less laborious to get a series of readings at different speeds. In order to do this a tuning fork electrically driven was introduced as an intermediate standard of time. The prong of this tuning fork was arranged to carry an obscuring screen, so as to open and close an observation window twice in each complete vibration. The rate of the fork was $63\frac{1}{2}$ vibrations per second. A toothed wheel painted on card was attached to the spinning coil, and observed through this periodically opening window. The interval between successive openings is $\frac{1}{127}$ second, and if the rate of rotation is such that this is exactly the time taken for one tooth to pass, then the disc will appear stationary. If the true speed of rotation is slightly more or less, then the wheel will appear in slow rotation either forward or backward as the case may be. The same effect may often be seen on the cinematograph, the spokes of a carriage wheel apparently moving forward, remaining stationary, or moving backward, according to the frequency of the passage of the spokes, compared with the frequency of the successive pictures, which in this case takes the place of the frequency of the tuning fork periodically opening an observation window.

By this arrangement the constancy of speed was tested and controlled. A small water motor was used, capable of producing a rather more rapid rotation, and it was slowed down by holding the driving string more or less tightly between the fingers, until the desired effect was produced.

Although as has been explained the strength of the earth's

magnetism does not enter into the result, it is obvious that changes in its *direction* will alter the zero from which the deflection of the magnetic needle is to be measured. This was provided for by an auxiliary magnetic needle, whose deflections were read from time to time as the experiment was in progress.

In the early trials, made in the summer of 1880, disturbances of the needle were found when the coil circuit was open so that no electric current could flow, and these were traced to a fanning action of the revolving coil, which pumped air in and out of the box, open at top, in which the needle with its attached mirror was hung. This, it was found, depends on the tendency of any such obstacle to set itself athwart a stream of air (or water). In this case the stream is alternating in direction, but for this particular action the reversal makes no difference, and an alternating stream of air is as effective as a direct one. Although the effect first presented itself as a disturbance to be eliminated, Rayleigh afterwards utilized it to make an instrument capable of measuring the intensity of the aerial vibrations set up by a sounding body, and in this way it led to an interesting by-product of the main investigation. These preliminaries occupied the summer of 1880, and in October the work was recommenced. The apparatus had been set up on the ground floor of the laboratory, in the room then known as the " magnetic room," and afterwards the scene of Sir J. J. Thomson's classic experiments which led to the discovery of the electron. The revolving coil was set up on a brick pillar, which now bears a tablet put up by Sir Ernest Rutherford, to record the fact. The observations were made late at night, to avoid magnetic and other disturbance. Rayleigh regulated the speed, Dr. Schuster took the main readings, and Mrs. Sidgwick recorded the readings of the auxiliary magnetometer.

The experiment is necessarily somewhat complicated by the self-induction, or electrical inertia, of the coil. The current induced in the coil is an alternating one, and such a current traversing a coil of many convolutions meets with an obstacle

of a different kind from the resistance to a steady current. The more rapid the revolutions the more important this obstacle becomes, and the result is that the deflection of the needle does not increase quite proportionately to the speed. The law of variation however did not conform to what was expected, and much trouble was encountered in tracing the causes of this, which were eventually found in more than one erroneous statement made by the original B.A. committee. For one thing, they had confused what they meant by the "breadth" and "depth" of the coil. The coil may be compared to a coil of rope on the deck of a ship, when the axial dimension would be called the depth, and the radial dimension the breadth. On the other hand, when we consider the coil as wound in a groove on a brass frame the radial dimension of the groove would be called the depth. There was no doubt that a confusion of this kind had arisen, and it is a striking lesson in the value of unambiguous language, at any rate outside the field of politics.

Partly but not wholly as the result of this confusion, they had arrived at a value for the self-inductance of the coil which Rayleigh and Schuster showed to be seriously wrong. This quantity is a purely geometrical one, but its computation from the measured dimensions of the coil was difficult and laborious. The result was confirmed by an electrical determination. After this the experiment was pursued to the best conclusion that could be got with the old apparatus, and it was found that the true ohm was about 1·1 per cent. greater than the B.A. unit. Most, but not all of the discrepancy was traceable to the wrong value which the committee had taken for the inductance, but the confusion about breadth and depth, which enters in another way as well, perhaps accounted for most of the remainder.

These experiments were concluded about the end of 1881. The reduction of the results was by Dr. Schuster, who had kept the notes, and the second part of the paper, giving the details of actual experiments, was written by him. The whole was published early in 1882.

It was considered that the residual error of the experiments was mainly in the difficult linear measurement of the dimensions of the coil, and before the first experiments were completed a new apparatus was under construction. This was much larger and heavier, and was designed more on engineering lines. The general arrangements and the personnel were the same as before, and in the early summer of 1881 readings were in progress. Rayleigh wrote on June 8th, 1881 :—

"Our little discrepancies in the B.A. experiment are annoying. The two sets at one speed (35 teeth) agreed so badly that we took two more sets yesterday afternoon, in the hope of swamping the error. There seems to be something in it that we don't understand. . . . There was a fire in the anatomical laboratory yesterday. Clark was showing somebody how safe the spirits [1] were."

These discrepancies were traced to shiftings of the paper scales used for reading the deflections, which were mounted on wood. This trouble was eliminated and a new series of measurements was undertaken.

About this time Dr. Schuster was appointed Professor of Physics at Manchester, and the part which he had taken devolved upon Mrs. Sidgwick. Lady Rayleigh often came to the laboratory to help by taking the readings at the auxiliary magnetometer, and other volunteers were occasionally pressed into this service. I think that Arthur Balfour was one of them.

In August a new trouble presented itself, recorded in these words :—

"It will be seen that the agreement is good except on Aug. 29th, in which case the deflections are as much as 4 or 5 tenths of a millimetre too small. These discrepancies, though not very important in themselves, gave me a good deal of anxiety, as they were much too large to be attributed to mere errors of reading, and seemed to indicate a source of disturbance against which we were not on our guard."

No light was obtained on the mystery until October, when

[1] Used for preserving specimens.

all attention was focussed upon it. Eventually, it appeared that a copper contact piece used to close the circuit of the coil was in fault. It was necessary to remove this every time the spinning coil was to be compared with the standard; and it was found that one of the legs, riveted into the place, had become loose and shaky. In all probability it had been accidentally dropped on the floor. The resistance of this piece had been indefinite, and accounted for the discrepancies. It was of course an error of judgment to rivet the piece, instead of soldering it, or better, making it solid. But it was one of Rayleigh's few oversights not to have attended to this detail himself, and he had to pay dearly for it in one of the most troublesome incidents in his career as an experimentalist.

At last all was right and the final series of observations was completed before the end of the year 1881. The result for the B.A. unit was 3 parts in 1,000 less than that from the preliminary investigation. This difference was very small compared with the total uncertainty previously existing. At the same time Glazebrook had completed a determination of the ohm by quite a different method which gave a result closely accordant.

Moreover, the new value gave a most satisfactory reconciliation of the dynamical equivalent of heat as determined electrically and mechanically.

12 Wardle Road, Sale, nr. Manchester,
Jan. 6, 1882.

My dear Lord Rayleigh,—

My last experiments with the agitation of water gave me 772·55 as the gravitation mechanical equivalent of heat, which multiplied by $g = 3{,}219$ gives 24,868 for the dynamical equivalent. The experiments on the heat evolved by electric currents in a wire whose resistance was measured by the British Association Unit (*Brit. Ass. Report*, 1867, p. 522) is 25,187. Your first correction (·989) of the Association Unit brings this to 24,910 and your second correction (·987) to 24,859. By the first my result from electric currents is 42 more than that derived from the friction of water, the last only 9 less! It is an extraordinary and gratifying result to all of us, and I congratulate your lordship and Schuster on

the admirable experiments you have brought to so successful an issue.

Believe me, my dear Lord,
Yours sincerely,
JAMES P. JOULE.

The Rt. Hon. Lord Rayleigh, F.R.S., etc., etc.

I have some experiments yet to finish which possibly may indicate a very small correction of my equivalent, but as that will apply equally to the two methods it will have no effect on their mutual relation.

The work done on the B.A. experiment was made the basis of a course of seven advanced lectures, probably given about this time.

Many people would have been content to let the determination of the ohm rest at the apparently very satisfactory point to which it had now been brought. Not so Rayleigh. On carefully reviewing the other methods available, he came to the conclusion that one which had been employed by Lorenz in 1874 offered important advantages.

This method depends on using the electro-motive force induced in a brass disc of measured diameter, rotating uniformly in a magnetic field generated by two coaxial coils, one above and the other below it. The current which traverses these coils also traverses the resistance under test: the electro-motive force at the terminals of this resistance is balanced against the electro-motive force of induction by adjusting the amount of resistance, and in this way a determination of the absolute value of the adjusted resistance can be made.

The quantities which enter into the calculation must, as always, fall under the headings of time and length. The time enters as the rate of rotation, and it was determined exactly as before; the same apparatus was brought into use for securing uniform rotation. The important difference was in the fundamental length measurements. These are the radius of the disc and the diameter and distance apart of the coils. It might appear at first sight that there was no advantage here over the B.A. experiment, for the need for measuring the diameter of the coil with full accuracy was the least satisfactory

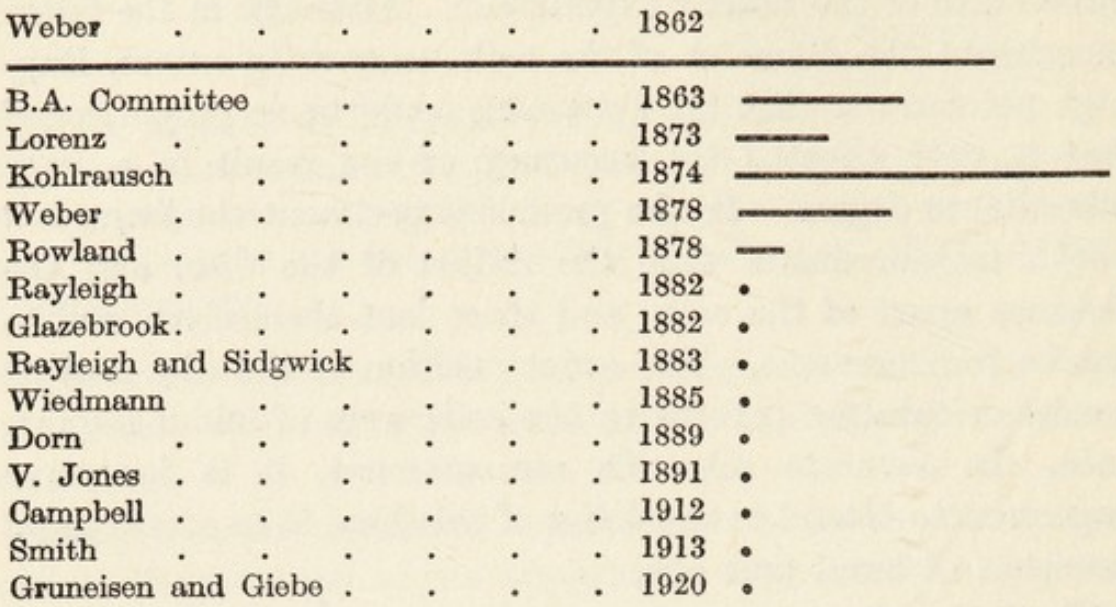
feature of that method. A coil of wire is by no means so good an object for precise measurement as, e.g., the accurately turned disc of the Lorenz experiment. Although in the latter experiment the diameter of the coils necessarily enters, Rayleigh pointed out that the apparatus could be so proportioned that it only affected the accuracy of the result in a very subordinate degree. In the present experiment the *important* length measurements were the radius of the disc, and the distance apart of the coils, and these lent themselves well to precise measurement. The exact position of the disc and its precise orientation parallel to the coils were of minor importance. In accurate scientific measurement, it is far more important to attend to subtleties of this kind than attain great precision of hand and eye.

The experiment seemed promising enough, but small and obstinate discrepancies persisted in making their appearance. "Two months' work had already been spent on the experiments, and we had begun to despair of a satisfactory issue, when it occurred to us that the connection of the coils for compounding the effective resistance was faulty." The fault, which was one of design, was a very subtle one, and it is hardly worth while here to enter upon the long explanations which it would require. When once detected, the remedy was easy, and all went smoothly afterwards. The final result was in excellent agreement with the B.A. method. The observations were finished in August, 1882, and published early the next year with Mrs. Sidgwick's name as joint author.

This concluded the three years of Rayleigh's work on the problem of the ohm, and it is of interest to review the results and to see how they compare with those of others. For this purpose they may be expressed in terms of the length of a column of mercury, 1 mm. square, which would have a resistance of 1 ohm. This length, according to the most recent determination of the ohm, would be 106·245 centimetres.

Taking this as the true value, it is of interest to exhibit graphically the errors made by the various experimenters at different epochs. The diagram exhibits the actual divergencies

in centimetres. For convenience, they are exhibited without distinguishing errors of excess from errors of defect.



The diagram shows clearly enough that the values obtained at the Cavendish Laboratory in the 'eighties were far more accurate than any done before. Moreover, though the size of the diagram is not large enough to show it, they were considerably more accurate than some done later. They practically left nothing to be desired. This is not of course said in depreciation of the admirable work of some of those who have attacked the problem since then, and have introduced important improvements of method. Full confidence can only be attained by a consensus of different experimenters.

The ampere, like the ohm, requires to be experimentally determined before the system of electrical measurement is complete: and this further task was undertaken by Rayleigh with Mrs. Sidgwick. The ampere cannot be well embodied in a secondary standard in the same sense as the ohm can be, and in this case the problem resolves itself into measuring an electric current absolutely in amperes, and determining at the same time at what rate it will deposit silver by electrolysis. Each part of the problem has its special difficulties. Rayleigh came to the conclusion that the most accurate method of current measurement would be by the mutual attraction of two coils conveying a current, which could be measured directly in

absolute units by weighing. The pull between the coils would seem at first sight to depend on how large they are, and thus to introduce the difficult measurement of their absolute dimensions. But this Rayleigh showed was not the case, so long as the sizes of the coils and their distance apart maintained the same *relative* values. By choosing the distance apart so that the attraction was at its maximum, the attraction depended practically on the *ratio* of the radii of the coils. And this could be determined by a purely electrical method.

It was only necessary to place the coils concentrically in the same plane and to send electric currents in opposite directions round each, so as to produce zero effect on a magnetic needle at the centre. The *ratio* of these currents would then give the required information about the radii of the coil. This may seem a roundabout way of comparing two lengths, but it was a dictum of Rayleigh's that an electrical measurement was incomparably easier to carry out accurately than any other. He meant, of course, a *comparison* of two electric currents or resistances, not the absolute measurement of either.

Much laborious work was spent in studying the process of silver deposition, with a view to accuracy, but this is not the place to dwell upon it. Closely concordant results were eventually got with nitrate and with chlorate of silver. The final result was that a current of 1 ampere would deposit 1·11794 milligrams of silver in 1 second.

The ohm and ampere had now been referred to the fundamental standards of length, mass, and time, and these measurements are sufficient to determine absolutely the volt, for by definition a current of 1 ampere, passing through a conductor of 1 ohm's resistance, implies an electro-motive force of 1 volt between the terminals of the wire. This, however, is not a convenient method of recovering the volt in ordinary practice, involving as it does a resort to the comparatively troublesome process of weighing silver deposits. The alternative is to have a standard form of voltaic cell and to determine its electromotive force once for all, utilizing the absolute measurements of the ampere and ohm for this purpose. Experimenters can

then readily make similar cells for themselves at any time, and the electro-motive forces of these will be known at once, from the standard data.

The form of cell selected was that known by the name of Latimer Clark,[1] which proved on trial to be much more constant than any other construction which had at that time been proposed. Rayleigh devised a special form called the H cell, from the shape of the glass vessel in which it was contained. In this zinc amalgam is used instead of rod zinc. It has since been widely adopted as a standard form, more especially for the modern cells using cadmium instead of zinc, which are preferable on account of their comparative independence of temperature changes.

The electro-motive force was determined by measuring the current with the current balance, and sending the same current through a resistance whose value was known in terms of the standard ohm. The absolute electro-motive force at the terminals of the resistance could then be calculated, and the Clark cell compared with it.

In this way the Clark cell was found to have an electro-motive force of 1·435 volts at a temperature of 15° C. This could be reproduced in new cells within about $\frac{1}{1,000}$ part, and the precautions to be taken in setting up such cells were carefully specified. The value considered most probable for the ampere as the result of recent determination is that it will

Mascart	1882	———————
Rayleigh	1884	.
Pellat and Potier	1890	———
Ayrton, Mather and Smith . .	1908	=
Janet, Laporte and Jonast . . .	1908	.
Smith	1910	.
Rosa, Dorsey and Miller . . .	1911	.

deposit 1·1181 milligrams of silver per second. As in the case of the ohm, it is interesting to exhibit in a diagram the de-

[1] It is believed that Dr. A. Muirhead took a leading part in the invention.

partures from this value in the various determinations. The diagram does not distinguish between errors of excess and errors of defect. To give some idea of the scale, it may be said that the largest error shown (that of Mascart's determination) represents about 2½ thousandths of the whole quantity in question. The diagrams show how Rayleigh's work has stood the test of time. The field has been gone over several times since, by skilled experimenters who had all the advantages given by the accumulated experience of their predecessors: but they have hardly established definitely that the ohm is either smaller or larger than the value that he arrived at. The same may be said of the ampere.

Whether a given wire coil or column of mercury is or is not of exactly 1 ohm resistance is of course not a legal question but a question of fact. Nevertheless, for the guidance of those who cannot form an independent opinion on such matters, and for the supply of electricity and electrical instruments, commercially, it is necessary to define for legal purposes what column of mercury is *to be deemed* to have a resistance of 1 ohm. The question was first raised at a conference held in connection with the Exhibition in Paris in 1881, and at this stage it was only agreed with difficulty that the ohm was to be used at all. Werner Siemens, in particular, was very anxious to retain the arbitrary standard known by his name of a column of mercury 1 square millimetre in section and 1 metre long. This would have had the merit of not legalizing any statement the exact truth of which might be open to doubt. Probably the exactitude attainable in determining the ohm was not then generally realized. Sir William Thomson was however most hostile to arbitrary units and was accustomed to express himself in no measured terms on such matters. Eventually Siemens was persuaded. Mr. J. Fletcher Moulton took him aside, and represented that the electrical units were now passing from the laboratory to the workshop and factory, and that the process would never be reversed. If this opportunity of putting them on a scientific basis were lost, there would never be another. Siemens ultimately agreed with a sigh. He said

that it was surrendering an important part of his life's work.[1]

The story of the Paris conference adjourned to 1882, so far as it concerned Rayleigh's work, is told in the following letter from Sir William Thomson. The other side of the correspondence is unfortunately not extant.

NETHERHALL, LARGS, AYRSHIRE,
Sept. 30*th*, 1882.

Have you received a notice *fixing* the time for the meeting of the international committee on electrical units ? . . . I hope you will be able to come. Dr. C. W. Siemens, who is with us just now, tells me that you *cannot* be in Paris on the 16th, but I hope this is not the case. We could get on but very badly without you, and in fact I suppose your ohm must be declared the one and true ohm for our generation. Could you not, in an international cause of such importance, and in direct relations with your professorial work, arrange to have lectures postponed, and laboratory work cared for in your absence for the week or ten days which should suffice for Paris and the Committee ?

Oct. 13*th*, 1882.

I have at last received a positive official notification that the 16th is fixed for the units committee, and I am going accordingly to Paris on that day. I don't leave London till Monday forenoon, so if you have anything more to send by me, I shall be able to receive it while here. Dr. Siemens had left us when your letter (which crossed mine to you) and paper for him came. . . . But will you not come to Paris, after all, *taking* leave of absence from Cambridge for the necessary time ? If you could come on Monday and remain a few days it would tend much to ease the way of the congress to the settlement of the ohm, not to speak of other questions. Let me have a line here unless you will cross over along with us.

HOTEL CHATHAM, RUE DAUNON, PARIS,
Oct. 19*th*/82.

We are all very sorry that you have been unable to be at the conference, and I am charged to express " des vifs regrets " that you have been prevented by illness.

The " sous commission " on the " fixation " of the ohm has held its last meeting to-day, and it is strongly impressed with the conviction that your number $\cdot 9865 \times 10^9$ is within 1/1,000th of

[1] I give these details on the authority of what the late Lord Moulton told me in conversation.

the true value of the BA unit. (Would you not rather, however, take as the most probable 9867, being the mean of your result by the old BA method and your two by Lorenz's method, ·9869 and ·9867 ?) I communicated this statement to them from your letter and also the statement that probably the comparison between the BA and the Siemens mercury unit made independently, is trustworthy to 1/4,000. I also communicated your printed slip, giving the results of your and Mrs. Sidgwick's comparison of the 4 tubes with the BA unit. This will all appear in the report of the meetings. I believe they would have decided on $(\cdot 9867)-1$ of the BA unit (or corresponding numeric of the S.U.) for the ohm, but that Friederich Weber was there defending some carefully made experiments of his own which gave him $\cdot 9550 \times 10^9$ for the Siemens unit, instead of ·9413 as you and Mrs. Sidgwick make it. It seems after all probable that it was the particular one or two standards *called* Siemens units which we had that caused the discrepancy, and that these results may after all be found to agree closely with yours.

Helmholtz proposed and the sous-commission adopted a resolution to move (viâ the French Government) on transmission of individual standard from one to another of persons who have made absolute determinations. This will be very valuable, but I shall try to arrange with Weber for immediate interchange of standards between you and him. Could you send me one here, which I could receive by Saturday evening, and give to him to take away with him ? That would settle the question no doubt in a few days. Or, with very little delay, there might be an interchange between you and him after he gets back to Zurich. He is himself under the impression that you are right, and so are Helmholtz and Wiedmann.

Our last general meeting will probably be on Monday morning, and terminate the whole business of the present session, and I think it is desirable it should be, because I think judging from the past good and useful work and convention will be promoted by at least one other meeting analogous to this and the last year's one. . . .

The conference of 1884 decided to adopt a column of mercury 106 cm. long and 1 square mm. section as representing the "legal ohm." This was obtained by taking a rounded mean of all determinations, but the English representatives, headed by Sir William Thomson, considered that it was unsatisfactory to average values differing among themselves by as much as

2 per cent., and that the recent (and, as it happened, the English !) determinations should have been given more weight. Accordingly the " legal ohm " was never adopted in England. In 1890 the question of a legal definition of the electrical units was reopened, with the advantage that the determination of the different workers could now be seen more in perspective. The Board of Trade appointed a committee to report on the question, with Rayleigh as a member, but this committee deferred their report till 1891, when the question was to be discussed at the British Association Meeting at Edinburgh. Helmholtz, who was the German representative, came on a visit to Terling before the meeting. Among other scientific members of the party were R. T. Glazebrook, A. A. Michelson and Mrs. Sidgwick ; while outside the purely scientific ranks were A. J. Balfour, W. Robertson Smith, George Wyndham, and several ladies, including Margot Tennant (now Mrs. Asquith). I remember that she made great friends with Helmholtz, and engaged him for a visit to the home of her family in Scotland. Preliminary discussions on the electrical units took place at Terling during this visit. Helmholtz found it an effort to talk English, and Robertson Smith took him out for an afternoon walk to give him a rest by talking German.

At Edinburgh the resolutions eventually agreed to by representatives of England, France, Germany and the United States were in effect that the ohm was to be taken as the resistance of a column of 106·3 cm. of mercury of 1 square mm. section at 0°. This was virtually Rayleigh's value, and fu ther resolutions adopted his figures for the silver value of the ampere, and the voltage of the Clark cell. The resolutions were communicated to the Board of Trade Committee, which adopted them.

They were also confirmed at an international conference held at Chicago in August, 1893, under the presidency of Helmholtz, and the names of International Ohm, Ampere, and Volt were given. Legal effect was given to these resolutions in the United Kingdom by an Order in Council dated August

23rd, 1894. In this way the commercial electrical units were fixed, though perhaps not one in a thousand of those who now use them in their daily work have any notion of how and by whom these units were determined.

To complete the story, I will mention here that an international conference was held on the electrical units and standards in 1908 under the auspices of the Board of Trade, and under the Chairmanship of Rayleigh, who was at that time President of the Royal Society. It was not thought necessary, however, to make any change in the ohm and the ampere as legalized in 1894.

Besides the work carried out by Rayleigh himself, or in collaboration with him, there were other investigations in progress at the laboratory, though not to the extent that was developed during the succeeding regime under J. J. Thomson. Glazebrook and Dodds made a determination of the ohm by a different method from any that Rayleigh used, and the former carried out a variety of other optical and electrical researches. W. N. Shaw published various investigations, chiefly on subjects connected with meteorology. J. A. Fleming was at work on resistance comparisons. Horace and George Darwin attempted to detect the lunar disturbance of gravity, J. C. McConnel was working on the rotatory property of quartz. Other workers in the laboratory whose names have since become known were R. Threlfall, L. R. Wilberforce, and J. C. Bose.

It was during Rayleigh's regime that J. J. Thomson made his earlier researches on the ratio of the electrostatic to the electro-magnetic units. This formed a natural sequel to the other absolute electrical measurements. Rayleigh designed the apparatus, and had contemplated directing the work himself. But he soon found that he had put it in the hands of one who even then hardly required guidance. "Thomson rather ran away with it," he said.

CHAPTER VIII

LIFE AND LETTERS DURING THE CAMBRIDGE PERIOD. THE BRITISH ASSOCIATION AT MONTREAL

During the tenure of the Professorship at Cambridge Raleigh lived with his family at No. 5 Salisbury Villas, in the Station Road. The establishment was somewhat cramped compared with what he had been accustomed to, but this was nothing to him. His day was taken up for the most part with work, and the lawn-tennis ground which almost filled the small garden at the back afforded him the opportunity of an occasional game with his brother-in-law and other friends.

He did not usually go to the laboratory in the mornings except to lecture, but worked at home at his writing table. The more important part of his scientific library had been brought over from Terling.

Family lunch followed and I remember the praiseworthy efforts he made to insist on French being talked, partly for my benefit and partly for his own—he was at no time able to speak it at all fluently. Frequent recourse to the dictionary was necessary, and the effort was not kept up very long. He was probably working too hard to have any energy left for extras of this kind.

Lady Rayleigh had an open carriage drawn by a pair of ponies, and she usually drove him down to the Cavendish Laboratory after lunch. Sometimes a short drive in the country would be taken first. The afternoons and sometimes the evenings also were devoted to experimental work. Tea was inaugurated in the Professor's room at the laboratory, and Lady Rayleigh often joined him and Mrs. Sidgwick. Other workers in the laboratory sometimes came too, and this gave

a valuable opportunity for intercourse and exchange of ideas on scientific subjects. An earthenware teapot with a broken spout was in use. Lady Rayleigh wished to replace it, but Rayleigh ruled that it would do very well. In the evening before dressing for dinner a few minutes would often be found for a visit to the nursery. I remember a competition as to who could count seconds most accurately, and a simple conjuring trick performed with the tongs.

At that time Frank and Gerald Balfour were in residence as Fellows of Trinity, and they with the Sidgwicks and my father and mother came together at a weekly dinner, at one of the houses or college rooms. Henry Sidgwick and the Balfour brothers were at that time inclined to be Radicals, and political discussions often waxed hot. There was some dining out with University friends, particularly the Darwins and the J. W. Clarks, and occasional dinner parties were given at 5 Salisbury Villas.

In July, 1880, Rayleigh's third son, Julian, was born. He was a weakling from birth, and his brief life of five years was a period of continuous anxiety to his parents.

In August they went to Roseneath in Argyllshire, which had been lent to Eustace Balfour and his wife by her father, the Duke of Argyll. They took me with them. Others of the party were Dr. Story, afterwards principal of Glasgow University, and Mrs. Anson, afterwards the Duke of Argyll's second wife. The Duke came in his yacht and joined the party. He was a keen naturalist, and used to discuss the flight of birds with Rayleigh. The discussions were not very fruitful, however. The Duke did not understand the principles of mechanics well enough to see the weak points of his own theories, and while Rayleigh was trying to frame some statement of his objections which he could understand, would pass on to something else, without realizing that Rayleigh considered his theory impossible.

The Duke took us an expedition in his yacht up Loch Long and through the Kyles of Bute to Inverary, where we stayed for a week or two. Here Rayleigh was very much interested

to learn that the Duke had often seen will-o'-the-wisps when a boy on the marshy ground near the castle, but that they had been less often seen in subsequent years, owing, he thought, to drainage operations.

YACHT *Lalla Rookh*,
FIRTH OF CLYDE,
Oct. 2nd, 1880.

DEAR LORD RAYLEIGH,—

My plans are of necessity very uncertain on account of the *Livadia*. I have to adjust two compasses on board, and two of my sounding machines, and to teach the Russian sailors how to use the sounding machine during some of the trial trips which are to be made probably next week. I should like if possible to see you either by meeting you in Glasgow, or which would be better, if I could persuade you to come and have a sail in the yacht to settle all questions of hydrokinetics, or to stay for a few days with us at Netherhall, our house at Largs. . . .

Remember me kindly to the Duke of Argyll and the ladies, and believe me yours truly,

WILLIAM THOMSON.

Life at Terling was not entirely given up during the Cambridge period. Rayleigh's brother Edward, who was now established as agent on the estate, lived in the house in a modest way, with his wife and growing family. During the Cambridge vacations we sometimes stayed with them. In this way the continuity of family Christmas gatherings was preserved with the customary distribution of a gift of bread and beef to each family in the village, which had been instituted in a former generation. The garden staff was greatly reduced.

The custom of staying at 4 Carlton Gardens in London before Easter had of course to be dropped, but a visit during June and July was substituted.

In March, 1881, Rayleigh was elected an honorary Fellow of Trinity, in succession to Maxwell.

He wrote, April 12th, 1881 :—

"I had to do the honours at Cambridge for Helmholtz, who came to get an honorary degree. He stayed two nights and brought his wife with him. There is not very much to be got out of him in conversation, but he has a very fine head."

I can remember this visit, and was very much impressed when I was told that the visitor was an even cleverer man than my father. I had a small horse-shoe magnet, as a childish treasure, and he suggested that I should fetch it: but it had been confiscated for some offence in the nursery. It was eventually released in honour of the occasion, and he showed me how the attractive power was concentrated at the ends.

In July, 1882, the family party at Cambridge was diminished by a tragedy. Frank Balfour was killed by a fall with his guide from the hitherto unclimbed Aiguille Blanche. I well remember when the telegram giving the news was brought in. I was in the drawing-room at the time, and was naturally sent upstairs at once, which I regarded as a grievance.

Although Frank Balfour's line of work—animal morphology—had little in common with Rayleigh's, his personal knowledge of Cambridge men had been of value to him when the Cavendish Laboratory organization was being built up.

Rayleigh, in spite of the caution and moderation of his character, always tended to be what is now called a "die hard" in politics. On the subject of Ireland he was a Conservative to the backbone. The Irish Land Bill of 1882 particularly roused his ire. "A thoroughly rotten Bill," as he described it. He wrote from Cambridge, August 12th, 1882:—

"As to the miserable business in the House of Lords, I feel at present inclined to join in the attack on the institution as useless. I got a telegram from Arthur [Balfour] about 1 o'clock saying that certainly I should be wanted [to vote] and went up with the idea that the thing was to be fought out. A second telegram sent after the meeting in Arlington Street was too late. I heard that not more than about *20* were in favour of standing out, though some more would have followed Lord S. in a division. Nothing could have been gained by pressure, because even if the amendment could have been insisted upon now, a retreat would have followed a second sending up of the bill, which Gladstone under these circumstances would have been sure to have done. I dined in Arlington Street afterwards. Arthur and Mr. Hope were there.

"The newspapers are down upon Lord S.; but he seems to me to have done right in a very difficult position."

In 1882 the British Association met at Southampton, and Rayleigh was President of Section A (Mathematics and Physics). His address was of a general character, traversing a wide range of topics, and explaining his general attitude towards the methods of physical research by means of illustrative examples. The address is reprinted in his *Collected Scientific Papers*,[1] and it is still well worth reading, but it is hardly necessary to dwell on it in detail here. I quote a characteristic paragraph.

"The different habits of mind of the two schools of physicists sometimes lead them to the adoption of antagonistic views on doubtful and difficult questions. The tendency of the purely experimental school is to rely almost exclusively upon direct evidence, even when it is obviously imperfect, and to disregard arguments which they stigmatize as theoretical. The tendency of the mathematician is to over-rate the solidity of his theoretical structures, and to forget the narrowness of the experimental foundation upon which many of them rest."

He wrote from Cambridge (August 14th, 1882):—

"I have cut the mesmerism out of the B.A. address, H.'s [2] opinion agreeing with yours. He thought that as all the other examples were taken from Physics, this looked rather dragged in. He repudiated the suggestion that it would be any improvement to call it Electrobiology."

During the later part of the meeting he went to stay with Lord Mount Temple at Broadlands, Romsey. He wrote from there (August 26th, 1882):—

"We have all come over here to stay till Monday at any rate and perhaps longer. . . . I find a nest of spiritualists. Crookes with wife, Barrett, B. Stewart, and one or two others whom I have not yet made out. I am glad I did not come here at first or my character would have altogether disappeared! Thomson gave a lecture on the tides last night which kept me in fits of laughter, and which I should have thoroughly enjoyed if having to second a vote of thanks had not been hanging over me. I brought in the story of the Oxford man who spoke of 'the influence of the moon on the tides and other popular superstitions.' I heard this

[1] Vol. II, p. 118.

[2] Henry Sidgwick's.

morning that I am supposed by some to have endorsed the view of the Oxford man in opposition to Thomson. To-day's rest has been very agreeable after being kept so close."

The following was written from Plymouth a few days later :—

"I have been living in a whirl since Wednesday. Yesterday I had an opportunity of seeing the telephone which everybody has been talking about. The extraordinary part of it is its simplicity. A good workman might make the whole thing in an hour or two. I held a conversation with Mr. Preece from the top to the bottom of the house with it and it is certainly a wonderful instrument, though I suppose not likely to come much into practical use.

"This morning I breakfasted with Sir W. and Lady Thomson on their yacht in company with Glaisher, father and son, Cayley, Foster our president of Section A, etc. I read a paper afterwards on the photographs of the ultra red, but did not succeed in eliciting any explanation of the discrepancy between my experiment and Herschel's. Adams gave us an interesting account of recent progress in the lunar theory. It seems too that Newton had made more [1] of a difficult point as to the revolution of the moon's apse than had been generally supposed. . . ."

In October, 1882, Rayleigh began to be troubled with feverish rheumatism, and it was feared there might be a return of the rheumatic fever which had nearly killed him in 1871. He was able to go to Hatfield for the coming of age of the present Lord Salisbury, but got worse after his return and had to take to bed, losing many weeks of attendance at the laboratory. At this time he was awarded one of the Royal Medals administered by the Royal Society, but was unable to attend to receive it. Sir George Stokes and Prof. Michael Foster, the secretaries of the Society, who were both Cambridge professors, came to Salisbury Villas to present it to him. During this illness the work of the laboratory was carried on by Glazebrook and Shaw, who frequently came to Salisbury Villas to consult him. When well enough to do so he amused himself with the six volumes of *Monte Cristo*. I remember that two or three years later, when we were out

[1] I.e. had gone further towards solving.

walking together, he entertained me with Monte Cristo's escape from the Chateau d'If. I pressed for the rest of the story. "The rest is not suitable," he said. "He devoted himself to revenge more than a Christian man should."

Rayleigh was advised to go abroad for the Christmas vacation. On the way through London Sir Andrew Clarke came in to see him professionally. He then went with Lady Rayleigh to stay with Gerald Balfour, who had recently given up his college rooms at Cambridge and settled in a villa at the village Alle due Strade, near Florence. The mornings were spent at work, and the afternoons in sight-seeing in Florence. They visited Pisa, and saw the leaning tower and Galileo's pendulum. The baptistery however was a still greater attraction with its wonderful echo in the dome, prolonging musical notes for many seconds. After paying one or two visits to friends in the South of France, they returned to Cambridge early in 1883.

The rheumatic attacks continued at intervals during the early part of 1883, and in the long vacation he was ordered to Homburg. There Rayleigh and Lady Rayleigh were in the same lodging as Lord and Lady Ashburton, but they occupied the humble *hinterhaus* to escape the constant sound of pianofortes in the street in front, which made mathematical work almost impossible. The ordinary regime of the place was followed, and the quiet time before lunch was occupied with long logarithmic calculations, probably those referring to the representation of Newton's scale of colours on Maxwell's colour diagram.[1] Rayleigh went through them first, Lady Rayleigh would check his results, and he examined her corrections until agreement was obtained. He had not the strength and facility in this kind of mathematical spade work that some mathematicians (Airey and Adams, for example) have possessed.

Various friends and acquaintances were there, among others Lord Strathmore and his son. His presence naturally reminded the other visitors of the stories about the Glamis

[1] *Scientific Papers*, Vol. II, p. 498.

ghost, and it was said that the heir, once told the secret of the undiscoverable room, never smiled again. But there was nothing in the cheerful aspect of Lord Strathmore to confirm this story. He told how they used to put out the lights and amuse themselves by telling ghost stories by the fire, and how there were various mysterious corners made when the staircase was altered. Lady Rayleigh diffidently said something about the secret room. "Ah yes," he replied, "but they will never find that." [1]

Rayleigh wrote from Homburg, July 7th, 1883 :—

"It is rather a change in our ordinary habits to be up two hours before breakfast drinking salt and water. I believe a handful of Tidman in a bucket of water would be much the same."

After Homburg they went to Heidelberg to see Prof. Quincke, with whom Rayleigh had corresponded on the copying of diffraction gratings and other subjects. They were hospitably entertained at a large luncheon party. Frau Quincke got up at intervals, presumably going to the kitchen to see after the next course ; and at each course the Professor brought out a fresh vintage wine laid down by himself, describing its history and merits, and calling upon Rayleigh to admire and enjoy. It is to be feared that his response was inadequate.

On leaving Heidelberg they drove to the station in uncertain mood as to whether they should return home or go to Switzerland, where an after-cure was recommended. They decided for the latter, and went by train to Interlaken, drove to Lauterbrunnen, and thence rode up to Murren.

Then Rayleigh was again laid up with feverish rheumatism, and he determined to take no more medicine, and to try whether the disease would not wear itself out. After a few days he was somewhat better and they ran for it. He was carried down the mountain in a chair by four men. They slept at Berne and went straight home to Cambridge.

During all these months Rayleigh was not without fear that he might be a cripple for life. However, things took a

[1] Query, perhaps because it has no objective existence ?

favourable turn, and the rheumatic attacks diminished. In the Christmas vacation he took the waters at Bath; afterwards, for whatever reason, there was no more of it.

Even while doing the cure at Bath an opportunity was found for scientific observation. He noted :—

"I found in the baths here that if the spread fingers be drawn pretty quickly through the water (palm foremost was best) they are thrown into transverse vibration and strike one another. This seems like Æolian string. . . . The blade of a flesh brush about 1½ inches broad seems to vibrate transversely when moved through water broadways forward. It is pretty certain that with proper apparatus these vibrations might be developed and observed."

Thirty years later this idea was put into practice.[1]

After his return to Cambridge he wrote (April 1st, 1884) :—

"I wrote to [Francis] Galton for advice as to joining the committee of the Athenæum, and he has risen in my estimation by advising me not to."

JOHNS HOPKINS UNIVERSITY, BALTIMORE,
March 6th, 1882.

MY DEAR LORD RAYLEIGH,—

When your very kind letter of last November reached me I employed a student to make the comparison with Warden, Muirhead and Clark's standard. I have delayed writing until I could give you the results. . . .

I have just completed in our workshop a machine for ruling gratings and it is a grand success, making gratings fully equal if not superior to Rutherfurd's and of larger size, the limit being 4 × 6 inches. Rutherfurd could only make one good grating out of very many, but my machine makes them as good as his *best every* time.[2]

The defining power is up to the theoretical limit. Rutherfurd's machine would only rule five lines a minute, while mine rules twenty or thirty, and even does good work for short lines at forty or fifty. I used an entirely new process for making the screw and it has no errors more than $\frac{1}{100{,}000}$ inch at any point. I thus believe I have solved the question of a perfect screw. I have also

[1] *Scientific Papers*, Vol. V, p. 315.

[2] I believe that the later experience of Rowland and his successors at Baltimore would necessitate some modification of this account.

invented an inverse method of treating diffraction problems by which I have discovered *concave* gratings which require no telescope. They will be invaluable for problems on heat distribution and the photography of the spectrum. The nickel line can be seen between the D lines by the naked eye, the image being on a sheet of paper. With an eyepiece the 1,474 line is seen double in the first spectrum and all the most delicate tests are easy in the higher orders. The machine rules any number of lines to the inch, but I generally prefer 14,438. I have made good gratings of 28,876 and even 43,000 lines to the inch. By good fortune the machine rules exact fractions of a mm. I intend to rule speculum metal with lines extending over a decimetre, making every hundredth line longer than the rest. With 400 to mm. this would rule ¼ mm. in the longer lines. Every physicist will then be able to compare his standard directly with the wave length of light by simply measuring the deflection angle.

The machine will also leave out every third line or any line desired. It can also introduce a periodic error making "ghosts" or any accidental error can be introduced at any point. I shall bring over specimens next summer and would be glad to try any experiments which you may desire and communicate results to you for publication.

Yours truly,
H. A. Rowland.

s.s. *City of Chester, Oct. 22nd*, 1882.

The Rt. Hon. Lord Rayleigh.

Dear Sir,—

I am on my way to Paris to see about those Electrical Units and hope to meet you there. I bring over a number of gratings and photographs of the spectrum which I would like you to see very much. If I do not meet you at Paris, I may stop a few days in London as I wish to ride on a regular English fox hunt, and also to go to the Royal and Physical Societies.

Yours respectfully,
H. A. Rowland.

The Mason Science College, Birmingham,
Dec. 14th, 1882.

Dear Lord Rayleigh,—

I thank you very much for the trouble you have taken about my paper. I shall be very much obliged if you will kindly communicate it to the R.S. for the Transactions, as if I fail there I shall have a chance for the Proceedings.

I have thought of the case of a single circuit with self-induction but did not put it in my paper. The steady cases are so much easier to follow, for there are the equi-potential surfaces to help one to imagine the flow. But if you think it advisable to add a section on self-induction it might come in my paper between the sections on reduced currents and velocity of light.

While it is quite clearly recognized that in self-induction energy goes out into the medium at make and comes thence at break, it is not yet so clearly recognized that this is merely a particular case of what is always going on, the energy at make going out to fill up the intervening space to the required amount for the steady state, the energy in the medium always moving from battery to circuit, and at break the amount still left outstanding on its way to the circuit has to come in.

I send herewith an additional section on self-induction. If you think it is worth while to put it in will you kindly insert it just before the velocity of light. If you do not please destroy it.

I had intended to limit the investigation of velocity of wave propagation to the case where the *form* travels on unchanged. In this case does it not follow that the energy passing a point corresponds to the energy of the passing waves ? My method depends entirely on the unchanged form. I had read your note in the Theory of Sound and it was I believe my starting point. I tried to get a simple proof of the velocity of sound and of transverse vibrations by a consideration of the flow of energy and an attempt to apply the method to Maxwell led to the law for the transfer of electro-magnetic energy. . . .

I enclose herewith the additional section on self-induction.

Thanking you once more for your kindness,

I remain,

Yours sincerely.

J. H. Poynting.

Rayleigh had been a diligent attendant at the British Association meetings from his early manhood onwards, and he was elected President for the Montreal meeting in 1884. This was the first meeting outside the United Kingdom. He was only 42 years of age, and there was no recent precedent for electing anyone so young. The circumstances were, however, somewhat special, and social considerations may have partly determined the choice. Sir William Siemens, the President for 1882–3, wrote (May 2nd, 1883) :

"The meeting at Montreal will be one of great importance for the future welfare of the Association as it is the commencement of a wider sphere of action! Contrary to the fears of many there is every appearance of success. Already over 400 members have signified their intention of going, and amongst them are 160 (about) of members of the General Committee, including leading members such as Sir W. Thomson, Sir Lyon Playfair, etc.

"It was the unanimous opinion of the council that the success of the meeting would be best secured under your Presidency, and I was empowered to communicate with you on the subject."

He accepted, but the thought of giving the Presidential Address was a nightmare to him, and in moments of depression he used to talk of cutting his throat!

(*Undated*) TRINITY COLLEGE, *Friday.*

I am so driven that I hardly know which way to turn. Having said nothing about it before, they now tell me they want the press proof of my address by to-morrow morning. . . . Arthur (Balfour)'s notes have reached me and I have made what use I can of them, but most of his criticisms go too deep. I gave up from the first being intelligible to most of the audience during the greater part, and so have made no attempt to explain some things which he would like explained. I believe the attempt to be useless.

Arthur Balfour wrote:—

WHITTINGEHAME, PRESTONKIRK,
August 15th, 1884.

MY DEAR JOHN,—

One line to convey to you my blessing. I expect you will have on the whole rather a good time.

Send me any extracts from the daily press of our transatlantic brethren which reflect on your dress, deportment, general appearance or proceedings generally; especially if the reflexions are of an unfavourable cast. Your responsibilities are appalling; for you represent not only Science but England and the Peerage! I have no doubt that the judgment of the Canadian press on the impending political crisis will largely depend on the effect which a living member of the hereditary chamber produces on them. The fate of the House of Lords may depend on the way you tie

those new cravats which the forethought of your wife has provided for you! Give my love to her; may neither you nor she have practical experience of the effect of violent oscillations on the stability of floating bodies, and of the reactions produced by the same on the nerve system of the stomach!

They sailed for Quebec on August 17th on board the Allan liner *Parisian*. Rayleigh's mother and his brothers Dick and Hedley accompanied them. Many leading members of the British Association were on board, including Sir Frederick Bramwell, G. Forbes, J. H. Gladstone, J. Dewar, Lord Rosse and W. Chandler Roberts (afterwards Robert Austin). The latter enlivened one of the evenings by delivering a nonsense lecture in the heaviest scientific manner, a feat which Rayleigh admired as hopelessly beyond his own powers. He was amused to find that some of the passengers had taken it *au grand sérieux*.

On arrival at Quebec they were met by an invitation from Lord Lansdowne, the Governor-General, and Lady Lansdowne, with whom they breakfasted and dined.

Rayleigh was somewhat alarmed by the crowded programme of receptions and entertainments which had been arranged, fearing that it might bring on one of his rheumatic attacks, and incapacitate him for the part he had to play. Fortunately, however, nothing of this kind happened. They left Quebec by steamer on August 26th, and arrived at Montreal the next day. Here they were hospitably entertained by Mr. Workman, a local magnate.

The Presidential Address was to be given in the Queen's Hall, and Rayleigh, who had omitted to bring a ticket, had an amusing controversy with the doorkeeper, who refused to admit him. "Well," he said, "the proceedings cannot go on until you do." The meeting was crowded.

Lord Lansdowne, who had come by the same steamer from Quebec, spoke first, dwelling on the advantages from an Imperial point of view which might be expected from the visit of the British Association. This was followed by a characteristic speech from Sir William Thomson, who represented

Cayley, the retiring President. Most of what he said was a warm tribute to the new President. "In reading some of the pages of the greatest investigators in mathematics one is apt to become wearied, and I must confess that some of the pages of Lord Rayleigh's work have taxed me most severely, but the strain was well repaid. . . . His book on Sound is the greatest piece of mathematical investigation we know of applied to a branch of physical science. The branches of music are mere developments of mathematical formulæ, and of every note and wave in music the equation lies in Lord Rayleigh's book." (Laughter and applause.) And more in the same strain.

Then followed the Presidential Address.[1] It did not break entirely new ground; nor, having regard to the fact that the speaker was only forty-two years of age, could it sum up a life's work, as some such addresses have done. Sir William Siemens, the President of two years before, whose letter conveying the invitation to become President has been quoted, had since died, and some reference to his career was fitting. This gave the opportunity for introducing topics of general interest, particularly the electrical industry, then in the course of rapid development. Rayleigh next proceeded to a review of recent progress in physics. The latter part of the address was chiefly devoted to a discussion of the rival claims of a classical and a modern education. The arguments on each side were fairly stated, and drew cheers alternately from rival parties in the audience. This part of the address throws light on his general views, and as it will appeal to many who have not convenient access to Rayleigh's scientific writings, I quote it here.

"From the general spread of a more scientific education, we are warranted in expecting important results. Just as there are some brilliant literary men with an inability, or at least a distaste practically amounting to inability, for scientific ideas, so there are a few with scientific tastes whose imaginations are never touched by merely literary studies. To save these from intellectual stagna-

[1] Vol. II, p. 333.

tion during several important years of their lives is something gained; but the thorough-going advocates of scientific education aim at much more. To them it appears strange, and almost monstrous, that the dead languages should hold the place they do in general education; and it can hardly be denied that their supremacy is the result of routine rather than of argument. I do not, myself, take up the extreme position. I doubt whether an exclusively scientific training would be satisfactory; and where there is plenty of time and a literary aptitude I can believe that Latin and Greek may make a good foundation. But it is useless to discuss the question upon the supposition that the majority of boys attain either to a knowledge of the languages or to an appreciation of the writings of the ancient authors. The contrary is notoriously the truth; and the defenders of the existing system usually take their stand upon the excellence of its discipline. From this point of view there is something to be said. The laziest boy must exert himself a little in puzzling out a sentence with grammar and dictionary, while instruction and supervision are easy to organize and not too costly. But when the case is stated plainly, few will agree that we can afford so entirely to disregard results. In after-life the intellectual energies are usually engrossed with business, and no further opportunity is found for attacking the difficulties which block the gateways of knowledge. Mathematics, especially, if not learned young, are likely to remain unlearned. I will not further insist upon the educational importance of mathematics and science, because with respect to them I shall probably be supposed to be prejudiced. But of modern languages I am ignorant enough to give value to my advocacy. I believe that French and German, if properly taught, which I admit they rarely are at present, would go far to replace Latin and Greek from a disciplinary point of view, while the actual value of the acquisition would, in the majority of cases, be incomparably greater. In half the time usually devoted, without success, to the classical languages, most boys could acquire a really serviceable knowledge of French and German. History and the serious study of English literature, now shamefully neglected, would also find a place in such a scheme.

"There is one objection often felt to a modernized education, as to which a word may not be without use. Many excellent people are afraid of science as tending towards materialism. That such apprehension should exist is not surprising, for unfortunately there are writers, speaking in the name of science, who have set themselves to foster it. It is true that among scientific men, as in other classes, crude views are to be met with as to the deeper things of

Nature ; but that the life-long beliefs of Newton, of Faraday, and of Maxwell, are inconsistent with the scientific habit of mind, is surely a proposition which I need not pause to refute. It would be easy, however, to lay too much stress upon the opinions of even such distinguished workers as these. Men, who devote their lives to investigation, cultivate a love of truth for its own sake, and endeavour instinctively to clear up, and not, as is too often the object in business and politics, to obscure a difficult question. So far the opinion of a scientific worker may have a special value ; but I do not think that he has a claim, superior to that of other educated men, to assume the attitude of a prophet. In his heart he knows that underneath the theories that he constructs there lie contradictions which he cannot reconcile. The higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment."

During the ensuing week Rayleigh made four communications to the Physical Section of the Association, which, though not unimportant, hardly call for detailed notice here. On Saturday there was a migration back to Quebec, where he stayed with Lord and Lady Lansdowne for their reception of the Association. The final meeting at Montreal was on Wednesday, September 2nd, when honorary degrees were conferred by McGill University.

After the Montreal meeting was over, an expedition was made westward by all the family party. The Directors' private car (Canadian Pacific Railway) had been placed at their disposal, so that the journey was made in comfort. They stayed for a few days at Toronto with Sir David and Lady Macpherson. They then went by steamer through Lake Huron and Lake Superior, taking rail again at Port Arthur.

The Canadian Pacific Railway was then under construction, and even those parts of the line which were open for traffic had been hastily constructed with temporary bridges. In the course of the journey the steward came in with a solemn face, and announced that a wire had come to say that it would not be safe for the train to cross the bridge in front of them. When they got there, it appeared that this message erred if anything on the side of moderation ; for there was a great

gap in the bridge, the trestles were lying about anyhow, a large iron crane hung suspended over the hole by one hook, and an engine lay on its side below.

However, the bridge did not take long to repair. They went on to Rat Portage (Lake of the Woods), which they reached on September 17th, 1884. Here the Mayor received them very civilly. Two items of his conversation are recorded. Rayleigh was much amused at his ideas of the Indian language. He asked him if he knew anything of it. "Well," said he modestly, "I know a few words." He was pressed for an example. "Well," he replied, "if an Indian wants to say good morning to you he says 'Bo jou.'"

Lady Rayleigh, in a moment of enthusiasm for the unspoilt wild country around, remarked, "There must be great pleasure in a wild life." "I thought so too once," he said, "but there are drawbacks—headache the next morning and so on."

They went on to Winnipeg and Calgary and thence to Laggan, which was the furthest point of the railway open for ordinary traffic: but Mr. Ross, the manager, kindly consented to take them down the Pacific slope to railhead in his own car. The journey was apparently considered somewhat adventurous, and it was suggested that the ladies should remain behind, but this suggestion they repudiated.

Railhead was then some ten days' journey from the Pacific coast, but the enterprise was considered too arduous. Rayleigh and Lady Rayleigh slept in a tent for one night. He was much impressed with the rate at which the work was being pushed on. They had a few words with Mr. Donald Smith (afterwards Lord Strathcona), one of the chief promoters of the enterprise, who came to railhead on a flying visit.

They started on the return journey on September 25th, 1885, their car being attached to a freight train. The ladies got into the cab of the engine and trusted to pick up Rayleigh and his brother Richard, who had gone some 3 miles further forward. But the engineer declined to stop the train on the steep upward slope when they appeared. The ladies were dismayed, as it entailed a 40-mile walk for two men, neither

of them over-strong. However, they both succeeded in boarding the train, and appeared at the next stopping-place.

At Winnipeg Rayleigh had a "pleasant chat" with the inevitable reporter. Looking about at the station at St. Paul they saw among the goods waiting to be forwarded a long deal box directed to Mrs. J. Stacey, and on a card attached; "This is to certify Mr. J. Stacey did not die of any infectious complaint." The journey passed without further incident. On October 2nd they reached Chicago, when they saw electrically driven tramways for the first time, and after a night there went on to Washington.

At Washington they met various astronomical friends, English and American, including L. M. Rutherfurd, Adams, and Newcomb, who was a resident, being connected with the Naval Observatory. Rayleigh saw the arrangements which Newcomb was preparing for a determination of the velocity of light, and met A. A. Michelson, who was at that time a young naval officer, and had been detailed at Newcomb's request to assist in this work.

From Washington they went to Baltimore on October 6th, 1884, Michelson travelling with them.

This was the occasion when Sir William Thomson's well-known Baltimore Lectures on optical theory were delivered. A full account of them will be found in S. P. Thompson's *Life of Lord Kelvin*. I recall a conversation with my father on the subject a year or two before his death. I mentioned the lectures, part of which I had recently been reading.

Rayleigh: What an extraordinary performance that was! I often recognized that the morning's lecture was founded on the questions which had cropped up when we were talking at breakfast. [At the hotel ?]

Self: I should have thought that when all the leading physicists in America had collected to hear him, they would have expected something more carefully prepared.

Rayleigh: On the contrary, they were very much impressed and he got some of them to do grinding long sums for him in the intervals.

Rayleigh only stayed during a part of the course, and left for Philadelphia about October 12th, 1884.

There he saw something of Prof. G. F. Barker and encountered Mrs. Bloomfield Moore, a widowed lady of large means, who was deeply interested in the supposed inventions of Mr. Keely, which she had financed. Rayleigh's mother had in the meantime stayed in Philadelphia and had met Keely. The following is her account :—

" Yesterday afternoon (Oct. 13th) we spent two hours at Mrs. Bloomfield Moore's and met Mr. Keely. He is a curious person and full of *fire* and I should say *not* an impostor, but I should not be surprised if he were *mad!* He talked away tremendously quickly, and used all kinds of words invented to suit his discovery, and I got quite exhausted trying to understand him ; all I could really make out was that he professed to have decomposed *hydrogen* and evolved a lighter element from it, and that his new force had something to do with *vibration :* that he multiplies vibrations almost infinitely, and can distinguish *divisions* of *tones* in an unusual manner.

" Those who have seen his experiments lately declare that *no* force with which scientists are acquainted could produce the same effects with the machinery used. ' If it is a trick,' he said, ' at any rate it is a trick worth knowing—if a pint of water can send a train from this to New York, which it will do shortly.' "

Barker told Rayleigh that he had gone with H. A. Rowland to Keely's workshop and had seen some apparently remarkable effects. Rowland, however, was dissatisfied as to whether what purported to be a wire was not really a hollow tube conveying compressed air, and he stepped forward to satisfy himself by cutting it. Keely flew at him to prevent this, and they rolled together on the floor !

From Philadelphia Rayleigh and Lady Rayleigh went on to Boston.

VENDOME HOTEL, BAWSTON (*sic*),
Oct. 19th/84.

MY DEAREST MOTHER,[1]—

I don't know exactly how to address this. . . . Our week at Baltimore was a success. Thomson had collected half the physi-

[1] She had left them when they went to Baltimore.

cists in America, so that we had almost daily discussions. His lectures were quite in the usual Thomsonian style, a sort of thinking out loud in an enthusiastic incoherent manner.

Rowland of the Baltimore University is about the first physicist in America, and happens to have worked at much the same things as myself, so that we appreciate one another ! and had good talks. . . . Mrs. Moore turned up in Philadelphia, but I could not go to see a thing that was made a mystery of, with the probability that anything I might say as a matter of civility would be exaggerated into a judgment of approval.

Prof. Barker has seen Mr. Keely's things for ten years, and believes him to be an impostor.[1] We arrived here this morning after a night journey attended with the usual railway accident."

While staying at Boston Rayleigh visited Harvard and saw the Jefferson Physical Laboratory under Trowbridge, and the Observatory under Pickering. At the Pickerings' they met Oliver Wendell Holmes, author of *The Autocrat of the Breakfast Table*; but there was little opportunity for conversation, as Rayleigh found that every member of the large party had to be introduced to him.

On October 28th, 1884, they went to New York. Here Rayleigh, in company with (I think) Prof. Barker, visited Mr. Edison and saw something of his electrical engineering work, which was then of course quite a novelty.

Edison was asleep when they arrived. It was said to be his habit to work some very large number of hours (I hesitate to give a figure) and then to "sleep it off." Rayleigh hardly liked to have him wakened, but someone (Barker ?) insisted on doing it, and they were very kindly received.

I believe also that Mrs. Henry Draper showed him her late husband's private laboratory.

Almost immediately on his return Rayleigh wrote to the Vice-Chancellor to resign his Cambridge professorship as from the end of the current term. He had probably almost decided on this step before starting, but it was characteristic of him

[1] I have seen somewhere a statement that after Keely's death his apparatus was examined, and it was found that his effects had been produced by trickery.

not to announce it any sooner than was absolutely necessary. The five years he had spent at Cambridge had been very strenuous ones, and in spite of a considerable amount of illness, and the work of teaching and organization, the number of papers published during that time was no less than sixty—almost double the number in the five years before and the five years after. This measure is of course only a rough one, but some of the Cambridge papers were long and elaborate. "I could not have gone on working as hard as I was doing then," he said in after years.

He returned to Cambridge to wind up his affairs there, staying with the Sidgwicks, and on December 8th he and Lady Rayleigh gave a farewell tea party at the Cavendish Laboratory, which was attended by almost all Cambridge. He said good-bye to the workers at the laboratory on December 12th, and left for Terling on December 13th.

The election of Rayleigh's successor was beset with peculiar difficulties, not from want of good candidates, but from the embarrassment of choosing between them. This led to a movement in some quarters for pressing him to stand for re-election.

However, eventually this was not done, and I have no doubt he was glad for his own sake not to have the question reopened. His attitude towards the proposal seems to have been rather sphinx-like. One of the electors wrote :—

"I wonder if I shall ever know if there was a chance of getting you back. I still think there was."

CHAPTER IX

THE LABORATORY AT TERLING

Rayleigh and Lady Rayleigh were enthusiastically welcomed back by the villagers at Terling on their return from America to make their permanent home there once more. There was an advance guard waiting at the gates to receive them, with illuminations and the ringing of church bells, and a huge crowd at the house.

At the stage which this narrative has now reached, Rayleigh settled down at Terling permanently, and it remained the headquarters of his scientific activity for the rest of his life. This seems an appropriate place to give a description of the laboratory, and the rooms in which he and Lady Rayleigh had their quarters. I shall not limit myself strictly to the arrangements of this particular epoch, where it is convenient to describe those of a later one.

The chief working rooms of the laboratory at Terling were adapted from their original purpose of a stable loft. The suggestion of the estate carpenter that the original flooring was altogether too rough, and that it *must* be replaced, was characteristically rejected, and the old floor remained and still remains. It was, however, made somewhat smoother by that dangerous and now obsolete tool, the adze.

The laboratory shelves were made by the estate carpenter. The brick walls remained bare, without plaster.

The available space was divided by a wood partition into two equal parts. The more westerly room, called the "black room," had the walls and ceiling painted dead black, with a composition of lampblack and beer (!) due to the inventiveness of the estate bricklayer. This was to mitigate the effects of

stray light in optical experimenting: the advantage was however rather heavily paid for, in the difficulty of finding anything readily or seeing the contents of drawers and cupboards.

Outside the west window of this room there was a suitable support for a heliostat, with a platform on which the experimenter could stand to adjust it in position. In this way the sunlight could be brought in. A part of the wall forming the southern façade of the house which obstructed the sunlight in winter was ruthlessly removed; it was complained that the house was being pulled down in the cause of science.

A beam of sunlight could, if necessary, pass through the entire length of the black room and the room beyond, for experiments requiring a long beam. When a diffraction grating was to be used, the black room was itself used as a camera, the grating being placed on a shelf about 25 ft. away from the shutter, with a long-focus lens in front of it, forming an auto-collimating spectroscope. Rayleigh always resented the attribution of this arrangement to Littrow, pointing out that the principle had been employed by Maxwell in his colour box, some years sooner, and that he had himself originally learnt it from this source. But the expression "Littrow spectroscope" has probably taken too strong a root in scientific literature to be now displaced.

On a visit from Prof. Rowland to Terling tests were made of one of his plane gratings (2-inch horizontal aperture) which had been received as a present. These gratings were an enormous advance on anything that had been made before, and had a world-wide reputation.[1] Rayleigh, however, found that the definition was improved by blocking out part of the ruled surface. "Rowland," he said afterwards, "did not like my doing that, and was inclined to blame the lens." It was of course a proof that the accuracy of ruling, in this particular example at least, still left room for improvement.

With this grating photographs were taken of the *b* group of the solar spectrum (due to the presence of magnesium in the sun's atmosphere), which though never published were

[1] See above, pp. 136, 137.

superior to anything previously done in that way. Many studies were also made with a view to improving the performance of the prism spectroscope, whether with glass or bisulphide prisms, and in the hope of getting something like the theoretical resolving power out of a powerful prism combination—such as might rival the grating. These were only partially successful. The main difficulty lay in getting sufficiently homogeneous glass prisms. Little of this work was published.

As well as the west window already mentioned, the black room had another facing more nearly south, which was available for work with the sun until about one o'clock.

It was at this window that the investigations on the reflection of light in the neighbourhood of the polarizing angle were made which showed how entirely the result depends on cleanness or otherwise of the surface. Rayleigh is shown working at this window in the very successful portrait of 1888 by Sir Philip Burne-Jones, of which the original is at Terling, and copies at Trinity College, Cambridge, and at the Royal Institution. It should be remarked, however, that the collection of coloured chemicals and other apparatus shown in this picture were arranged for artistic effect, and do not represent what was actually used in any particular investigation.

Other optical work not involving the use of sunlight was also carried on in the black room; for instance, the numerous studies involving the use of interference bands, the work on refractivity of gases, and later the measurements of wave length by modification of Fabry and Perot's method.

The other large upstairs room was used for general experimenting. The central object was a large Töpler mercury pump, which at the time it was procured (about 1885) was regarded with satisfaction as the last word in efficiency for the rapid production of high vacua. Yet it took a whole morning of intermittent attention to exhaust one of the 2-litre globes used for weighing gases, a painful contrast to the rapidity and facility with which the modern rotatory pumps will do such work. Mounted in connection with the Töpler was the

standard manometer, by which the pressure of gas in the weighing globe could be adjusted to balance a column of mercury, equal in length to the distance between two points on a standard steel rod. It was in this room that argon was first isolated by sparking with an induction coil actuated by five cells of Groves' battery.

The battery stood outside the window. The space near the ceiling was traversed with a tangle of cotton-covered wires passing in various directions, and the ceiling itself in later years at least was more nearly black than white. Certainly it could not have been whitewashed without imminent danger to the many fragile constructions which occupied the room. For instance, a long quill tube, one continuous piece of glass, passed from a remote corner of the room to the Töpler pump. This was used in the investigations on the weighing of gases, in order that the tube furnace used for the purification might stand in the draught cupboard.

In this room also were carried out the experiments on the induction coil. The problem was to determine whether or not the function of the condenser in improving the length of sparks was to make the break of the primary circuit abrupt; and it was decided to try whether cutting the wire with a bullet would have the same effect. Some success was attained using the bullet from one of a pair of Derringer pistols which Rayleigh had taken to Egypt as a measure of precaution on the occasion of the voyage up the Nile in 1871.

Later, a service rifle was borrowed from an enthusiastic sergeant of volunteers in the village, and with this still more striking results were obtained.

I remember that a young lady visitor of about eight or nine years old came about this time to stay at Terling with her parents. She was afterwards required to write an account of the visit as an exercise in English composition. It opened by stating that on their arrival "Lord Rayleigh was shooting in his lavatory"!

The bedroom used by the Rayleighs was separated by a thick brick wall from the room which has been described. This

wall served as an additional obstacle after the long box of sawdust used to stop the bullets.

A tubular hole was made through the wall into the bedroom, for acoustical experiments, when it was desired to isolate the experimenter from stray sounds proceeding from the apparatus in use. The experiments on hearing with the two ears as a means of locating sounds were carried out in this room, listening tubes or telephone wires being led through the hole in the wall; any spare space in the channel was filled up with tow, as a non-conductor of sound. This research will be described in a later chapter. If afterwards came very much into prominence when submarine hunting by acoustical methods became of such vital national importance.

However, to return to the description of the laboratory.

A wooden staircase led down to a small workshop on the ground floor. Here there was a foot lathe of simple construction, and a fitter's bench with a vice. Rayleigh used the lathe a little himself before 1879 when he had not the services of an assistant. Afterwards, constructional work was left almost entirely to his two successive assistants.

Out of the workshop opened a small room fitted with a chemical bench and sink. This was called "the schoolroom," having been used for that purpose when Rayleigh and his brothers and sisters were children. The old wall paper remained, but shelves covered with chemical glass ware had been put up, and one end of the room was partitioned off as a photographic dark room.

In this room there was arranged a turn-table on which could be placed a large circular sponge bath. This was used for observations on fluid motion, such, for instance, as Scott Russel's phenomenon, of the production of a train of waves by a stationary obstacle in the moving water, for observations on ship models, and so on.

It was in this room and its annexe that photographic development was done—the only manipulative work that Rayleigh kept in his own hands, during the time that Gordon was with him; "the only thing," as he said, "that I can do better

than he can." In fact, his interest and pleasure in photography never flagged, from his school days till the end of his life. However, he abandoned the art of producing landscapes or portraits after about 1872. His subsequent practice of photography was purely in its scientific applications.

During the closing years of his life, such experimental work as he did was largely done in this room. It was well warmed, while the upstairs part of the laboratory was far from being so ; and it was conveniently situated next his study.

The study itself, known as the "book-room," is a large room well lighted both from the north and from the south. The room was in many respects very characteristic of its occupant. Appearances were absolutely disregarded. The writing table was a large one, covered but not littered with books and papers, except for the small space actually used for writing. An old brown leather despatch box, a present received when he came of age, occupied one corner of the table. It was used for holding money, and important papers. On the right hand was a book-rack, containing notebooks, including the notes he had taken at Stokes's lectures as an undergraduate, and a copy of the *Theory of Sound*, full of notes on loose sheets of paper. On the shelves at the writer's back was a copy of his collected scientific papers. The volumes were well thumbed, and it was a family joke that if he was seen reading anything with special interest, it was sure to be his own writings ! Considering the amount of mathematical detail they contain, it is not surprising that it was often necessary to refer back to past work which was in course of further development.

Over the arm-chair in front of the fireplace was a gas bracket, which appeared to the conventional observer to be of very extraordinary construction. Rayleigh had noticed that the brittle Welsbach mantles, when carried on a pendant from the ceiling, suffered very much from shaking when anyone walked across the floor above. The remedy for this was to carry the lamp on the wall. But no wall was suitably placed behind the arm-chairs. It was necessary therefore to carry out a bracket

of about 8-foot span from the wall above the mantelpiece. This was achieved on correct engineering principles, by means of two strips of plain deal, one from each side of the fireplace, which were in a horizontal plane about 2 feet from the ceiling, and intersected at an apex from which the lamp was to be supported. The strips were in longitudinal compression. A stout iron wire, which was in tension, proceeded obliquely upwards from the apex to a point at the top of the wall, over the centre of the mantelpiece. A " compo " pipe ran along one of the strips and conveyed the gas to the Welsbach burner, which was of the inverted type.

As already mentioned, gas was from a small private gas-works, which was stoked by one of the under-gardeners. It was installed soon after Rayleigh succeeded to the place. In later years he frequently discussed the project of putting in an electric installation, and I think there can be no doubt that his experimental work suffered in some degree for lack of this convenience. One consideration was that gas was useful if not indispensable for glass-blowing and other laboratory work. Another was the constitutional dislike of spending, and the annoyance and loss of time which he anticipated in making the many small decisions involved. Most men enjoy this kind of occupation. He emphatically disliked it. One remark, made I think about 1895, was that a perfectly reckless expenditure in zinc and acids (used in primary batteries for laboratory purposes) would be far cheaper than an installation.

To return, however, to the description of the book-room.

The walls were covered with books, chiefly serial publications, such as the *Proceedings of the Royal Society*, the *Philosophical Magazine*, the *Annalen der Physik*, and others. The Cambridge mathematical textbooks in the familiar green covers which Rayleigh had used as an undergraduate were in evidence, many of them very dilapidated with long use. There were a great number of loose copies of scientific pamphlets sent by the authors. Some of these were classified according to subject in two bedroom chests of drawers, but latterly the

attempt to classify them was abandoned, and they accumulated in heaps on the top of the drawers.

Although the general aspect of this room was untidy, Rayleigh was rarely at a loss to find anything of importance. The temporary loss of a volume of the serial publications disquieted him seriously. I remember one occasion when Lord Kelvin had caused a volume of the *Philosophical Magazine* to be packed, in anticipation of consent to borrow it; but the permission was not given, the owner explaining that he never took the volumes away himself. The refusal was taken in good part.

Rayleigh's natural taste for economy made him reluctant to throw away anything that might be utilized, and this led to a large amount of space being devoted to brown paper, cardboard boxes, postal tubes, and large used envelopes.

There was an old piano which had been in the room ever since Rayleigh's boyhood (see p. 51), and which served as a table for miscellaneous objects of a scientific character. Lady Rayleigh had a roll-top desk in this room, and used to write her letters here, though it was necessary to withdraw for domestic interviews. Rayleigh was not specially sensitive to disturbance; but he had an extreme dislike of hammering, whether he was at work or not. I think part of his opposition to "improvements" may have arisen from this cause.

The bedroom occupied by the Rayleighs, together with dressing-rooms for each, were above the book-room. Rayleigh always resisted a bedroom fire, urging that the fire went out in the early morning, making the room colder, so that the bedclothes which had been adequate were no longer so, and sleep was disturbed by making the change! It was his custom to have a cold or nearly cold morning bath until the end of his life.

The book-room is connected with the body of the house by a conservatory, in which Rayleigh would often pace up and down in the morning in the intervals of his mathematical work. There are steps leading down from the passage outside the book-room to a tunnel which runs under the conserva-

tory to the basement under the centre of the house. This tunnel has remarkable acoustic properties, imitating on a small scale those of the well-known echoing caverns known as the "Ear of Dionysius" at Syracuse. Like the latter, it has opposite curvatures at the two ends. He wrote:[1]

"There is an underground passage in my house in which it is possible, by singing the right note, to excite free vibrations of many seconds' duration, and it often happens that the resonant note is effected with distinct beats. The breadth of the passage is about 4 feet and the height about $6\frac{1}{2}$ feet."

At the further end of the tunnel there was a space where experiments could be carried out which required an absolutely steady foundation. The various applications of interference bands were a favourite subject with Rayleigh, and one of the experiments carried out at the end of the tunnel was the use of the free surface of water as a natural test plate, by which a worked glass flat could be tested. The method cannot be called a convenient one, and would only be used in the absence of a standard glass test plate of ascertained flatness. It would be useless to attempt to carry it out in a town, where the surface of water is always disturbed by tremor. The large flat mirrors to which it is applicable are chiefly of use in solar research.

Another arrangement[2] here used was a Michelson interferometer, attached to the wall with the motion vertical, instead of horizontal, as is usual. The movable mirror was a mercury surface, which after displacement would automatically readjust itself to parallelism with its original direction, independently of the accuracy of the slide by which it was moved. It was curious to notice how the interference fringes could be displaced by pushing at the supporting wall with the forefinger.

The principal piece of experimental work which Rayleigh had in view on his return from Cambridge had already been

[1] *Theory of Sound*, Vol. II, p. 72, *footnote*.

[2] This arrangement, though published, seems to have been unnoticed or forgotten. It has recently been re-invented by Prof. Michelson himself.

foreshadowed in the address to Section A of the British Association in 1882. An extract from that address will show what was his point of view in taking up this work, which in its ultimate dramatic development was to lead to his best known discovery. Turning to the British Association address, we read :—

"The other subject on which, though with diffidence, I should like to make a remark or two, is that of Prout's law, according to which the atomic weights of the elements, or at any rate of many of them, stand in simple relation to that of hydrogen. Some chemists have reprobated strongly the importation of a priori views into the consideration of the question, and maintain that the only numbers worthy of recognition are the immediate results of experiment. Others, more impressed by the argument that the close approximations to simple numbers cannot be merely fortuitous, and more alive to the inevitable imperfections of our measurements, consider that the experimental evidence against the simple numbers is of a very slender character, balanced, if not outweighed, by the a priori argument in favour of simplicity. The subject is eminently one for further experiment ; and as it is now engaging the attention of chemists, we may look forward to the settlement of the question by the present generation. The time has perhaps come when a redetermination of the densities of the principal gases may be desirable—an undertaking for which I have made some preparations."

An extract from a letter of that time shows what sort of preparations these were :—

"CAMBRIDGE, *Aug. 8th*, 1882.

"The glass balloons for weighing hydrogen, etc., are come, but they are at least twice as heavy as they ought to be, and I am afraid are useless. No delicate balance will carry more than about 2 lb. I had told them to make it as light as possible consistent with strength, but now it is clear I ought to have gone into it more thoroughly. Such is life."

The principle of this determination of the densities and hence the atomic weights of gases is very simple, compared with the complex problem of absolute electrical measurements ; but it may be questioned whether the work itself proved easier in execution.

The chief difficulty and source of error in the work was possible impurity in the hydrogen. Whatever impurity may enter is sure to be many times as dense as hydrogen, and thus produces an altogether disproportionate error in the weighings. Everything therefore depended on getting the hydrogen chemically pure. Rayleigh was asked by a quondam pupil about the subject of a research which the latter proposed to undertake. "Well," he replied, "if you want a thoroughly troublesome job, take up some chemical problem."

In outline, the process of determining the densities was simply to weigh a glass globe empty of air and of known volume, to fill it, e.g., with hydrogen, at a determinate temperature and pressure, and to weigh it again. In this way the weight of a cubic centimetre of hydrogen could be found, and a similar determination could be made with oxygen. For comparison of densities the process was still simpler, not requiring a knowledge of the volume of the globe. The problem of primary interest was whether oxygen would prove to have exactly sixteen times the density of hydrogen.[1]

Since the number of atoms in a cubic centimetre of hydrogen is to a high degree of approximation the same as the number in a cubic centimetre of oxygen, the exact ratio of densities would imply that an atom of oxygen was sixteen times as heavy as an atom of hydrogen, and the inference of the essential unity of matter would be irresistible. I represent the question as it then appeared, without reference to knowledge which has been gained in the ensuing thirty years.

It was a case when the attainment of a definite answer, whether positive or negative, was of supreme importance for a knowledge of the constitution of matter. The answer ultimately attained was in the negative. This might seem to some a disappointing result, but eventually, as will be related, the labour undertaken was more than rewarded by positive discovery. Altogether apart from this, the actual value of the ratio of masses of oxygen and hydrogen is a fundamental

[1] This statement is not quite accurate, but sufficiently so for the present purpose.

datum for the problems of the present hour. Although it is difficult to argue the matter in a cogent way for those who are not in sympathy with the scientific spirit, experience gives ample proof that the labour spent in fundamental determinations of this kind does not fail of its eventual reward in scientific progress.

It would scarcely have been practicable for Rayleigh to undertake the tedious preparatory routine with his own hands, and he had arranged with George Gordon (see p. 104) to leave the Cavendish Laboratory and accompany him to Terling. Gordon was assigned an old Tudor house just outside the grounds, of some antiquarian interest,[1] and established himself there with his aged parents, his widowed sister, Mrs. Moffat, and her daughter. His father was a retired sea captain. He was a familiar figure with his venerable white beard, and was usually seen fishing with a worm in the Ter, the small stream on which the village is situated. The best hours of his day were spent in this way. But I believe it was very rarely that his perseverance was rewarded. Mrs. Moffat and her daughter were prominent figures in the church choir, and at village gatherings ; but Gordon was inclined, as Rayleigh phrased it, to practise the Scotch virtue of " keeping himself to himself." He was known, however, on one occasion to give a popular scientific lecture in the village. His abilities as a mechanic very much impressed the estate carpenters, who held him in high respect. He was sometimes questioned by the villagers about what went on in the laboratory, " his Lordship's *ply* [play] room," as it was called in the broad Essex dialect. " That ain't much good, is it ? " he would be asked. " They think Lord Rayleigh is a kind of dreamer " was Gordon's impression, and doubtless it had some foundation. In later years, however, Rayleigh's humbler neighbours gradually learnt that many in the great world considered the mysterious proceedings in the laboratory by no means futile : and they

[1] It is figured in the *Inventory of Historical Monuments in Essex*, published by H.M. Stationery Office, Vol. II, facing p. 229.

were content to take this opinion on trust, and adopt it for themselves.

At the time Rayleigh undertook the work, nothing had been done on the problem of weighing gases since the time of Regnault, whose work was published in 1845, and the figure he had arrived at was so near the whole number that there was no evidence at all to exclude the exact ratio 16 : 1. Rayleigh in pondering the problem had detected a subtle source of error in Regnault's experiments which was one of the reasons that determined him to repeat them. This I will now explain.

In all accurate weighings it is necessary to have regard to the buoyancy of the air. No one will require to be convinced that the apparent weight of an object (say a man) is changed when he is immersed in water. Air, it is true, is about 800 times less dense than water, but for all that its buoyancy is by no means negligible in accurate work.

Let us make abstraction for the moment of any changes in the density of the air which occur from time to time. The buoyancy resulting from the displacement of air depends on the external bulk of the weighing globe. It was tacitly assumed by Regnault that this bulk, and consequently the buoyancy correction, remained the same whether the globe was exhausted, or full of gas at atmospheric pressure. Rayleigh detected this tacit assumption, and saw that it was not justified. When the globe is exhausted, the internal support is removed, and the external air pressure crushing down upon it on all sides causes it to shrink. When the figures are gone into, it appears that this is an important consideration ; and to ignore it appreciably affects the final result.

When the actual weighing was begun it was found that the temperature conditions were not good enough for the balance in the upstairs laboratory, and one of the cellars under the body of the house was cleared for the purpose. As a further precaution, an inner chamber with water-proofed brick walls was built for the balance, and the atmosphere in it was kept dry by the simple expedient of placing a large well-dried woollen

blanket in it.[1] The blanket would often gain 2 lb. in weight from the moisture absorbed in twenty-four hours.

The globe hung in a cupboard below the balance proper, which was left swinging over-night, ready for the temperature to settle down. The final readings were taken the next morning, without entering the balance room. A window with suitable optical arrangements allowed the pointer to be read by an observer in the outer cellar.

The buoyancy correction for the weighing globe is dependent on the density of the air, and therefore liable to constant variation with changing barometer and thermometer. This is by no means a trivial consideration. If nothing is done to deal with it, it might in extreme cases make more apparent difference whether the barometer was high or low than whether the globe was vacuous or filled with hydrogen. To effect a compensation, Regnault had used the device of a dummy globe permanently closed, and of equal volume to the working globe. This was suspended from the other arm of the balance, and changes of buoyancy would affect both equally, so that compensation is automatic. Now the ordinary methods which the glass-blower uses in making such globes is not well adapted to securing a size exactly predetermined. In blowing the globe he has to judge this by eye, and therefore it is not surprising that the compensating globe used by Rayleigh had not exactly the same displacement as the working globe. To make up the deficiency, a closed U-shaped piece of glass tubing was used, which was permanently hung round the neck of the dummy globe.

The early experiments made on the weighing of gases were embarrassed with most puzzling discrepancies. The balance by itself seemed to behave well enough, but when the globes were put on, then small changes of zero appeared from time to time for which no explanation could be found. These changes of zero did not occur from hour to hour, but rather from

[1] I do not know who originally invented this method, but I have heard at Cambridge that Clerk Maxwell used it, and it is possible that Rayleigh learnt it from him.

day to day, as the globes untouched hung on the balance. It seemed impossible to conjecture what they were conditioned by. In particular, no sufficiently definite connection could at first be traced with the changes of temperature or atmospheric pressure. The experimenter and his assistant were almost driven to despair. It was a remark of Rayleigh's that his normal position when experimenting seemed that of having to choose between opposite impossibilities. Never was it more so than in this case. He said long afterwards that he doubted whether he would ever have got to the bottom of it had it not been for the long duration of an exceptionally low barometric height. This had its counterpart in the behaviour of the balance, and gave the necessary clue. It was found that the U-shaped volume piece which has been described above was not effectively closed. At the end, where it had been sealed in the blow-pipe, a minute crack had appeared, which however was so small as not to be distinguishable to the eye. Its existence was afterwards proved by means of the air pump. This minute leak had partially prevented the volume piece from performing its proper functions, its buoyancy being affected like that of a leaky ship or, what is a better comparison, a submarine filling or emptying its tank.

If the external barometric pressure rose, air would enter making the piece heavier. If it fell, air would pass out, making it lighter. If the leak had been larger, the trouble would readily have been located. The volume piece would then have been altogether inoperative, and the change of apparent weight would have followed immediately on changes of the barometer. As it was, only *long-continued* depressions below or elevations above the mean barometric height had time to assert their effects, which only accumulated slowly owing to the extreme smallness of the leak.

Only those who have experienced it can appreciate how painful an obstacle of this kind is to an ardent experimenter. At times he is almost tempted to doubt if truth is attainable at all, and feels inclined to abandon the exhausting struggle. The uniformity of Nature, it has been said, could never have

been discovered in a laboratory. The moral which Rayleigh drew from cases of the kind was to have some alternative experiment to turn to. "I always like to have two or three things simmering," he said; "if one of them does not get on, [perhaps] another does."

When these preliminary skirmishes had been fought and won, the work went slowly but steadily forward. It formed the staple occupation of Gordon's mornings to exhaust the globe and to make any necessary changes in the apparatus for preparing the gas. Though not trained in youth as a glass-blower, he acquired at the Cavendish Laboratory, and later, a serviceable knowledge of the art. It must be admitted that his glass-work left something to be desired in point of elegance. But that did not trouble his employer at all. As soon as matters had got into a routine Gordon also read and recorded the balance readings and I think did a part of the arithmetical reductions,[1] on loose sheets of paper. Rayleigh checked them and copied them into his notebook.

The fillings of the globe were usually done between tea and dinner. Rayleigh always superintended these himself. As anyone will realize who has attempted work of this kind, accidents occur leading to much waste of time and disappointment. Any experiment which could even be suspected as unsatisfactory in any one of the numerous details, had to be ruthlessly rejected. Many weary hours, too, were spent in locating minute leaks in the complicated train of apparatus required for preparing and purifying the gas. The reader will perhaps think that I am dwelling on these matters at too great a length. I do so because I believe the layman has no conception of them. One visitor at Terling who was shown something of the work in progress betrayed by a casual remark that he thought that the weight of hydrogen would be finally ascertained when the balance was read the next

[1] Rayleigh destroyed the notebooks of this work, some twelve years before his death, remarking to me that no one else would be able to make them out. I now regret that I did not make an effort to save them as historical relics.

morning! He was not a little astonished to learn that the determination had been in progress for years. The first publication on the relative densities of oxygen and hydrogen was in fact in 1888, three years after the work was begun, and other publications followed in 1889 and 1892. The result for the ratio of densities was 15·882, and appeared definitely to contradict the idea of an atomic weight of 16 for hydrogen. In the meantime other experimenters had been at work on this and allied problems, and their conclusion was substantially the same.

Throughout this work the chief anxiety was as to the purity of the hydrogen. Various methods were tried for obtaining an independent check upon it. In October, 1891, he wrote: "Dewar has tempted me to try the spectrum of my hydrogen. I hope it won't be the beginning of an immersion in the bog of spectroscopy."

I may mention here that in 1900 he again returned to the subject to try whether lighter and therefore purer hydrogen could not be obtained by freezing out the moisture by liquid air, which in the meantime had become available. In all previous work chemical desiccation by phosphoric anhydride had been used. The result was negative. Later still, he thought of trying hydrogen obtained by the evaporation of liquid hydrogen: but this was never done. "I doubt if anything different would come of it," he said, "and besides I am rather tired of the job."

This is only the first chapter of the work on weighing gases. The second and more dramatic one is to follow later.

CHAPTER X

THE LATE 'EIGHTIES

At this time the ninth edition of the *Encyclopædia Britannica* was in course of preparation, and Rayleigh undertook the articles on Optics, and on Wave Theory of Light, at the request of Robertson-Smith the editor, who was a personal friend and stayed more than once at Terling. Robertson-Smith, though best known as an Oriental scholar, was one of the very few men who have combined this with knowledge of physical science—he had been assistant to Prof. Tait at Edinburgh. Tait wrote the article on Light, which gave the more elementary aspects of the whole subject, and Rayleigh's contributions were to carry it further. The shorter article on Optics is devoted in the main to geometrical optics, though without pedantic limitation.

The article on wave theory is severely technical, not to be mastered by anyone but an earnest student of science. It was in fact by far the most profound discussion of the subject in our language; in that respect it may be considered the lineal successor of Sir John Herschel's article "Light" in the *Encyclopædia Metropolitana* of 1830, which for long remained the standard treatise. Some of the best subsequent books on Optics have drawn largely on Rayleigh's article.

It may perhaps be questioned whether an encyclopædia is the best place to publish an abstruse treatise. The following extract from one of the editor's letters shows that doubts of this kind were felt at the time, and defines his attitude:—

"I have always found that a good deal of pressure and a good deal of diplomacy have had to be used to get the publishers to

print long papers on abstract subjects of which they cannot understand one word, and I see that it is a severe shock to them to have suddenly to contemplate an addition of 40 pp. to the estimate for the article on Wave Theory. However I am not going to sacrifice your subject or mutilate your article merely on that account. Only I cannot but see that there is a great deal to be said against the long article from a business point of view and if the article does run to 50 pp. we shall have to bring pressure on other contributors to keep the volume within compass. While therefore I will do my best to give you what you need, it will be a real relief to me if you can manage to leave out whatever is not really necessary to do justice to yourself and the subject. Messrs. Black would be glad to publish your fuller statement in a separate volume and from my own experience I am sure that you would find them fair and honourable publishers and also that they would bring out the work in as good style as any firm I know.

"From the letter received to-day I fancy that a shortened article, containing references for fuller details to a volume to appear simultaneously, would be the solution of the difficulty most acceptable to them, and I venture to hope that such a solution would not be disagreeable to you. I am to hear further on the publishers' ideas in the course of a day or two; but if you can give me any suggestions as to the course you would like me to take, I shall be glad to have these to help me in my next communication with Messrs. Black."

As a result perhaps of these difficulties, part of the article dealing with the more speculative parts of the subject was sacrificed. It was afterwards printed in *Nature* under the title "Aberration."[1]

Possibly Rayleigh may have felt doubts himself; he said, "I did it rather elaborately because there was nothing of the kind in existence." On another occasion he remarked that he had heard that some of the farmers of the Western States in America bought the *Encyclopædia* as a complete library, and started to read it out from A to Z to the assembled family of an evening. "I often wonder how they get on with my article on the Wave Theory," he said. Anyone who glances at the text of the article may well share his wonder.

[1] See further, Chap. XX.

Both of the articles are now more conveniently accessible in his *Collected Scientific Papers*, Vols. II and III.

In the tenth edition of the *Encyclopædia Britannica* (published 1910–11) the treatment of optical subjects generally was re-cast and divided up into separate headings. Rayleigh contributed the articles "Diffraction" and "Interference," in which much of the substance of the old article "Wave Theory" was reproduced, with the necessary additions to bring it up to date. He wrote in addition the articles "Argon," "Capillary Action," and "Sky." In some of these paragraphs from his previously published papers are introduced unaltered.

In the year 1884 Rayleigh had served on the Council of the Royal Society. Sir George Stokes had been secretary for thirty years, and had unsparingly devoted himself to the duties of the post. He was now to become President. Michael Foster, the secretary for Biological Sciences, wrote to Lady Rayleigh:

"Correspondence, etc., ought not and certainly will not in the future be as great as Stokes has made it. It has been painful to see how his energy has been wasted in this way. Mr. Rix is a very competent person, and can be entrusted with much more than he now has, and the council I think will distinctly approve of this kind of work being taken off the secretaries. What we want is knowledge and judgment, and we can find it nowhere as we can in Lord Rayleigh. I hope you will bid him say yes."

Rayleigh accepted, taking up the office in November, 1885.

One of the most important duties is to superintend the censorship of papers sent in to the Society for publication. It was the duty of the secretary to suggest (practically to appoint) Fellows of the Society who have specialized knowledge of the particular subject to act as referees. The referees send in confidential reports, and their names are not disclosed. As a general rule this resort to special referees is not necessary in the case of authors of established reputation, but I remember Rayleigh telling us with a good deal of amusement about one such case, in the department of his co-secretary, Michael Foster. He mentioned no confidential names, but he told

us that an eminent physician who specialized in a particular disease, had had his communication pruned in a manner which he resented. Foster tried to appease him by saying that the part of his paper which had been accepted would not have been received from any less authority than himself. He thought he knew who was the author of the unfavourable report, and said, "You should not have referred it to X——. You ought to have referred it to Y——," naming the very man it had been referred to! Foster had of course to swallow this in silence.

Referees are naturally liable to make mistakes like other men, and Rayleigh was instrumental in remedying a long-standing injustice in a case of this kind. The neglected author's name was J. J. Waterston. Rayleigh's attention happened to be caught by some of his published though little known writings, and in them he found references to an unpublished paper of 1845 in the Archives of the Royal Society, with enough of a clue to its contents to excite his curiosity. On his next visit to the Society's rooms he asked the assistant secretary, Mr. Rix, for the manuscript, and it was produced in a few minutes. On studying it, he found that here for the first time was clearly enunciated the conception that the temperature of a gas is to be measured by the *vis viva* or, as it is now called, kinetic energy, of colliding molecules. Waterston enunciates the principle that "in mixed media the mean square molecular velocity is inversely proportional to the specific weight of the molecules." His paper further contained the first calculation of the molecular velocity. All this was ten or fifteen years in advance of his time, anticipating much of the work of Joule, Clausius, and Maxwell. Rayleigh took steps to have the paper published by the Royal Society, as a tardy act of reparation, and in the meantime he wrote to Prof. Tait of Edinburgh to have inquiries made about the author, who had resided there. Tait wrote in reply (February 13th, 1891):—

"This promises to be a somewhat sensational inquiry—worthy of a detective! I have reached *this* point that J. J. W. was a

Civil Engineer, a near relative of people living here now, and that he was in the employment of the East India Co., but *disappeared* about 7 years ago, and not a trace of him has been discovered in spite of the most anxious search."

Later, Rayleigh got into communication with Mr. George Waterston, a nephew of J. J. Waterston, and he wrote :—

"Some personal details regarding my uncle may be of interest to you. He was born in Edinburgh and educated at the High School, passing to the University, not to take a degree but for certain classes he thought would be of use to him in the profession of Civil Engineer. He took the first prize in Prof. Leslie's [Natural Philosophy] Class in the University. After being in the employment of an Edinburgh firm of Civil Engineers he took a similar situation in London and afterwards entered the employment of the hydrographer's department of the Admiralty, but as his mind was given up to Mathematical and Physical study he relinquished civil engineering and became Naval Instructor to the Indian Navy (E.I. Coy.). He took this place as likely to afford him leisure and a salary on which he could soon retire, which he did in 1856 and returned to Edinburgh.

"At that time I was about 18 years of age and my uncle and I continued very intimate till I married in 1866, and during that time I have often heard him talk on scientific subjects but he never mentioned this paper. He talked however in a manner that seemed to me strangely contemptuous of scientific men with but few exceptions. He had not a word of complaint nor did he speak of being neglected or ill-used, but I distinctly remember the Royal Society was characterized in very strong terms useless now to repeat. We have it on record what they thought of his paper.[1] He returned the compliment in no measured terms. He would not attend the Meetings of the Royal Society of Edinburgh though some friends sent him billets, and rather avoided the society of scientific men. He was of a most social, kind disposition, enjoying the society of young people. He never married, and besides his mathematical work he was fond of Music, Chess, and Billiards.

"When in India he published a book *Thoughts on the Mental Functions*. It had no sale and he continued his Physical and Mathematical studies, contributing papers to the *Philosophical Magazine*.

[1] This alludes to the report of one of the referees quoted in Rayleigh's introduction to the paper. The referee said: "The paper is nothing but nonsense, unfit even for reading before the Society."

"He was a man of strong feelings and strong prejudices—especially so in regard to anything that looked like self-seeking on scientific matters.

"We could never understand the way in which he talked of the learned Societies, but any mention of them generally brought out considerable abuse without any definite reason assigned.

"My uncle's disappearance was very remarkable. We did all we could through the police and by private detectives but have found not a trace of him. Latterly he had become very absent-minded and dreamy when walking in the streets. His sight also was not very good, and he was addicted to smoking large cheroots, so that when anyone met him it always appeared like waking him up before he brought his mind to bear even on an ordinary salutation. He was very fond of walking out by a new breakwater recently built at Leith—very well exposed to a fine sea breeze, but from its construction very dangerous to foot passengers. At this place the tide runs out very fast, and if he had fallen in he would have been carried out to sea. We know of no place near Edinburgh where he could so easily have disappeared, and no one who knew him thought of suicide as likely in his case.

"His friends share your surprise that he should not have put forward more definite claims, but he was of a very retiring disposition. To me he appeared to put forth his papers like some mathematical question for others to tackle, and not being a scientific man I was never sure whether all his contemptuous words of other scientific men were not the fruit of some exaggerated views of the importance of his own work, though he was always so simple and straightforward that I put it down to his not stating his views in a sufficiently practical manner. I remember at one time in a popular magazine seeing his name coupled with that of Mayer in regard to the heat of the sun, and when I spoke to him about it he simply made a grimace.

"The last time I remember him very angry on a scientific subject was in regard to Mr. Crookes and his radiometer, as to which he used some unparliamentary language."

Rayleigh usually went up to London from Terling only on Thursdays when the meetings of the Society are held: but no doubt he attended much more often when he was residing in London before Easter. At Terling, the business on other days was done by post. I believe that the general, as apart from the strictly scientific, business of the Society was conducted in the main by the senior secretary, Sir Michael Foster.

Rayleigh served as secretary for 11 years, from 1885–1896, under the presidencies of Stokes ('85–'90), Kelvin ('90–'95), and Lister ('96).

When Kelvin had accepted nomination for the office, Rayleigh wrote :—

"I need hardly tell you that I am delighted to hear this, and look forward to frequent meetings and discussions. What fun it will be!"

As secretary of the Royal Society, Rayleigh was very anxious to get recognition for Willard Gibbs of Yale. Gibbs' work was put forward in an abstruse and highly generalized form, and no doubt his readers were few. I think it was the Davy Medal, allocated to chemistry, that Rayleigh proposed should be awarded to him. For this he could get no adequate support. Some chemical members of the Council took the view that Gibbs' work was "not chemistry," though, as Rayleigh pointed out, the title of his great paper on "The Equilibrium of Heterogeneous Substances" would serve as a general definition of that science. Lord Kelvin also was antagonistic ; he wrote (September 13th, 1891) :—

"I feel very doubtful as to the merits of Willard Gibbs' applications of the 'Second law of Thermodynamics' referred to by J. J. Thomson. Do you attribute merit to them?"

and again (February 9th, 1892) :—

"I find *No* light or leading for either chemistry or thermodynamics in Willard Gibbs."

"I daresay," said Rayleigh, "that Kelvin had many of the ideas in his own mind ; but then he should have brought them forward properly."

Time has brought its revenges. The Copley Medal, which is the most valued distinction of the Royal Society, was later awarded to Willard Gibbs, and his methods are everywhere expounded in chemical lectures and textbooks. About the time of the abortive attempt to give him the medal, Willard Gibbs wrote (June 27th, 1892) :—

"I thank you very much for your kind interest in my 'Equilibrium of Heterogeneous Substances.' I myself had come to the conclusion that the fault was that it was too *long*. I do not think that I had any sense of the value of time, of my own or others, when I wrote it."

During the latter part of Rayleigh's tenure of the secretaryship, there was a continuous stream of carping criticism of the administration of the Royal Society in the pages of the *Times*. Part of it was editorial, and part came from anonymous correspondents, "A Critic" and the like. There is nothing worth quoting in these criticisms, which consisted of malicious and quite mistaken gossip and innuendoes about the management of the Society's finances and the awards of its medals, together with vague suggestions that it was becoming "fossilized." It was suspected that these were inspired or written by one or two Fellows of the Society who resented not being themselves elected to the Council. However this may be, Lord Lister, the President, wrote to Rayleigh at the time of his resigning the secretaryship (December 1st, 1896):—

"You have no doubt seen the malicious article in to-day's *Times* about the R.S. The statements regarding the award of the Rumford Medals and the second Royal Medal are of course quite false, and equally so is the insinuation that you declined to remain in the Council because you disapproved of its ways. It is felt by Rücker and others as well as myself that a few words from you in the *Times* would do a very great deal of good."

Rayleigh was staying with Gerald Balfour at the Chief Secretary's Lodge, Dublin. He had been much discomposed by the article, and had already written his protest before receiving Lister's letter. It was as follows:—

Times, Dec. 4th, '96.

SIR,—

In your issue of Tuesday, after some too flattering remarks regarding my tenure of office, you say that I have taken the unusual step of declining to sit on the council, and that no one can doubt that my refusal is significant. There seems to have been a suggestion that my retirement is due to a difference with my colleagues

—colleagues with whom I have worked for 11 years in complete harmony, and for whom I retain the highest regard.

Permit me to say that my retirement is significant only of a desire to escape engagements involving journeys to London, and of a possibly mistaken impression that the position of an ex-secretary as an unofficial Member of Council would be a little anomalous.

I am, Sir,

Yours faithfully,

RAYLEIGH.

Some years later (1903), certain Fellows of the Royal Society, among whom I think Sir Andrew Noble was prominent, presented a portrait of Rayleigh, by Sir George Reid, to the Society. It now hangs in the meeting-room at Burlington House. They also generously presented a copy, which was touched up by the artist, to Lady Rayleigh. This is now in the dining-room at Terling.

In 1890, during Rayleigh's tenure of office as secretary of the Royal Society, the question of colour-vision came up for consideration, and he took a leading part. In order to collect in one chapter what there is to be told on this subject, it will be convenient to go back ten or twelve years.

We have already seen, in Chapter III (p. 46), that some of his earliest experiments were directed to producing the subjective yellow colour by means of absorbing media.

These early experiments on the subjective yellow were followed a few years later by an interesting development. An arrangement was made somewhat on the principle of Maxwell's colour box, by which it was possible to mix pure red and pure green taken from the spectrum, so as to match pure yellow, also taken from the spectrum. Having made this match to his own satisfaction, Rayleigh showed the arrangement to his brother-in-law, Gerald Balfour, and was surprised to find that the latter was altogether dissatisfied with the match. He said that the mixture was far too red, "almost as red as red sealing wax." Frank and Arthur Balfour were afterwards found to be of the same opinion. A fourth brother and the three sisters agreed with Rayleigh

himself. After this had been established, Rayleigh remembered a dispute some years before as to the colour of a dichromatic liquid, which appeared to him green, while one of the Balfours maintained that it was red : " but," he wrote, " the observation was not followed up as it ought to have been, each of us, I suppose, regarding the other as inaccurate."

This peculiarity was quite a new discovery, and by no means to be confused with colour-blindness in the ordinary sense. The Balfours were able to distinguish red from green as well as most people. Where they differed from others was in the proportion of red and green required to give the subjective yellow.

Investigations have been made since by others to determine the statistical distribution of this peculiarity, but these later developments hardly concern us here.

It is difficult now to trace exactly what led to the appointment of the Royal Society's colour-vision committee in 1890, but it appears that the Board of Trade had been severely criticized for their policy, or want of policy, in instituting adequate tests of colour-vision, when failure in this respect may be dangerous, as, for instance, in the case of sailors or railway employés who must distinguish between red and green lights. Thus Mr. T. H. Bickerton, of Liverpool, a witness before the committee, said :—

" It seems to me that the action of the Board of Trade all through has been inexplicable. At first they would not believe in the existence of colour-blindness ; then, when the dangers of colour-blindness could not be denied, they said that the number of colour-blind cases was very small ; and now they say that the number of cases were so numerous that it would cause great hardship to rid the [Mercantile Marine] Service of them all."

To what extent these criticisms were justified it is not necessary here to enquire, but it seems likely enough that the Board of Trade may have thought that an enquiry by the Royal Society would be more satisfactory to the critics than one held directly under their own auspices. The formal

proposal was made by the Society, and the Board of Trade wrote to welcome it, and to offer co-operation.

Rayleigh was appointed chairman of the committee, and Captain Abney (afterwards Sir William Abney) acted as secretary.

The point of chief interest was what test would be adequate to determine whether the candidate's colour-vision was normal. It is to be noticed that the signals to be discriminated are red and green, and unfortunately this is precisely the discrimination in which the colour-blind are most likely to fail.

For instance, as Rayleigh would often mention, colour-blindness has been detected in some cases by children who were gathering holly berries saying, "It is so difficult to see the berries among the leaves." Again, in his bound set of the *Philosophical Magazine*, the buff-coloured volumes have green leather tickets on the back, but in one volume the ticket is red. This he would often show to visitors. He felt no doubt that the workman who bound it was colour-blind, and considered it a good match for the specimen volume sent as a pattern.

If it were feasible to use a blue light instead of a green one, mistakes would be much less likely. Unfortunately a blue glass cuts down the light of oil or gas lamps too much: moreover, blue light has much less penetration in a foggy atmosphere.

The committee heard evidence from some of the railway officials who tested the engine-drivers. The number of cases they reported as colour-blind was suspiciously few: about 4 per cent. of men are in fact abnormal. The test was by matching coloured wools. When the method had been explained the chairman said:—

"It would be satisfactory to see exactly how you carry it out in practice. We will get in Mr. Rix [1] from the next room, and ask you to put him through the test."

This was done, and Mr. Rix was pronounced normal. When some of the astonishing matches which Mr. Rix had made were shown to the witness, he admitted it was an eye-opener! In

[1] Then assistant secretary of the Royal Society.

this case the test was faulty from the choice of unsuitable colours.

The tests for the Marine Service as laid down by the Board of Trade were also criticized unfavourably in the committee's report. They had test cards, and coloured glasses, and the candidate was required to name the colours correctly. It was possible to buy these from dealers, and there was reason to suspect that candidates who were colour-blind could be, and were, "coached" to pass the test. This would be done partly by taking advantage of the difference of brightness, and partly by such imperfect colour sense as they possessed.

Thus, many people who cannot distinguish between red and green, can recognize blue, and a bluish green signal could be recognized by these from the tinge of blue that they would see in it.

It seems plausible at first sight to argue that if they could distinguish the test colours, that is enough for practical purposes, and that it is unnecessary to enquire how this was done. It may be admitted that if the candidate could be tested under practical conditions, with all the variations in distance, and in atmospheric clearness, which come into question, and if in repeated tests he could always at once give accurate answers, then nothing more need be asked. But the conditions of such a test only need to be stated to show the difficulty of applying them in practice to great numbers of men. The only method which is really safe and practical is to make sure that the candidate's colour-vision is normal. Experiments, made at a much later date for a departmental committee under the presidency of Sir Arthur Acland, of which Rayleigh was a member, showed plainly enough that those who failed with the wool test failed also with tests on distant coloured lanterns.

Naturally enough, border-line cases were occasionally found, whom the authorities would not take the responsibility of passing, but who felt bitterly the loss of their professional prospects by what they considered an unreasonably exacting test. However, this later committee, which reported in 1912, did not feel able to relax the test appreciably.

But, to return to the findings of the Royal Society committee—they recommended the test by means of coloured wools, according to Holmgreen's specification. A test skein of wool is given to the candidate, and he is required to match it by selection from a heap of skeins which lie upon the table. The test skeins are to be a light green, a pale purple or pink, and a bright red. A great variety of other colours in various gradations are provided to afford a match to these. The report was adopted by the Board of Trade, and was made the basis of their practice.

Rayleigh would often tell of his experiences in connection with colour-blindness when entertaining non-scientific visitors at Terling. It is an easier topic to interest laymen in than most of the subjects which he had specially studied : partly, perhaps, because they come to it with some notion of what the subject of enquiry is.

The test with Holmgreen's wools would be tried. Some of Lady Rayleigh's relatives were marked examples of colour-blindness, and the selections they made excited so much astonishment that politeness was apt to be forgotten. "The difficulty is to get people to behave themselves," as he said. It was most curious to notice how abnormality became apparent the moment an abnormal subject began to handle the wools, and before he committed himself to a match. I say *he* advisedly, as women are hardly ever colour-blind, though, as in some of the individuals in question, the defect may be inherited through the mother, who herself has normal vision.

Rayleigh seldom spoke in the House of Lords, but on one question which came to the front at that time he probably felt himself to be better informed than most Members of the House. That was the question of amendment of the Electric Lighting Act of 1882, which, as is now generally admitted, was far too hard in the conditions which it imposed on the would-be promoters of enterprise. The following is an extract from his speech on the second reading of one of the amending Bills (March 26th, 1886) :—

"But no capitalist would care to invest his money in electrical enterprises on such terms as the Act imposed. Those terms in effect were that if the enterprise failed the loss should fall exclusively on the capitalist; but if it should be successful the Local Authorities should have power to step in and buy the concern on utterly inadequate terms. How could such a clause permit the proper development of any scheme of electric lighting?

"At the time the Act was passed it was generally supposed that the electric light would be a great success and the Board of Trade desired that the benefit of that success should be shared by the public and that it should not go entirely to private companies. It seemed to him that there was too much jealousy at the present day of a private company making large profits. Most undertakings were more or less of a speculative character, and if some were great successes others were miserable failures. So that one ought to be balanced against the other. The promoters of the Act, in endeavouring to advance the public interest, really inflicted great injury on the public by preventing them from having any chances of getting the electric light for many years. From another point of view he thought they need not altogether regret that something in the nature of a wet blanket was thrown by the Act over the feverish speculation of that time. But during the last two years he thought the Act had done a great public injury."

There were two other Bills with the same general object before the House, at the same time as this one, one of them a Government measure introduced by Lord Houghton. But for some reason which does not appear on the record, none of them eventually passed into law, and there was no mitigation of the existing conditions until 1888. Rayleigh took no further part.

In the 'eighties he often remarked that he thought the prevalent idea that gas would immediately be superseded by electric light was altogether premature, and recommended his friends to buy gas shares, which he thought were absurdly depreciated. I cannot find, however, that he did it himself. Perhaps the reason was that in the depressed condition of agriculture he had no money to invest.

In 1885 Rayleigh attended the British Association at Aberdeen, and read a paper on "A Theorem relating to the Time Moduli of Dissipative Systems," and several others of which

nothing further need be said here. He, along with Sir Lyon Playfair and I think one or two other scientific men, were invited to Balmoral for a night. He wrote from Whittingehame, October 2nd, 1885 :—

"I am glad to have seen Balmoral, but it was rather a racket at the time. We arrived at 7 p.m. and left at 7 a.m. the next morning. It was a very formal affair. We were kept in readiness for nearly half an hour until past 9 o'clock, when after a rapid presentation we followed the Queen to dinner. She sat next her grandsons, but I had a good view across the table. Dinner was so rushed through that one had to make the most of one's opportunities. Afterwards in the other room the Queen came round in turn to the new-comers and gave us a few minutes each. I was impressed with the graciousness of her manner, but all were kept standing. After we left, some presents of books were sent us, for which we wrote our humble acknowledgments."

The books were copies of the Queen's *Journal of my Life in the Highlands*, with autograph inscriptions.

Visits were paid afterwards to Lord and Lady Aberdeen at Haddo, to Sir William and Lady Thomson at Largs, to Arthur Balfour at Whittingehame, and to the Duke and Duchess of Cleveland at Raby. From there they went to Manchester, to a great Conservative meeting that Arthur Balfour was holding in his constituency. While there, Rayleigh dined at Prof. Schuster's house to meet Joule, which he had long been anxious to do. But it was almost too late. Joule's powers were failing, and this was probably the last time that he went out.

The following letter will be read with interest, though the circumstances which led to its being written cannot be given :

Confidential. HATFIELD, *May* 7.

MY DEAR RAYLEIGH,—

If I remember right — was one of the men who applied to me for office last year, but to whom I was unable to give it for want of room. I earnestly trust I may never again have the odious duty of cutting up the loaves and fishes for distribution, but if I should have to do so at any time, any request I was to make to . . . now, would be something in the nature of a promissory note.

I hardly ever go to an evening party in London even now—for the faces of the unsatisfied, who cannot forget, scowl at me from every quarter.

Ever yours affectionately,
SALISBURY.

On July 20th, 1886, Rayleigh's youngest son William Maitland Strutt was born.

During these years politics played a rather important part in the atmosphere of Terling. Thus in the summer of 1885 a branch of the Primrose League was formed, and Rayleigh took his part in the speech-making and entertaining which this involved. His brother Charles stood as Conservative candidate for Saffron Walden division of Essex at the general election of 1885, and the ladies of the family threw themselves into the contest: it was, however, unsuccessful.

Arthur Balfour, at that time Chief Secretary for Ireland, came to stay for Whitsuntide, 1887. At that time he was constantly receiving threatening letters, and was under police protection. The Chief Constable was directed to provide as many men as he might consider necessary. He wrote or called to consult Rayleigh, who characteristically pointed out that the number ordered was as many as *he* (the Chief Constable) might consider necessary. The latter decided to be on the safe side, and the village community was much intrigued by the arrival of twelve policemen for the week-end. Later in the year a visit was paid to the Chief Secretary's Lodge in Dublin, and then to Sir William Thomson, at Glasgow.

GLASGOW UNIVERSITY, *April 4th*/87.

I have been having a hard-working but interesting day, Fluid Motion, White's (Instrument Maker) Laboratory, etc., with very little intermission.

Sir W. Thomson has had a letter from the Duke of Argyll, with reference to Huxley's criticisms. He agrees more nearly with the Duke than I do.

Yesterday we went to hear the University Sermon. I am afraid I was not awake the whole time, but what I heard was good.

They seem to approve here of my accepting the Royal Institution.

I shall come back Wednesday night, or perhaps Thursday by day. We might go on Friday to hear the Messiah if there is a performance.

Many visits were paid to country houses during those years, but for the most part nothing of special interest remains on record about them. When Rayleigh was visiting in the Highlands his various hosts were disposed to be afraid that as he took no part in the outdoor sports which were the staple occupation of the place, he must be bored. This however was by no means the case. He usually filled up his mornings with work at the writing table, and enjoyed Highland scenery and afternoon expeditions.

I remember well a visit in the autumn of 1890 to Stornoway Castle in the Isle of Lewis, to which my brother and myself were invited with our parents. The hostess was Louisa Lady Ashburton, a lady of strong individuality, and with striking gifts in many directions. She had taken the castle from Lady Matheson. Lady Ashburton's views were strongly evangelical, and she had a touching belief in the efficacy of tracts. I remember that we were invited to use the billiard table whenever we wished, but this was more easily said than done, as it was covered with a layer of this kind of literature, a foot or more in depth. When our kind hostess took us for a drive, a pile of edifying pamphlets were placed on the seat beside her, and they were flung out to any passer-by. If no one was in sight, the good seed would often be sown on the roadside.

Rayleigh was at that time occupied with his studies of capillarity, and was much interested to notice the foaming of the brown peaty water of the Highland streams. Sometimes the bubbles were as much as a foot in diameter. He obtained one or two clay tobacco pipes in the town, and tried blowing bubbles from the water for himself, but without success. It was found, however, that bubbles could be blown from the liquid obtained from collected foam. Evidently the substance responsible for the foaming was concentrated there. But what was this substance? There are very few things that have the property of bubble making, and any precise defini-

tion or means of measuring this quality is still lacking. There is a remarkable substance called saponine which is present in an infusion of horse chestnut bark, and bubbles can be blown with it which have the extraordinary property of wrinkling like a shrunken apple if the air is allowed to pass out through the stem of the pipe. Contrary to expectation, it was found that bubbles blown from peaty water did not behave like this, but remained smooth and round like an ordinary soap bubble. So far as I am aware the nature of the substance giving rise to them is still unknown.

TERLING, *Oct.* 14*th*/91.

There is something wrong about [Willie's] bringing up, for when to-day, distinguishing from Charlie's, I spoke of my dog-cart, he corrected me, saying You mean Mama's dog-cart I suppose ! . . . I am having another fling at Tait. He brings disgrace upon the country by depreciating all the work of foreigners. I am more than ever an admirer of Van der Waals.

It looks as if A.[1] was to lead the House. I suppose at present he does not know much more about it than other people. I can't reconcile myself to his giving up Ireland.

The reference in this letter is to a prolonged though quite friendly controversy with Prof. Tait on the subject of Van der Waals' writings on the equation of state and kindred subjects. This controversy was by private correspondence as well as in the columns of *Nature*. Part of what Rayleigh wrote is reprinted in his collected papers. Neither party succeeded in convincing the other, but it would be useless to attempt here to explain the questions at issue.

As we have seen, Rayleigh's decision to go to Cambridge as professor was partly determined by the need for economy, resulting from the agricultural depression.

By the year 1878 the tenants were in arrears, and the home farms brought in nothing. In 1883 things seemed a little better, and this may have been a factor in his decision to leave Cambridge ; but the position was far from satisfactory : thus he wrote to Lord Kelvin from Terling on November 14th, 1886,

[1] Arthur Balfour.

in connection with the appointment of a Professor of Physics at the Royal College of Science, South Kensington :—

"It may perhaps surprise you to hear that I had some idea of going in for it myself ; but I found that the lecturing would be more than I could do without giving myself entirely up to it. I should prefer to live on here as during the last two years, but it is doubtful whether I can do so without uncomfortable economies unless farming matters improve."

During the years from 1879 to 1892 the amount of land in hand gradually increased as the tenants fell off owing to the bad times. Originally the land farmed by him amounted to 1,000 acres only, but at the end of the period mentioned it had risen to 6,000 acres. His brother Edward had been managing the farms and estate from 1876, on a salary ; but in 1883 it was felt that some change was called for. From that year onwards the farms were conducted on a profit-sharing basis as between the brothers, after Rayleigh had been paid his rent, and the current rate of interest on the farming capital. This arrangement, with modifications in detail, continued until his death. "I find the money, and my brother Edward finds the brains," as Rayleigh was wont to express it.

The low prices of wheat in the years about 1885 led to the development of dairy-farming on the Terling Estate, which is favourably situated for the London market. At that time there were perhaps twelve cows on Rayleigh's farms, and one man and a boy to milk them. At the end of his life in 1919 there were about 800 cows and sixty milkers.

In order to be independent of middlemen, a shop with the legend "Lord Rayleigh's Dairies" was established in Great Russell Street in 1887, and this was followed by others in different parts of London. At the end of Rayleigh's life there were eight in all. Rayleigh's name perhaps gained more notoriety with the general public from these shops and from the milk carts seen in the London streets with his name upon them than from his scientific activities. When the portrait by Sir George Reid was exhibited at the Royal Academy about 1904, a friend was amused to overhear: "No. 342"

—referring to catalogue—" Lord Rayleigh. Oh! He's the milkman." But whether this was the train of thought which the name usually evoked outside scientific circles it is difficult to say.

His name also achieved considerable notoriety about the year 1900 from a cow on one of the farms, " Captive " by name, whose yield of milk was at that time unprecedented, amounting to 1,760 gallons in a year. The cow was not of pure breed, though it might be described as a shorthorn, and was purchased casually on the strength of its generally satisfactory appearance. It gained many prizes, but its wonderful milking power was apparently a " freak," as its progeny were nothing out of the common. The account of its achievements brought enquiries to Rayleigh from many parts of the world. He was delighted with the name of a Dutch correspondent " Van Bosh." This love of a childish joke, which was perhaps only known to his family circle, was one of the most endearing features of his character.

The following is from his manuscript jest-book :—

" In the retail milk business it is necessary to bind the men who go round, when they leave your service, not to compete in the same district for two years. When a case arose, the man promptly began to compete, and it was necessary to take out an injunction. The radical *Star* took up the matter, and represented me as a rich man trying to prevent a poor one from earning his living. To annoy me a man came down to Terling and by an oversight was admitted to the laboratory where I was at work. On entering the room he put two guineas into my hand.

" ' What in the world does this mean ? ' said I, who had never heard of the matter.

" ' It is something about milk, and is a summons to Bow Street.'

" ' But I know nothing about it; the summons must be meant for one of my managers.'

" ' Oh no ! My orders were for you personally, and it is for to-morrow.'

"I did not go and got the injunction, but was fined £5 for contempt of court.

"A little while afterwards in the lobby of the House of Lords[1]: —— 'It is a shame to have you up like this?' 'Yes,' said I, 'I should have been doing much more good in the laboratory.' 'Oh,' he replied, 'I meant at Bow Street.'"

By about 1899 the worst of the agricultural depression was over, and from that time onwards, and particularly during the war, the farming was increasingly prosperous. Rayleigh was not further troubled with financial difficulties. Visitors were often taken to see the farms during a Sunday afternoon's walk. "But does it pay?" they would ask, apparently with the idea that such a result would be paradoxical. "We should not be able to entertain you here if it didn't," Rayleigh would answer.

[1] Whither he had been summoned by an urgent "whip."

CHAPTER XI

THE DISCOVERY OF ARGON

After completing the weighings of hydrogen and oxygen, Raleigh turned to what he probably supposed would be an easy problem, the weighing of nitrogen; in this case, unlike the case of hydrogen, no impurity is to be feared which is many times heavier than the gas to be weighed, and therefore it seemed there would be no great difficulty in getting a satisfactory standard of purity in the gas, evidenced by consistent weighings when different methods of purification were used. We live in an atmosphere consisting mainly of nitrogen, and the easiest and most obvious way of obtaining the pure gas was to remove the other constituents of air. Oxygen, for example, was removed by means of red-hot copper, and any hydrogen which might be accidentally introduced in this process was taken out by copper oxide.

This was carried out with success, and seven weighings were made, the extreme difference between which was only about $\frac{1}{10,000}$ part of the whole weight of gas in the globe. The latter amounted to about 2·3 grammes.

This was quite satisfactory so far, particularly as the absolute density of the gas agreed within about $\frac{1}{10,000}$ part with the value obtained by Leduc two or three years previously. Many people would have been satisfied, and would have let the matter rest there; and I think it would be difficult to blame them for doing so.

Rayleigh, however, with characteristic caution wished to confirm the result by a different method of preparing the gas. In order to do this he used a plan originally due to Vernon Harcourt, which was suggested to him (I think in conversation)

by Professor William Ramsay. This method is to allow the air to bubble through strong ammonia, and *then* to pass it over hot copper and drying material. In this case the air takes up ammonia gas from the solution, and in the hot tube the hydrogen contained in the ammonia unites with the oxygen of the air, and the resulting water is absorbed. The nitrogen residue is thus derived partly from air and partly from ammonia. In practice the process is convenient because the copper is not oxidized but remains good indefinitely, while affording a safeguard against the passage of any unburnt oxygen.

It was found, however, that the nitrogen thus prepared was lighter by about $\frac{1}{1,000}$ part, 2·3 milligrams on the actual weight, than the nitrogen prepared from air by copper, without using ammonia.

2·3 milligrams is not much, when we consider that the object to be weighed was a bulky globe, which had to be immersed in ice, in order to control the temperature while filling with gas, and then taken out of the ice and wiped, before it could be weighed. But, in seven weighings conducted with different samples of nitrogen all prepared by treating air alone with hot copper, the extreme range of weights had been only about a quarter of a milligram, and thus the 2·3 milligram discrepancy was ten times as great as any error incidental to the process of weighing.

This 2·3 milligrams discrepancy was the whole clue which subsequently led to unexpected, and, as many then thought, incredible conclusions.[1]

Two different densities had thus been found for nitrogen, and clearly one of them must be wrong. Which was it? Either the atmospheric nitrogen must be too heavy, or the ammonia nitrogen must be too light. As the former density

[1] Unfortunately the original notebooks in which the weighings were recorded were destroyed by my father, who remarked to me when he did so that no one else than himself would be able to make anything of them. This has made it impossible to determine definitely whether the earliest weighings were those made without the use of ammonia in preparing the gas. The accounts he gave in print in 1892 and in 1895 seem difficult to reconcile. I have followed the former.

was in agreement with previous determinations by Leduc and others, it was naturally supposed at first that the ammonia nitrogen was too light. Why was it too light ? What impurity could it possibly contain which would make it so ? The only plausible answer seemed to be that it contained hydrogen. But how could it contain hydrogen, when the gas had been passed through a long tube of hot oxide of copper, which, according to all ordinary laboratory experience, oxidizes hydrogen most efficiently and easily ?

It certainly ought to have done so, according to received ideas. But perhaps after all it did not. If hydrogen was *intentionally put in*, would it succeed in running the gauntlet of the hot copper oxide ? This experiment was tried, and the answer was quite decisive. For it was found that the atmospheric nitrogen could not be made any lighter by intentionally introducing hydrogen into it before it had been over the copper oxide. So this explanation failed.

The other alternative was next considered that the nitrogen prepared from air by hot copper might be too heavy, agreement with other people's determinations notwithstanding. The difficulty here was that if the discrepancy was due to imperfect removal of oxygen, as much as 1 per cent. of oxygen would have to be postulated : for oxygen is only one-seventh part heavier than nitrogen. It was not difficult to prove directly by a chemical test that no such quantity of oxygen was present. So both the alternative explanations apparently failed, and it seemed impossible to see any daylight.

Rayleigh's early training in chemistry had been limited, and with the idea that something might suggest itself to others who had specialized in that science, he wrote a letter to *Nature* (September 29th, 1892) detailing his results so far. In view of subsequent developments the following letter is of interest :—

12 Arundel Gardens, London, W.
20th Nov., 1892.

Dear Lord Rayleigh,—

I read your letter in *Nature* some weeks ago about the discordance between the densities of nitrogen prepared in the old way,

and by the way I told you of. I could think of no reason for the difference, and I can't now; but it has just struck me that ordinary ammonia contains traces of amines . . . but I really don't see what such an impurity could do. . . . The impurity could not be marsh gas surely? The only possibility that occurs to me is that as there is always excess of ammonia, so that the gas after it has passed the hot copper has to be washed with sulphuric acid, it is conceivable that the oxygen of the air is all used up, for the hydrogen of the ammonia, and the CH_4 possibly formed escapes burning and absorption.

But then you pass over copper oxide, which would certainly remove CH_4. So I give it up. But I thought it worth while to tell you of the possible impurity.

I am experimenting on surface tension. . . .

Yours sincerely,
WILLIAM RAMSAY.

Sunday morning.

You will doubtless think me a bore for all this; but I have been sleeping over the nitrogen and have revolved these points in my dreams.

1. The weight of nitrogen was compared directly with that of oxygen = 16, wasn't it?
2. Were your weights standardized?
3. Would the latitude correction not apply equally to each, and moreover wouldn't it be too small anyhow to make any difference?
4. Is it not likely, judging from the similar temperature difference between the critical points of N_2 or O_2 and the ordinary temperature, that they should be equally divergent from Boyle's law? That is that the attraction of the molecules should equally lower the pressure required to give a given volume at a given temperature?
5. The atomic weight of O_2 agrees with its density, does it not? Why shouldn't that of N_2?
6. Revolutionary. Is it conceivable that the energy parted with by N in passing from combination to the state of N_2 should affect the balance? Oxygen gains energy, I suppose, in passing from its compounds to the free state of O_2.

I have turned up Stas's actual results in his own book and they seem the same as what Clarke gives. But there is a little uncer-

tainty about vacuum weighings of $AgNO_3$ and of Ag which I haven't had patience to unravel.

W. R.

Please don't trouble to reply ; we can discuss the matter if you think it worth while when we meet. I merely want to put these ideas on paper so as to preserve a record.

I shall continue the story with a quotation.

" In order if possible to get further light upon a discrepancy which puzzled me very much, and which at that time I regarded only with disgust and impatience, I published a letter in *Nature*, inviting criticism from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia.[1] For, before going further, I ought to explain that in the nitrogen obtained by the ammonia method some, about a seventh part, is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically derived nitrogen were partially dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

" The next step in the enquiry was if possible to exaggerate the discrepancy. One's instinct at first is to try and get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake, and as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia although the greater part remained of common origin in both cases, the application of the principle suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmosphere in the ammonia method, so that the whole instead of only a part of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified

[1] This suggestion, however, was contained in his own letter to *Nature*.

some five times. The nitrogen so obtained from ammonia proved to be about ½ per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call 'atmospheric nitrogen.'"

At this point it was difficult to know what to think. No attitude seemed possible except a general one of suspicion towards the various methods of purifying the gas. In short, the only thing to do was to give up thinking until more facts had been accumulated. But at any rate there was now a healthy discrepancy of about 10 milligrams to be explained—an immense gain over the original 2 milligrams, as every experimenter will realize.

Several other ways of preparing nitrogen were now tried, and as a variant of the *oxidation* of ammonia, the *reduction* of nitric oxide by red-hot iron was tried. This gave the same weight as ammonia nitrogen. Nitrous oxide was then reduced in the same way; and this again gave the same result as ammonia nitrogen.

Material was now accumulated which made it possible to try again to think what it could all mean; and it emerged very definitely that nitrogen prepared in these different ways from different chemical sources gave all the same weight, 10 milligrams less than the same globe full of nitrogen prepared from the air.

The balance of suspicion was now turned very definitely against the latter kind of nitrogen, as being imperfectly purified, and other ways of removing oxygen from air were tried. The oxygen was removed by hot iron instead of by hot copper, but the weight was the same as when copper had been used.

Perhaps, however, there was something wrong about the use of hot metals? It was difficult to imagine what this could be, but it was better not to think but to try. There are various methods of getting rid of oxygen "in the wet way," and the absorbent that seemed least likely to introduce any other impurity was freshly precipitated ferrous hydrate. This was tried accordingly, but it gave exactly the same result as

before. All the samples of atmospheric nitrogen had one weight, all the samples of chemical nitrogen had another.

Even now, however, Rayleigh was not fully satisfied. He wished to weigh chemical nitrogen which had not been purified by means of hot metals. It was attempted to do this by getting the nitrogen from urea, but the gas thus obtained was not free from oxides of nitrogen, and could give no trustworthy result without using the hot iron purification. When this was used, the gas weighed the same as other chemical nitrogen. Finally, however, it was found possible to get from ammonium nitrite a sample of chemical nitrogen which did not require purification with hot iron. It behaved like all the other samples of chemical nitrogen.

As the result of about two years' work Rayleigh was thus at last thoroughly satisfied that nitrogen of chemical origin was about ½ per cent. lighter than the nitrogen of the air. Amidst all the doubts and perplexities that the future was to bring, he never felt the slightest further question that so much at least was true. "That, I take it, is a fact," he said.

Although for continuity of narrative I have told the whole story of the nitrogen weighings to the end, they were interrupted when Rayleigh came to London for his usual term at the Royal Institution from February to about the 20th of March, 1894. At this time the difference of density between the chemical and atmospheric nitrogen was practically established. In fact, anyone who did not possess his extreme caution would have already been more than satisfied. This being so, his thoughts were naturally concentrated on the question of what it meant. In his own words :—

"At this point the question arose what was the evidence that all the so-called nitrogen of the atmosphere was all of one quality ? And I remember (I think it was about this time last year or a little earlier [i.e., about April, 1894]) putting the question to my colleague Prof. Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. That advice I quickly

followed, and I was rather surprised to find that Cavendish had himself put this question as sharply as I could put it. . . . Cavendish not only asked himself this question, but he endeavoured to answer it by experiment."

Cavendish's experiment, published in 1899, consisted in mixing air with excess of oxygen, and passing electric sparks from a frictional machine through the gaseous mixture, which was in contact with alkali. In this way the nitrogen of the air was caused to unite with oxygen, and the resulting compound (nitrogen peroxide) was absorbed by the alkali. The process was very tedious, only about 1 cubic centimetre of mixed gases being absorbed in an hour, under the most favourable circumstances. Cavendish, however, carried it on to the bitter end, until no more contraction was perceptible. The rest may be told in Cavendish's own words :—

" Having by these means condensed as much as I could of the phlogisticated air [nitrogen] I let up some solution of liver of sulphur to absorb the dephlogisticated air [oxygen] ; after which only a small bubble of air remained unabsorbed which was certainly not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube. So that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$ part of the whole." [1]

Rayleigh's account in his Royal Institution lecture of how he consulted Dewar was written about two years after the event. Some doubt subsequently arose as to whether it *was* from Dewar that he first learnt of it. Prof. Ramsay's recollection at the time of the lecture was that *he* had first drawn Rayleigh's attention to Cavendish. I am confident

[1] Some writers have suggested on the strength of this passage that Cavendish discovered argon. I entirely repudiate this view. It was wonderful that Cavendish removed the nitrogen as completely as he did, considering the miserably inadequate appliances he had. The residue might well have come out of solution in the liquids employed, and he would have been rash indeed to attach any importance to it. Moreover, there is no reason to think that he *did* attach importance to it.

that Ramsay was mistaken on this point. He may of course have mentioned it to him later, but he was certainly not the first to do so.

My father wrote to me at Eton that he had just asked Dewar, and that he was going to look up Cavendish.[1] The letter must have reached me before March 22nd, 1894, when the school broke up and I returned home.

Prof. C. V. Boys, who came on a visit to Terling from May 19th to 21st, 1894, confirms this, writing (June 3rd, 1915): "I remember very clearly that your father mentioned to me in the earliest days of his argon researches . . . that Prof. Dewar had told him of the burning up of nitrogen by means of oxygen with the aid of electric sparks made by a frictional electrical machine of Cavendish's." Dewar himself confirmed this account in conversation with Sir Arthur Schuster.

Finally it may be remarked that Dewar would be the chemist Rayleigh naturally would ask, as he saw him every day at the Royal Institution.

After this Rayleigh further pondered the subject, and he asked Sir Henry Roscoe, whom he happened to meet, whether there was any reasonably good absorbent available for nitrogen. Were any of the more active metals, such as sodium or magnesium, any good? Roscoe said no, so far as he knew sodium or magnesium were no good. My father told me this when I was at home for the Easter holidays, which began on March 22nd.

Not unwilling to air my schoolboy knowledge, I eagerly contradicted what Roscoe had said. I told him that I had heard Dr. Porter, my chemical teacher at Eton, when lecturing on nitrogen, refer to the known fact that nitrogen would combine directly with magnesium, and that he had, on the spur of the moment, tried the experiment of plunging a piece

[1] I was at that time a boy of eighteen, but I had a fair knowledge of elementary chemistry, and was quite able to appreciate the questions at issue. I did not then know Ramsay's name, but I had frequently seen Dewar, who was kind enough to take some notice of me, at the Royal Institution.

of burning magnesium ribbon into a jar of nitrogen, and that it continued to burn, though less brilliantly than in air. I do not think he was convinced by what I said, but he told me I ought to repeat it for him to see. Intent on boyish pursuits of my own, I neglected the golden opportunity. I mention the incident merely in order to show that I have good grounds for relying on my recollection of what he said passed between him and Roscoe, and that his mind was working in this direction. The fact that magnesium would unite directly with nitrogen was in print, but not widely known.[1]

Rayleigh returned to Terling for Easter on or about March 20th, 1894, with his plans matured for absorbing atmospheric nitrogen by the process which Cavendish had used.

"I think I must have a proper induction coil before I can begin repeating Cavendish," he said. An old coil by Ladd, insulated with sheet guttapercha, had formed part of the very first apparatus he possessed when beginning serious experimental work as a young man.[2] This was got out, and, as had been suspected, it was found to have deteriorated very much. Desiccating the bobbin under an air pump failed to effect a remedy, and the instrument was sent back to Ladd's successors Hervey and Peak, to have the bobbin replaced by a new one. In the meantime the weighings of nitrogen were resumed, and were not finally completed until the middle of July.

A paper was read at the Royal Society on April 19th, "On an Anomaly encountered in Determinations of the Density of Nitrogen Gas," embodying the results obtained up to that time. In the paper as printed no opinion is expressed as to the cause of the discrepancy. It may be that something was said in the course of the discussion, but of this I have found no record. "Reading" a scientific paper usually means giving a general account of the results, and authors often allow them-

[1] At the time of writing (1923) I have repeated the experiment as I saw Dr. Porter do it. The magnesium can usually be induced to burn, and the product, treated with caustic alkali, yields ammonia. This succeeds equally if air is used, nitride being formed as well as oxide.

[2] See p. 45.

selves more freedom in speech than they do in print. The letter that follows throws some light on what passed, and is interesting as showing that the nitrogen weighings at once suggested to other minds the step that Rayleigh was already preparing to take.

KENSINGTON PARK GARDENS,
April 20th, 1894.

DEAR LORD RAYLEIGH,—

I was thinking of making the following suggestion yesterday during the discussion on your most interesting paper on the different densities you found for nitrogen according to its source, but I had not quite thought it out before the discussion was over.

There can, I think, be little doubt that the chemical nitrogen you have prepared from many different sources, always having the same density, is likely to be purer than atmospheric nitrogen (or rather than the inert residue from air, always considered to be nitrogen).

I suggest that you prepare nitrogen from air, and take its density. This you have already done. Now mix this identical lot of nitrogen with oxygen, and pass electric sparks through the mixture, absorbing the products with caustic potash. Remove the oxygen, and take the density of the nitrogen that will be left. Again mix with oxygen spark, absorb product, remove excess of oxygen and take the density of what is left. Repeat this series of operations as many times as the nitrogen originally started with will hold out. If the greater density of atmospheric nitrogen is due to an admixture of another inert gas of greater density, the density would probably get higher and higher as you went on till at last you might even have the other gas left behind. I am assuming that the other gas does not combine, when sparked, with O to form a body absorbable by potash, but in searching into the unknown we must assume something.

Most likely this idea has already occurred to you. It is so obvious that I should not have ventured to mention it had you not invited suggestions from chemists. In any case no harm can be done by putting it down on paper.

Believe me,
Sincerely yours,
WILLIAM CROOKES.

The weighing of gases as Rayleigh conducted it, with the tedious exhaustion of the globe, and the system of leaving

it on the balance overnight, could not be hurried. In the intervals of this work during April, May and June he was occupied with acoustical experiments. It will probably seem strange to the reader that he should not have pushed forward the work on absorption of atmospheric nitrogen at once. It is true that he was waiting for the renovated induction coil, but he could easily have bought or borrowed a new one at once.

But it was not in his character to do things in a hurry. He was making steady progress in hardening his conviction about the anomaly in the density of nitrogen, and that was enough. I think he rather liked the prospect of its taking a long time to clear the matter up. " It would keep Gordon [the assistant] occupied for a long time to come," he said. Friends were tempted to make a comparison with the legendary demon of Michael Scott who had to be found a task lest he should turn on his master.

By about the beginning of July the induction coil had been received, and an experiment was started on the lines of Cavendish. The only source of current available was a primary battery of five Grove cells, giving a current of less than 3 amperes through the primary of the induction coil. The equipment, though it would eat up nitrogen thirty times faster than Cavendish's frictional machine, was miserably insufficient for the problem in hand, and absorption was very slow. Several of the first experiments were futile, because the sparking electrodes cracked away from the glass, through which they were sealed. This construction had to be abandoned, and the electrodes were brought up from below, through U-shaped glass tubes. Encouraging results were soon got, but the quantity of residue left after the nitrogen had been got rid of was miserably small. To accumulate more at a reasonable rate, it was necessary to work for long hours, prolonging the work far into the night. The hammer break of the coil was liable to stick, so that constant watching was necessary. Rayleigh had a telephone arranged so that the hum of the induction coil upstairs in the laboratory was

transmitted to him as he dozed in his arm-chair in the book-room. If the noise stopped, he woke with a start, and went up to set the coil going again.

The process of absorption was very slow indeed at the end, but finally after removal of oxygen about a cubic centimetre (say a thimbleful) of gas was obtained which was evidently not nitrogen, because it did not show the slightest trace of the nitrogen spectrum in the electric spark taken through it.

The new constituent of air had been isolated shortly before the British Association meeting at Oxford. Mr. and Mrs. Crookes and Prof. A. M. Mayer came to stay at Terling (August 14th). They were shown the experiments in progress, and Crookes spoke of the large induction which he possessed, and of the method of using it simply as alternating current transformer. He had noticed the flame-like discharge obtained in air under these conditions, and the rapid oxidation of nitrogen under its action. He offered to show this at his own laboratory, and to lend the large coil if desired. Crookes' laboratory was in his own house at 7 Kensington Park Gardens, and my father went there on his way through London to Oxford on August 8th, taking me with him. There we saw the flame-like discharge and a test was made of the rate at which nitrogen could be absorbed. This proved to be about twenty times as fast as with the primary battery at Terling. Crookes also showed us the high-tension discharge between the ends of two glass rods, which when warmed with a spirit lamp became quite sufficiently conducting to carry the current. I remember how much we were struck with the scrupulous tidiness and neatness of everything in his laboratory. There was none of the miscellaneous lumber and débris of disused apparatus which formed a conspicuous feature of the laboratory at Terling.

On the afternoon of the same day we travelled to Oxford, and in the train my father told me of the work which had been done by Professor William Ramsay of University College, London, on the isolation of the new gas by means of magnesium.

This part of the story is of some delicacy. The exact share in the discovery of argon which was properly due to Ramsay has been a subject of somewhat bitter discussion, both public and private, though the personal relations of the principals concerned were always excellent, and neither wished for the intervention of third parties.

It will appear I think from the facts and dates here given that Rayleigh's own isolation of argon owed nothing to any suggestion that he received from Ramsay. It will also appear that Ramsay, starting from the discrepancy of densities which Rayleigh had established, isolated the gas independently and at about the same time.

Rayleigh always deprecated any attempt to minimize Ramsay's share in the joint discovery, and Ramsay, I feel sure, did not intentionally claim anything that was not his due.[1]

The following letter, though belonging to a later date, will best find a place here :—

12 Arundel Gardens, W.
7th June, 1898.

Dear Lord Rayleigh,—

—— has sent me the enclosed letter [2] and I consider it very friendly of him. If there is any feeling in your mind that I should do anything, please let me know, and I will do my best. If, on the other hand, you have no such feeling, I will let things alone. In the *Gases of the Atmosphere* and elsewhere I have tried to give

[1] I think, however, that his memory betrayed him on some points, as, for instance, in a lecture which he gave to the Pharmaceutical Society on November 8th, 1898. The obituary notice of Ramsay in the Royal Society's *Proceedings* is, I think, also liable to mislead on this matter. It is fair to recall that Ramsay did not trust his own memory as to the exact circumstances. He wrote to Rayleigh, "I have not a historical memory."

[2] The letter is not now in my hands, but I was shown it at the time, and remember the substance of it. —— stated that some of his friends told him that Rayleigh had the gas *in a bottle* before Ramsay came into the matter at all, and that Ramsay had therefore no claim as a joint discoverer. He disclaimed any personal knowledge, but (as a friend) thought Ramsay ought to know what was being said.

a true historical account of events. If you think the account might be amended, or is not accurate in any particular, I shall do my best to rectify it, provided you will show me where.

Yours sincerely,
W. RAMSAY.

P.S. Please be quite plain with me. You treated me so very generously at your first talk on the matter that the last thing I should dream of doing would be to deprive you of a particle of credit, either by doing or abstaining from doing anything.

TERLING (?), *June* 11*th*/98.

DEAR RAMSAY,—

Your letter followed me to London, and then back again here.

I hope that your writing does not imply that you think that I am in some way at the bottom of the remarks of our over-zealous friends. I have done my best to suppress any such manifestations, and I am no less annoyed by them than yourself.

I don't know what exactly the newspapers have said, but there is certainly nothing to contradict in the statement that you were the "co-discoverer of argon." Naturally I wish it recognized that it was my work that gave the clue, but as this was already published the case seems clear enough.

But the discussion of personal claims is very disagreeable to me, and I doubt whether I am the best person to give you advice.

I return ——'s letter, to whom you may show this if you think proper. Crookes was saying that these new gases were very explosive.

Yours very truly,
RAYLEIGH.

But to proceed with the narrative of events.

It was almost certainly after the reading of Rayleigh's Royal Society paper on April 19th and under the stimulus of what he had just learnt from that paper, that Ramsay said to my father that he (Ramsay) intended to try absorbing atmospheric nitrogen by means of hot magnesium.[1]

[1] It is clear at any rate that this must have occurred between March 22nd, when I came home, and which is therefore the earliest possible date for my conversation with my father about magnesium, and the 23rd of April, when Ramsay actually began experimenting in this direction. See W. A. Tilden's *Life of Ramsay*, p. 129.

This expression of intention had no element of the interrogative about it. It was not in form a question, and it was not treated as such by the person it was addressed to. It may be doubted whether the intention expressed was agreeable to him. It may occur to the reader to ask why, if this was the case, he did not say so. I think this was partly due to a natural reticence. Moreover he himself said, when attacked on the subject, that he would have disliked on principle to say anything which might discourage research, or in any way retard the progress of science. Ramsay very naturally took silence for consent, and implied afterwards that Rayleigh had given permission. "Well, no, I did not exactly do that," he said.

It was apparently on May 24th, 1894, that Ramsay reported verbally to Rayleigh his first results, for on that date he wrote as below :—

"I intended to ask you to-day what is probably quite unnecessary, not to say anything about the gas which I think I have got. It may turn out a mare's nest, and it would be well that no one should know of its existence.

"Another thing occurs to me. I have got a large amount of nitride of magnesium, which when treated with water, gives ammonia, and I shall be glad to give you it, if it can be conveyed to you in any way : or what might perhaps be better, I could give you the ammonia as chloride of ammonium, and you could liberate the ammonia and pass it, mixed with oxygen, over red-hot copper.

"I find on making a rough calculation that on adding my 60 c.c. of gas of sp. gr. 16 to the nitrogen from which it was obtained, it would amount to 3 p.c. of the total ; and that such a mixture of N = 14 with X = 16 would give a gas of the density you find. This is so far encouraging ; but I must try to further purify the gas ; I think that it still contains some nitrogen, and moreover it will be none the worse of another treatment with hot magnesium."

This letter, which merely amplified a conversation of the same day, was apparently not answered.

The following, more than two months later, was the next communication which passed :—

Private.

UNIVERSITY COLLEGE, GOWER STREET,
4th August, 1894.

DEAR LORD RAYLEIGH,—

I have isolated the gas. Its density is 19·075 [1] and it is not absorbed by magnesium. The last passage of the gas mixed with nitrogen over red-hot magnesium eight or ten times yielded only 3 milligrams of ammonium chloride from the magnesium nitride formed. I think there is some 1 p.c. in the nitrogen of the air; but unfortunately owing to leakage in my gas holders at the beginning of the absorption, which I didn't discover till later, I can't be sure of the amount of nitrogen which was absorbed. I should think my 104 c.c. have come from 10 or 11 litres of nitrogen.

The nitrogen from the magnesium nitride, converted into ammonium chloride, gave a product with the usual amount of chlorine. The number agrees exactly with that found in pure resublimed NH_4Cl.

Even the last absorption which removed the last 20 c.c. of nitrogen, though only a little magnesium was used up, gave nothing but nitride; the ammonium chloride produced from it was normal in its content of chlorine.

The nitrogen prepared from magnesium nitride is chemical nitrogen; i.e. it has a density 1/230 below that from air (your experiment). The value of the chemical N_2 is identical with yours. I have been watching the density of X creep up as absorption proceeds; so you see this is no chance determination with a possible source of error.

I have filled two vacuum tubes with the gas. The results are very curious. My impression is that it gives no spectrum—no visible one. Perhaps a blue green line or band is due to it. The band is just visible in the spectrum of chemical nitrogen, but is bright in that of X. It appears still to contain a trace of nitrogen, for the lines of N_2 are still visible, though not very strong.

I am going to spark the gas with oxygen and with chlorine on Monday; also to try to liquefy it by pressure. I have arranged my critical-point apparatus and made a tube to fill. I have also arranged to start with a big gas holder of nitrogen 89 litres. It is very dreary work absorbing nitrogen day after day making firmly-divided magnesium, etc., etc. However "le jeu vaut la chandelle."

I should much like to talk to you about this. Are you going

[1] Later determinations gave 19·94.

to be at Oxford ? If so we will meet there. I didn't want to trespass on your preserves and yet I feel that I have done so.

I had a long wrestle with boron as an absorber, but it is worse than magnesium. It coats itself over with nitride, and ceases to absorb after a little. It is difficult to get boron, too, in a suitable state. I haven't tried titanium, not having any.

Hoping we may meet at Oxford,

Believe me,

Yours sincerely,

WILLIAM RAMSAY.

TERLING PLACE, *Aug. 6th*, 1894.

DEAR PROF. RAMSAY,—

I believe that I too have isolated the gas, though in miserably small quantities. When I spark away (after Cavendish) 50 c.c. of air with oxygen added as required, I get a residue of ·3 c.c. which is neither oxygen nor nitrogen (nor hydrogen).

The same operations conducted upon 5 c.c. of air give a very small approximately proportional residue only. I had estimated that the gas was but 1/150 or less of the nitrogen, and the density correspondingly higher than what you give. I have concentrated X by diffusion, collecting at the end of a tobacco pipe 1/30th of the air, which goes through the pores into a vacuum. The air so prepared seems to contain twice as much X as ordinary air. I am preparing to develop this method further. My attempts to accumulate larger quantities than 1 c.c. have only partially succeeded, I think because of the solubility of X in water, but I am to try sparking on a larger scale in London. However, the 1 c.c. was sufficient to allow of the spectrum being observed at *atmospheric pressure* between platinum points. Like you I could find no new line as I had hoped, but the nitrogen lines were conspicuously *absent* or only very faint.

As to publication. I had thought of giving at Oxford some definite results of work (with urea, etc.) undertaken to settle the question of the unity of chemical nitrogen, and perhaps of throwing in such results as I have from the repetition of Cavendish. But it seems now so much mixed up with your work as to be difficult or impossible to treat separately. My own feeling is that the only solution is a joint publication. Doubtless your last results go further than mine, and are probably better established. But as you suggest the whole is founded upon work which I had carried to a certain point and was continuing. If this is the course to be adopted, the question arises whether anything should be said as yet. If not, I would keep back also my further results as to

chemical nitrogen. I shall be at Oxford staying with the Warden of Merton, and shall be glad to hear your views.

Yours very truly,
RAYLEIGH.

Ramsay replied (August 7th, 1894) :—

"To take the last part of your letter first, I think that joint publication would be the best course, and I am much obliged to you for suggesting it, for I feel that a lucky chance has made me able to get Q in quantity (there are two other X's, so let us call it Q or Quid ?)."

He then gave further details of his experiments.

At the Oxford meeting of the British Association the communication on a new gas in the atmosphere was deferred till August 13th, near the end of the meeting, when a joint session of sections A and B was held. The substance of this communication was the same as in the letters exchanged between Rayleigh and Ramsay a week previously. Rayleigh was the speaker, and there was no published official abstract, though of course there were reports of what he said in the daily papers as well as in the various weekly scientific journals. I was not present, but so far as I can remember to have heard, no comments of much significance were made beyond civil remarks by the Chairman, and a suggestion of Mr. H. G. Madan that the gas should be called argon (Greek *ἀργόν*, idle) on account of its chemical inertness.[1] This suggestion was ultimately adopted.

Although there was no public expression of scepticism at the Oxford meeting, I have reason to think that doubts were freely expressed in private. It seemed very difficult to believe that after all the attention that had been given to the composition of the air for a century, there could still be an undiscovered constituent amounting to about 1 per cent. of the whole. There was a kind of hazy idea that it was implied

[1] The word occurs in the New Testament in the parable of the labourers in the vineyard, some of whom "stood idle in the market place."

that every analysis of air that had been made was inaccurate to that extent.

No doubt in a sense this was true, since the inert residue was taken to be nitrogen, whereas 1 per cent. of it was not nitrogen at all. But this was an error of hypothesis, not of measurement. It takes time to realize the bearing of a new discovery orally explained, and often imperfectly understood. But at any rate the density determinations had been properly set out, and were before the world. Was not that enough ? I do not think that they were generally believed in, though naturally it is rather difficult to produce evidence of this.

Some, it may be, without taking the trouble to examine the figures closely, vaguely supposed that the difference was not outside the possible limits of experimental uncertainty. For instance a fellow-undergraduate at Cambridge shortly afterwards told me that at a university lecture he had heard the discrepancy of weights slightingly referred to, and dismissed with some facile explanation of experimental error.

Again, somewhat later, Dr. Ludwig Mond told Rayleigh that some of his chemical friends had supposed that Gordon had " fudged " the weighings, to save himself the trouble of carrying them out properly. Such things have been done ; but to anyone who knew Gordon's stern rectitude of character, it would be difficult to imagine a more ludicrous misapprehension of what *he* would be likely to do. Moreover, he had not the chance. Rayleigh had taken enough personal part in the routine work to exclude the possibility of it.

There was some expression of dissatisfaction and scepticism in the Press. For instance, the *British Medical Journal* (September 11th, 1894) feared that " a sad blunder had been committed," and added that " official management having burked discussion in the section, criticism makes itself heard through the public Press."

During the week at Oxford, Rayleigh had been maturing his plans for isolating the new gas on a large scale. He negotiated with his successor, J. T. Thomson, the loan of a de Meritens' permanent magnet alternator, which belonged

to the Cavendish Laboratory, and this arrived at Terling soon after his return. It was to be driven by a three-horse power gas engine, which had been installed for driving a centrifugal cream separator in connection with the dairy farming. It was in the old brewhouse, built by Colonel Strutt for domestic brewing, but disused for its original purpose since 1879.

Gordon constructed a large switch, which looked rather as if "someone had made it himself," but served very well. A leather driving belt was required, and this was the only part of the whole outfit which was bought for the occasion. There was a delay of a day or two about its arrival. While waiting for it Rayleigh was more nearly in a mood of impatience than I have ever seen him about anything of the kind. The truth is that he was not happy at this time. He had been hurried by circumstances into a more hasty publication than really commended itself to him. He had only been at work for six weeks on the isolation of the gas, which may be contrasted with the two years which he had taken to satisfy himself about the density of nitrogen; and the quantity he had separated was miserably small. Ramsay's experiments, it is true, had resulted in the isolation of a larger quantity; but he had seen nothing of these experiments with his own eyes, and as he afterwards remarked, at this time he much undervalued them.[1]

At length, however, the driving belt arrived, and work was started. The brewhouse itself, where the engine was situated, was the scene of operations, for there were no leading wires to convey the current to the laboratory, which is about 200 yards away, separated by the entire length of the house with its two wings. I remember being rather scandalized by seeing Gordon put caustic soda solution into some of the tinned dairy vessels used in the cream-separating. Absorption was started on August 31st, 1894, and proceeded at the rate of about 700 c.c. of mixed gas per hour. The induction coil got very hot, and the life-blood was seen issuing from its side in the form of a pool of melted paraffin wax. However, it

[1] I have no doubt that if he had been an indifferent third party, he would have pointed out the improbability of their being mistaken.

survived this somewhat brutal treatment, and still remains serviceable at the time of writing, twenty-nine years later. The work was continued each day till late in the evening. Rayleigh took the evening shift, though Gordon relieved him while he was at dinner. Later in the evening my mother and I would pay him a visit during his vigil in the old brewhouse, which, with the noise of the machinery, contrasted unfavourably with the comfortable surroundings of the library where we had been sitting. I remember on one occasion we discussed what Cavendish would have thought of the operations in progress, and how long it would take him, if he could come back, to make out without help the working of the gas engine, dynamo and transformer.

The admission of air with oxygen was continued for seven days, until about 8 litres of air had been admitted. On the eighth and ninth days oxygen only was admitted, to make up for the contraction. The contraction became very slow, and finally ceased, and soon after nitrogen faded out from the spectrum of the electric spark. The excess of oxygen was now removed by slowly adding hydrogen, passing electric sparks the while. I will give the issue in a quotation :—

"If the nitrogen had been completely removed, and if there were no unknown ingredient in the atmosphere, the volume under this treatment should have diminished without limit. But the contraction stopped at a volume of 65 cubic centimetres, and the volume was taken backwards and forwards through this as a minimum by alternate treatment with oxygen and hydrogen added in small quantities, with prolonged intervals of sparking. Whether the oxygen or hydrogen was in excess could be determined at any moment by a glance at the spectrum. At the minimum volume the gas was certainly not oxygen or hydrogen. Was it nitrogen ? On this point the testimony of the spectroscope was equally decisive. No trace of the yellow line could be seen even with a wide slit and under the most favourable conditions."

Rayleigh wrote to Lady Frances Balfour :—

TERLING, *Sept.* 9/94.

The new gas has been leading me a life. I had only about a quarter of a thimbleful, and that was not much to go upon. To

get larger quantities I had to set up a dynamo, and work it for days. I now have a more decent quantity, but it has cost about 1,000 times its weight in gold!

It has not yet been christened. One pundit whom we consulted suggested äeron, but when I have tried the effect privately, the answer has usually been, "When may we expect Moses?"

On September 23rd Ramsay came to Terling for the week-end, to talk matters over, and to discuss the programme for further work. It was the first time I saw him. It was decided that he was to work out the chemical properties of the new gas. "But I don't believe it has any," Rayleigh said, and the event justified him.

Ramsay was shown the sample of the new gas which had just been isolated, and Rayleigh was about to show him how the volume would go through a minimum when sparked with alternate feeds of oxygen and hydrogen. "No, don't put anything into it," was Ramsay's instinctive remark. However, the demonstration was eventually given.

Lady Constance Lytton, who became known afterwards in connection with the militant suffrage movement, was often at Terling in those days, and one of her visits coincided with Ramsay's. She was an accomplished pianist, and in the evening Ramsay sang and whistled to her accompaniment.

Rayleigh's mind was now considerably relieved. The large-scale experiments had given substantially the result expected, and all his experimental facts mentioned at Oxford were thoroughly confirmed. Perhaps it was now beyond reasonable doubt that there was a new gas in the atmosphere. It might be, and indeed was, suggested that the violent treatment of exposure to the electric arc or to hot magnesium had in some way manufactured the inert residue which had been found. But then what of the original discrepancy of weights? It could only be supposed that it had nothing to do with the matter, and that there was some other unknown cause for it. This was improbable enough, but perhaps not impossible. Was there any way of, at least partially, separating the inert constituent from air by a mild process, not open to the

suspicion of manufacturing any unknown substance ? The method of diffusion suggested itself ; for by this method the heavier constituent of a gaseous mixture can always be partially separated from the lighter one. It seemed possible, too, that a useful concentration of the new gas might be got in this way, with a view to its preparation on a larger scale. Some diffusion experiments had been tried already, but it seems that they did not give complete satisfaction, and the matter was now resumed.

The diffusion was carried out using the stems of " churchwarden " tobacco pipes. The air passed slowly through a series of these, which were contained in a glass tube in which a vacuum was maintained, so that the air stream soaked through the walls of the porous tubes like a river running through a sandy bed, which gradually diminishes by the water soaking into the sand. Under these conditions the lighter constituents of the air will leak away most, and the small residual stream which survives is enriched in the heavier constituents. It would therefore be richer in oxygen, and also in the suspected (or more than suspected) new constituent.

To get a marked enrichment it was necessary to let fifty times as much air soak away as was ultimately allowed to get through, and thus constant and vigorous pumping was required to maintain the vacuum. No power-driven air pump was available, and the house and laboratory water supply had not enough pressure to make a water-jet pump effective. The old hydraulic laboratory, where Rayleigh had worked with Mr. Mallock in the 'seventies with the water supply from the swan pond (pp. 3, 72), had been dismantled in the meantime and a hydraulic ram installed in the same shed for pumping water up to the kitchen garden, and to one of the farms. A tap was now put on the high-pressure side of the ram, and afforded the necessary supply for a jet vacuum pump. There the diffusive apparatus was set up. It was not a particularly pleasant place to work in, as the floor was ankle-deep in water from the discharge of the ram. However, a plank afforded standing room, and the exhausting power proved adequate, though

without much margin. The treated air was slowly collected in a glass aspirator bottle and taken to the laboratory. The collection for each experiment took about sixteen hours.

Oxygen, water vapour and carbonic acid gas were removed from it by absorption, and the residual "nitrogen" was weighed as usual. In every experiment this nitrogen was heavier even than atmospheric nitrogen. The difference in the best experiments was about 3½ milligrams. The excess weight of atmosphere over chemical nitrogen it will be remembered was 11 milligrams. These experiments were concluded about November 6th. They were interrupted during the early part of October, when Rayleigh and Lady Rayleigh were visiting in Scotland and elsewhere.

Rayleigh's and Ramsay's conviction about the whole matter was now hardening in quite a satisfactory way, but this was by no means the case with others, and numerous murmurs began to make themselves heard.

Thus Rayleigh wrote (October 26th, 1894):—

"Kelvin tells me that some German chemists have been jeering (privately) over the new gas."

The reply was:—

"Let those Germans jeer—they don't know your appalling caution."

A French satirist was said to have nicknamed the gas "Oxfordgen." Lord Halsbury told Lady Rayleigh in a spirit of banter that he understood the gas was known as "Mrs. Harris."[1]

Again, on December 3rd, 1894, a Fellow of the Royal Society wrote to Rayleigh privately to say that he had "proved to his complete satisfaction" that the action of magnesium on chemical nitrogen also furnished the new gas.

More serious than these various manifestations was the attitude of Rayleigh's Royal Institution colleague, Professor

[1] The reference is, of course, to Mrs. Gamp's imaginary friend in *Martin Chuzzlewit*.

James Dewar. His pioneer researches on liquid air were then exciting general attention, and he had written to the *Times* on August 15th, immediately after the Oxford meeting, raising the question of how the new gas behaved when air was liquefied. As a comparatively dense gas it should freeze to a solid with comparative readiness. Was it contained in the solid residue usually associated with liquid air ? On the whole he believed not.

In a second letter (August 16th) he suggested that its non-appearance as a solid indicated that it was not present in the air at all, but was manufactured in the process of getting rid of nitrogen, of which he thought it might be an allotropic form.

1 Scrope Terrace, Cambridge,
4th Dec., 1894.

Dear Lord Rayleigh,—

I am going to give a little paper at the Chemical [Society] on Thursday on the examination of gases at low temperatures. Indirectly this in some points bears on the work you have in hand, and in case of discussion I would like to know the exact facts. From my experiments it is clear that chemical nitrogen treated with magnesium produces a new gaseous product. So that the magnesium method of separating chemical nitrogen from the new aerial constituent breaks down. The question arises, how has the chemical nitrogen got changed ? Does it come from a change in the reagents, or is it due to the reagents, such as magnesium, glass, or gases in the metal [illegible] in the latter case, or acting [illegible] in the former. This can be very easily settled. In the meantime the point of chief interest is the result of the comparison of your product with that of Ramsay. *Am I right in assuming that your substance is substantially identical with the Ramsay product ? ?*

If this is the case, it appears chemical nitrogen can be transformed into the air product by the action of magnesium, or the air product is dissolved in magnesium or results from its action on glass ? ?

A note addressed to the Royal Institution would prevent any mistake on my part. I have just learnt —— has confirmed the action of chemical nitrogen on magnesium.

Yours very truly,
James Dewar.

TERLING, *Dec. 5th*, 1894.

DEAR PROF. DEWAR,—

I have received your letter of yesterday, which places me in rather a difficult position. Prof. Ramsay and I are as you know working in concert, and have published nothing as yet, beyond an oral statement at Oxford in a few sentences of what we had done, and announcing that we were pursuing the subject. Matters are not yet ripe for publication. Under these circumstances it appears to me to be contrary to scientific usage for others to bring forward the questions involved.

You speak of wishing to know the exact facts in case of a discussion at the Chemical Society to-morrow, and in particular whether my "substance is substantially identical with the Ramsay product." I should be glad to tell you what I know privately, but not for the purpose of a public discussion, which in my view ought not to take place. The discrimination between us [1] at all is founded upon private information, and not upon anything published.

Would it not be better to wait until we have published our work, and then, if we make mistakes, as is not unlikely, to correct them?

Yours very truly,

RAYLEIGH.

ROYAL INSTITUTION, *6th Dec.*/94.

DEAR LORD RAYLEIGH,—

If I had thought that anything I had to say this evening would either directly or indirectly be displeasing to you, I would rather not appear at the Chemical. I never had any intention of treating of your work. My informant —— may be all wrong, and my question was out of mere curiosity; never intending to use the same in public. I do not want to rush in where Angels fear to tread. My hands are full enough in all conscience, and I would be the last person to desire to take part of other people's work.

Yours very sincerely,

JAMES DEWAR.

The next morning the following report appeared in the *Times*. It is here slightly abbreviated.

THE NEW ELEMENT

There was an unusually large attendance at the Chemical Society last night, in anticipation of a discussion upon the new element announced by Lord Rayleigh at the meeting of the British Association. None of the scientific societies were at that time in session,

[1] I.e. between what particular part of the work Rayleigh and Ramsay respectively had done.

but our readers may remember that a certain amount of discussion on the subject took place in our columns. In the five months which have elapsed since the announcement was made, chemists naturally supposed that definite and unassailable conclusions would have been reached. This expectation was naturally very powerfully confirmed by the language of the President of the Royal Society,[1] who in his presidential address treated the discovery as fully authenticated, and described it as the greatest scientific event of the year. It was therefore a serious disappointment to the Chemical Society last night to discover that not one of the men known to have been engaged in working at the new element came forward to give information as to the results so unequivocally proclaimed. Some astonishment was also felt when the president of the society observed that a good deal of feeling had been called forth by this question, notwithstanding its purely scientific character. It was evident from his remarks that the discoverers of the new element are anxious to forbid discussion, on the extraordinary ground that as they have not published their conclusions, discussion can only proceed upon private and confidential information. It is obvious, as he pointed out, that when chemists are informed that they have entirely failed to comprehend the constitution of a substance upon which they have bestowed so much labour as upon the atmosphere, they have an indefeasible right to carry out whatever experiments they may think fit. Moreover it is absurd after not only the facts of the discovery but details of the preparation and properties of the new element have appeared in the public press to pretend that there is any breach of social or scientific etiquette in discussing them. . . .

Prof. Dewar described last night the methods of applying liquid air to the investigation of the properties of gases. It appears from his experiments that chemically prepared nitrogen liquefies at the same temperature and boils off at the same rate as nitrogen obtained from the atmosphere. Yet according to the discoverers of the new element, one contains a substance which is not present in the other, the density of which is nearly half as great again as that of nitrogen. It follows either that the new substance does not liquify at all, even at temperatures which condense much rarer gases, or that it behaves in exactly the same manner as nitrogen. Chemists will fully appreciate the extreme singularity of a substance with the assigned density which fulfills either condition. It is not too much to say that its discovery would revolutionize chemical theory. But the whole ques-

[1] Lord Kelvin.

tion becomes infinitely more obscure if, as seems to be the case, chemically prepared nitrogen passed over red-hot magnesium behaved in a manner undistinguishable from that of atmospheric nitrogen treated in the same manner. Confirmation of this result would at once prove that the new substance is a manufactured product, which may indeed be present in the atmosphere, but cannot be a new element. With these grave uncertainties brooding over their discovery, it is remarkable that Lord Rayleigh and Professor Ramsay should prefer to keep silence, though all doubts might have been settled in almost as many days as months have elapsed since the meeting of the British Association."

It is not perhaps surprising that this report of what had passed made Rayleigh extremely angry. He wrote to ask Dewar whether the concluding paragraph was a correct report of what he had said. Dewar gave an answer which Rayleigh took as a disclaimer. Rayleigh suggested in reply that the *Times* report could easily be repudiated in a public letter. This suggestion was not adopted, Dewar maintaining (correctly, I think, so far) that the last paragraph did not profess to be a reproduction of his words. Relations became very strained for a short time, but eventually the matter was allowed to drop by mutual consent, and Rayleigh and Dewar resumed their usual friendly relations when he came back to the Royal Institution in the ensuing February.

In the meanwhile, other work on the new gas was proceeding. The dissolved air was extracted from water, and the "nitrogen" from this air was weighed. It was found to be 24 milligrams heavier than chemical nitrogen, and therefore to contain more than twice as much of the new gas as atmospheric nitrogen does.

Again, the spectrum of the new gas had to be investigated. This is a matter requiring special experience. Generally a great many lines will appear in the spectrum of any gaseous sample, and particularly when vacuum tubes are used, impurities often assert themselves very strongly, while a main constituent may not show its spectrum at all. For these reasons the spectroscopic work was deputed to two friends, Mr. W. Crookes and Prof. A. Schuster. Samples of the gas were sent

to them, and both reported about the same time a characteristic spectrum, the same whether the gas was isolated by magnesium or by sparking with oxygen. Rayleigh was much relieved to hear this, as it tended to dispel the doubts raised by Dewar's observations.

One final point remained to be investigated, which might seem simple, but which proved to be very troublesome. This was to establish that the gaseous residue would shrink to nothing if chemical nitrogen was used as the raw material instead of air or atmospheric nitrogen. This would finally vindicate the processes used from the suspicion of manufacturing the gas in the course of separation. Such proof may seem redundant at the stage now reached. The official explanation of the joint authors was: "We have thought it undesirable to shrink from any labour that would tend to complete the verification."

Rayleigh wrote, however, to Lord Kelvin (November 11th, 1894) describing the diffusion experiments, and proceeding, "I regard this as a demonstration, but for the sake of weak brethren am sparking to the bitter end 3 litres of chemical nitrogen, and hope to find nothing."

The difficulty of this experiment is that if any air leaks in or gets accidentally introduced in the course of the complicated manipulations, the sharpness of the test is spoilt. Owing to causes of this kind, none of the experiments gave an ideal result, but the residue was very small indeed compared with what atmospheric nitrogen had given. Tests of this kind were made both by Rayleigh and by Ramsay. I remember that about the middle of December, 1894, the latter came down to Terling for a week-end, and produced a corked test tube (upside down, with water above the cork) from his inside breast pocket. This contained the residue from 3 litres of nitrogen, which had been reduced to small volume with magnesium. He proposed then and there to spark it down with oxygen. There was some consternation when 3 c.c. of residue was found to survive. This it was true was only about 1/10th of what atmospheric nitrogen would have given. But why was there any residue? On

reflection it appeared that it was probably derived from the considerable quantity of water used in collecting the gas in the first instance. A later experiment with some improvements gave a fourfold better result, and was considered good enough.

The time was at last ripe for collecting the results and laying them before the world. While the work above described was going on at Terling, important progress had been made by Ramsay at University College, but it does not fall within my scope to dwell on this in detail. Ramsay had isolated a good supply of the gas by the magnesium method, and had determined its density with considerable accuracy. He had tried many experiments to induce it to enter into chemical combination, but with a negative result. Finally, he had determined the velocity of sound in the gas, from which the ratio of specific heats can be determined. This ratio was found to have approximately the value $1\frac{2}{3}$, which is also possessed by mercury vapour. Mercury vapour is known on chemical grounds to be monatomic, and the value for the ratio of specific tests was held to be confirmatory of this, though on grounds which appeared somewhat insufficient at that time, and appear still more dubious now. Argon was found to behave like mercury, and was classed with it as monatomic. Whatever may be thought of the reasoning, the conclusion has stood the test of time and is now well established on quite independent evidence.

Some preliminary progress in drafting the paper had I think been made during Ramsay's visit to Terling in December, and at Ramsay's suggestion a paper had been sent in then to the Smithsonian Institution of Washington in competition for a prize of $10,000 from the Hodgkins Fund "for a treatise embodying some new and important discovery in regard to the nature and properties of atmospheric air." Prior publication would have disqualified for this prize, and this was one reason why nothing had been published in connection with the Oxford announcement. S. P. Langley, the secretary of the Smithsonian Institution, was present at Oxford and indicated that there was no objection to an oral statement. The paper which was to be

presented to the Royal Society was substantially the same as the Smithsonian one. The details were finally settled during a week-end visit on January 26th, 1895. Though the work had to be hurried through in a short time, Rayleigh was not to be diverted from his usual attendance at morning church. However, he compromised by coming out before the sermon.

From such echoes as I heard, the tendency seemed to be for Rayleigh to try and restrain Ramsay from stating positively conclusions which he thought were insufficiently confirmed. One of these was the notion Ramsay had that the gas leaked out through the walls of a red-hot iron tube. I think too that he insisted on toning down what Ramsay had written as to the inference to be drawn from the ratio of specific heats.

Rayleigh was greatly impressed with Ramsay's skill as an experimenter. But on questions which were to be decided purely by reasoning they did not always see eye to eye. He urged on Ramsay that the preliminary absorption of nitrogen would be more simply carried out if the hot magnesium were contained in a cul-de-sac, and the atmospheric nitrogen were allowed to pass into it as long as it would. No, said Ramsay, you must circulate it over the magnesium, or the action will be too slow. No doubt, said Rayleigh, that will be so when the argon has concentrated to a large proportion of the whole, but what good will the circulating do in the early stages ? But Ramsay was unconvinced. " You must circulate it," he said. " I could not altogether make him out about that," my father remarked, in repeating the conversation just given.

He had, however, a great admiration for Ramsay's skill and facility as an experimenter, and his prompt energetic attack on any problem that came up, as well as the devotion he was able to inspire in his pupils and assistants. He was always most solicitous that Ramsay's merits should be duly recognized ; thus he gave a hint that it would be difficult for him to accept the Barnard medal of the U.S. National Academy of Sciences unless Ramsay could share the honour. His regard for Ramsay seems to have been reciprocated. I learn from Lady Ramsay that during his last illness Ramsay

spoke of a friend in a different line of life, saying, "He is I think the second greatest man alive." Asked who he put first, he answered "Lord Rayleigh."

The paper, entitled "Argon, a New Constituent of the Atmosphere," was read at a meeting of the Royal Society on January 31st, 1895. As the applicants for admission were very numerous, it was decided to hold the meeting in the large lecture theatre of London University which then existed at the back of Burlington House, and an invitation was issued to all members of the Physical and Chemical Societies. As Rayleigh had made the announcement at Oxford he felt it was Ramsay's turn, and the exposition was given by the latter. I remember that Ramsay said he had been asked by friends to *show* some argon, and he produced a sealed glass tube to satisfy them, though of course there was nothing to be seen. Rayleigh said afterwards, "I did not know you had as much as that." "I did not say what pressure it was at," replied Ramsay. "I was not going to risk losing a valuable stock by the tube being broken!" It was a tube from which nearly all the argon had been pumped out before it was sealed up!

Two supplementary papers were read. The first was by Dr. K. Olszewski, a Polish chemist, who specialized in low-temperature work, and to whom Ramsay entrusted the investigation of argon under these conditions. The second was by Crookes, on the spectra of argon. He had found that an argon vacuum tube gave a blue or a red glow according to the conditions of the discharge, and he kept speaking of "red argon" and "blue argon." Rayleigh was at first rather inclined to scoff at this, and he said it reminded him of two powders labelled *red electricity* and *blue electricity*, which he had taken to satisfy the importunity of a lady relative who had great faith in their virtue for healing rheumatism. However, he visited Crookes' laboratory with me soon afterwards, and was very much struck with the phenomenon.

But to return to the discussion which followed the reading of the paper. There were several speakers, including the respective presidents of the Chemical and Physical Societies.

It was agreed by all that the case for a new gas in the atmosphere had been fully established. The admission on the part of the President of the Chemical Society was, Rayleigh thought, somewhat grudging. "No doubt the paper will meet with very considerable criticism throughout the world." "But apart from the facts brought forward in this paper, there is a portion which is purely, one almost might say—if I may be allowed the expression on such an occasion—of a wildly speculative character . . ." and so on. In fact, Rayleigh was somewhat disappointed that discussion turned almost entirely on the rather dubious conclusion about the monatomic character of the gas as deduced from the ratio of specific heats.

After Ramsay had given the main account, Rayleigh was called upon by acclamation from all parts of the hall: but he reserved himself till near the end. He then said, "I have very little to add. . . . I am not without experience of experimental difficulties, but I have never encountered them in anything like so severe and aggravating a form as in this investigation. Every experiment that one attempts takes about 10 days or a fortnight to carry out to a definite conclusion. . . ." He further replied to various points that had arisen in the discussion.

Letters of congratulation came in the next day or two. I give two of them.

High Cliff, Lyme Regis, Dorset,
1*st Feb.*/95.

Dear Lord Rayleigh,—

It was a very great disappointment to me not to be able to be at Burlington House yesterday. But I have read the abstract and it is a very long time since I enjoyed anything so much. I am astonished at the progress you have made during the last few months and I greatly admire your beautiful research in all its aspects. And how grandly Crookes and Olszewski clinch your conclusions.

I see from the *Times* to-day that Armstrong complains that you had not put the matter as *logically* as he would have liked! I cannot imagine what he can mean.

Please excuse all defects, as I write in bed. Do not think of answering this but

Believe me, with hearty congratulations,

Yours very sincerely,

JOSEPH LISTER.

INVERARY, *Feb. 2nd/95.*

MY DEAR LORD RAYLEIGH,—

One word of hearty congratulations on your having added your name to the list of great *discoverers* after a most difficult and laborious investigation.

It reminds one of a fine sentence of Owen:

"Nature never proclaims her secrets with a loud voice, but always whispers them."

The whisper was a small one in your case.

Yours very truly,

ARGYLL.

It might perhaps have been anticipated that the newspaper campaign against argon would now have died a natural death, but it died hard. The *Electrical Review* in particular maintained the fight, and published an article on February 28th on "Argon, the supposed New Element in the Atmosphere," containing the following intelligent suggestion.

"Lord Rayleigh appeals to spectrum analysis in order to settle the question as to whether the 65 c.c. (of unoxidized residue) is argon, or whether it is a mixture of commoner gases.

"There is a way of dealing with the 65 c.c. by well-known methods of gas analysis, but apparently that method has not been resorted to."

Lord Kelvin wrote to the Editor to suggest that the writer should submit himself to the "silent discharge," but this did not prevent the appearance of another article on "The Argon Myth" early in April.

Rayleigh's usual season in London and at the Royal Institution now began, and in April he gave a Friday evening lecture on Argon. The lecture room was crowded to overflowing. The ground covered in the lecture was of course that which we have already traversed, but towards the end a few remarks of a more general tendency were made, reviewing the whole subject. I quote one of these.

"The result (ratio of specific heats) is no doubt very awkward. Indeed I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all that we can now do is to apologize for ourselves and for the gas."

I believe that in these sentences we have the only public reference he made to the constant stream of carping criticism to which he and his co-worker had been subject for months. It must not be supposed however that he was unaffected by it. About this time he said to me, referring to the discovery of argon as a whole, "I have got more pain than pleasure out of it so far."

His earlier work had been chiefly on the mathematical side of Physics, where, as a rule, none but well-considered opinions were offered. But he now found himself in a different atmosphere. "I want to get back again from Chemistry to Physics as soon as I can," he said. "The second-rate men seem to know their place so much better." Another remark in this connection was: "I find that the only chemistry I can remember is either what I knew as a boy, or what I have just read up for the immediate purpose."

He said too that in earlier work he had been generally able to improve on the experimental methods in use before, but that in the argon work he had not been able to do much in that direction.

Another matter of interest came up at this time. Berthelot, the celebrated French chemist, considered that he had induced argon to combine with benzene under the action of the silent electric discharge. Ramsay and others were disposed to take this very lightly, and the event has apparently shown that they were right. I mention this because it drew characteristic remarks from my father. "I don't understand all this poo-poohing of Berthelot," he said. "It is poor fun writing nonsense. A young man might do it in order to make a reputation, but Berthelot has a tremendous reputation in France."

During the season in London before Easter, 1895, Rayleigh was occupied at the Royal Institution in carrying out the

absorption of nitrogen on a much large scale than hitherto. The vessel was of 20 litres capacity, and the mixed gases were taken up at the rate of 7 litres per hour. Enough crude argon was accumulated to allow of weighing on the same scale as the other gases, and the argon was taken down to Terling at Easter, where it was finally purified and the density determined with full accuracy. Some time later Rayleigh returned to the problem of enlarging the scale of the operation, and he read a paper, "Observations on the Oxidation of Nitrogen Gas," before the Chemical Society. The rate of absorption was pushed to 21 litres per hour. This is some 21,000 times the rate at which Cavendish was able to work with his electrical machine.

This process of oxidizing nitrogen is now carried out in Norway on a scale more than 21,000 times larger again, for the purpose, not of isolating argon, but to obtain nitrogen in the combined form for fertilizers and explosives. In this connection I quote from the address given by Dr. J. A. Harker before the Chemistry Section of the British Association at Hull, in 1922. "I do not think it has been sufficiently recognized that the arc process in its industrial form owes its initiation in a very great measure to scientific researches carried out mainly by British investigators. When during the war I was privileged to see the whole of the developments at Notodden, also the much larger and newer plants at Rjukan, officials of the Norsk Hydro Company told me that Prof. Birkeland used to recognize frankly that his inspiration to found an industrial process was derived from the famous British Association address of Sir William Crookes, and especially from the quantitative experimental work of the late Lord Rayleigh. I am sure I am not wrong when I say that Lord Rayleigh's big flask mounted on a wooden stool, and provided with a pair of metal poles and an internal potash fountain which still reposes on a high shelf at the Royal Institution, is the lineal ancestor of all the great Norwegian plants of to-day. The experiments made by Lord Rayleigh employing one or two horse power in this apparatus, in which he carefully measured for the first time the relation between the energy consumed

and the amount of nitrogen fixed, pointed the road to all that has since happened in Norway."[1]

The yields obtained by Rayleigh were 40–50 grams of nitric acid per kilowatt hour, while at the present day the yields in the best technical large-scale practice are only from 60–70 grams. In 1911, when the financing of these projects was in progress, attempts were made to get his authority to back them. But I doubt whether he was willing to commit himself on the commercial outlook.

It was about March 24th, 1895, that Ramsay discovered helium in cleveite. He invited Rayleigh to help in working out the discovery, but Rayleigh thought it better not to accept the offer. I do not think working in double harness was very congenial to his habit of mind. Some give-and-take is of course inevitable in joint authorship. Rayleigh was very fastidious as to exactly what he committed himself to, and carried this so far that he rather disliked signing any joint memorial. "It isn't written as I would have written it," he would say.

Later on, however, he did some work on helium by himself. He was the first to establish its presence in the gas arising from the hot spring at Bath, and, later, he showed that its presence in minute proportion in the atmosphere could be demonstrated by the method of diffusion.

I have dwelt at considerable length on the discovery of argon, as it was the most dramatic event in Rayleigh's scientific career. At the time, the question was often asked in conversation, by people belonging to the large class who do not distinguish between a discovery and an invention, "What is it going to do for us ?" It was of course impossible to give any answer which they thought satisfying. It has since had at any rate one important industrial application in affording the best gas for filling the "½ watt" incandescent electric lamps which are now in use in shops and private houses everywhere. Every such lamp bulb contains what, at the time I have been writing of, would have been a valuable store of argon. The

[1] Reference may also be made in this connection to a paper by Birkeland, *Proc. Faraday Soc.*, Vol. II, pp. 100–101.

gas is separated industrially by a process which may be described roughly as a fractional distillation of liquid air. But in scientific eyes this application is not the important fruit of the discovery. It will be instructive to trace what have been its scientific consequences, all flowing from the original discrepancy in the weight of nitrogen. These are mainly through the discovery of the other inert gases, and especially of helium, by Ramsay. The discovery of argon led directly to these. It showed that a gas reported as nitrogen by indifference to chemical reagents could not be safely taken to be such. In the light of this consideration Ramsay re-examined the supposed "nitrogen" given off by certain rare minerals, and found it to be the hypothetical "helium" recognized and named many years before by Lockyer in the spectrum of the solar prominences. Later, it was emphasized by Rutherford that helium was usually found in minerals containing uranium and radium, and he conjectured that it was probably a product of the spontaneous breaking up of the atoms of these substances. Later, Ramsay and Soddy showed by direct experiment that radium continually generated helium, and Rutherford proved that the atomic projectiles shot off by radioactive bodies are in fact helium atoms. In this way the whole scheme of radioactive disintegration was unravelled. It is very doubtful, to say the least, if this goal would have been reached even now, but for the clue afforded by the original nitrogen weighings of 1893. Again, neon was found by Ramsay and Travers, as the result of a search to see if argon was homogeneous. This led in the hands of J. J. Thomson to the first discovery of isotopes, outside the region of the radioactive elements: and hence followed the exploration of this field by Aston, who showed that many of the common elements are mixtures of two or more constituents, which, though of different atomic weight, are inseparable by purely chemical means. This has afforded in great measure the explanation of the apparent departure of the atomic weights from whole numbers, which, it will be remembered, formed the starting-point of Rayleigh's investigations on the weighing of gases! (See p. 158).

CHAPTER XII

IN THE 'NINETIES

Mention has been made in the last chapter of the Oxford Meeting of the British Association in 1894, when argon was first publicly announced. I will now go back to the Oxford Meeting to mention some incidents which were passed over to avoid interrupting the story of argon.

Lord Salisbury was the president. He had written to Rayleigh from Hatfield (February 10th, 1892) :—

"As to the other matter—(The British Association), I feel very highly honoured by the suggestion—but, apart from other very obvious disqualifications I think my political position at the present moment is a fatal difficulty. To accept that nomination would be to announce that I feel certain of not being in office next year. *Toute verité n'est pas bonne à dire.* I should not be justified in casting a damper on the younger and more hopeful members of the party by telling them they were certain to be beaten at the elections. So I am afraid it would not do to accept it. I should be an infamously bad choice."

However, in 1894 this objection no longer held, and he accepted the office.

20 Arlington Street, S.W.
June 16, '94.

My dear Rayleigh,—

The British Association have informed me that they want the copy of my address in the middle of July. Why cannot they take it down like any other speech, at the time ? However, the request involves my troubling you with the enclosed. Will you kindly look through it and see if there are any scientific blunders in it ?

My predecessors, as a rule, have made much longer addresses.

But this, at my usual rate of speaking, will take over an hour; and I cannot consent to condemn an unoffending audience to any more.

Yours very truly,
SALISBURY.

20 ARLINGTON STREET, S.W.
July 4, '94.

MY DEAR RAYLEIGH,—

Many thanks. I have adopted your corrections. The most important one—on the question of the analogy between luminous and electric undulations—affected a sentence on which I had great doubt when I wrote it. But I could not find any proof of the impermeability of metals to the electric wave. . . . A sentence of Lord Kelvin's last year seemed to point the other way.

I enclose Schuster's letter. I find in the April *Edinburgh* (p. 372) a paper, evidently by some great gun, in which it is plainly stated that "neither sun nor stars give any trace of being supplied with oxygen." So I may take that as the dictum of popular science, up to date.

Ever yours affly.,
SALISBURY.

At Oxford, Rayleigh, Lady Rayleigh, and I stayed with the Warden of Merton, G. C. Broderick. Huxley, with Mrs. Huxley, was also of the party. He was then an old man, and he died shortly afterwards, but his comments at the breakfast table on some passages of Lord Salisbury's Presidential Address of the night before were vigorous enough. These passages pointed out difficulties in the Darwinian theory. "Lord Salisbury may be sound enough on physics and chemistry," he said, "but on biology he isn't. He does not understand the theory of evolution."

I ventured to press him as to what answer he had to give to Lord Salisbury's objection, that Kelvin's estimate of the age of the earth was painfully small for the Darwinian view. Although I was only a boy, he kindly answered me, and said that he had always taken a position intermediate between the physicists, who advocated a brief age for the earth, and the geologists, who advocated a very long one. The progress of

knowledge has of course left to the views of that time only an historical interest.

The second edition of Rayleigh's *Theory of Sound* appeared in 1894–1896. Two thousand copies were printed, the earlier edition of 1,000 being exhausted. Each of the volumes was considerably enlarged, so that the entire work bulked nearly half as large again. The additional subjects treated included bells, of which he had made a special experimental study, reed pipes, singing flames, the *blowing* as distinguished from the *resonance* of organ pipes, the repulsion of resonators, the effect of friction and heat conduction on acoustical problems, capillarity and capillary waves and ripples, vortex motion and sensitive jets, and facts and theories of audition. There is also a chapter on the telephone, leading to a discussion of alternating currents, and their distribution in a network of conductors.

Among the subjects treated in this chapter is one which was presented to the British Association at Birmingham in 1886, under the title, " An Experiment to show that a Divided Electric Current may be greater in both Branches than in the Mains." It recalls an amusing incident. His name was either omitted or accidentally detached, and the Committee " turned it down " as the work of one of those curious persons called paradoxers. However, when the authorship was discovered, the paper was found to have merits after all. It would seem that even in the late nineteenth century, and in spite of all that had been written by the apostles of free discussion, authority could prevail when argument had failed !

In the summer of 1895 Rayleigh once more took part in psychical research, joining the séances which were held at Cambridge with Eusapia Paladino. I think the scene was Frederick Myers' house. Prof. and Mrs. Henry Sidgwick took part, and Rayleigh was very much amused with the way the former exerted himself to entertain the medium with a very limited knowledge of conversational Italian—the only language she could speak.

Rayleigh himself was no use in this way, but the medium professed that his presence gave " much power." However,

as later developments made it very probable that the whole thing was a fraud, this is not of fundamental interest.

The following letters give the only contemporary record of his experiences, but there was a brief public mention of the subject in his presidential address to the Society for Psychical Research in 1919.[1]

To the Dowager Lady Rayleigh, from Dieppe, August 30th, 1895 :—

" The bathing is pretty good, and one can fill up the day somehow ; but I usually find holidays more fatiguing than work. I suppose however that one's brain has a chance of recruiting itself. To-day we are to lunch at Puys with ' La Princesse ' [Lady Salisbury] as she is called hereabouts. Only the ladies and children are over as yet.

" We had your letter yesterday. I had 3 sittings at Cambridge. At the first I was on Eusapia's right, holding right hand, feet, and with my left hand round the back of her neck. The back of this hand was touched. Of course this would be easy enough, if her left hand were free, but we had the assurance of Myers at the moment that he was holding it. This may be said to depend upon M. but afterwards the same sort of thing occurred when he was out of it. The last day Evelyn was with me, and several things occurred when Eusapia was held by E. and me. There was usually light enough to see where people were. The impression left upon me is that it is not a question of holding her (at the moment) nor of an accomplice. But I felt all along that I should not be convinced by such things under such conditions. We are looking forward to Maskelyne's views as to what can be done by mechanical contrivances.

" I should say that there were some bad features such as Eusapia wearing black (but her collar was mostly visible). The good feature is that there was almost always notice, so that one's attention was on the alert. This goes for a good deal with me."

Lady Rayleigh wrote from Terling, August 25th, 1895 :—

" *I* was at one séance when my chair was pulled from under me, and lifted between the Medium and me on to the table we were sitting at—and when we were sure we had possession of her hands and feet.

" I feel as if we ought to be convinced and yet it is difficult to

[1] See Appendix II to the present volume.

know the resources of conjuring, and it was almost dark in the room."

The Royal Institution, founded by Count Rumford in 1799, though under Royal patronage, is a private institution, supported by the subscriptions of its members, with only small endowments. It has a scientific library and reading-rooms for the members, but it is chiefly famous for the laboratories and lecture theatres, associated with the immortal discoveries of Davy and Faraday, who were Professors in the Institution. In 1887 the professorship of Chemistry was held by (Sir) James Dewar. The other, known as the Professorship of Natural Philosophy, had been held by Tyndall, whose name has already occurred in these pages, and who had an unrivalled reputation as a popular lecturer, and great merits as an experimental investigator. Tyndall's health had for some time been failing, and Rayleigh was asked by Sir Frederick Bramwell, the Honorary Secretary of the Institution, whether he would accept the appointment. He replied as follows :—

"*Apl.* 2/87.

"I have been carefully considering the proposal you made to me last Sat. on behalf of the Managers. Let me in the first place record my high sense of the honour done me by the offer of the professorship of Natural Philosophy. The principal ground of my hesitation has been that being already provided with a Laboratory in the country, where I pass much the greater part of the year, I should not make adequate use of the Laboratory in Albemarle St. I understood you to say that in your opinion the Managers would not regard it as of importance whether the Scientific work of the Professor was done in one Laboratory or another. I am very anxious that there should be no misunderstanding, for if elected to the office I should wish to feel quite free in this respect. Of course the preparation of lectures for the Institution would be carried out mainly in Albemarle St.

"Please bring this letter before the Managers. If they should still think that my appointment would be to the interest of the Institution I shall be willing to accept the nomination."

These terms were accepted. The fixed duties of the Chair were not heavy. A course of six lectures on Saturday after-

noons were given before Easter each year, and one Friday evening lecture, which was mainly devoted to original results all the lectures being elaborately illustrated by experiments The arrangement suited Rayleigh very well, for the lectures were all to be given during the time that he was ordinarily in residence in London, and his brother-in-law's house in Carlton Gardens where he lived was situated conveniently near. Gordon was appointed as his Royal Institution assistant, and came up with him every year from Terling, usually bringing a good deal of apparatus.

Access to a laboratory during his season in London was an advantage which he appreciated, and the facilities there in the way of an electric supply made up in some degree for the lack of this convenience in the country.

Rayleigh's laboratory at the Royal Institution consisted of three rooms on the first floor, opening out of one another : the same rooms, I believe, that Tyndall had worked in. Most of the work was done in the first and largest room.

The investigations undertaken there were not usually such as to require any elaborate assemblages of apparatus, or long-continued preparation. The season in London was not long enough for work of that kind, and many investigations were barred out by the vibration caused by the machinery running in the basement. Dewar was at this time carrying out his pioneer investigations on the liquefaction of gases. The prevalent idea at the time was that this work was very dangerous, an idea perhaps exaggerated. Gordon used to look with satisfaction on the large window at the end of Dewar's laboratory, which he thought would act as a safety valve in the case of an explosion, and save the floor above on which he (Gordon) was working.

Rayleigh wrote to Lord Kelvin, February 16th, 1888 :—

"I am now established in the R.I. laboratory. The apparatus has been allowed to fall behind altogether, of which I may give you an idea when I say that there is not an ohm in the place ! "

He also apparently wrote to Tyndall to the same effect.

16 *Dec.*, 1887.

HINDHEAD, HASLEMERE.

DEAR LORD RAYLEIGH,—

You are quite right, but our poverty as to apparatus was self-imposed. We did not buy, but we borrowed, and paid for the loan. This was Faraday's plan, and mine. It answered. Besides, we were often able to put together, through the exercise of mother-wit, apparatus which, had we resorted to the philosophical instrument maker, would have cost a ten-fold sum. We never lacked the necessary apparatus ; but we declined to heap up dead stock at a time when each year's advance made the apparatus of the preceding year defective. By such methods the Royal Institution was raised from a position of poverty and difficulty now happily unknown.

Besides the apparatus I mentioned to you you will find a very beautiful Dove's Syren, a wave-machine, and various other things which I handed over to the Institution. For a considerable number of years there was a free exchange of apparatus between the R.I. and South Kensington. It might be worth your while to re-establish this relationship, which was disturbed by some " tiff " between the assistants.

Yours very truly,
JOHN TYNDALL.

Faraday used proudly to say to his continental friends—" Our show of apparatus is humble, but if I wanted 1,000 pounds for experiment to-morrow our members would give it to me."

The afternoon lectures were for the most part elementary and easy to follow. They were illustrated with many experiments, the more difficult of which were arranged and rehearsed in the laboratory during the week. The lecture-room was occupied for the evening lecture on Friday, so that the work of setting up the experiments had all to be done during three hours of rather feverish haste on Saturday morning.

Gordon had charge of the experiments, and proved an efficient lecture assistant, though perhaps he did not always give enough attention to appearance. I remember Rayleigh pointing to a muddy-looking liquid on the lecture table, prepared for some experiment, and saying, " Anyone else lecturing here would have had that filtered clear, but perhaps it is not

fair to expect Gordon to do one thing [1] at Terling and another here."

The audience at the afternoon lectures, which are open to the public on payment of a fee, was of a very varied kind. At one end of the scale were eminent scientific discoverers, such as D. E. Hughes, the inventor of the microphone, and Mr. Justice Grove. At the other end were a curious kind of residuum difficult of classification, seekers after truth in their own way, no doubt, but without any precise notion of how the search should be conducted. Some of these were, I think, mystics and dabblers in the occult. They were often eager in asking questions after the lecture. "Do you believe in the moon?" Rayleigh was asked on one occasion by a lady of somewhat eccentric aspect. "I have never seen any reason to disbelieve in it," he replied. I remember asking what he thought would be the result if the audience were required to pass an examination on what had been said; but this he did not like to contemplate. The numbers of the audience were, I think, somewhere about 300.

The subjects taken for these afternoon lectures were :—

Year.	No. of Lectures.	Subject.
1878	4	Colour.
1887	6	Sound.
1888	7	Experimental Optics.
1889	8	Experimental Optics (Polarization: Wave Theory).
1890	7	Electricity and Magnetism.
1891	6	The Forces of Cohesion.
1892	6	Matter: at Rest and in Motion.
1893	6	Sound and Vibration.
1894	6	Light—with special Reference to the Optical Discoveries of Newton.
1895	6	Waves and Vibrations.
1896	6	Light.
1897	6	Electricity and Electrical Vibration.
1898	3	Natural Philosophy.
1899	7	The Mechanical Properties of Bodies.

[1] I.e. to have regard solely to efficiency.

Year.	No. of Lectures.	Subject.
1900	6	Polarized Light.
1901	6	Sound and Vibration.
1902	6	Electrical Developments.
1903	6	Light—Its Origin and Nature.
1904	6	Life and Work of Stokes.
1905	3	Controverted Questions of Optics.

Many of the courses are reported in "Engineering." But there is no other record.

The first of these courses (1878) was given long before the question of Rayleigh's appointment as Professor at the Royal Institution had arisen. The second was I believe given when the appointment was under consideration; or at all events when Tyndall's health made it impossible for him to give his usual course.

The lectures in 1892 followed somewhat closely the lines of those which had been given by Thomas Young in the same place nearly a century before, at the birth of the Royal Institution. These were fully written out in Young's *Natural Philosophy*, published in 1807, and many of the identical pieces of demonstration apparatus figured in that work, which had been preserved in the museum of the Institution, were brought out and used.

Rayleigh's habit in lecturing was to speak extempore from a few headings on a half-sheet of notepaper. Difficult experimental demonstrations do not always succeed at the first trial, and it was his rule not to allow a pause while the assistant was trying to make good, but to *keep talking the whole time.* There can be little doubt that he was right. If there is ultimate failure, it is only emphasized by long waiting before it has to be admitted. Failures were not frequent, but they are bound to occur sometimes.

In June, 1891, the Royal Institution celebrated the centenary of Faraday's birth, and commemorative lectures were given by Rayleigh on Faraday's electrical discoveries and by Dewar on the liquefaction of gases. The Prince of Wales (Edward VII) presided at Rayleigh's lecture. H.R.H. had

himself attended Faraday's lectures as a boy. The lecture was illustrated with a repetition of Faraday's experiments, with some of their modern developments. Among these were the lighting up of a small incandescent lamp attached to a coil of wire when the latter was brought over an alternate-current electro-magnet. A small piece of by-play is worth recalling. In order to let it clearly be seen that the effect occurred at a distance and without any kind of visible contact, the lecturer instinctively pushed back his shirt-cuff, to prevent it obstructing the view of what was being done. To what extent H.R.H. may have been interested in the experiments I do not know, but he seemed to find this movement suggestive, at all events Rayleigh noticed that it raised a smile !

Another experiment originally due to Elihu Thomson was the repulsion of a light aluminium ring from the alternate-current magnet. When the current was switched on the ring was thrown up 2 feet into the air, and the lecturer deftly caught it. He was a good deal chaffed afterwards in the family about the amount of practice that this feat must have required !

The Royal Institution celebrated its centenary in June, 1899, and Rayleigh gave a commemorative lecture on that occasion also, the Prince of Wales again presiding. This time he dealt chiefly with the work of Thomas Young, one of the first Professors of Natural Philosophy in the institution. The subject was a congenial one. Rayleigh had studied Young's *Lectures on Natural Philosophy* (1807) already mentioned and had found them a mine of interesting matter. The pencil marks in his copy show how closely he had gone over the book, and in the lecture he brought to notice some of the good but forgotten work which he had found there and also in other writings of the same author. One of the most striking points was Young's estimate of the size of molecules, which depends on a comparison between the cohesion of liquids and the tension of the surface.

Young estimated the cohesion of water at 23,000 atmospheres, or 345,000 lb. to the square inch. That is to say, he concluded that a direct pull of 345,000 lb. would be necessary

to break a column of water 1 square inch in cross section. It is very difficult to arrange to break a column of water by a direct pull in the way contemplated. Any development of bubbles violates the conditions. However, in modern experiments considerable tensions have been sustained by liquid columns. It is not certain how Young made his estimate. In this as in many cases he was so brief as to leave his readers unsatisfied, but he may have assumed that liquids were as strong as solids. At all events, this was his estimate, and it agrees substantially with modern ones.

Tearing a liquid column in half in this way creates two surfaces. These surfaces have a tension, and Young showed that the range of molecular forces could be found by comparing the surface tension with the tensile strength. He made it to be $\frac{1}{2\,000,000}$ of an inch. Further, he argued that water vapour would condense to liquid when the particles or molecules, as we should now call them, came within this distance of one another. The proportional contraction which resulted when condensation occurred, and the particles came into contact, brought their distance into comparison with the dimensions of the particles themselves, and in this way Young was enabled to estimate the molecular diameter as " between the two-thousand and the ten-thousand millionth of an inch." This is a wonderful anticipation of modern knowledge, and antedates by more than fifty years the similar estimates of molecular dimensions made by Kelvin. Until Rayleigh drew attention to it, it had fallen completely out of notice, even if we suppose that it had ever attracted notice, and of this there is no evidence.

Rayleigh's Royal Institution lectures were illustrated with many original demonstrations. One of the subjects which he developed largely for this purpose was the demonstration of interference and diffraction of sound waves.

As has been recognized from the time of Young and Fresnel, the essential distinction is one of *scale*. The scale of apparatus used for experiments on interference and diffraction in optics are in general an enormous multiple of the wave length, and

as the wave length of ordinary sounds is about 4 feet, this would make the experiment impracticable, at any rate without huge structures built in the open air, and high up from the ground.

Rayleigh had therefore studied how to make sounds of the shortest possible wave lengths. This he did by means of a "bird call," a kind of whistle of special construction. Waves only a quarter of an inch in length could thus be produced, and he was able to illustrate interference and diffraction of sound by means of apparatus not too big to be used in the lecture room. Such high notes are inaudible, at least to adults, and it may be considered a stretch of language to speak of them as "sounds." But the high-pressure sensitive flame originally used by Tyndall responds very well to them, and allows the places of loud sound to be shown to an audience by its flaring.

By this method he was able to show the nodes and loops of stationary vibrations, formed by reflexion against a wall, the action of a zone plate in concentrating or focussing a sound, the formation of interference bands analogous to the optical bands of Fresnel, and the bright or rather the *loud* spot at the centre of the shadow of a circular disc.

The latter was shown by using a glass screen, with a circular aperture large enough to admit the first two Fresnel's zones. On blocking off the central zone by a metal disc considerably smaller than the aperture the flame flared, showing greatly increased loudness. This was shown at the Centenary lecture, and on leaving the room a lady in the crowd was heard to remark, "I have always noticed that you feel more draught from a window when it is nearly shut!"

As Professor, Rayleigh was called upon to give one of the Friday evening lectures each year, and each of these was devoted to some part of his own original work. He gave twenty-three altogether, and the abstracts of nearly all are reprinted in his *Scientific Papers*.

He resigned the Professorship in 1905, partly from the feeling that adequate use was not being made of the physical laboratory of the Institution. He was elected Honorary

Professor, and gave further Friday evening lectures in 1910 and 1914.

During his tenure of the Professorship he frequently went in to hear the afternoon lectures given by others; and both then and later he regularly attended the Friday Evenings while he was in London.

CHAPTER XIII

FRIENDSHIP WITH LORD KELVIN

Rayleigh's friendship with Lord Kelvin [1] has already been noticed, but it formed so important an element in his scientific as well as in his private life as to deserve special and separate treatment.

It was during the tenure of the Cambridge Chair, and afterwards, that the friendship fully ripened. Kelvin was frequently at Cambridge; and the annual British Association meetings provided another common ground. Afterwards, he frequently visited at Terling. "I have always regarded Kelvin as the foundation of a scientific party," Rayleigh said; and certainly he was the life and soul of such gatherings, with his infectious and boyish enthusiasm about any scientific novelties of an experimental kind. When anything new was on view in the laboratory, his excitement was unbounded. "Look, look! The most wonderful thing in the world!" he would say. "There!" "There!" "There!" at each repetition of the *dénouement* of any experiment. Rayleigh noticed that he always treated Gordon, the assistant, like an old friend, warmly shaking him by the hand.

He was equally eager in discussion of theoretical views, but in his mature years at any rate he was by no means so laudatory about the theories of other workers as he was about their experimental results. Indeed, to those who did not realize the tremendous record of achievement that stood to his name, his way of discussing new views might well have seemed not a little perverse. As Rayleigh often said, "He

[1] I call him so throughout this chapter, though his peerage was not conferred till 1892.

is a most interesting personality, not only for his powers, but also for his limitations."

I remember well one visit, probably about 1895, when the now generally accepted theory of electrolytic dissociation, connected with the names of Van't Hoff and Arrhenius, was under discussion. Lord Kelvin had learnt something of it in conversation with friends, and was full of indignation against it. At the same time he showed some desire to learn more of the accursed thing, and a small text-book was produced from Rayleigh's shelves in which the theory was expounded. On this occasion he showed more disposition to read than usual, but in a page or two he came on a thermodynamic argument which, if not incorrect, was certainly inconclusive. It was argued that the work done against osmotic pressure in expelling pure water through a semi-permeable pot containing a dilute salt solution was to be measured by the heat liberated: the weak point being that the solution was at a different concentration at the end of the process, and that therefore its internal energy might be changed. I remember meeting Lord Kelvin in the conservatory as he was leaving the book-room, and I was going to it. He waved the book in triumph as he crucified the fallacy.

"H is *not* equal to *pdv*," he said triumphantly, repeating the words several times with emphatic relish. "It is Meyer's old mistake of 1842, and here we have it over again in 1895!"

However, his indignation abated somewhat as he read further. "He will think before long that he discovered it himself," Rayleigh remarked, after the visit was over.

He was equally antagonistic at first to the new views of gaseous conduction which were being developed by Schuster, J. J. Thomson, and Rutherford. Rayleigh said something about the carriers [of the electric charge], a term which had been used by one or other of the writers named. "Why do you call them carriers?" said Lord Kelvin indignantly. "I thought it was intended to be a non-committal term," replied Rayleigh mildly.

Rayleigh was much interested in what is known and

accepted as the " displacement law " for black-body radiation, developed by W. Wien. In this derivation, the conception of work done by a piston moving against radiation pressure is introduced. I remember that Lord Kelvin stigmatized it as " Thermodynamics gone mad ! "

Lady Kelvin always accompanied him on these visits, and it was touching to see the way he depended on her, and how eager he was that she should enter into all his scientific enthusiasms. No one could doubt her devotion. " But I doubt if she listens to him very much," Rayleigh remarked.

Lord Kelvin too was full of friendly interest in my brothers and myself as children. My first definite recollection of him was when we went with him at Cambridge to see the electric light installation which he had presented to Peterhouse. He gave me the charge of his watch while he went to examine the dynamo. It was a very necessary precaution with the dynamos of those days, which were not of the modern enclosed type and which produced much more " stray " magnetic field. A little later, he brought on a visit to Terling as a present for me a fascinating electrostatic toy. Pith figures were contained in a shallow box with a glass lid, and when the lid was electrified by rubbing with a pad, they executed the most violent evolutions.

Later again, we had an underground abode in the grounds which we had dug and roofed over ; Lord Kelvin paid a visit of ceremony there, and left his card, at the entrance !

When my brother was a midshipman, Lord Kelvin used to cross-examine him closely about all kinds of nautical matters, in which he took the keenest interest.

Sometimes Lord Kelvin would talk of his early days. I remember in some detail one such conversation. It was after dinner—the ladies had left the dining-room, and only Lord Kelvin with my father and myself were present. The conversation turned, I forget how, on Challis, who had been Plumian Professor of Astronomy at Cambridge, and who is most often mentioned in connection with his unsuccessful search for the planet Neptune.

Rayleigh : He had a curious knack of getting hold of the wrong end of the stick. Do you remember what he called his "law" in hydrodynamics ?

Kelvin : Yes. He set it when he examined me for the Smith's prize. I could not see that it was right, and I took it round to Stokes in the evening, but he knew about it before.

Rayleigh : Stokes knew it wasn't right ?

Kelvin : Yes. Parkinson [1] did it though.

Rayleigh : Did it according to Challis, you mean ?

Kelvin : Yes.

Self : And I suppose scored many marks by it ?

Kelvin : I have no doubt he did.

I remarked once to my father on Lord Kelvin's habit of saying "What ?" under conditions when he seemed to be attending closely and one would have supposed that he could hear sufficiently well. My father thought that the explanation was that he was not content, like most people, to fill up conjecturally those words of a sentence which he could not distinctly hear. He must hear *every* word before he would be satisfied.

Rayleigh and Kelvin had a very wide range of scientific interest in common, in fact, it would hardly be an exaggeration to say that the whole field of their respective interests in pure science coincided, though Rayleigh, unlike Kelvin, never entered the field of applied science as an inventor, or as an expert witness. Electrical measurement, optics, capillarity, hydrodynamics, and kinetic theory of gases, all these were subjects of constant debate between them. One subject which they had *not* in common was an interest in "spiritualistic" phenomena. On this subject Lord Kelvin could not express himself with patience. "That wretched miserable superstition of spirit-rapping," he would say. Rayleigh naturally kept off the subject, in which, as already shown, he was a good deal interested, though unconvinced. He always thought that there must have been some personal reason for Kelvin's strong feeling, and that perhaps someone

[1] His successful rival in the Senior Wranglership.

whose welfare was dear to him had come to grief over spiritualism. But he knew nothing definite.

Rayleigh to Kelvin, Terling, January 2nd, 1888:—

"It is pleasant to see that you spoke so warmly of Arthur [Balfour]. I believe indeed that the country is very fortunate in having him at their disposal just now."

Rayleigh to Prof. A. Schuster. From Netherhall, Largs, Ayrshire, October 4th, 1888:—

"Sir W. [Thomson] is full of a *froth* theory of the ether! This will lend itself to sarcasm even better than the jelly theory."

Lord Kelvin's scientific discussions or arguments with my father were often on abstruse questions, and I cannot now attempt to reproduce any of them from recollection. But it was as good as a play to hear them. "I cannot see the shadow of an argument in that," Lord Kelvin would say. "Well," Rayleigh would reply, "I regard it as rigorously proved: and I think you will be convinced if you will only read it as I have set it out here in half a page of print." But it was not easy to get him to do this. He would take it up, but the first line or two would send him off on some train of thought of his own, and his eye would wander from the printed page.

I have fortunately been able to find a verbatim report of a discussion between them in a blue book,[1] which, though not on a subject of much intrinsic interest, will give an idea of the style and manner of each speaker.

It is necessary to preface it with a short explanation of the subject discussed. The value of a steady electric current is defined for practical purposes, as we have already seen, by the amount of silver deposited in one second, and if the strength of the current varies with the time, its effective value for such purposes as electro-plating, at any rate, may still be defined by the amount of silver deposited over a given interval of time. When, however, we come to deal with alter-

[1] Minutes of Proceedings of the Board of Trade Committee on Electrical Standards, 1891.

nating currents, it is obvious that this method of measuring currents must be abandoned. What is done in one phase of the current is undone in the next, and the result is that no silver is deposited at all. We must, for measuring alternating currents used in electric lighting, adopt some other definition, not consistent with the one already given. In other words we must attach some different meaning to the word ampere, and cases may arise when this causes ambiguity as to what is meant when we say that a current has the value of so many amperes. The definition appropriate to alternate currents for heating or lighting purposes is to take the square root of the average square of the current at individual moments. This is more usually distinguished as the R.M.S. (root mean square) current, where any ambiguity can arise, and the distinction has not been found a serious stumbling-block in the development of electrical technology. But there is a type of "practical man" who demands that definitions should be short and simple, even if they mean nothing particular, and some anxiety was not unnaturally felt as to how this sort of person would regard the definition proposed.

From Report of Electrical Standards Committee of the Board of Trade (1891), p. 28.

Chairman [Mr. Courtenay Boyle of the Board of Trade]: This is what I took down—an alternating current is one ampere if the square root of the time average of the square of its strength at each instant in amperes is unity.

Lord Rayleigh: Is it correct to say "it *is* one ampere"?

Sir William Thomson: It is defined as one ampere; that is the definition.

Lord Rayleigh: Can you say "is"? "Is" must refer to a moment of time.

Sir William Thomson: An alternating current is one ampere.

Lord Rayleigh: I suggest "may be treated" or "may be considered" one ampere. That is what I am driving at.

Sir William Thomson: "Is."

Lord Rayleigh : Is it ?

Sir William Thomson : That is the proper average.

Major Cardew : It is not absolutely the same thing as what we have defined to be an ampere before.

Lord Rayleigh : You cannot say that one particular man is 5 feet 8 inches because the average height of men is 5 feet 8 inches.

Sir William Thomson : It is the ampere for an alternating current for all legal purposes.

Lord Rayleigh : Is it consistent with the other definition ?

Sir William Thomson : Perfectly ; it is the proper average. In reckoning the alternating current you are to use the proper average current, and the proper average is as stated here. It is continually used in the trade, and I do not think it is at all desirable that that usage of the trade should be altered.

Lord Rayleigh : I was not proposing that ; I was wondering whether it would not be more accurate to say " may be treated as " or something like that, instead of " is."

Sir William Thomson : " May be treated as " is an expression for approximate measurement.

Major Cardew : May be taken as.

Sir William Thomson : Shall be.

However, after some further discussion Sir William practically yielded the point.

Later on :—

The Chairman : Now, with great deference I ask whether you can put in an Order in Council the square root of the time average.

Sir William Thomson : You cannot put in anything else, sir, it is correct. " Average current " would mean nothing ; literally it means zero in the case of the alternating current.

And later again :—

Lord Rayleigh : We are only telling the practical people what they have meant all the time.

Sir William Thomson : And now they do know it ; it is in the electric journals constantly.

The Chairman : I am bound to tell the Committee that I

did speak to Sir Michael Hicks Beach,[1] about this particular definition, and that it certainly frightened him a great deal.

Sir William Thomson : A page on the theory of averages would frighten him still more.

The Chairman : Sir Michael had consulted a gentleman of some practical knowledge of electricity on that point, but if you tell me that the square root of the time average is a phrase which is understandable——

Sir William Thomson : There is no other phrase which will express it correctly. I dare say Sir Michael Hicks Beach's friend said, " Oh, it is the average current," but the average current in the sense of Board of Trade averages is zero for the alternating current.

Lord Kelvin was one of Rayleigh's most constant correspondents. His letters and post cards, which were all carefully preserved, are very characteristic. They range over the whole period of their acquaintance from 1872 up to Lord Kelvin's death in 1907. Many of them are reproduced in the second volume of S. P. Thompson's *Life of Lord Kelvin*, and a few of these with others have been reprinted in the earlier parts of the present book. Unfortunately the other side of the correspondence was only partly preserved.

Lord Kelvin's letters, which were often written off in a red-hot fit of enthusiasm about some new idea, ranged from abstruse mathematical analysis to the strongest denunciations of Mr. Gladstone's home rule policy, to which, as a Belfast man, he was furiously opposed.

I reproduce here some further letters in full, and extracts from others :—

March 30th, 1890.

I am delighted with your $1{\cdot}6 \times 10^{-7}$ film of oil on water.

Think next of the calming effect of oil on water. Osborne Reynolds published something on that and gave us a lecture on the seaman's orphanage night on board the *Servia* on our way to the B.A. in '84 ; but I am not sure if it was very clear or satisfactory. Perhaps it was : I daresay you know.

[1] President of the Board of Trade.

Try ripples made by a tuning fork to measure the true surface tension of a large area of water. Michie Smith has been doing this in Tait's laboratory, for mercury with good results : Capillary tension found very close to Quincke's . . .

NETHERHALL, LARGS, AYRSHIRE,
Oct. 16*th*/92.

DEAR RAYLEIGH,—

The appointment 10.30 a.m. Oct. 27 for Board of Trade Committee on Electric Measurements holds. Cardew writes me that he will have a reminding circular sent. The *Anti-mercatorizing* (i.e. the reduction to drawing geodetic lines) of the problem of a pair of masses connected by a spring, moving in the line joining them between two soft (?) planes is curious. It may possibly help towards the question of distribution of Kinetic energy between that of their centre of gravity, and of their relative motion.

We were greatly delighted with the victory by 3 of yesterday. What do you think of Mr. Morley's beginning ? The dullest of apprehensions are beginning to learn, but the most shocking part of the thing is that such object lessons are required to teach them. What *does* Mr. Gladstone deserve ? The 22 cows with the tails cut off, the murder, and the effects of the assault on Inspector Lily are a first instalment of the grand sacrifice to his vanity entailed by the resolution of electors to give the old man another chance.

I hope Mr. Balfour is getting a good rest, and getting it *fast*, because it seems less likely to be long than it did six weeks ago.

Yours,
KELVIN.

THE UNIVERSITY, GLASGOW,
March 24*th*/94.

DEAR RAYLEIGH,—

That is a splendid idea of yours to explain the approximately periodic character of the twinning in the iridescent chlorate. I am going to introduce it into my Oxford Boyle Lecture (which now, under influence of minatory letters from successive Secretaries of the Oxford Junior scientific club, I am preparing for press) unless you say no, which I hope you won't.

You should experiment with different depths of bath, and try if the greater depth gives more approach to monochromatic iridescence : and to find conditions on which frequency of occurrence of iridescent crystals depends. . . .

Yours,
KELVIN.

NETHERHALL, LARGS, AYRSHIRE,
Easter Sunday, 1894.

DEAR RAYLEIGH,—

Have you found any rule or tendency to rule as to whereabouts the iridescent stratum is in a chlorate crystal ? If your explanation is correct, would it not be generally on the underside of a plate as it lies in the mother liquor ? A flake falling would probably not become united on its lower face to the bottom on which it falls, and it would grow from its upper face upwards.

You should try the effect of artificial horizontal oscillating motion of the liquid. This might be done by giving an oscillation (by a little motor, or by a weight wound up) to the basin containing the liquor. Or you might do it (less easily) by aid of a plunger.

The more I think of it the more probable your explanation seems. You may, if it is true, learn to make very regular iridescent strata of any thickness at pleasure, for any wave length you choose.

Yours truly,
KELVIN.

TRAIN TO GLASGOW. [I return to Largs this Evening.]
Oct. 7th.

DEAR LORD RAYLEIGH,—

I have been trying to get a continuation [circuital geodetics] of Periodic Motion written for the Nov. *Phil. Mag.* or I should have answered you sooner re Boltzmann-Maxwell; but I find I can scarcely manage it. Look at the case I gave you with the tube and you will find it gives a perfect dynamical "System" which does not fulfil Maxwell's "conclusion" and which has no exceptional character according to which it could fail to fulfil his "probable fundamental assumption." You will I think find the same in respect of my Madeira post-card test case.

All the test cases in which I find a decided violation of the conclusion are cases in which the time under the influence of force is comparable with the time of free (or [in a system] time of adynamic motion). In his highly worked out paper on the Kinetic theory of gases Maxwell expressly assumes that the time of each molecule being under influence of another is infinitely small in comparison with the time of its free motion. Except for this case there is absolutely no proof in anything I have ever seen of his or others to show that, or to make it probable that, in any large class of cases, the alleged equality of Kinetic energies is true.

I tried lately a question (I don't call it a test case, because it is incomplete) analogous to yours of last May. It was this :

⊕ [Arrival Glasgow; relapse into circuital geodetics; discovery of a new theorem in plain kinetic Trigonometry, being extension of Gauss' *curvatura-integra* and the old "Spherical excess"; writing this last night to Dr. Francis; have caused delay of this till now. Oct. 8th again in train for Glasgow.]

⊕ [Continued] Let there be i massive vibrators given, each vibrating through $\pm$ unit range, all in same phase; mass of each $1 - m$, frequency $2\pi/\omega$. Let i particles, each of mass m, start all at the same time at equal distances from the middle positions 0_1, 0_2, etc. of the vibrators: each with velocity q, towards 0_1, 0_2, etc., respectively and in the lines of the vibrations. Let q and m be each small fractions of unity. I find, and if I am right, as I think I am, you will find (q_1', q_2' etc. being velocities of recoil)—

$$\Sigma\tfrac{1}{2}q^{12} - \Sigma\tfrac{1}{2}q^2 * \doteqdot i \times 2(1 - m)\{- mq^2 + \tfrac{1}{2}\omega^2(2 - 3m)\}$$

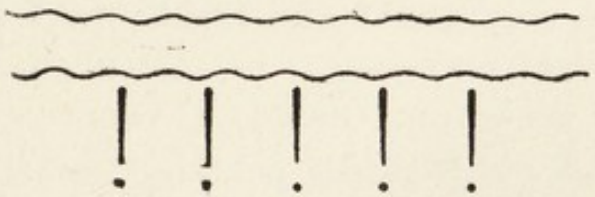
Hence, in order that the recoiling corpuscles may have the same sum of energies that they had before the impacts, we must have

$$\tfrac{1}{2}mq^2 = \omega^2\left(1 - \frac{3}{2}m\right)$$

or

$$\tfrac{1}{2}mq^2 \doteqdot \omega^2(1 - m).$$

Hence the kinetic energy of all the impingers must be a little less than but approximately equal to the sum of the kinetic and potential



energies of the vibrators, not to add to or take from the total energy of the vibrators.

Finished in a cab, but I flatter myself not less easily read than your letters written to me on terra firma, and in ink (which are *not* easily deciphered always).

Yours,
W. Thomson.

This letter gives a sample of one side of the prolonged debate between Lord Kelvin and Rayleigh on the Boltzmann-Maxwell doctrine, or as it is now more often called, the law of equipartition. The subject is too abstruse and difficult for a book like the present, and the reader who wishes to gain some idea of the controversy would do well to read Lord Kelvin's

* $\Sigma\tfrac{1}{2}q^2 = i \times \tfrac{1}{2}q^2$.

lecture, "Nineteenth-century Clouds over the Dynamical Theory of Heat and Light."[1]

Rayleigh was inclined to consider that Maxwell's proof of the law was valid, or at all events that it had not been successfully challenged up to that time. Lord Kelvin, on the other hand, "had never seen validity" in Maxwell's demonstration. But he was anxious to go further, and to disprove it by a "decisive test case." The above letter gives one of his attempts in this direction, and there were others.

One of them was published in the *Philosophical Magazine* (1892) as a "Decisive test case disproving the Maxwell-Boltzmann Doctrine regarding distribution of Kinetic Energy." But some years later Rayleigh published a refutation of this refutation. Kelvin admitted that this particular test had failed to convict: but, so far as I know, he rejected the doctrine itself to the end of his life.

This was an example of one of Rayleigh's strongest points —not to be taken in by a bad argument. He recognized this himself, and he even said to me once that so far as he could see, it was the only quality he had! but I think that this was in a mood of paradox.

Lord Kelvin wrote from Glasgow (November 27th, 1896):—

"Has not Osborne Reynolds published something about the mitigating effect of oil on waves? . . . The result is I think, without doubt, due to doing away with the tendency to the formation of ripples which are produced when wind blows over the surface of non-oily water, but I do not quite see how to connect this with the diminished capillary surface tension produced by oil. And I do not think it can be explained by any kind of surface film viscosity of the oil. *You* know the true reason I believe, so please tell me or give me a reference so that I may answer Stokes."

Undated, Dec. 1893 ?

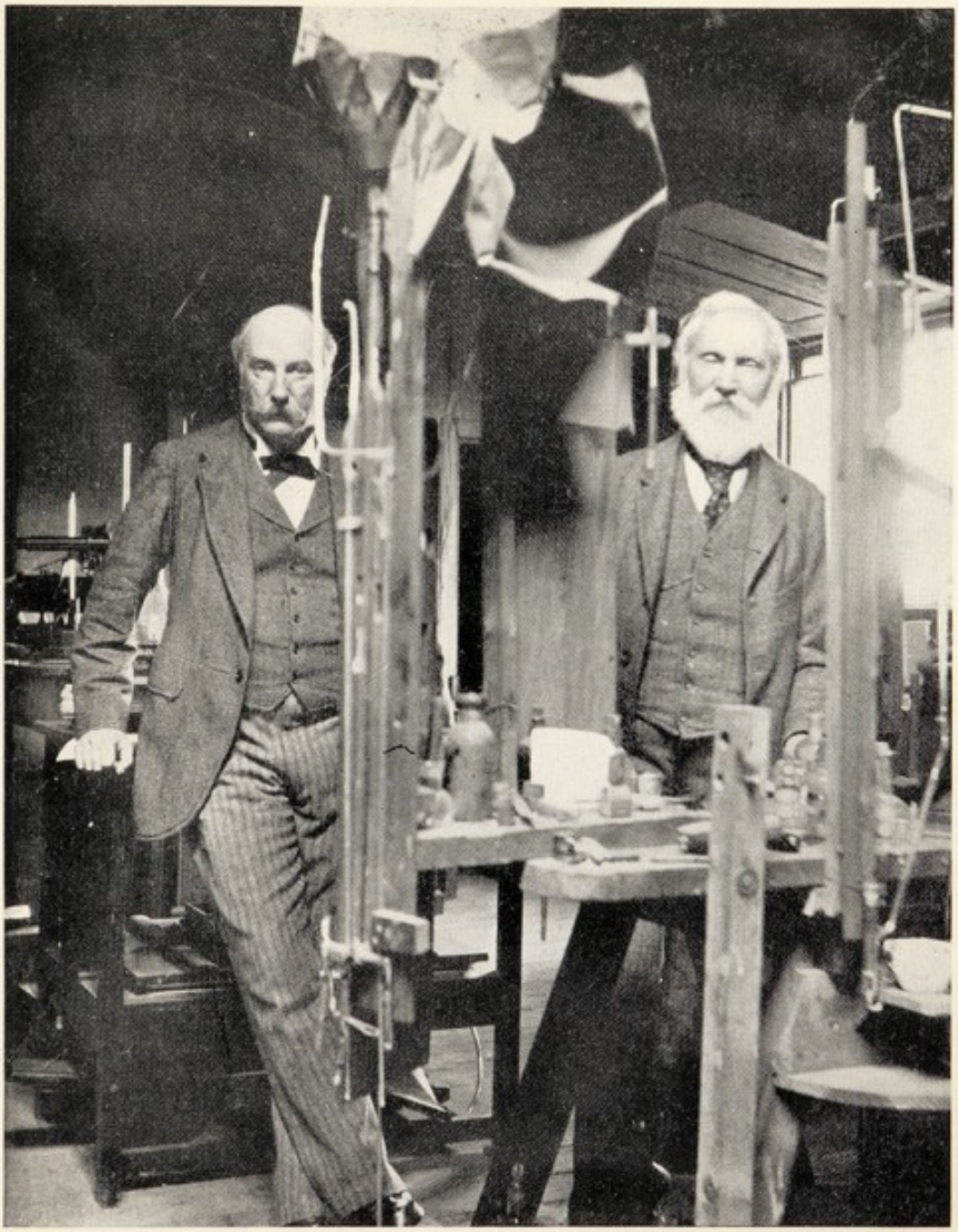
NETHERHALL, LARGS, AYRSHIRE.

DEAR RAYLEIGH,—

Is the 18th of Jan. wholly preoccupied or would there be time for a dose of homogeneous division of space illustrated by Models ?

[1] Royal Institution *Proceedings*, April 27th, 1900, or *Baltimore Lectures*. Appendix B.

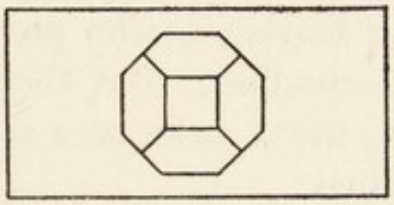
[2] At the weekly meeting of the Royal Society.



LORD KELVIN AND LORD RAYLEIGH IN THE LABORATORY AT TERLING, JULY, 1900.

From a photograph by Prof. A. G. Webster.

So many things have come on since the killing of the Home Rule bill and Aix-les-Bains, ending with my abstract of Isoperimetrical problems which I despatched on Friday night, and three patents the provisional specification of the last of which I got off yesterday morning, that it was not till coming here in the train yesterday afternoon that I got through the last of my difficulties; and Lady Kelvin commenced in the evening making a tetrakaidekahedronal pin-cushion which will make all clear. White will make a larger affair with 24 pin-cushions at the 24 corners of a skeleton tetrakaidek^{n} of six squares with their corners connected pro



perly (36 stiff little iron rods each 10 cm. long, and their ends strongly soldered together by threes) and 24 glass-headed hair or bonnet pins for explaining to the R.S. If Jan. 18th is already full, the 22nd of Feb. will do as well, and I shall have the paint on a Model to illustrate a communic^{n} 'On the Molecular structure of Quartz' dry by that time.

In any case however I hope to have the Homogeneous Division of space written out (and in type if wanted) before the 18th of Jan. I have been at it so long on and off, since the Home Rule Bill came on in 1886, and so much *on* since last May that I expect to be able to make the paper very short.

Lady Kelvin joins in Christmas greetings to Lady Rayleigh and Willy and the Captain R.N. and the Etonian.

Yours,
KELVIN.

Do you know any place in England where four parishes meet?

Lord Kelvin wrote, April 2nd, 1899:—

"I am glad you find molecules of N or O sufficient for *blue* sky. It seems impossible that spherules of water could be uniform in all climates and places and seasons to explain what we [see]. The whiteness of all grades is easily enough by fineness and quantity of mist."

FLEMING'S HOTEL, *Jan. 25th*, 1905.

DEAR RAYLEIGH,—

It is quite certain that whatever be the laws of force during collisions or complexity of the molecules, we must have viscosity $\doteqdot \varrho D$ and thermal conductivity $\doteqdot JK\varrho D$ when the free path is

large in comparison with distance between centres of colliding molecules. Will you come to the House of Lords on Tuesday at 4, when I am to be one of the supporters for the introduction of Lord Avebury. This will give us an opportunity of settling precisely the meaning of $\doteqdot$ in the preceding, and some other important questions; also of getting you to read me your letter of the 23rd.

Yours,
K.

I believe that the suggestion of conferring a peerage on Sir William Thomson originated with Rayleigh, who pressed it on Lord Salisbury's not unwilling attention. But there is no means now of verifying this belief, which rests on a recollection of what I heard said at the time.

At all events he, with Lord Sandford, officiated in introducing him when he took his seat in the House of Lords, according to the ancient forms.

The only other new peer for whom Rayleigh did this was Lord Fisher of Kilverstone, whom he only knew slightly, but who always expressed admiration for him.

The last time that he saw Lord Kelvin was during a visit to Sir Hugh and Lady Alice Shaw-Stewart at Ardgowan, when Lord Kelvin came over to tea. This was on September 25th, 1907. About three months later he was acting as one of the pall-bearers at his friend's funeral in Westminster Abbey.

It fell to Rayleigh, as President of the Royal Society, to say something at the Anniversary Meeting in 1908 in memory of his friend. After a reference to Lord Kelvin's scientific work he said :—

"My acquaintance with Kelvin was limited, until about 1880, a time when I was occupied with measurements, relating to the Electrical Units, and received much appreciated encouragement. From thence onwards until his death I enjoyed the privilege of intimacy, and, needless to say, profited continually from his conversation, as I had done before from his writings. Our discussions did not always end in agreement, and I remember his admitting that a certain amount of

opposition was good for him. Such discussions often invaded the Officers' Meetings [at the Royal Society] during the time that we were colleagues, not always to the furtherance of the Society's business. But I must not linger over these reminiscences, interesting as they are to me. We shall never see his like."

CHAPTER XIV

DOMESTIC AND SOCIAL LIFE AT TERLING

The day at Terling was begun with family prayers, at about nine o'clock. This was attended by the family and household, and absences (usually due to late rising) were frowned upon. "Prayers is on the table" was a form Rayleigh often used when he was in good spirits. A short interval while breakfast was being brought in was passed in the library and was devoted to opening letters. Some of these from foreign or miscellaneous correspondents would be addressed in the most extraordinary manner. "Lord O. M. Rayleigh" lingers in my memory, and presumably has reference to his being a member of the Order of Merit.

If the family were alone the reading of correspondence would continue intermittently during breakfast, and he was usually rather silent, even apart from this. His children, and later his grandchildren, would be playing about in the room, and he would romp and joke with them at intervals. This he thoroughly enjoyed doing up to the end of his life.

After breakfast, he went to the book-room and began by answering letters. At ten o'clock Gordon arrived (he lived only a few hundred yards away) and passed through the book-room on the way to the laboratory. A few words would pass as to what work he was to go on with.

Rayleigh then usually spent the rest of the morning in reading and writing work. He began by writing any necessary letters for the post which went out at midday, and these he contrived to keep within moderate limits. As a correspondent, he was not negligent: but on the other hand he was not always prompt, taking a week or more to consider

his answer if there was any difficulty about framing it. He never employed a secretary at any time in his life.

If a number of the *Philosophical Magazine* or the *Annalen der Physik* had come by post, half an hour would be spent in his arm-chair on looking through it, before it was put on the shelf. He would then sit at his writing table, and work for the greater part of the morning at the numerical reductions of experiments, or at mathematical analysis, or in writing out an investigation for publication. He was able to do work of this kind for long hours without getting tired or stale, and continued able to do so to the end. At the same time he would sometimes return to the arm-chair to read something bearing on the work, or to meditate—often with the help of paper and pencil. On a sunny day he would pace up and down the conservatory, built by his father, which united the book-room with the body of the house; sometimes he would pace up and down the garden path outside.

As a rule not much laboratory work was done in the mornings, unless something was on hand which required the sun. In this respect he was less independent than experimenters who have an electric arc conveniently at their disposal. During the course of the experiments on reflection in the neighbourhood of the polarizing angle he had often to await the sun, and would rise frequently to look at the sky through a dark glass to see what the prospect of any continuous sunshine amounted to.

He would usually look in once or twice in the morning to see what progress his assistant was making with preparatory work, and to give the necessary directions for it.

Half an hour before lunch was sometimes devoted to a lesson in mathematics to one of his children or grandchildren. These lessons were not perhaps always as highly appreciated at the time as they should have been. They were very much the antithesis of "cram." Emphasis was laid on the fundamental and essential points, and the pupil was led forward by questions directed to guide him in finding his own way as far as his capacities would allow. At the same time he

was given glimpses of higher things. It is possible that a more didactic system might have been better: but after all these lessons were rather intended to supplement school-teaching than to replace it: and (in the case of his grandson) to give him some meeting-ground with the younger generation.

Luncheon followed, and the afternoon's plans would be settled. In his prime, he usually went for a walk by himself, but occasionally, if the weather was pleasant, a drive with Lady Rayleigh in the pony-carriage was arranged. In this case, Rayleigh could rarely be induced to make any constructive suggestion as to when the carriage should be ordered.

"Well, I don't know: what do you think?" would usually be the reply. Then, if some concrete suggestion was made, he would accept it or propose some modification. The same trait appeared if any attempt was made to induce him to give some estimate of "how many?" or "how much?" Those who knew him well and wished to elicit his opinion would introduce some tentative estimate of their own into the question, and there would then be some chance of getting it confirmed or modified in the right direction.

After luncheon, a preliminary study of the newspaper was made in the Library. When the custom of coffee after luncheon came in, he usually made a protest against the encroachment of luxury before helping himself!

The afternoon walk was often through some of the nearer farms on the estate, which, as elsewhere mentioned, were farmed by his brother Edward in partnership with himself. The pigs and their habits were, I think, a special object of interest.

In the summer, lawn tennis was sometimes substituted for a walk. This did not continue much beyond the age of fifty. Up till that time he played a fairly good game. He was more willing to play on Saturdays than on other days, when he thought that it made a demand on energies that ought to be devoted to his work.

On coming in he worked for an hour or so in the laboratory,

and then had tea. He often remarked that he found a cup of tea an effective stimulant. A difficulty in his work would often resolve itself almost immediately when he had had one. After tea, more particularly in the winter, he would devote himself to amusing his children or grandchildren. He would show them the pictures in *Punch* or (in my own case at least) the steam cranes and steam hammers illustrated in *Engineering*, a paper which he always took in. Sometimes there would be games with cushions and hassocks, and a peculiar diversion which he had christened "Seeing London." The child, standing with the legs separated, bent down and put the arms between the legs. It was then seized by the hands, which were pulled up so that a complete somersault was executed. Children generally were delighted with this, and on one occasion some embarrassment was caused when a child staying in the house eagerly demanded that the governess should have the same enjoyable experience!

Rayleigh was not at all a stern disciplinarian with children, but any definite disobedience was promptly and drastically dealt with. These occasions, however, were very few. I believe that they cost him a good deal. "I hate thrashing a child," he said later. "It makes me feel ill all the rest of the day." But we never suspected this at the time.

One point on which he insisted very strongly with us boys was the necessity of care with firearms. He never lost an opportunity of insisting on this, though it was done with good humour. On one occasion I was walking with him in the garden. My youngest brother, aged about seven years, jumped out from behind a bush, covered us with the dilapidated remains of a toy gun, and called to us to stand and deliver.

Rayleigh: You should never point a gun at anyone, particularly if it is not loaded.

Self: And even if it has no barrel?

Rayleigh: Well, that does diminish the danger somewhat, no doubt.

However, to return from this digression to the daily routine; at six o'clock, when Gordon returned from tea, Rayleigh

returned to the laboratory, and worked on till about a quarter to eight. He then put on a black coat for dinner, but did not wear dress clothes unless there were visitors.

After dinner, he resumed the study of the *Times*, and various weeklies, such as the *Spectator*, *Punch*, *Nature*, *Engineering*, or the *Photographic News*, which latter he had taken in from boyhood. In middle life, he read few novels, and not many other books of general interest, unless he was away for a holiday.

He was glad to get a game of whist, or later, bridge, when three other players could be mustered.

When still a comparatively young man, he sometimes worked in the laboratory or at his writing table for an hour before going to bed. But afterwards this was discontinued. In later life he usually dozed in his arm-chair between eleven and twelve o'clock, and then woke up and went to bed—a curious habit which I, at least, have not met with in any-one else.

He never smoked, and disliked the smell of it in quite the old-fashioned way.

The domestic regime at Terling was punctual, though not pedantically so, and lunch and dinner were rarely more than five minutes late. The latter particularly was regarded in the old-fashioned way as somewhat of an observance, and if as school-boys we showed any tendency to "rag," we were reminded that "Dinner is a ceremony."

Work went on as usual on Saturday afternoons, but on Sundays, though he wrote letters before church in the morning, he seldom did any other regular work, though no doubt he pondered scientific problems. I never remember him to have missed morning church, unless considerations of health compelled it. He often went in the evening as well.

After dinner on Sundays Lady Rayleigh played on the organ, which was a fairly large instrument, situated at the end of the library, behind his arm-chair; if she made no move to do so on her own initiative, he did not fail to ask for it. He enjoyed music, though he did not care to spend time on cultivating his taste in that direction.

Mrs. Sidgwick's regular co-operation in experimenting had ceased from the time when Rayleigh left Cambridge in 1885: but her visits to Terling were not infrequent, and on these occasions she was always ready and anxious to give help in the laboratory. Sometimes, when Rayleigh knew she was coming, he would reserve a piece of work in which an extra hand could be of use, so that she still had the sense of co-operation.

She found it delightful to talk over his work with him. He never minded explaining and could put things very clearly. It was his habit to get for himself at the fundamental points, and to find the simplest form of the problem he was occupied with, both while looking for the solution and after he had found it. This made it useful and profitable to explain it to an intelligent sympathetic hearer. He often had long conversations of this kind with Mrs. Sidgwick when walking or driving with her.

There were frequent week-end parties at Terling in the summer, during the years from 1885 to 1914. Though a fair proportion of these were scientific, the majority had a social or political complexion. The place has a great advantage in being within easy reach of London, which made it possible for hard-worked politicians to come.

The guests usually expressed a wish to visit the laboratory, and Rayleigh was always glad to show it to them, or even to children who might be staying in the house. The visit would usually be on Sunday afternoon or after tea, and Rayleigh would show some of his more spectacular experiments with a few words of explanation, which were always simple. He never made the mistake on that or any other occasion of trying to say too much. He was not very quick himself in getting the bearings of a subject which was unfamiliar. "I can never understand when anybody tries to explain his apparatus to me," as he once said. There is no doubt that most people tend to forget that what is familiar to them may be quite unfamiliar to the person they are showing it to. In this respect he always did as he would be done by.

Some of the favourite experiments to show to visitors were the movements of camphor scrapings on clean water, and their arrest by the slightest trace of grease, as for instance from the fingers.

Then the special model of sensitive flame which he had designed, which would work on the ordinary gas supply without requiring a high pressure of gas from a special holder. It had a chamber through which the gas passed, provided with a thin diaphragm of tissue paper which was acted upon by the aerial vibrations. Such a flame would duck when exposed to a sound of high pitch such as a hiss, or the rattle of a bunch of keys.

Some explanation would be given of the more elaborate investigations, but the incidental comments sometimes showed how wide was the gulf between the world of affairs and the world of science. Then there were tests of colour-blindness (see p. 178) and experiments with Maxwell's colour discs.

There was often a Sunday afternoon walk for the more active members of the party, and Rayleigh would take them a round, the furthest point being Toppinghoe Hall, a ruin of Tudor date, or earlier, on the estate, part of which was utilized as farm buildings. We used to regard it as monastic, but a study of the county history fails to confirm this impression. The place was remarkable for a magnificent group of cedars, of which a part remains, and may be seen from the Great Eastern Railway, between Chelmsford and Hatfield Peverel.

This walk took the party through Terling Hall Farm, and they would frequently look in at the large cow-house there, and watch the milking, and the cooling of the milk by trickling over cold-water pipes.

Rayleigh was, I think, popular as a host, and if any of the visitors expected him to be formidable, or to talk over their heads, they seldom came away with the expectation confirmed.

Often current superstitions were laid before him for a scientific judgment, and they had to be foolish indeed before he could be provoked to anything like scorn.

When appealed to for his opinion, he always gave it modestly, and without cocksureness. "I think you may take it that, etc.," was perhaps about the strongest form in which it was ever cast, and he had to be very sure before he would say so much.

Like most Englishmen, he was fond of political discussion, and liked to draw out any of his guests who had interesting things to tell. But he was well content to be a listener, if his intervention was not needed to keep the conversational ball rolling.

After the ladies had gone to bed, some of the male guests usually went to the smoking-room. Rayleigh so disliked the smell of smoke, that he rarely accompanied them. He was, however, glad to talk to any who preferred staying with him in the library.

The Terling visitors' book was an institution. It contained more than a mere record of names; visitors who had artistic gifts were pressed into service to illustrate it. In particular it contains many specimens of Sir Philip Burne-Jones's handiwork, illustrating the numerous week-end parties at which he was present. The book only begins in 1886, and there is no record of earlier visitors. Here are some interesting signatures that it contains other than those of relatives. The date is in each case the earliest under which that signature appears.

1886. St. John Brodrick. G. H. Darwin. M. Foster. Francis Galton. Oliver Lodge. J. Norman Lockyer. Arnulf Mallock. William Thomson. Margot Tennant. Argyll. Herbert McLeod. Arthur Schuster.

1887. A. C. Benson. George Curzon. Edw. Cantuar. Horace Darwin. Edmund Gurney. Asa Grey. R. T. Glazebrook. William Huggins. Alfred Lyttleton. G. G. Stokes. Albert Grey. John Tyndall.

1888. Herschell. H. H. Asquith. Edward Burne-Jones. Humphry Ward. Mary A. Ward. Henry White.

1889. John Evans.

1890. J. Chamberlain. W. de W. Abney. W. R. W. Peel. C. V. Boys.

1891. Charles Bowen. H. G. J. Du Bois. William Crookes. Lytton. Neville G. Lyttleton.

1892. Albert A. Michelson. J. J. Thomson. George Wyndham.
1893. A. A. Common. Herman von Helmholtz. R. B. Haldane.
1894. E. Mascart. Alfred M. Mayer. Raphael Meldola. John Morley.
1895. W. T. Thiselton Dyer. L. J. Maxse. F. Paschen. C. Runge. R. B. Finlay.
1896. Balcarres.
1898. H. F. Newall. John Hay.
1899. W. Gatacre. A. Cornu. Felix Klein. W. C. Roberts-Austin. Joseph S. Ames. Walter H. Long. Peel. A. Sommerfeld.
1900. Augustine Birrell. Joseph H. Choate. Halsbury.
1901. Balfour of Burleigh. J. N. Langley. Alexander Scott. Goschen. H. J. Gladstone. Ian Hamilton.
1902. Theodore Lyman. Frederic W. H. Myers. Percy A. McMahon.
1903. S. H. Butcher. A. E. W. Mason. Simon Newcomb. Winston S. Churchill. Austen Chamberlain.
1904. Prof. H. Kayser. Prof. Dr. Otto Lummer. R. W. Wood. James Dewar. Donoughmore. Arthur H. Lee.
1905. Selby. Knut Angström. J. W. Brühl. J. A. Ewing.
1907. Edward W. Morley. Frank Dicksee.
1908. Milner. Cromer. Dunsany.
1909. John Buchan. George E. Hale. Charles Hardinge.
1910. E. F. Benson.
1912. Prince B. Galitzin. Arch. Geikie.
1914. Kr. Birkeland.
1915. Walter H. Page. Charles A. Parsons.

Mr. Joseph Chamberlain's first visit was in 1890. Rayleigh's acquaintance with him was improved in 1891 when he sat as a Member of the Royal Commission on Explosions from Coal Dust in Mines, of which Chamberlain was Chairman. He was impressed with the quick way in which Chamberlain seized the technical points when put before him: though rather scandalized when he leaned across, and asked him sotto voce, "Then is coal gas lighter than air?"

Later on his visits to Terling became almost a yearly institution until his health broke down. In 1893 he came to speak at Braintree for Rayleigh's brother, Charles Strutt. The following letter was written in connection with that visit.

40 Princes' Gardens,
Nov. 28th/93.

Dear Lord Rayleigh,—

It is stated in some of the papers that I have "accepted the Presidency" of the Conservative Club at Braintree.

This is not accurate, and of course it would give rise to misconception if a Liberal Unionist leader accepted such an honour.

I understand that I am coming as a Unionist, to address a Unionist meeting, and am not either asked or expected to accept any official position in regard to the Club.

Please ask the Secretary to correct the misstatement as "the enemy" has already begun to blaspheme. See *Daily Chronicle* of this morning.

Yours very truly,
J. Chamberlain.

Rayleigh's pencilled note runs :—

"I think my first letter asked C. to address a meeting on the occasion of the opening of the —— Club. I don't remember whether I said a Unionist Club."

Chamberlain was a tower of strength as a conversationalist, for in this regard he never flagged. "He takes his exercise that way," as Rayleigh would remark. Certainly he did not take it in any other, for his longest excursion during a week-end visit would be round the garden. For the most part, he sat near the house smoking strong cigars : though, on one of his earlier visits, he was known to make a not very successful attempt to play lawn tennis, wearing his tall hat.[1] He was not at all embarrassed by his ill-success at the game. Indoors, he would often try his hand with the cup and ball which stood on the library table. The host was quite an expert at this.

In connection with his dislike of exercise, Mr. Chamberlain amused us once very much by describing how his enthusiastic constituents at Birmingham had insisted on unharnessing the

[1] No doubt this was unusual at the time, but by no means so grotesque as it would seem now. I remember Lord Kelvin saying that in his undergraduate days at Cambridge no member of the University would have liked to be seen in the town except in tall hat and frock coat.

horses from his carriage and dragging it towards Highbury where he lived. Long before the objective was reached their enthusiasm had flagged. He was stranded on the high road two miles from home, and was for once reduced to walking.

Mr. Chamberlain was most considerate in manner to even the younger members of the party. He was willing to enter into long conversations with them, and would take up and illustrate any remark they might make in the kindest way. But it would have been easy to gather from his conversation that he was a born fighter, even if one had not known it otherwise.

One interesting occasion was the week-end of June 23rd, 1895. The party were sitting after dinner in the garden just outside the laboratory, on the Sunday evening, when a letter was brought out for Mr. Chamberlain. He read it and put it away without saying anything, but one of the other guests whispered a suggestion that the Government was out, and this was a message from Lord Salisbury inviting him to take office. Some of the party were much interested and intrigued, feeling that they were in the thick of great events ; but they felt rather crestfallen when it was found that the exact situation had been revealed by the messenger, and was being discussed in the servants' hall !

Rayleigh and Lady Rayleigh paid many return visits to the Chamberlains at Highbury, and these were continued even after the breakdown of Mr. Chamberlain's health. The last time they were there was in September, 1913, shortly before his death.

The subject of Mr. Chamberlain leads, by a natural sequence of ideas, to Rayleigh's views on the fiscal controversy which raged at the time of Chamberlain's resignation from the Unionist Government. As might be expected, he was not forward to volunteer an opinion on obscure and almost indeterminate questions : he limited himself to an unassailable position. It might be doubtful who paid the import duties, but there was at any rate no doubt who got them !

In addition to the parties which have been described, there

were others when the guests were mostly scientific friends and their wives. Lord and Lady Kelvin were usually of the party, and his eager enthusiasm went far to make it a success. Speaking generally, the order of the day would be much the same as when guests were non-scientific, but naturally more time was spent in the laboratory.

Sometimes the scientific and non-scientific class of visitors would be asked together. On one such occasion (Sept. 26th–28th, 1891) the late Lord Lytton was a fellow-guest with Sir William Crookes, whom he had been anxious to meet. The conversation turned a good deal on spiritualism. Lytton shared the interest which his father the famous novelist had felt in this subject ; and Crookes, encouraged by the presence of a sympathetic listener, expanded more than he was wont. He said something to the effect that he was usually reticent about it because he did not like being taken for a knave or a fool. The most extraordinary incidents, however, were told by Lady Crookes, whom one would not have taken for an unduly imaginative person. She mentioned that at the séances they had been sitting round a particular table for some days, then they put it aside and used another. The first table came out from its corner, apparently to attack the other, which leaped on to the sofa, was pursued by the first, and they had a fight there !

Crookes, when appealed to, said he knew nothing about motives, but corroborated the facts.[1]

Apart from the regular scientific parties Rayleigh would frequently take the opportunity of asking foreign scientific men who happened to be visiting England to spend a night or two at Terling, without arranging a party to meet them. Generally speaking, the impression produced on them was one of surprise at the homely appliances with which his

[1] Although the substance of this conversation is written down long after it occurred, I have no doubt that the account of what was said is substantially accurate. I heard it soon afterwards, and Lady Rayleigh has often told it in my father's presence with his tacit assent. She confirms it in writing now (1923).

experimental work was carried out. Sealing wax, string, rough unplaned woodwork, and glass tubes joined together by bulbous and unsightly joints, met the eye in every direction. The Terling laboratory was far from being a temple of "the brazen image which the Instrument Maker has set up." [1]

In concluding the chapter one visit of a very different kind must be referred to: Rayleigh's brother-in-law, Henry Sidgwick, has several times appeared in these pages. In the summer of 1900, he had serious symptoms, which left no hope of ultimate recovery, and made an immediate operation necessary. It seemed possible that he might live to wind up his literary work, and he came to Terling, as it was hoped, to complete his convalescence from the operation. But he rapidly grew worse during the hot weather at the end of July. During August many of his devoted friends came to see him for the last time, and towards the end of the month he died, under the roof where in previous years he had spent so many happy days.

Rayleigh's studies had nothing in common with his except in the matter of psychical research. But they had a wide circle of mutual friends, and a keen appreciation of the same kind of humour. Moreover, they both thoroughly enjoyed discussion and argument on current events. Sidgwick is buried in the family corner of Terling churchyard.

[1] The phrase is borrowed from Mr. C. V. Boys.

CHAPTER XV

PUBLIC WORK

A new phase of Rayleigh's activities opened with the following letter :—

Jan. 12*th*, 92.

My dear Rayleigh,—

I am sorry to plague you, but I do not see how I can help proposing to you to take the Lord Lieutenancy of Essex (which will be an unmixed bore to you) or how you can well avoid accepting it. Carlingford has resigned. Braybourne [1] won't have it on account of age and sickness : and you are the only other available 'Swell.' I have therefore submitted your name to the Queen which she has graciously approved ; and I hope, as a patriot, you will not refuse.

Yours affectionately,

Salisbury.

This letter was judiciously calculated for the purpose in view. I think there can be little doubt that if the Lord Lieutenancy had been represented by Lord Salisbury in any other light then as an irksome duty it would have been refused. As it was, a reluctant consent was given. "I allowed the Prime Minister to get round me," he said afterwards, in groaning over some of the to him uncongenial duties of the post.

The most important of these was the recommendation to the Lord Chancellor of candidates for the magistracy, which virtually amounted to making the appointments. In this matter he was largely guided by the local knowledge and experience of his brother Charles, who was chairman of the local Bench and later chairman of Quarter Sessions.

All went smoothly for a short time until the Liberal Govern-

[1] Braybrook is doubtless meant.

ment came into office later in the year. An agitation then began to secure that the balance should be held between the political parties for appointment to the Bench. The Lords Lieutenant were for the most part Conservatives, and since the appointments they recommended were also in the main Conservative, it was easy to represent this as the result of political bias. The Lord Chancellor was Lord Herschell, and he put pressure on some of the Lords Lieutenant to recommend Liberals for the Bench. The difficulty was that, as the law then stood, there was a minimum landed-property qualification. This limitation made it extremely difficult to find many Liberals who were suitable, as the landowning classes are of course mainly Conservatives.

Rayleigh was pressed in this sense by Lord Herschell, but reported that after due enquiry he was unable to find suitable names to send forward. Lord Herschell replied by mentioning three names which had been suggested to him, though naturally he did not claim to be able to guarantee them. The result of Rayleigh's enquiries was amusing. One of the suggested three had a doubtful reputation for commercial honesty, and had left the country, to avoid (it was thought) a possible prosecution; the second had eloped with his neighbour's wife; and the third was in a lunatic asylum!

Rayleigh sympathized with Herschell, with whom he was privately on friendly terms. "I am sure he hates being made to write to me like this," he said. Herschell ended one of these letters by wishing him a happy Christmas, which he said was more than he was likely to have himself under the circumstances.

Clergymen of the Church of England, it was understood, were not regarded as acceptable candidates by the party in power. There was, however, one such in the county who afterwards rose to a position of dignity in the Church and was known to be a Liberal and *persona grata* with some members of the Government. Rayleigh tentatively suggested him, and was amused to find that the proposal was eagerly accepted. He felt unable, however, to take the responsibility

of recommending some of the nominations which were suggested, and it ended, I believe, in appointments being made by the Lord Chancellor over the Lord Lieutenant's head.[1]

In 1897, at the time of Queen Victoria's Diamond Jubilee, it fell to Rayleigh as Lord Lieutenant to organize the local effort of Essex to commemorate the occasion. The result was the Essex County Cottage Nursing Association, in which Lady Rayleigh took (and continues to take) a leading part.

In 1901, after the Boer War, military organization was in progress, and new duties were to be imposed on the Lord Lieutenant of a kind that Rayleigh felt little qualified to undertake. He wrote to Lord Salisbury to resign. The latter told us that the resignation had been written "in pencil on a scrap of paper," and that in consequence a great scandal had been caused in High Quarters! Probably the "scrap of paper" was a fanciful description, but about this time Rayleigh had taken to using an aniline pencil for most of his writing work, including his scientific manuscript. This practice was continued till the end of his life. He found the use of a pen unduly tiring to his hand.

Mention of the South African War recalls an amusing incident which happened about this time. Rayleigh was called upon to propose the toast of "The Imperial Forces" at a dinner held at Witham to celebrate the return of his brother Charles to Parliament at the "Khaki Election." After referring to the courage and good conduct displayed by the rank and file, he said :—

"The Officers too have shown that bravery which we have always been led to expect. Whether our officers have taken their duties quite so professionally as I should like to see I am not quite so sure. . . . I can only speak of a very small portion of the War Office authorities, although I can judge from what I have seen [on the Explosives Committee], and from that I take a more favourable view than is generally put forward on such subjects in the

[1] I have written the above chiefly from personal recollections of what I have heard my father say at the time. He does not seem to have kept the correspondence, so that there is no opportunity of verification.

newspapers. The men I have come across are very able men, thoroughly up to their work and taking a keen interest in it. . . . I have often wondered whether military training has been brought up to the requirements of the present day; when the conditions of war are very different from what they were when Military tradition was first fixed. The recruit when he joins is put through what is known as the goose-step. He then learns to walk in a line, keeping his eyes straight in front. Now, being a sceptic in these matters, I don't feel at all sure that at the end of such displays he is better prepared to meet the Boer in the field than he was before he began (Laughter and Hear, Hear). Some years ago there was a picture in *Punch* where a sergeant was reviewing and drilling a squad of recruits, and he said to them: "There you go, staring about you just as if you were in church." (Loud laughter.) I don't endorse the sergeant's description of what it is proper to do in church (laughter)—but I cannot see that staring in front of you is the best way of seeing what is going on around you. (Laughter). . . ."

Shortly afterwards a Conservative smoking concert was held at Maldon, and a baronet well known in sporting circles chose this opportunity to make a violent attack on the Conservative member's elder brother for his remarks at the Conservative dinner.

"He thought it well to reply to some of the remarks made by Lord Rayleigh at Witham. The remarks had been greatly resented by many officers in the army. . . . For a man who had remained at home in luxury, who had never seen a shot fired in anger, who probably had never run any great risks from tropical malaria, and who had probably never been seriously hungry or thirsty in his life, he (Sir ——) called it a —— piece of impudence, and an insult both to the officers of the British Army and the intelligence of his audience to criticise the officers as he did. Lord Rayleigh ridiculed the goose-step . . . would Lord Rayleigh have every British soldier slouching about wall-backed? . . . Among the continental armies the one army which was most particular about these parade movements was the Prussian, and their record proved the value of the movement. Look at the slack Frenchman, and they would see how it was that the Teuton beat the Latinian.[1]

[1] In reading this now (1923) one is reminded of the maxim: "Never base your argument on a fact: for if the fact is disproved, what becomes of the argument?"

"Lord Rayleigh quoted an amusing instance from *Punch*, in which a sergeant told his men not to be staring about as if they were in church, and said: 'I do not endorse the sergeant's description of what it is proper to do in church.' The sergeant did not tell them what was proper to do in church, but what was done. Could it be wondered that people did stare about, for in most families, with a few brilliant exceptions, if a man were too big a fool to pass into the diplomatic service, or the Army or Navy, they put him into the Church and then expected other fellows to go and listen to him. He (Sir ——) was not surprised at men and women staring about them under these circumstances when they were not asleep.

"He hoped he had not unduly condemned the noble lord, but he noticed that on certain state occasions he wore a rapier, and he believed his servants sported a cockade, which, if he knew his heraldry, was a fighting badge. Therefore, he presumed Lord Rayleigh was capable of looking after himself, and if he thought that he (Sir ——) had defamed him, he was quite at liberty to take him on, either privately or publicly. . . ."

These remarks not unnaturally excited some local interest. Shortly afterwards Lady Rayleigh was making some purchases in Witham. The subject came up in conversation with the salesman and he remarked, "What *we* say is, that if Sir —— came to Terling Place, his Lordship would destroy him with electricity before he got within a mile of the door."

However, the powers of the magician were not put to the test, as it does not appear that the knight-errant made any attempt on his enchanted castle.

Towards the end of 1895 Rayleigh was offered the post of Scientific Adviser to the Trinity House. The post had been held by Faraday and by Tyndall, but after that it had been in abeyance. "I am in two minds about it," Rayleigh wrote to Lord Kelvin, asking the latter's advice, which was in favour of accepting. Eventually he did so. He had always taken an interest in nautical matters. When, later on, he was considering how his engagements could be lightened some one suggested that he might give up the Trinity House work. "No," he said. "I enjoy that."

During the years 1897–1913 inclusive, he made several

trips with the Elder Brethren on the yacht *Irene* inspecting lighthouses, and making tests of lights and fog-signals. He had been rather sceptical about the extraordinary caprice in the audibility of fog-signals which Tyndall had reported, but was astonished and convinced by his own experience. After his attention had thus been drawn to it, he would often notice the same thing at Terling. Sitting out in the garden on warm summer nights, he would listen for the noise made by the Great Eastern Railway trains, about $2\frac{1}{2}$ miles distant, on the far side of the park. On some nights the trains would be heard with extraordinary distinctness approaching from many miles away. On other nights, not obviously different in atmospheric conditions, nothing could be heard. He did not feel much doubt that in these cases the sound was deflected upwards; but later on proposed that the question should be settled if possible by observations from an airship. So far as I am aware no attempt has yet been made in this direction.

He wrote from the *Irene* (May 12th, 1901):—

"We had a nice day yesterday compassing the old St. Catherine's signal with a new disc siren. The new one is driven independently, so that the sound starts on a new pitch and gives a better æsthetic effect. But alas the 'whoop' of the old signal proved to be best part of the whole and was heard at 10 miles when all else was lost! Otherwise there seems to be very little difference between any of the things we have tried yet.

"I hear plenty of talk over the misdeeds of captains, and on the other side, of the moral courage it requires to take precautions which your subordinates and the passengers think unnecessary. 'What is the old man afraid of now?'"

Again, he wrote:—

"The Elder Brethren have various tales as to the principles followed by captains of ships in dispensing medicines at sea: one made it his ideal to keep the levels of the drugs in the various bottles uniform. Another used only two drugs, one for diseases above the navel, the other for diseases below."

On his return from one of the *Irene* trips, he told us that tests had been attempted of a new type of lighthouse lamp, which was to be substituted for the old one at a prearranged

moment. Each of the observers on the *Irene* independently wrote down their impression. Some thought the new lamp was an improvement, others thought it was the reverse. But when they got back ashore it was found that owing to some misunderstanding the change had never been made at all!

I remember also (though imperfectly) his report of a conversation on the supposed danger of sleeping with one's face in the moonlight. Some of the Elder Brethren were strongly impressed with this danger, and (I think) a story was told of how one side of a man's face had become paralysed by neglecting it. "You don't believe that?" he was asked, for no doubt his demeanour had betrayed his thoughts. "I have a difficulty in believing it," he replied; and he explained that moonlight after all was of the same nature as sunlight, differing only in respect of intensity.

As might be expected by those who knew him, caution was generally the note struck in the advice he gave in his reports to the Trinity House. Thus on the question of lightning conductors for lighthouses, he wrote (July, '97):—

"If the question were regarded as a purely theoretical one, there is much to be said in favour of a proposal to erect the lightning conductor outside the tower, and to connect it with metal rings embracing the tower at various heights. . . .

"Seeing that the system now in operation has given satisfaction over a long term of years, I should be very loth to recommend an alteration, of which probably the consequences could not be fully foreseen."

Again, he argues against incurring the expense of very powerful lights. Thus (May, 1900):—

"It must be admitted that the French lights already mentioned present a very splendid appearance when seen on a fine night. But if the object of the light be, not so much to astonish the passer-by as to convey information of importance to the mariner, the advantage does not seem to be so clear. It seems hardly to be disputed that while in clear weather almost any moderately powerful light is good enough, in really thick weather the range of the most powerful lights is so small that the difference between an ordinary

light and a very powerful one practically disappears. But it is argued that in certain intermediate states of the weather the powerful light would have distinctly greater penetration. That this will be so to a certain extent must be admitted, but I believe that the increased penetration is much less than would naturally be supposed. When occupied with lights reckoned at from 100,000 or 1,000,000 candles, it is difficult to remember that through a really clear atmosphere a *single* candle has a considerable range. Experiments made on a fine night with a half moon have shown me that a candle would be visible through *absolutely transparent* air to a distance of nearly three miles."

The suggestion was made of floating the revolving mechanism of a lighthouse on mercury so as to minimize friction. Some one remarked that this would be costly on account of the large quantity of mercury required.

Rayleigh : "Not necessarily. You must remember that it is the mercury which is *not* there which floats it."

This was found a hard saying. But on reflection it will doubtless be admitted by the reader.

The chief contribution which Rayleigh made to the practice of the Trinity House was in what came to be known as the Rayleigh Horn for fog-signals. The object was to spread the sound horizontally over the surface of the sea, avoiding as far as possible the waste of sending it upwards. It would seem plausible at first sight to use a horn of elongated section, with the large dimension of the mouth horizontal. Rayleigh, however, was led by a consideration of the theory to conclude that it ought to be vertical.

His reasons are set out in the following note to the Trinity House.

Note respecting the Horns of Elliptical Sections.

By Lord Rayleigh. [1901 or 1902 ?]

"If the object were to send as much sound as possible in *one direction* from a siren using air at high pressure, it would be best attained by associating with the siren a conical horn of small angle, and carrying this out to such a length that the diameter of the aperture is a considerable multiple of the wave length of the sound.

"If on the other hand it be desired to distribute the sound in all directions, the diameter of the aperture must not much exceed the half wave length; otherwise there will be serious interference between the parts of the sound proceeding from the various parts of the aperture. For example, if the diameter has precisely the value above-named, the sound emitted in a direction perpendicular to the axis of the horn is diminished, since the waves proceeding from the nearest and furthest parts of the aperture reach the observer in exactly opposite phases.

"In practice it is usually desired to distribute the sound horizontally through at least 180°. If the horn be horizontal, the horizontal diameter of the aperture is thus limited not to exceed the half wave length. If the section be circular, and of the above diameter, as much sound is sent to the zenith as along the horizontal arc. This sound must be regarded as wasted. The remedy is to elongate the vertical diameter of the aperture, retaining the limitation on the horizontal diameter. We are thus led to the elliptical form of section, the axis of the horn being horizontal, and the major axis of the section vertical. In this way we obtain a concentration upon the horizon, analogous to but of course much less complete than the concentration of rays by the lens used with a fixed light.

"It is important to remark that the dimensions of the aperture, determined upon these principles, depends entirely on the wave length, i.e. upon the pitch of the sound: so that it is impossible to design a horn until the pitch is chosen."

In order to convince the responsible authorities of the Trinity House, Rayleigh arranged a demonstration at Terling in 1903, and Sir George Vivyan, the deputy master, Mr. Thomas Matthews, the chief engineer, and one or two others, came down to see it.

The sound used was a reed organ pipe of high pitch, giving sound waves about 8 inches long. For these comparatively short waves a trumpet of moderate dimensions was enough, and the trumpet used was a wooden one pyramidal in form, 6 feet long, and measuring 36 × 4 inches at the mouth. This was mounted so that its mouth projected from the laboratory window on the ground floor. The observers were on the lawn outside, and the trumpet could be turned on its axis, so that the long dimension of the mouth was either

horizontal or vertical. When the observers were 30 degrees or more away from the axis, the sound was heard much louder with the mouth vertical, as the theory predicted.

This demonstration was convincing, and led to large-scale experiments. In passing from the high pitch used in the model to the deep note used in fog-signals the length of the sound waves was increased about six times. It is necessary to magnify the trumpet in the same ratio, so that a length of 36 feet, with a mouth measuring 18 feet × 2 feet, is indicated. This is a very large structure, but in 1913 it was carried out at Trevose Head in Cornwall. The results on the large scale did not altogether come up to the expectation formed from theory, and from the model experiments. It is probable that the inequalities of the atmosphere which render long-distance transmission so uncertain intervenes in this case also. However, the large horn at Trevose is still in use, and is thought to be superior to the old Service type with circular mouth. Horns on the same principle have been installed at Alderney, Flambro', Flat Holme, Nash, Round Island, Souter Point, South Bishop, and Whitby.

The scheme of instituting a National Physical Laboratory was publicly urged in Sir Oliver Lodge's presidential address to Section A of the British Association in 1891. He had some correspondence with Rayleigh on the subject at that time, witness the following ;—

AMPTON HALL, BURY ST. EDMUNDS,
Oct. 23/92.

MY DEAR LODGE,—

I am afraid I shall not be able to take up the National Physical Laboratory question, having too much business on my hands as it is. Besides, I am not clear as to the advantage of moving at the present time. That there is no chance of getting the thing done by Gladstone and Harcourt is perhaps not conclusive, as a move now might prepare the way for the future. But it seems to me that the first thing wanted from the Government is the extension of the South Kensington School and laboratories, which would perhaps to a certain extent supply the want.

You will not misunderstand me as doubting the propriety of a

National laboratory, but the question is, what can best be done practically.

Yours very truly,
RAYLEIGH.

Later, 1895–96, the project was again brought to the notice of the British Association by Sir Douglas Galton, and the matter was keenly taken up. A deputation waited on Lord Salisbury, and was not unsympathetically received, though of course the usual financial difficulties were emphasized.

As a result, it was decided to appoint a Treasury Committee to consider the subject, and Rayleigh was asked to call on the Chancellor of the Exchequer, Sir Michael Hicks Beach, to discuss it. Before the interview he had a few minutes' conversation with Sir Francis Mowatt, the Permanent Under-Secretary. "You must expect the Chancellor to blow you up," Mowatt said. "He tells me once a week that I am no use." Hicks Beach was certainly not personally sympathetic to the proposal. "I was not scientifically brought up," he said. But he had apparently made up his mind to it as inevitable, and asked Rayleigh to be chairman. "They all tell me you are the proper person," he said, and Rayleigh did not see his way to deny it, though I think he would have been willing enough to do so. The Board of Trade and the Treasury were represented on the Committee, but for the rest it consisted entirely of scientific men, so that there was no doubt from the outset what the tenor of the report would be. Witnesses were heard at some length, but they were preaching to the converted. Rayleigh was absent from many of the sittings, as this was the time of his Indian trip.

The report emphasized the backward position of this country in lacking such an institution. There were, indeed, the Standards Office, and the Electrical Standardizing laboratory of the Board of Trade and the Kew Observatory, the latter almost entirely self-supporting. But these institutions, though efficient for the limited purpose for which they were founded, had not the personnel or the money required for a policy of expansion, capable of meeting new demands as they arose,

and for undertaking the kind of researches on standards which were beyond the scope of private enterprise, or of university laboratories. No doubt Joule's determinations of the mechanical equivalent of heat, and Rayleigh's electrical measurements, were striking examples of what could be done in this way, but, as Sir William Thomson said in his evidence: "Although we are most grateful to wealthy amateurs of science for what they have done—and a great deal of English science is owing to them—we must not fold our hands and wait till they give results which are of great practical good."

As regards practical policy, it was proposed to extend and develop the Kew Observatory in Richmond Park, since it was considered that this institution successfully exemplified on a small scale much of what was aimed at.

It was also proposed that the Royal Society should control the institution and nominate the governing body, with due regard to the representation of commercial interests as well as of academic science. Annual reports were to be furnished to the Government, with audited accounts. This was very much the principle on which Kew Observatory had hitherto been governed.

Most of these proposals were accepted, but when the intention to sacrifice more of the Old Deer Park at Richmond became known, formidable opposition was organized by some who were influenced by æsthetic considerations. Lord Balcarres [1] led their movement. Rayleigh was not unsympathetic to their point of view, which he thought was usually none too much regarded, but it was a wrench to abandon the idea of using Kew Observatory as the nucleus of the new institution, as the following extract shows:—

"Athenæum Club, *July 12th*, 1900.

"A rumour having got about that things were going wrong, I have spent the morning interviewing Beach and Arthur [Balfour]. B. pooh-poohs Balcarres and Co. and is practically on our side. A. is exposed to various influences and has started a new proposal

[1] Now Earl of Crawford.

that we should go to Bushy Park. This was rejected upon the ground of expense, and I don't know yet whether it is feasible from our point of view."

Bushy House (near Teddington) was a Royal Palace which had been the home of William IV as Duke of Clarence, and afterwards of his widow, Queen Adelaide. Later, it had been occupied by the Duc de Nemours, son of Louis Philippe, from about 1866–72. From that time on it had been empty, though the Duc de Nemours was responsible for its upkeep until his death in 1896. Apart from the objection of distance from Kew Observatory, the building had no efficient drainage system, and no convenient access to the station by road. On the other hand, the grounds of 30 acres allowed room for expansion.

In the meantime the Royal Society had appointed an Executive Committee for the laboratory, consisting for the most part of the same individuals as the original Treasury Committee.

The President of the Society was nominal chairman ex-officio, but Rayleigh was vice-chairman, and in fact did all the duties of chairman.

It was probably by Rayleigh's influence that the Committee had already taken the important step of appointing Dr. R. T. Glazebrook as director. Glazebrook, as we have seen, had been Demonstrator under him at the Cavendish Laboratory fifteen years before. After leaving Cambridge he had been Principal of Liverpool University. He had frequently visited at Terling. In this and other ways he had remained in touch with Rayleigh, who had a high opinion of his scientific and administrative abilities. There was little reason subsequently to regret the choice.

The Committee eventually accepted the offer of Bushy House, with an additional grant to make good the deficiencies. The terms were arranged at an interview between Rayleigh, Lord Esher, Sir F. Mowatt and Dr. Glazebrook.

It soon appeared that some of those who had been most forward in pressing for the laboratory were unwilling or

unable to shoulder the work of administering it. Rayleigh took this with good-humoured resignation. "There is ——," he said, "who agitates for this thing, and then leaves all the hard work of it to me!"

The laboratory, once established at Bushy House under Glazebrook, underwent a steady and healthy development. Of Rayleigh's share in it Sir Richard Glazebrook writes:—

"Perhaps the most important part of his work depended on the fact that he was personally trusted by every one whether in matters of science or administration. His position was such that he could approach members of the Government or high officials without difficulty, and put before them his views in a manner which necessarily carried weight and called for action. As Director I was constantly in touch with him; I never brought forward at the Committee any matter of importance without consulting him and learning his views, and while of course discussion at the meetings led frequently to modification of details the proposals or suggestions which he supported were always accepted by his colleagues."

The otherwise smooth course of the laboratory's early development was disturbed in 1903 by an incident which gave rise to much trouble; though if viewed in proper perspective, it will perhaps appear like a storm in a tea-cup.

One of the assistants brought in a sample of cod-liver oil, and told the Director that he had been asked by a private friend, Mr. M——, living close to the laboratory, if it could be analysed. Mr. M—— was a prominent man in the neighbourhood, and he and his wife had been very kind and hospitable to some of the laboratory staff. The Director had some hesitation, but finally, to oblige Mr. M——, he consented, and signed a certificate of the analysis. Some time later he was a good deal troubled by hearing that the matter was not a private one of Mr. M——, but that he was the chairman of a wholesale drug company, and that there was a lawsuit pending about the oil.

The matter thus came to the ears of the Society of Public Analysts and of the Institute of Chemistry. These bodies were evidently not sympathetic to anything which they thought

might tend to the nationalization of their industry. They were led to scrutinize closely the chemical testing work done in the laboratory. As a matter of fact such work was rarely done, and only in quite special cases. These were usually on material sent to the laboratory for other tests, and the annual value of the small residuum averaged only about £30 in fees. This was carefully explained to the objectors, both orally and by letter, and they professed to be satisfied. Thus the secretary of the Institute of Chemists wrote (January 29th, 1906) thanking Rayleigh for his letter and saying, "The Council cordially accepts the assurances herein contained."

However, the event proved that they were not satisfied. Sir William Ramsay, as Vice-President of the Institute of Chemistry, wrote to Mr. Asquith, then Chancellor of the Exchequer, urging that no commercial tests of material, mechanical or chemical, should be executed by the laboratory. He pointed out certain passages in the report of the original Treasury Committee which were considered to forbid them.

The executive committee of the laboratory were not prepared to accept this view of the position, and they pointed out that they themselves were for the most part the same individuals, and had the same chairman, as the original Treasury Committee, whose views they might, therefore, be supposed qualified to interpret. Moreover, though the report to the Treasury might not be strictly self-consistent, there were passages which *did* contemplate commercial tests of material.

However, the clamour continued, and questions were asked in Parliament reflecting on the conduct of the laboratory, and in one case even suggesting doubts as to whether that institution was giving any return for the public money spent upon it! "Can the Secretary of the Treasury give particulars of the results of any research which can be considered in any way an adequate return for the expenditure incurred?" etc., etc. The answer given was that copies of the two volumes of published researches had been placed in the library of the House. Perhaps the Honourable Member read them through,

and felt satisfied. At all events he does not seem to have returned to the charge.

Finally the Treasury resorted, no doubt wisely, to the usual expedient of harassed Government departments. They appointed a committee to enquire, with Mr. Gerald Balfour as chairman. The committee heard evidence and reported in due course. They quoted my father's evidence. Lord Rayleigh, when asked whether he considered the practice of the laboratory, especially with regard to the testing of materials, had, as a matter of fact, observed the spirit of the recommendation of the [original] Treasury Committee, replied, "I am not so sure about the letter. I think it has the spirit, except, perhaps, in some very small matters. I think the letter is a little ambiguous." He further referred in his evidence to influence of the Treasury in urging the laboratory to obtain funds as far as possible from fees. "Perhaps that has pushed us a little in the direction of undertaking what we might not otherwise have done," he said.

The chief point of the Committee's recommendation was that as a general rule the laboratory should not undertake the ordinary testing of materials to ascertain whether their quality and behaviour were in accordance with the requirements of contracts unless for Government. Otherwise the laboratory was to be quite free. This small concession was readily agreed to, and the matter was at an end. Much ado about nothing!

In the meantime the laboratory increased and flourished. Many new departments were added, including those of electrotechnics and metrology, the experimental tank for determining the resistance of ship models, and the department of aerodynamics, of which more will be said in a later chapter. Work on the electrical standards had been carried on in the laboratory from the commencement with the chairman's warm support. He was always ready to assist the Director and Mr. F. E. Smith in the researches with the Lorenz apparatus placed at the laboratory by the Drapers' Company, in memory of Viriamu Jones, or with the Ayrton Mather current balance.

These instruments were glorified and improved editions of the apparatus which he had used in his own electrical measurements at Cambridge, with all the refinements which modern engineering construction could give. He was not at first very much attracted to the proposal for the current balance. The suspended coil was to be wound on a marble bobbin, and he feared that the dead weight thus added would more than neutralize the advantages. He also feared possible magnetic disturbances from the gunmetal adjusting slides. Moreover, the great cost of the instrument violated his economical instincts. However, I believe that he was afterwards converted to its merits.

The work culminated in the great international conference held at Burlington House, which has been mentioned in a former chapter. Rayleigh was the chairman, and under his guidance resolutions were adopted which have been the bases of legislation throughout the world.

As the laboratory developed, the question of finance was an ever-recurring difficulty. Something could be earned by fees for testing, but the Treasury were always over-sanguine as to what could be done in this way. In a National Laboratory, as in a University, the fees earned are no true measure of the value of the work. The original arrangement between the Treasury and the Royal Society made the latter responsible for balancing income and expenditure, and any deficit would fall, theoretically at least, on the slender resources of the Society. This situation was tolerable at first, in the period of small beginnings, but as the laboratory grew, it became less and less so. To conduct a big business with no capital is always risky and difficult. By the beginning of 1913 the Society had guaranteed a considerable overdraft at the bank, and it was clear that the position needed stabilizing in some way. Rayleigh interviewed the Prime Minister, Mr. Asquith, who was a private friend, and learnt that he would receive a deputation. But at this point the War broke out.

In the early stages of the War the financial difficulties

were serious : then the value of the work which the laboratory could do began to be more fully realized. Work of all kinds came in with corresponding growth and expenditure, which was met, during the War, by payments from the Ministry of Munitions but which promised to leave the laboratory in a bankrupt position on the approach of peace. Meanwhile, the Department of Scientific and Industrial Research had been established, and it was understood that for the future, assistance in the application of science to industry was to come through the Department. It was about this time that Rayleigh wrote to the then President of the Royal Society, Sir J. J. Thomson (October 4th, 1916) :—

"The N.P.L. is continually in difficulties owing to our scale of pay not being high enough. Has the Royal Society come to any decision as to going under the Privy Council scheme ? I am getting more and more in favour of it, of course supposing the details can be arranged satisfactorily. I hope the matter will not be postponed more than can be helped. If it were decided not to make a move in this direction, I think we shall have to go to the Treasury, and see what they say."

When it came to details, there were difficulties. It was hard to combine that freedom which the laboratory had enjoyed when its funds were under control of the Royal Society with the stringent rules which the experience of Government departments has found to be necessary for the expenditure of public money when there is direct responsibility to Parliament. Rayleigh found it hard to reconcile these competing claims in his own mind, finding that there was great weight in what was urged on both sides. These difficulties culminated only a few months before his death. He was himself a member of the Advisory Committee of the Research Department, and did what he could to arrange matters at conferences with Lord Curzon and Mr. H. A. L. Fisher, who were the responsible Ministers concerned. The following letter was written about this time, and seems to have been successful in averting one change which is deprecated therein.

To Mr. H. A. L. Fisher, 1919(?).

"I recognize that my long association with the laboratory may lead me to take too conservative a view of practices which have been established largely under Treasury pressure. But there is one aspect of the separation of testing and research work which is very likely not appreciated by those who have not been closely concerned with it. When a system of testing has been organized, much of the practice may be in the hands of people incapable of research. But when it comes, as it often must, to a question of improving a method, research is required, and this can hardly be carried out without a familiar acquaintance with the routine. It therefore seems to me that if the two kinds of work are under the control of different departments, there is great need for a kind of co-operation which I fancy is not usual, involving transfers of personnel as occasion may require."

Shortly afterwards Rayleigh was compelled to resign by failing health, and the Director retired under the age limit. This epoch of the laboratory's administration was at an end.

Rayleigh had presided at 165 out of the 185 meetings which had been held while he was chairman. A great national institution had been created. When work was begun at Bushy House in 1901 the personnel numbered about fifteen and the annual income of the laboratory was under £6,000. In 1918, at the time of Rayleigh's retirement, the personnel was upwards of 400 and the expenditure more than £100,000.

Finally, it is to be observed that this was not a hot-house growth, fostered at the public cost in order to satisfy the instincts of scientific men. It had grown and expanded in response to the public need, and the public experience of the benefits which its services could give.

CHAPTER XVI

RECREATION AND MORE PUBLIC WORK

In 1897 Rayleigh had the feeling of staleness over his scientific work which showed the need for a holiday, and he decided to take the only opportunity which was likely to occur in his lifetime to see a total eclipse of the sun, visible near Poonah in India early in 1898. After paying one or two farewell visits, he sailed on October 22nd on the S.S. *Arcadia* for Colombo. Lady Rayleigh joined him at Brindisi.

At Colombo they were the guests of the Governor, Sir West Ridgway, and his wife. I think it was there that some of the Governor's Staff had been puzzling themselves over a simple conundrum which they thought should easily admit of algebraical solution, but the solution they got did not seem to fit, and they brought the question to Rayleigh. Before they had propounded it he told them that they had doubtless taken the wrong root of a quadratic equation. It proved to be so. "How in the world could you guess that?" they said.

However, apart from trifling incidents of this kind, he wished to get away from his usual pursuits, and this proved easy enough, because hardly anyone he met in India or Ceylon seemed to know or care anything about them. He found that he was received with a good deal of respect as a peer, but the people he met seemed to know nothing about him in connection with science.

From Colombo they started on November 18th, 1897, for Candy, and thence they went to Newara Elia. They returned to Candy and on to Matale, where they slept at a Government rest-house and continued the journey by coach to Damboul and

Anurajahpura—sleeping at rest-houses, and kindly entertained by Government officials, and shown the sights. He noted in his pocket-book, "November 26th. Fireflies at about 100 feet were comparable with the brighter stars."

At Damboul on the return journey the rest-house keeper persuaded them to make an expedition into the forest with *his* bullock cart to see the ruins of the huge rock fortress of Sigri. They started at 4.30 a.m. Unfortunately the bullocks were not up to the work and jibbed a good deal, and finally, seeing Rayleigh, who had got out to relieve the strain, walking in front, they took fright and broke the yoke. They were stranded in the jungle, with food exhausted, and no apparent means of return. However, the native servant was able to get fresh bullocks from a Government farm with a supply of green coco-nuts, and they eventually got back to the rest-house none the worse. On the return journey their luggage got drenched by the rain. For the rest of their travels Rayleigh's white flannels showed red stains from the lining of the portmanteau, but this did not disconcert him at all when wearing them.

They spent some further time with the Governor at Colombo on their return. One night they dined out in a native house, and on their departure large wreaths of flowers were hung round their necks as a mark of respect. They feared to meet the chaff of the Governor's Staff on their return with this absurd decoration, but their fear proved groundless. The Staff were so accustomed to it that their only interest was to consider whether the wreaths were as good as the occasion demanded.

They sailed on December 1st for Calcutta, stopping for a few days on the way at Pondicherry and at Madras. They stayed at Government House, Calcutta, as the guests of Lord and Lady Elgin. From there they went to Darjeeling, and returned to Calcutta for Christmas. Thence to Delhi, Amritsar, Lahore, and Peshawur, where they stayed with Mr. Merk, the Acting Commissioner. Rayleigh recorded the following :—

"When we were in India at the time of the Tirah Campaign, Mr. Merk, Acting Commissioner at Peshawur, wished to take us to Jamrud (?) at the mouth of the Khyber. To this end he approached the military authority for permission, which to his surprise was refused. All he could do was to take us to about a mile short of Jamrud, the limit of his jurisdiction.

"Many years afterwards I was sitting at a Royal Academy dinner (the only one I have ever attended) next an agreeable man, who seemed to be military. We got talking about India, and I related the above incident. 'When was that?' he said. 'It must have been January, 1898,' I replied. 'I could fix the date by the eclipse which we saw soon afterwards.' '*I* [1] was the Military Authority,' he said.

"Among the humours of the Tirah Campaign Mr. Merk told us that the Afridis, against whom we were fighting, had sent their wives and children to Peshawur for safety.

"There was also a question whether Afridis who had served in our army and were in receipt of a pension should still be paid, in spite of their fighting against us. Mr. Merk thought they should. The pension was for work done, and without further conditions. Moreover, an Afridi would have little choice. He would have either to fight or else leave the country."

They were a good deal impressed with all they heard at Peshawur, and Rayleigh was surprised when they next saw Lord Elgin, the Viceroy, that the latter did not seem the least interested to hear how things had appeared to him.

After Peshawur they visited Gwalior, Agra and Benares. At the latter place pilgrims were flocking from every side into the holy city to bathe in the Ganges during the expected eclipse. The Viceroy had a camp near Berar, where the eclipse was total. Here they joined the Viceroy's party. Rayleigh had been quite firm from the first that he would not take any part in the work of astronomers who had come to observe the eclipse. He had come on a well-earned holiday, and was determined to enjoy the eclipse as a spectacle, without being distracted by the responsibility of an astronomer during those few feverish and anxious minutes. He looked at it with an opera glass only, and got a good view of the

[1] Nicholson. Havelock had been killed in the Khyber a little time before.

hydrogen flames and the corona. In respect of darkness, however, the eclipse was a disappointment, and colours were never undistinguishable. As the sun began to reappear, a cheer broke from a crowd of natives assembled near the camp.

After the eclipse was over, they went to Lucknow, where they were the guests of Mr. Hardy. Mrs. Hardy was the daughter of Admiral Luard, a near neighbour in Essex, but she was away. Mrs. Steel, well known as the authoress of *On the Face of the Waters*, was there with her husband. She was a student of native life, and Rayleigh was much interested in what she had to tell him.

Here are some incidents which he wrote down from her conversation :—

"When Mrs. Steel published her book *On the Face of the Waters*, dealing with the siege of Delhi, she had numerous letters. One was from a man whom she had spoken of as having watched Delhi 'as a cat watches a mouse.' This man she had never seen, and did not know was living. He wrote, 'What you say is perfectly true, but how in the world could you know it?' What she went on was that his name had been mentioned more than once as the person who first noticed something in Delhi.

"Another pathetic letter was from a man who had lost all his family in the Cawnpore tragedy. He thanked her, saying that her book had helped him to do what he had thought he never could do, to forgive. The best native opinion had always condemned this business."

Rayleigh was anxious to see a specimen of the Indian conjuring of which so many marvellous tales are related, and Mrs. Steel did the best she could for him : but the performance was not found very impressive. At a second visit the magician descended to selling his tricks for 4*d*. each !

Mrs. Steel afterwards wrote :—

"I think still more often of you sitting on a sofa covered with enormous chintz carnations in Mr. Hardy's drawing-room, eliminating impossibilities from a conjurer's string tricks, and coming

out victorious again to the verandah, the same trick discovered. It is my favourite example of abstract thought."

Military spectacles were not much in Rayleigh's line, but at Lucknow he went on horseback to an early-morning parade of the 18th Lancers, not at all embarrassed by the want of riding clothes. This was perhaps the last time he was ever seen on horseback. After Lucknow followed visits to Cawnpore and Jeypore, where they stayed with the Resident, Colonel Hendley. Lady Rayleigh, like most other travellers, liked to purchase for herself the wares of the countries she visited, but Rayleigh never did this, and indeed it seemed almost painful to him to see things bought, even by some one else, which as he considered were not required.

From Jeypore they went on to Bikaneer, Jodpore, Adjmere, Mount Abu, Ahmedabad and thence to Bombay, where they stayed with Lord Sandhurst (the Governor) and Lady Sandhurst.

March 1st to 10th, 1898, was spent on an expedition to Hyderabad, beginning with a visit to Ellora to see the cave temples there. At Ellora they were the guests of the Nizam, and a Parsee gentleman who had his meals with them was sent from the Nizam's Court as a guide. One of the Governor's staff at Bombay had warned them to have their pockets full of small change for the priests, who were persistent beggars. The Parsee guide, however, made short work of them, driving them off with the remark, "Why should we give to them? We are not of their caste." If he looked away for a moment, the begging by gesture was immediately resumed.

From Hyderabad they returned to Bombay and sailed for England on March 12th on board the P. & O. *Egypt*. Dr. Campbell, now director of the Lick Observatory, and President of the University of California, had been out to observe the eclipse, and was returning on the same ship. He wrote, in 1920:—

"Aside from my interest in Lord Rayleigh's contributions to knowledge, I shall always treasure the memory of associating with him on the ocean voyage from Bombay to Suez in the year 1898.

At that time I was a young man with my career to be determined in the future, yet Lord Rayleigh sought me out on the ship daily, and gave me many inspiring hours. I could not but think of him as a great man, aside from his scientific attainments."

They landed at Marseilles and arrived in London on April 1st. They went to 10 Downing Street, the official residence of the First Lord of the Treasury, where Arthur Balfour was now established.

The explosives committee of the War Office originated in 1900. Lord Lansdowne, then Secretary for War, was anxious about our national position in this respect, and Lord Goschen, First Lord of the Admiralty, shared his anxiety. Lord Lansdowne happened to encounter Mr. R. B. Haldane, M.P., on a railway journey, and the latter suggested following the example of the French Government, who were guided by a committee under Berthelot, the well-known scientific chemist. He further suggested that Rayleigh should be asked to preside over it. They afterwards dined together for further discussion. As the result Lord Lansdowne wrote to Rayleigh strongly pressing him in this sense. "Your name," he said, "would inspire great confidence, and the two members of whom I have just spoken [Noble and Haldane] have both told me that there is no one under whom they would prefer to serve."

He accepted. The other members of the Committee were —Sir Andrew Noble, the Chairman of Armstrongs', Sir William Roberts-Austen, Chemist to the Mint, and Sir William Crookes, with Captain Tulloch as secretary.

Haldane, Crookes, and Roberts-Austen came to Terling for a week-end visit (June 16th–18th, 1900) for preliminary discussions.

The Committee soon came to the conclusion that what was needed to ensure that this country should not fall behind was an experimental establishment under the charge of a research chemist who would be free from routine testing work, and able to devote his time to the improvement of existing methods. Their views in this respect were promptly met,

and the establishment was set up at Woolwich, under the direction of Dr. O. Silberrad, who was in turn under the direction of the Committee.

I do not know that the work was specially congenial to Rayleigh, but his patriotic feelings had been strongly moved by the country's anxieties in the South African War, and he felt that this was his opportunity to do something to improve our position for national defence.

Rayleigh's own part in the Committee's work was rather judicial than constructive, Noble and Crookes did more in the way of experimenting.

As might be expected, Sir Andrew Noble was a tower of strength, bringing unrivalled knowledge to bear. Rayleigh was amused to observe his diplomatic methods in discussion. He would put his own views into somebody else's mouth—"as you were saying." The person addressed was perhaps not conscious of having expressed any definite views, but supposed that he must have done so, thus an adherent was gained!

One of Rayleigh's contributions is reprinted in his *Scientific Papers* [V, p. 103], where he discusses, on certain approximate assumptions, what form cordite should take in order to maintain the pressure approximately constant during a definite part of the travel of the shot inside the gun. He concludes for tubular cordite containing more than one tunnel in each stick. He also pointed out advantages which might be gained by using cordite of two different sizes in each charge. So far as I have been able to learn, these proposals have not been taken up.

The problem of most urgent importance was the erosion in big guns, a kind of washing away of the steel by the hot and highly compressed gases which are generated by the explosion. This soon makes the bore of big guns so irregular as to prevent accurate shooting.

The propellant chiefly in use was cordite, a gelatinized mixture of gun cotton and nitro-glycerine, containing 58 per cent. of the latter. This formula, which had been worked out and specified chiefly by Sir Frederick Abel some ten

years earlier, was very efficient as a propellant, but gave rise to serious erosion. The problem was to modify it in some way that would diminish this trouble with as little detriment as possible to the value of the powder as a propellant. Powders consisting of pure nitro-cellulose were on the market, and gave rise to much less erosion than cordite, but with the need for a larger charge to give the same ballistic effect.

The Explosives Committee tried various expedients, such as incorporating a temperature-reducing agent like carbon or naphthalene in the cordite itself, but this reduced seriously the propelling power. The probability is that the two requirements are necessarily antagonistic, and that all that can be done is to make a judicious compromise. The tests made for the Committee led to the conclusion that it would be best to reduce the nitro-glycerine in cordite from 58 per cent. to 30 per cent. A modified cordite named officially cordite MD was thus brought into extensive use in the service.

I believe also that the Committee were the first to introduce the use of trinitrotoluene (T.N.T.) as a high explosive.

Besides the question of erosion, the Committee made extensive investigations on the keeping qualities of explosives at the high temperatures which prevail in tropical countries. "Climatic huts" were provided at the experimental establishment, in which the explosives were stored, at sufficient distances to prevent harm when the huts "went up." I believe that Dr. Silberrad visited one of them only five minutes before this occurred.

Many other technical questions were considered during the five years that the Committee sat, but the details are not of general interest, and indeed hardly belong to my subject. At the end of this time, Mr. Haldane, now Secretary of State for War, who had himself been active in originating the Committee, and had sat as a member, decided that its work was done.

Rayleigh received a letter in June 1901 from his brother-in-law, Gerald Balfour, then President of the Board of Trade, offering him the appointment of Chief Gas Examiner. This

was rather a judicial than a scientific appointment. It may be explained that the London gas supply is regularly tested for purity and illuminating power at fixed stations. This is done under the relevant Acts of Parliament in the interest of consumers. If the gas falls below standard the companies are liable to a forfeit. The Chief Gas Examiner hears appeals from the companies against the result of the official test, if they wish to make them, and his decision is final.

The post had been held by Prof. A. W. Williamson, the well-known chemist. His failing eyesight made it impossible for him to go on ; indeed, it was said that he had been reduced to appealing to the shorthand writer to determine whether the paper used in testing for sulphur in the gas was appreciably discoloured or not !

The post was well paid and the work light, and Rayleigh was at first afraid that if he accepted it, corrupt motives for the appointment might be suggested by the Opposition in Parliament. However, nothing of the kind happened, and he continued to hold it until his death.

There were some anomalies in the gas legislation for the Metropolis at this time, and the Board of Trade decided to have an amending Act. A departmental Committee was appointed early in 1904, with Rayleigh as chairman, to report on what changes were desirable. After hearing evidence at length they reported in May, 1904.

It was not found possible to do much in the way of simplification. Two changes which the Committee proposed are of some interest for this narrative. The companies complained that when the Chief Gas Examiner had ruled against them in respect of a test of the quality of gas falling below standard, they had to appear before the magistrate in company with common malefactors. They objected to this, and with reason. The quality of gas may become deficient for a short time owing to some cause which is so obscure that it may fairly be called accidental ; and the person technically responsible under these circumstances naturally objects to being summoned to petty sessions, and having his case called next in order (it

may be) to a man summoned for brutally beating his wife. To meet this, the pecuniary penalty was to be imposed directly by the Chief Gas Examiner.

The other point was more technical. Under the earlier Acts all the companies had to supply gas giving 16 candle power when burnt at the rate of 5 cubic feet per hour in the standard burner. The Gas Light and Coke Company continued to do this, but the other companies had obtained Parliamentary sanction to supply gas of a nominal 14 candle power, of course at a lowered price.

So far as the South Metropolitan Gas Company was concerned, their Act of 1900 contained a very peculiar provision, due to the ingenuity of Sir George Livesey, who was the leading spirit in the company, and fought well for his shareholders. Rayleigh had greatly admired his resourcefulness on another occasion in fighting the obstructive policy of the Trade Union, and he was now brought into personal contact with it. The 14 candle power was not to be determined by the more obvious procedure of burning the gas at the standard rate of 5 cubic feet per hour. It was to be tested at a rate which would yield 16 candles, i.e. at a rate of 5·71 cubic feet per hour. This gave the company a marked advantage, because it brought out the illuminating power about 1 candle power higher than the other method of testing.

It is difficult to understand how a Parliamentary Committee were induced to swallow this. Perhaps it slipped through unnoticed, or without any clear appreciation of its effect. Livesey, in his evidence before the Committee in 1904, made a spirited defence of it, but without success. The Committee reported in favour of a system by which the gas was uniformly tested at the rate of 5 cubic feet per hour, though they did not propose that the actual standard of quality should be altered. The Act of 1905, which substantially gave effect to their report, went further on this particular point, and required 14 candle-power gas to be supplied by the South Metropolitan Company.

To go back, however, to Rayleigh's normal duties as Chief

Gas Examiner. One or two examples of his judicial manner in hearing appeals may not be without interest.

Thus (June 27th, 1907) :—

Chief Gas Examiner : " Without attributing to the company any desire to ignore their proper obligations, I think one must see that if they were liable to no penalties or forfeitures whatever, the deficiencies would be a good deal more numerous ; so that it is really in the interest of the public that some definite notice should be taken of deficiencies which occur, supplying some kind of motive to the company beyond good intentions to keep the gas up to the standard intended by Parliament at all the stations. I do not understand how this clause 14 can be read in any other sense but that the minimum penalty is £25 and the maximum £100. I understand the question before me at this moment to be merely a discrimination between these two amounts or the fixing of some intermediate figure."

And again later on in the report of the same proceedings :—

Chief Gas Examiner : I will take an intermediate figure and adjudicate a forfeiture of £50 for each of the three days—the 18th November, 1906, the 2nd December, 1906, and the 27th January, 1907, in respect of the deficiencies reported at the Vincent Terrace Testing Station.

Mr. Goulden : [1] Should I be in order, my Lord, in enquiring as to the ground on which the larger penalty was given, seeing that we had only one station out of 14 showing deficient illuminating power ?

Chief Gas Examiner : I do not know that I am bound to say any more.

Mr. Goulden : I fully understand that, my Lord.

Chief Gas Examiner : But my view is really that the money penalty is a very small one on a large company, and as the hearing takes place now under circumstances exposing the company to no undue odium, there is no special reason why the pecuniary figure should be cut down to the lowest point.

Mr. Goulden : Thank you, my Lord.

[1] Representing the Gas Light & Coke Co.

Again, on another occasion (May 8th, 1913):—

Chief Gas Examiner: I do not think I shall decide before I am actually called on to do so whether Good Friday is a Sunday or not.

In 1901 the Central London Tube railway was newly opened and trouble arose from vibration caused in the houses below which it passed. The complaints of the householders were loud; and the Board of Trade appointed a committee to see what could be done. Rayleigh was the chairman, and Sir John Wolfe Barry and Prof. J. A. Ewing were the other members.

As soon as they began to consider the question, they were unfavourably impressed with the design of the electric locomotives.

The heavy armatures of the motors were not spring-borne, but were on the axles of the road wheels. The field magnets were also of course in a definite position relative to them. In this way some simplicity of construction was gained, but at a heavy cost in respect of smooth running. The load carried without springs by each wheel was no less than 4 tons. The rails were not perfectly straight, since they usually come curved from the rolling mills, and have to be corrected by local bending. This is bound to produce waviness, and the conveyance of the heavy armatures over this without any easing by springs would readily account for the vibration. Pending the construction of a new trial locomotive in which this defect would be remedied, the only thing that could be done was to determine whether individual locomotives were specially bad. Tests were made by the Committee with the assistance of a number of other observers, including the present writer. The individual locomotives were timed at a station and the vibrations in the houses above were also timed and their intensity estimated. There was no definite indication that one locomotive was worse than another.

Mr. Arnulf Mallock was employed to experiment for the Committee, and devised an ingenious vibration recorder, by which he obtained records of the vibrations in the tunnels

and in the houses. He also made a detailed examination of the irregularities of the rails, and of the wheels.

Finally, when the new locomotives arrived, in which the armatures were carried on springs, one of the houses was specially connected by telephone with an observer in the tunnel below, and the contrast in behaviour between the old locomotives and the new was satisfactorily verified. The Committee reported in this sense, the old locomotives were "scrapped," and so far as I am aware no further trouble has ever arisen.

Besides the public work described in this and the previous chapters, Rayleigh served at various times on the following bodies :—

Senate of London University.

Royal Commission on Explosions from Coal Dust in Mines, 1891.

Governing body of Rugby School.

Governing body of Felstead School.

Board of Visitors, Greenwich Observatory.

Board appointing to 1851 Exhibition Scholarships.

Advisory Council of the Department of Scientific and Industrial Research.

CHAPTER XVII

SOME RESEARCHES IN MATURE YEARS

Rayleigh's early researches on the blue sky, and its explanation in the scattering of light by small particles, have been discussed in Chapter III. At that time he did not pledge himself as to what kind of particles they were. But after the publication of his early papers Clerk Maxwell wrote to him to suggest that the molecules of air themselves might be concerned and that on this view something might be found out about the molecules.

In 1899, twenty-six years after Maxwell's letter, he took up the subject from this point of view, in an epoch-making paper. Only a slight indication of the line of argument can here be attempted.

Since the scattering diverts energy laterally from a beam of light, it is clear that the primary beam must be enfeebled. The question is how much ? Can this enfeeblement be usefully expressed in terms of other quantities which we know, or should like to know ?

Given the volume and optical density of a scattering particle, the older calculation shows at what rate it causes energy to leak away in each direction. If the losses in all directions are summed up, we shall get the total loss from the primary beam due to a particle and hence from any number of particles that may be present in the unit of volume. This by itself is not of very much help since we do not know what the volume and optical density of a particle may be.

If, however, these considerations are combined with certain others, an important result emerges. In the direction of original propagation the secondary disturbances emitted by

the particles combine with the original disturbance, and the result is that the phase of the resultant disturbance is somewhat behind what it would be in the absence of the particles. This retardation can be recognized from an experimental point of view as the slower propagation of light in air than in vacuo, and is consequently expressible in terms of the refraction of air. In this way Rayleigh got another relation involving the optical density and number of the individual particles, and the refractive index. Combining the two, an equation emerged, which did not involve the optical density of the particles, but only their number. This equation expresses the fraction of energy lost per centimetre of path, in terms of the length of the light waves, the refraction of air, and the number of molecules in a cubic centimetre.

If then we know the number of molecules in a cubic centimetre, we can deduce the opacity of pure air; for the other quantities concerned are well and accurately known.

At the time of Rayleigh's paper, neither the number of molecules nor the transparency of air was well known. As regards the former Maxwell's estimate (from the viscosity of air) was 19×10^{18} per c.c. This would make the opacity such as to reduce the light from 2·7 to 1 in 83 kilometres. The actual transmission as inferred from Bouguer's old observations on the stars at various altitudes was about three times less than that calculated, and much of the difference was attributable to dust. If allowance was made for this, Maxwell's estimate of the number of molecules was confirmed by methods totally different from those used by him.

Rayleigh's conclusion was "the light scattered from the molecules would suffice to give us a blue sky not so very greatly darker than that actually enjoyed."

Since the time of Rayleigh's paper, experimental knowledge on the number of the molecules and the transparency of the air has been very greatly improved. In particular, methods based on radioactivity and a knowledge of the ionic charge have given us a knowledge of the number of molecules within a fraction of 1 per cent. The observations made by Abbot

and his collaborators, of the solar radiation at various altitudes of the sun, have given an accurate value for atmospheric transparency. Their observations on Mount Wilson, away from the dust which infests the lower regions of the atmosphere, have especial value. These new numbers confirm Rayleigh's relation very closely, and show that the scattering and resultant opacity of pure air are precisely calculable by his method.

He never attempted laboratory observations on scattering, by pure air, and was not disposed to think it feasible. More recent work, however, has shown that it is so.

Mention has often been made in these pages of Rayleigh's faithful and valued assistant, George Gordon. He was somewhat older than his chief, but the latter had hoped that he would be able to help him to the end of his own working career. Some abatement of Gordon's activity would be no disadvantage, for he expected that his own requirements would become less exacting as time went on. However these hopes and plans were disappointed. During the years 1903 and 1904 Gordon was visibly declining. On one occasion Rayleigh found him asleep at the blowpipe table in the laboratory, but pretended to notice nothing. In the summer of 1904 his health finally gave way under distressing circumstances, and early in December he died. He was buried in Terling churchyard, near his parents. My father and I attended the funeral in pouring rain.

Rayleigh had thought it doubtful whether he would care to face the effort of starting with a new assistant, and the question remained in suspense for a time. During an interval of more than a year, he remained without regular assistance, but for a considerable part of the time I was at home, and was able to help with glass-blowing and metal-working when necessary.

Eventually, chance put it in his way to secure a new and efficient assistant. This was J. C. Enock, an engineer who was engaged in erecting a refrigerator plant for milk near Hatfield Peverel Station in connection with the dairy farming on the estate. Rayleigh fell into conversation with him while this work was in progress, and engaged him to help in the

laboratory. His previous experience had been purely that of the engineering workshop, and, as he said, he was now introduced into a new world. However, he quickly adapted himself, and the arrangement was in every way satisfactory. He started work at Terling in September, 1905, and left early in 1908.

As early as the year 1876 Rayleigh had been interested in the question of how it is that we are able to distinguish the direction of a sound. It is to be noticed in the first place that this is very different in its nature from the way in which we recognize the direction of a light with the eye, when a picture or image of the source is formed on the retina. During the War, what may be called "acoustic eyes" were tried for locating enemy aeroplanes. In this case a concave mirror replaced the lens of the eye, but for our present purpose this difference is unessential. The important point is that the "eye" for sound had to be 4 feet or more in diameter, to give it the necessary size in relation to the wave length of sound. Nature does not, and indeed hardly could, make use of this method to enable us to tell which direction a sound comes from.

In 1876 Rayleigh made experiments on the subject, to find out in the first place how good the power of discrimination was. The tests were made on the lawn at Terling, and it was found that an observer with his eyes closed, could point within five or six degrees to the direction from which a human voice proceeded.

It appeared however that a pure sound, that is, one from which the octave and higher overtones are absent, gave a different result. A tuning fork with a resonator was the source used, and in this case if the sound came from the side it was easy to say with confidence whether it was right or left. It was, however, impossible to say with certainty whether a pure sound came from in front or from behind.

These results pointed to the conclusion that, in the case of pure tones at least, the difference of effect at the two ears came into play. This was never really doubtful. There

could be no difficulty in telling which of the two ears a small tuning fork was being held to. If the near ear is closed with the finger, the sound is much diminished, whereas closing the other ear makes little difference.

The conclusion that both ears were concerned was confirmed by experiments with Mr. Francis Galton, whose name is well known for his studies on inheritance and kindred subjects. Mr. Galton was almost absolutely deaf with one ear. He came on a visit to Terling in January, 1881, and experiments were tried in the park. I was for the first time privileged consciously [1] to take part in experimental work. Galton made mistakes as regards back and front, like ordinary hearers. But unlike them, he was unable to distinguish right and left.

It appeared clear, therefore, that the two ears work together in discriminating right and left. But, on further consideration, a serious difficulty presented itself. When the pitch is fairly low (say the middle C of the piano or higher) the waves of sound are about 4½ feet long, and therefore fairly long in comparison to the size of the head. But, as is very well known, in connection with optics, in such a case the waves bend round the obstacle, and there is no definite shadow: so that the sound from a distant source is not much less intense at the near ear than at the further one, and it seems difficult to see how this explanation of localizing power can hold good.

Rayleigh had shown in his *Theory of Sound* how to calculate the condensation and rarefaction of the air and hence the intensity of sound at points round the surface of a sphere when plane waves of sound fell upon it, in cases like the one under discussion, when the sphere is not large compared with the wave length. The human head is not of course spherical, but can nearly enough be so represented. On the basis of this calculation it appears that for the middle C, the difference of intensities at the two ears would be only one-tenth of the

[1] I make the reservation because I have heard my father say that as a baby he had made me cry with a sound from a "bird call" so high in pitch that adults could not hear it!

whole intensity. This is little enough for a discrimination; an octave lower the difference would be considerably less than 1 per cent. of the whole, and may be definitely regarded as too small to count. Yet right and left discrimination remains quite easy. The difficulty was explicitly pointed out by Rayleigh in 1876, and seems to have been present in his mind for thirty years, before he returned to the attack, and definitely cleared it up, in 1906.

There is pretty obviously another difference than that of loudness which may enter, which had been in his mind throughout. That is the difference of *phase*. Sound at a given point in air is mechanically an alternate condensation and rarefaction. The maximum condensation at each ear may be simultaneous (agreement of phase). Again maximum condensation at one ear may be at the moment of maximum rarefaction at the other (opposition of phase) or we may have any intermediate case. Was it possible that this gave the clue ?

He had been reluctant to accept this point of view. Generally speaking, if one explanation of a natural phenomenon fails and has to be bolstered up, so to speak, by the partial application of another and quite different explanation, there is reason to suspect that we are on the wrong track altogether. The second explanation savours of an excuse for the failure of the first, and the whole has the appearance of a weak compromise. In any case the explanation by phase difference could not help except for sounds of not very high pitch.

Take the case of a sound wave of such length that maximum condensation reaches one ear at the moment when maximum rarefaction reaches the other. The distance round the head from one ear to the other is about 1 foot, thus the sound wave fulfilling this condition will have a length of about 2 feet, and this corresponds to a pitch rather more than an octave above the middle C of the piano. Now after a small fraction of a second the condensation and rarefaction are reversed in position, and so on more than a thousand times a second for this particular note. Clearly therefore in this particular case we

could have no material for discrimination. It is still more evident that when the wave had half this length, so that maximum condensation at one ear coincided with maximum condensation (of the succeeding wave) at the other ear, there would be nothing to distinguish the right or left position of the source. So that generally it seemed impossible to apply the explanation of phase difference except to the lower pitches, for which the half wave would be longer than the distance between the ears. However, it was precisely for these lower pitches that the alternative explanation, depending on a difference of loudness, was found to fail.

Rayleigh's next step was to design a crucial experiment which would decide whether a right and left effect could in fact be got by a difference of phase. The idea was to use two separate sources of sound, one for each ear; these sources were to be nearly but not quite of the same pitch. If they started fair, then the higher-pitched sound would gain a little on the other at each vibration, and maximum condensation would begin to reach (say) the right ear a little before it would the left. This should give a sensation of sound to the right. The difference of phase accumulates, until the slower fork is one whole period behindhand, and then the cycle is complete; but just before this, maximum condensation would reach the left ear a little before it would the right ear, and the sound would appear to be on the left. The sound should *apparently* be transferred from right to left once during each cycle.

The experiment was arranged with tuning forks and resonators as the sources of sound. They were kept sounding continuously by a well-known electrical device, practically the same as that used in the ordinary electric bell. Each fork was in a different room, and the sound from each was led through a separate pipe to a third room where the observer was seated at a table with his head between the ends of the pipes. The forks used vibrated about 128 times per second, and were put slightly out of tune, by loading one of them, so that the cycle, as judged by the beats when they were listened to together

in the ordinary way, was five seconds. At the very first trial the sound apparently changed over from right to left and back again, in every cycle. The experiment was varied in many ways, and the conclusion was decisively verified that for low notes right and left were distinguished by making use of the difference of phase. As the pitch was raised the sensation was found to become less and less marked, as had been expected from theory, and was found to vanish somewhere near the region of pitch which had been expected.

The question had now been cleared up. It may appear that a theory which appeals to one cause for high notes and a totally different one for low notes has a lack of satisfactory simplicity. However this may be, experiment proved it to be true. It is one more instance of nature's resource in adapting living beings to their environment. Where one expedient ceases, in the nature of the case, to be available, she is ready with another.

As the reader will be aware, original scientific work is published almost entirely in papers contributed to scientific periodicals, and not in book form. This is necessary in view of the rapid progress in recent times. But it has the disadvantage that the average reader can only get at the publication by going to a library where the complete bound series of the periodical is kept. This naturally leads to the custom of scientific authors collecting their papers in one accessible form. Some distinguished authors, for instance Sir David Brewster and William Froude, have not done this, and the omission is a serious detriment to science. Republication has seldom proved a self-supporting venture, and it is sometimes embarrassing to deal with matter that has not stood the test of time.

Rayleigh began this republication in good time, I think on the initiative of the Cambridge University Press, who took the financial risk. In the event the cost to them has been covered, leaving a small profit to be divided with his executors. The edition printed was 1,000 copies. He wrote to his mother (Terling, February 3rd, 1899) :—

"We go to London to-morrow and my lectures begin next Saturday. I feel as usual that I have nothing much to say, but perhaps shall get on well enough when the time comes. The Cambridge Press are going to bring out an edition of my collected works, going back now over 30 years. It will mean a lot of work in writing notes and correcting proofs, but I am rather interested in going over the old ground again. It will run to 3 or 4 big volumes! *Theory of Sound* of course not included."

He tells an amusing incident in this connection as follows: "When I was bringing out my *Scientific Papers* I proposed a motto from the Psalms, 'The works of the Lord are great, sought out of all them that have pleasure therein.' The Secretary to the Press suggested with many apologies that the reader might suppose that *I* was the Lord."

A compromise was reached by putting the motto on another page.

The *Scientific Papers* eventually occupied six large quarto volumes. These appeared as follows:—

I (1869–1881), appeared 1899.
II (1881–1887), appeared 1900.
III (1887–1892), appeared 1902.
IV (1892–1901), appeared 1903.
V (1902–1910), appeared 1912.
VI (1911–1919), appeared 1920 (posthumously).

It will be seen that in about four years' time the republication had been completed approximately to date. At this time he said, "I suppose the fifth will contain an obituary notice." But in the event it appeared seven years before his death. While the material for Vols. V and VI were accumulating, the papers were reprinted to about one year behind the current date, though owing to War conditions they had fallen behind somewhat at the end.

The number of pages is of course a very crude measure of scientific work accomplished; still, if we are considering a single author who always expressed himself in as few words as possible, this measure may serve. It will be found that the rate of production scarcely falls off to the end of the

author's life. Its total number of papers is 466, though some few of these are only short notes.

The part of the sixth volume which had not been passed for press by the author was very carefully revised by Mr. W. F. Sedgwick. It was decided for various reasons not to include an obituary notice, but to prepare the present work as a more adequate account of the author's life. There was little material left in his manuscripts of the same quality as the published papers: and on consideration it seemed undesirable to lower the standard by publishing what he had deliberately laid aside. Two papers which had been partly written out for publication were alone included.

The six volumes of *Scientific Papers*, together with the two volumes of *Theory of Sound* (second edition), contain practically everything of importance that Rayleigh wrote. A few incidental gleanings from other sources are to be found in this book. Some material which had already been republished in *Theory of Sound* is not included in the *Scientific Papers*.

The general style of Rayleigh's papers is concise, though this habit is not carried to the extreme lengths of his hero, Thomas Young. There is a conspicuous absence of anything remotely resembling "purple patches." When the subject is mathematical, the utmost is done by way of illustration by simple cases to bring home to the reader the common sense of the matter before entering on any highly generalized investigation. He often found it easier to begin his paper in the middle, leaving the introduction to be filled in when he was warmed up to the work. Great care was taken over the composition, and it was not done very readily. He remarked late in life that he regretted not having acquired the habit of using a dictionary of synonyms.

It will be noticed by those who study his works that when occasion arises he quotes his own former writings instead of expressing the thought anew. In conversation, too, he often explained things in what was practically a quotation from his published writings. It cost an effort to get it into shape, and this was done once for all.

He confessed to a dislike of shortly summarizing a paper at the end; though he admitted that he was grateful when he found it done by other scientific writers. In a brief summary some qualifications are necessarily left out. It commits the author to more than his fuller statement does. This was perhaps what went against the grain.

It has only been possible to notice in this book a small minority of the subjects treated in his six volumes, and naturally the selection has been made rather from the experimental than the mathematical work. Much of the latter can only be accessible to the few. To give a general idea of the whole I will quote the appreciation of Sir J. J. Thomson in his memorial address in Westminster Abbey (December, 1921) :—

"Among the 446 papers which fill these volumes, there is not one that is trivial, there is not one which does not advance the subject with which it deals, there is not one which does not clear away difficulties; and among that great number there are scarcely any which time has shown to require correction. It is this, I think, which explains that while the collected papers of scientific men often form a kind of memorial tablet in our libraries, respected, but not disturbed, those of Lord Rayleigh are among the most frequently consulted books in the physicist's library.

"The first impression we gain on looking at these volumes is the catholicity of Lord Rayleigh's work. Mathematics, light, heat, electricity, magnetism, the properties of gases, of liquids and solids are all represented in fairly equal proportions. If I were asked to explain in what department of physics Lord Rayleigh's work was most important, I should be quite at a loss to do so. In these days when we speak of electricians, of molecular physicists, elasticians, or even if we take the wider classification of mathematical physicists and experimental physicists, it is refreshing to come across one who like Kelvin and Stokes was each and all of these. Lord Rayleigh took physics for his province and extended the boundary of every department of physics. The impression made by reading his papers is not only due to the beauty of

the new results attained, but to the clearness and insight displayed, which gives one a new grasp of the subject. No subject passed through Lord Rayleigh's mind without being clarified and having its difficulties either removed or brought so strongly into the light as to be subject to attack on every side.

"The impression that one gets after reading a paper by Lord Rayleigh is that the subject, if I may use a homely phrase, has been tidied up. Law and order have been substituted for disorder. There are some great men of science whose charm consists in having said the first word on a subject, in having introduced some new idea which has proved fruitful; there are others whose charm consists perhaps in having said the last word on the subject, and who have reduced the subject to logical consistency and clearness. I think by temperament Lord Rayleigh really belonged to the second group. Certainly no man ever revelled more in that greatest of intellectual pleasures, working at a subject which was all obscured and tangled, and bringing it to a stage where everything was clear and in order. When we take Lord Rayleigh's papers we find some purely mathematical in which, with his characteristic directness of attack, and simplicity of means, he obtained most important results. We get others almost purely experimental, such as the determination of the absolute unit in electricity, in which, again with simple apparatus, he attained results which rivalled in accuracy those of Regnault and Joule. But in the majority of writings we have a combination of mathematical analysis and experimental work, and his papers, I think, afford the best illustration of the true co-ordination of these two great branches of attack on the problems of nature. The physical ideas direct the mathematical analysis into the shortest and most appropriate channels, while the mathematics gives precision and point to the physics."

In supplement to the above, an obituary notice by Sir Joseph Larmor [1] may be quoted.

"The same caution [as he showed in connection with

[1] *Philosophical Magazine*, September, 1919.

psychical research] seems to have affected his physical work, in inhibiting discussion of problems depending on disconnected and shifting hypotheses : in treating of molecular physics his main weapon was general dynamical reasoning, far-reaching principles of the strict classical type, and their statistical application. Though he may fail to attain to the desired goal, he is not under temptation to bridge the discrepancy by unproved suggestion ; the analysis may be supplanted by a better one for the purpose in hand, but it retains the character of a true investigation and an addition to real knowledge. Molecular thermodynamics and theory of radiation are prominent, but special hypotheses and models of molecular structure are largely avoided, even where they would not imply any discordance with normal dynamical theory. In subjects so well founded in many respects as the electron theory, he has refrained from giving public expression to his thoughts perhaps in part for similar reasons, but possibly also from a sense that the problems belonged to others. He was gentle in criticism, and most anxious that ability, of whatever kind, should not be discouraged or overlooked."

CHAPTER XVIII

PUBLIC REWARDS AND ORNAMENTAL OFFICES

A chronological list of the various honorary distinctions which Rayleigh received at different times will be found in Appendix I. Some, however, demand more special notice.

In 1902 the Order of Merit was instituted by King Edward, and Rayleigh was one of the twelve original members. The others were Roberts, Kitchener, Kelvin, Lister, Henry Keppel, E. H. Seymour, Huggins, Wolseley, John Morley, Lecky, G. F. Watts.

Its originally declared intention was to be the reward of exceptionally meritorious service in the Navy or Army, or in Art, Literature and Science. Politicians as such were pointedly omitted. In practice however the tendency has been increasingly in the direction of conferring the Order upon them. Rayleigh with others went to Windsor to be invested, and about the same time a dinner was given to the members of the Order at the Athenæum Club with Lord Avebury in the chair. Rayleigh, in replying to the toast of his health, said that "the only merit of which he personally was conscious was that of having pleased himself by his studies, and any results that may have been due to his researches were owing to the fact that it had been a pleasure to him to become a physicist."

Speaking generally, he did not take honorary distinctions and orders very seriously. I have heard him quote with approval a remark that "they reduced men to the same level as women—not knowing what to wear." But I have the impression that the Order of Merit pleased him, though I do not remember his saying anything explicit to this effect. At

the time of his death he was the senior member. In 1912 he represented the Order at Lord Lister's funeral, acting as a pall-bearer.

Towards the end of 1903 a hint from Stockholm was received by some of Rayleigh's scientific friends that he would be an acceptable candidate for the Nobel Prize for Physics. The formal proposal to the Nobel Committee was made by Lord Kelvin and dwelt particularly on his work on the density of gases, leading up to the discovery of argon, and his subsequent close examination of the laws of pressure of gases.

A number of other leading physicists signed another copy of the same statement, which was forwarded independently: and in due course the prize was awarded. At the same time the Nobel Prize for Chemistry was awarded to Sir William Ramsay. Rayleigh had himself earlier proposed Lord Kelvin, but the view was apparently taken that the more important part of the latter's work dated too far back.

Lord Kelvin wrote, December 27th (1904):—

"It is very kind of you to write as you do about the Nobel Prize. But your own work of the last 20 years is absolute proof that the award is right.

"It was very sad to see poor Gordon's breaking down these last two years."

The proceedings at Stockholm are best told in Rayleigh's own words.

He wrote from the Grand Hotel, Stockholm, December 11th, 1904:—

"The big function is happily over. At 7.30 we met in the large music hall, the King in the Chair, and several members of the Royal Family present, with the rank and fashion of Stockholm. There were long speeches interspersed with songs. After general laudation of Nobel and his objects, our individual merits were set out at great length.

"All this part was in Swedish, and I need not tell you that I did not even know when to blush. Finally we were each (Russian and all) addressed in our own tongues, and the King handed the medal and diploma. At about 9.30 we sat down to the banquet at this hotel, the Crown Prince in the Chair. The toasts followed,

mine coming last. The affair was very polyglot. My health was given in *Latin* by Prof. Hasselberg. I spoke loud and slow in reply, and I believe I gave satisfaction.

"The whole thing was very interesting, and I have scarcely ever met more distinguished people. The hospitality and kindness were wonderful, but regard for forms seems to be extreme, and their customs differ rather widely from ours. After supper I talked with the Prince, who seemed quite to put one at one's ease. The conclusion did not come till past 2 a.m."

He wrote, December 13th, 1904, from Stockholm :—

"The whirl continues. Last night Ramsay and I dined at the palace. The old King was very kind, leading me by the hand to show me his treasures! All the time I have occasion (tell Robin) to curse my inability to talk French. But nothing could exceed the kindness of the people.

"On Sunday we went into the suburbs to lunch with Mittag-Leffler—great ceremony, English flag flying, speeches, etc. He has a magnificent mathematical library, more like a public than a private one.

"This afternoon we lunch at 1, and dine at 7, and I lecture at 3.

"People here all sympathize with Japan, and are indeed apprehensive that Russia may go for them at any time. We were told that there were spies about at Mittag-Leffler's."

Before leaving Sweden, he visited Upsala, and on the home journey he and Ramsay made a short stay at Copenhagen. There he had the opportunity of seeing Prof. Ch. Bohr, and discussed with him the curious anomaly in the behaviour of oxygen at low pressure, which Bohr had found, but which Rayleigh had not been able to recover. The discussion did not throw much light on the discrepancy.

At Copenhagen he also took the opportunity of seeing Thorwaldsen's statue, Christus, a copy of which had been placed by his brother Richard above the altar of Terling Church.

He decided to present the Nobel prize money, amounting to about £7,700, to his Alma Mater, Cambridge University. He consulted his successor in the Cavendish Professorship,

as to the allocation of the gift, and the latter wrote (May 14th, 1905) :—

"I have been thinking a great deal about your donation ever since your first letter, and have talked over the needs of the laboratory with many people here. I think there can be no doubt that our most pressing need is now space for research ; at present we are much overcrowded. We have generally about 30 researchers at work, and they are packed like herrings in a barrel. I have not had a room to myself for some years. The scheme I have in my mind is to build a research laboratory on the site of Mortlock's garden, just beyond the room in which you used to work. I think it would be advisable to make this in some degree a separate building, and not merely an extension of the Cavendish Laboratory.

"My reasons for this are (1) that I am sure the University would wish to make as prominent as possible the fact that it once had you for a Professor, and that could not be better done than by having a Rayleigh Laboratory. (2) At some future time it may be possible for the University to have two Professors of Physics, in that case I think it would prevent friction if they could have separate laboratories.

"As far as I can estimate a laboratory sufficient for the purpose I have indicated could be built for about £5,000."

Accordingly £5,000 was allocated to the proposed extension of the Cavendish Laboratory ; the remainder, at the suggestion of J. W. Clark, was to go to the University Library, to form a fund for the purchase of foreign scientific books, especially periodicals.

After Rayleigh's term of office as secretary of the Royal Society the question of his occupying the more ornamental office of president was frequently raised. Thus Sir Michael Foster wrote (1900, undated) :—

"I hope you fully recognize that you are due as P.R.S. next Nov. 30th. We shall have to arrange before we disperse in July. You *must* take it.

"Of course we would lighten your work as much as possible—you need not attend ordinary meetings."

The other secretary of the Royal Society, Sir Arthur Rücker, wrote (June 8th, 1900) :—

"I don't want to put any unfair pressure upon you, but I doubt if you quite realize how deep the general disappointment will be if you are not the next President. Everyone wants you."

An informal meeting of the Council was held on October 18th, 1900, and the following memorandum was signed by the retiring president, Lord Lister, and the other members present.

"The members of the Council of the Royal Society, after due consideration of all the circumstances, unanimously desire to convey to Lord Rayleigh the expression of their belief that his acceptance of the office of President at the present time would greatly promote the best interests of science in this country.

"In the event of his permitting himself to be nominated, the Council would be anxious that the duties of the office should be so far lightened as to interfere as little as possible with the scientific work which they hope that Lord Rayleigh will be able to accomplish for many years to come."

The document was forwarded by Sir Arthur Rücker, who wrote (October 18th, 1900) :—

"I may perhaps be allowed to add that in the belief of those present the action they have taken to-day represents the unanimous opinion of the Fellows of the Society."

Rayleigh wrote in reply :—

"I was sorry to receive the document signed by many members of the Royal Society, because it puts me in the disagreeable position of having to decline a very great honour. But a man must decide for himself what he is capable of undertaking, and the more I consider the matter, the more I am convinced that the necessary work of the presidency, combined with the work to which I am pledged (e.g. in connection with explosives) would entail a practical abandonment of my own experimenting. I am not prepared for this at present, and I even flatter myself that it would not be in the interest of Science."

Sir Michael Foster reproached him when they next met, but he added, "And what's worse, I believe you are right."

Sir William Huggins was elected.

At the end of his customary tenure of five years, the proposal

came up again, rather unexpectedly, as a biologist, in the ordinary course, would have naturally been chosen.

Sir William Huggins wrote, October 27th, 1905 :—

"I do hope that you will consent to serve the Society as President. There is no doubt that you are the *only Fellow* whose election *will not be opposed*. The feeling both in the Council and outside is *very strong* in your favour."

This time he consented.

He gave the usual presidential addresses at the anniversary meetings on November 30th, during his time of office (1906, 1907, 1908). A few extracts may fittingly be given from these. I have taken slight liberties in abbreviating passages and putting them into consecutive form.

1906.

Much of the activity now displayed in Mathematics has taken a channel somewhat remote from the interest of the physicist, being rather philosophical in its character than scientific in the ordinary sense. Much effort is directed towards strengthening the foundations upon which mathematical reasoning rests. No one can deny that this is a laudable endeavour ; but it tends to lead us into fields which have little more relation to natural science than has general metaphysics. One may suspect that when all is done, fundamental difficulties will still remain to trouble the souls of our successors. Closely connected is the demand for greater rigour of demonstration. Here I touch upon a rather delicate question, in which pure mathematicians and physicists are likely to differ. However desirable it may be in itself, the pursuit of rigour appears sometimes to the physicist to lead us away from the high road of progress. He is apt to be impatient of criticism, whose object seems rather to be to pick holes than to illuminate. Is there really any standard of rigour independent of the innate faculties and habitudes of the particular mind ? May not an argument be rigorous enough to convince one thoroughly imbued with certain images clearly formed, and yet appear hazardous or even irrelevant to another exercised in a different order of ideas ? Merely as an example, there are theorems known as "existence theorems" having physical interpretations, the object of which is to prove formally what to many minds can be no clearer afterwards than it was before. The pure mathematician will reply that even if this be so, the introduction of electrical or

thermal ideas into analysis is illogical, and from his own point of view he is of course quite right.

As more impartially situated than some, I may perhaps venture to say that in my opinion many who work entirely upon the experimental side of science underrate their obligations to the theorist and the mathematician. Without the critical and co-ordinating labours of the latter we should probably be floundering in a bog of imperfectly formulated and often contradictory opinions. I shall not be supposed, I hope, to undervalue the labours of the experimenter. The courage and perseverance demanded by much work of this nature is beyond all praise. And success often depends on what seems like a natural instinct for the truth—one of the rarest of gifts.

1907.

Accuracy of measurement appeals less to the lay and scientific public than discoveries promising to open up new fields; but though its importance at any particular stage may be overrated, it promotes a much-needed consolidation and security in the scientific edifice.

The history of science shows that important original work is liable to be overlooked, and is perhaps the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston, and B. Stewart will suggest themselves to the physicist. In looking into the more recent progress of geometrical optics I have been astonished to find how little correlation there has been between the more important writings. That Coddington should have remained unknown in Germany and Von Seidel in England need not greatly surprise us; but in this subject it would appear than a man cannot succeed in making even his own countrymen attend to him. Coddington seems to have heard nothing of Cotes and Smith, and Hamilton nothing of Airy and Coddington.

It is true that no two writers on theoretical subjects could differ more in taste and style than do Hamilton and Coddington. The latter addressed himself to special problems which seemed to have practical importance. Among his achievements was the rule relating to the curvature of images, generally known as Petzval's, though Petzval's work was of much later date. Hamilton, on the other hand, allowed his love of generality and of analytical developments to run away with him. In his memoir on Systems of Rays with its elaborate and rambling supplements there is little to interest the practical optician, though the mark of genius is throughout apparent. It was only in two or three pages of a later

paper that he applied his powerful methods to the real problem of Optics. As Finsterwalder has remarked, his "six radical constants of aberration," expressing the general properties of a symmetrical instrument, are at once an anticipation and a generalization of Von Seidel's theorems. But the published work is the barest possible summary. If Hamilton had been endowed with any instinct for Optics proper, he could have developed these results into a treatise of first-class importance.

I have spoken of English work that lay neglected, but a scarcely less notable instance is the splendid discovery of the microscopic limit by Fraunhofer, a man who combined in the highest degree practical skill with scientific insight. Thanks to the researches of Abbe and Helmholtz, it is now well known that there is a world that lies for ever hidden from our vision, however optically aided; but neither of these eminent men realized that the discovery had been anticipated by Fraunhofer. Some perhaps may doubt whether Fraunhofer's argument, founded upon the disappearance of spectra from gratings of extreme fineness, is of adequate cogency. To this I may reply that I was myself convinced by it in 1870, before Abbe or Helmholtz had written a word upon the subject.

1908.

Dr. Sorby belonged to a class on whom England has special reason to congratulate herself—men who pursue science unprofessionally. The names of Cavendish, Young, Joule, and Darwin at once suggest themselves. It is to be feared that specialization and the increasing cost and complication of experimental appliances are having a prejudicial effect in this regard. On the other hand, the amateur is not without advantages which compensate to a certain extent. Certainly no one who has the root of the matter in him should be deterred by fears of such difficulties, and the example of Sorby suffices to show how much is open to ingenuity unaided by elaborate appliances.

A very interesting observation published during the year is that of Hale upon the Zeeman effect in sun-spots, tending to show that the spots are fields of intense magnetic force. Anything which promises a clue as to the nature of these mysterious peculiarities of the solar surface is especially welcome. Until we understand better than we do these solar processes on which our very existence depends, we may do well to cultivate a humbler frame of mind than that indulged in by some of our colleagues.

Rayleigh was not, I think, specially successful in promoting discussion when he occupied the chair at the reading of papers

before the Royal Society. The difficulty of such discussion always is that the author is or should be much better informed than anyone else. If others are to contribute, beyond the mere asking of questions, they must be prepared to a certain extent to risk "giving themselves away." It was not natural to Rayleigh to do this—caution and restraint were the key-notes of his scientific character. He felt too the difficulty arising from increasing specialization, and the want of consideration of authors for the limitations of their audience. Privately he often quoted with relish a saying attributed to Dalton when in the chair at a scientific meeting: "Well, this is a very interesting paper for those that take any interest in it."

At the biological meetings, he seldom felt qualified to say anything, and often deputed the presidential duties on these occasions.

After three years of the office, he retired, in the face of some opposition on the part of colleagues, who wished him to complete the usual five years' tenure. One reason was that he wished to take a long holiday abroad. Other reasons are explained in the following extract from a letter to Sir Richard Glazebrook :—

TERLING, *Jan. 5th*, 1907.

"As to R.S. I think I shall have had enough after three years, which is really all that I ever contemplated. I am not specially efficient, being rather deaf, to hear easily at Council, and being so bad a linguist that I cannot take a proper part with foreigners. Some people forget that if I had wanted a life of administration and functions I should never have given attention to science at all."

In 1891 it had been suggested that Rayleigh should become High Steward of the University of Cambridge, but he decided not to stand for this office.

In 1908, on the death of the Duke of Devonshire, the question arose who was to be his successor as Chancellor of the University. Rayleigh's name was suggested among the Fellows of Trinity, and the suggestion gained favour in a wider circle.

An informal meeting of members of the Senate was held, and his name received the support of a large majority of those present. A memorial in favour of his election was signed by many members of the Senate, including eleven heads of colleges and twenty-six professors. It was felt that he would not care to run the gauntlet of a contested election.

On April 11th, 1908, he was unanimously elected.

His formal letter of acknowledgment ran thus :—

TERLING, *April 18th*, 1908.

MR. VICE-CHANCELLOR AND GENTLEMEN OF THE SENATE,—

It is with feelings of pride that I accept the exalted office to which you have done me the honour of electing me. Though lacking in many of the qualifications possessed by the honoured chancellor whose loss we deplore, I may perhaps claim that my connection with the University has been intimate, and that my interest in it has remained unabated for nearly 47 years. In the course of nature many of my early friends and teachers to whom I owe so much have left us ; but I am still in close relation with some of the younger and active men among you who are carrying forward the lamp of Cambridge learning and science, in accordance with the great traditions which they have inherited.

It will be my endeavour, as far as in me lies, to assist in maintaining your privileges, and in the furtherance of the great work in which the University is engaged.

I have the honour to remain,
Your obedient servant,
RAYLEIGH.

The election was a considerable departure from precedent : for since 1688 previous Chancellors with one exception (Marquis Camden) had all been of the rank of a Duke, or higher. I think he was pleased at this mark of appreciation from the University, but he was himself inclined to think that they would have done better to choose some one more influential in public affairs. "I do not understand what they want a scientific person for," he said.

He was congratulated by the Chancellor of a sister University on the dignity he had attained without the unpleasantness of a contested election !

Prof. G. D. Liveing had kept the Duke of Devonshire

informed on University matters, but he was contemplating immediate retirement and did not wish to continue. He said further: "May I add how glad I am that you have undertaken the office of Chancellor. It is centuries since we have had anyone in that post who had ever taken an active part in the ordinary business of the university."

Prof. J. J. Thomson, Rayleigh's successor as Cavendish Professor, undertook to keep him in touch with what was going on.

The inaugural ceremony took place on May 1st, 1908, in the large drawing-room of Arthur Balfour's house, 4 Carlton Gardens. The object of having it in London was presumably because it would have been almost impossible, at short notice, to arrange the ceremony of installation at Cambridge with the customary conferring of honorary degrees; and the University could not constitutionally remain without a Chancellor in the meantime.

The principal officers of the University attended. The Chancellor entered in his robes, attended by his page.[1] He was welcomed in a speech by the Vice-Chancellor, Dr. E. S. Roberts. In his reply he dwelt on how much he had owed to his own University training, and made some reference to his work on the Commission of 1877 and to the financial needs of the University.

A number of members of the Senate were present, and I fell into conversation with one of them—an elderly country clergyman, who I believe did not identify me. His remarks gave what I thought was an interesting sidelight. He said: "I am very glad Lord Rayleigh has been chosen as Chancellor. I once took the duty in Terling Church, and he was present at both morning and evening services. A *very* good churchman."

The Installation at Cambridge took place on June 17th. It is the customary privilege of the Chancellor to nominate the recipients of honorary degrees on that occasion, and I remember my father discussing whom he was to choose.

[1] Lord David Cecil.



Installation of Lord Rayleigh as Chancellor of Cambridge University, June, 1908. To the right are seen Mr. Asquith, the Duke of Northumberland, Lord Halsbury, and Sir John Fisher (the

I mentioned a view which had been advocated by Herbert Spencer, that it was an opportunity to lead public opinion by honouring younger men who were insufficiently recognized. But this point of view did not commend itself. He said that a pageant fell flat if people were not familiar with the names of the actors in it.

The list was ultimately made up thus:—

The Rt. Hon. H. H. Asquith.
The Duke of Northumberland.
The Earl of Halsbury.
Admiral Sir John Fisher.
Sir Hubert von Herkomer.
The Hon. C. A. Parsons.
The Rt. Hon. Sir G. O. Trevelyan, Bart.
Sir James H. Ramsay, Bart.
Sir Andrew Noble, Bart.
Sir W. Crookes.
Mr. Rudyard Kipling.
Mr. Alfred Marshall.
Professor G. D. Liveing.

Sir Hubert von Herkomer in recognition of the compliment presented to the University a portrait of the Chancellor in his robes. It hangs in the Fitzwilliam Museum.

The Chancellor, with Lady Rayleigh, arrived at Cambridge on June 16th, 1908, and was received by the Vice-Chancellor and Registrar, who conducted him to Trinity College, where he was met at the great gate by the Master and Fellows.

In the afternoon he went in state to the Cavendish Laboratory. I happened to pass the procession in the narrow approach in Free School Lane, and I saw a twinkle in his eye as it met mine. No doubt he felt there was an element of the ludicrous in being conducted in this way, in scarlet gown, with two mace-bearers walking before him, to the place where he had daily laboured with his hands at the hard work of electrical measurement.

The new wing of the laboratory which had been built out of the funds which he provided (p. 315) was now complete, and he formally opened it. There was a reception afterwards with experiments on view. Some of these were in illustration of Rayleigh's work.

The next morning the Chancellor held a levée in the Fitz-

william Museum, where the heads of colleges, and then the Council and other members of the Senate, were presented. A luncheon in the hall of Caius College followed, with speeches. Sir Andrew Noble announced that several of Lord Rayleigh's friends, non-resident members of the University, proposed, in order to express the gratification of the scientific world at his election, to offer to the University a fund large enough to provide an annual award to be associated with the name of the Chancellor. This took effect in the foundation of a Rayleigh Prize, awarded under similar conditions to the much older Smith's Prize. After luncheon, the usual procession was formed round the Senate house yard. The Chancellor wore his academic robes, and underneath wore evening dress, with knee breeches, and the Order of Merit. His youngest son Willie acted as page. After taking his seat he made a short speech, referring to his predecessor in office, and thanking the University for their confidence. The honorary degrees were then conferred, and the procession left the Senate house.

Later in the day there was a garden party at King's College, and a dinner with speeches at Trinity. The next morning he called at all the colleges, and returned to London at midday.

As Chancellor he did his best to continue the work of his predecessor in collecting money for the rapidly expanding needs of the University. He had already made his own contribution of the money he had received as the Nobel Prize. It may be that most of the likely givers had already been approached by the Duke of Devonshire ; at any rate I do not think that Rayleigh's efforts in this direction were specially successful. He wrote to Andrew Carnegie, and received the following reply :—

Skibo Castle, Sutherland,
Sept. 2, 1908.

My dear Lord Rayleigh,—

It was a matter of great rejoicing to me, and to many in the United States, when you were made Chancellor. It was indeed a sign of the times. I am not ignorant of the position Cambridge

has attained in the scientific world, and especially of the work of your Professor Thomson.

Let me confess that I should have some hesitation in offering money to Oxford or Cambridge, whose alumni are so rich.

Mr. Gladstone asked me once to give £9,000 to bind the books in the Bodleian Library. I replied: "I have not impudence enough. The rich alumni could not but feel that it was a reflection upon them for a stranger to step in." I know how Americans would feel if Britain had to make gifts to their institutions, ranking them with other heathens to whom large contributions go.

With the Scottish Universities it was different, but after reading the report of the commission on these, I could not entrust money to their faculties and selected a committee, of which your honoured brother-in-law, then Prime Minister, is a member. When I explained this, his was the first voice I heard, saying, "Not a penny, Mr. Carnegie, not a penny."

Frankly, I feel the same about Oxford, and also about Cambridge in a lesser degree. Both of these institutions commit the offence of requiring scientific students to waste time studying a dead language, an insult to Science, which has been the Cinderella of the family of knowledge quite long enough. I believe, with your great brother-in-law, that it is to science that we have chiefly to look for the future progress of Man.

Pray excuse this long epistle, but I have watched your career so long with such admiration that I had to give you my views.

Very truly yours,

ANDREW CARNEGIE.

Rayleigh also made a public appeal for funds in the *Times* (October 12th, 1908).

It is not often that the intervention of the Chancellor in University affairs is called for, but there was one such case—the appointment to the Lowndean Professorship of Astronomy and Geometry, vacant by the death of Sir Robert Ball in 1914. The electors to the appointment were evenly divided, and the Vice-Chancellor, Dr. M. R. James, thought it better to refer it to the Chancellor rather than to give the casting vote himself. Dr. H. F. Baker was appointed.

In one other case it appeared likely that he would have to intervene. The Master of one of the colleges was past his work, but could not readily be brought to realize the fact,

and the Chancellor was approached with a view to his holding a visitation. Fortunately the necessity was averted, as the Master was prevailed upon to appoint a deputy.

It was Rayleigh's duty as Chancellor to give interpretations of the statutes in cases where difficulty arose: a story was current in the University that on one occasion the reply came back so promptly that he could hardly have resorted to high legal opinion as was customary. But, on hearing of the surmise, his answer was that as one of the University Commissioners of 1878 he had helped to make the statute, and so must be presumed to know more about its intention than any legal expert.

The visits of Chancellors had previously been rare events and were treated as such, and Rayleigh was known to say humorously that he could not well go to Cambridge for a quiet visit as his presence there would upset the working of the University.

He went officially for the Darwin centenary in 1909, and, as Sir Joseph Larmor informs me, it was a matter of remark among the delegates from Continental Universities, to the great content of Cambridge men, that only in England could a man who had shed distinction on an ancient University as a Professor be called to its highest ceremonial office.

On the evening of June 23rd, the Chancellor received the delegates at the Fitzwilliam Museum, and on June 24th gave an address in the Senate house in his robes. The following are the more interesting passages of the speech :—

"They had met to celebrate the centenary of the birth of Charles Darwin and the 50th anniversary of the publication of the *Origin of Species*. He was old enough to remember something of the stir caused by the latter event. To many, the results of Darwin's speculations were unwelcome, and probably remained so, at least in their application to the origin of man. Fifty years ago it would have seemed a bold prophecy to predict that day's celebration. They might perhaps take it as proving that Cambridge was not held so fast in the bonds of mediævalisn as some would have them suppose. They were prepared to face whatever strict methods of investigation might teach to be the truth. He need not remind

them that on many important questions raised by Darwin's labours opinions still differed, and he imagined that Darwin would hardly recognize as disciples some of the distinguished biologists who were met there to do honour to his name. He did not attempt even the briefest survey of those labours. They would presently hear appreciations from men of distinction well qualified to instruct them. What appealed to all was the character of the man, loved by every one who knew him and admired by every one with a spark of the scientific flame. It was a pleasure and a stimulus to think of him working on in spite of ill-health in his study, his garden, and his hot-houses, and from his retirement moving the minds of thinking men in a manner almost without parallel.

"He (the Chancellor) esteemed himself fortunate that a visit nearly 40 years ago, which he owed to his friend now Sir George Darwin, enabled him to picture the scene. He was struck as were others with his wonderful modesty. On his (the Chancellor's) propounding some difficulty in connection with colour-vision and the theory which attributes the colours of flowers to the presence of insects, he remembered that Darwin asked time for reflection before making a reply. His enthusiasm also impressed him much. That characteristic must have remained to the end. Commenting on it only a short time before the deaths of both of them, Frank Balfour, himself a strenuous and sympathetic worker, remarked to the Chancellor that he wished he could be as much interested in his own subject as Darwin was in other people's subjects."

The delegates then presented their addresses to the Chancellor, and two of them, Hertwig and Metchnikoff, addressed the meeting. The next day the Chancellor conferred the honorary degrees.

In 1910 it fell to him to present an address from the University to King George on his accession.

In 1911 the University authorities were alarmed at proposals which, they considered, threatened to hamper the right of the University library to free copies of all books. Rayleigh spoke in the House of Lords against an amendment to the copyright Bill in this connection (Hansard, December 4th, 1911) :—

"It is natural enough," he said, "that the publishers should object, one cannot be surprised at that. The publishers are men of business—many authors think the publishers are rather too much so, I believe—but anyhow they do not undertake the publi-

cation of a book unless they see their way to a probable profit upon it, and they take care that at any rate part of the burden should be shared both by the writers and by the readers of the book. It seems to me that it would be a step very much in the wrong direction to hamper the action of the Universities in this matter. . . ."

Rayleigh also spoke shortly on April 22nd, 1914, in defence of the view of the University that religious tests should not be exacted for Degrees in Divinity. Bishop Moule, of Durham, had moved in a contrary sense, but did not press the matter to a division.

Rayleigh's later visits to Cambridge as Chancellor were :—

June 14th, 1911, to confer honorary degrees.

July 15th, 1912, for the same purpose. The recipients were chiefly foreign delegates to the 250th anniversary celebration of the Royal Society.

August 23rd, 1912, to receive the International Congress of Mathematicians.

June 9th, 1914, for the opening, by Prince Arthur of Connaught, of the new physiological laboratory, which the Mercers' Company had presented.

This was his last official visit. Naturally all University ceremonials were in abeyance during the War. So far as I can trace, he was never at Cambridge again.

CHAPTER XIX

LATER YEARS

Rayleigh had always had a wish to see the South Sea Islands, and he and Lady Rayleigh contemplated spending the winter of 1908–9 in making an expedition there, when Lord and Lady Selborne invited them for a visit to South Africa, of which he was then High Commissioner. This was too good an offer to be refused, and they tried to think that they could take it on their way to the South Seas. However, on consideration, the cold and rough voyage from the Cape to Sydney was a deterrent, and Rayleigh never saw the Pacific.

They started from England on November 7th, 1908, with Lady Selborne to join Lord Selborne, who was already in South Africa.

After an interesting stay at Cape Town, Rayleigh and Lady Rayleigh went on alone to see Kimberley and the Victoria Falls.

In the train on the way back to Pretoria he was taken ill with dysentery, but fortunately they were able to get to Government House before he was unable to travel, and so he was laid up among friends. Alarming and unauthorized reports of his condition appeared in one of the London dailies and gave unnecessary anxiety to his friends.

He wrote from Government House, Pretoria, December 27th, 1908 :—

" You have heard I believe what a miserable time I had the day before arriving here. After a week or so in bed I am supposed to be recovered, but feel very weak. . . . I enjoyed the visit to the Falls, but yet feel that I am getting too old for long travelling.

Probably we shall give up most of our projects, and come up to the Mediterranean by the East Coast. . . . I was amused to see by the *Times* that I delivered an address at Burlington House on Nov. 30th.[1] The only thing they mention is my remark by the way about flying machines."

The South African trip ended in Natal, where they visited the Governor, Sir Matthew Nathan, at Pietermaritzburg, and sailed from Durban on the *Amiral*, belonging to a German line which had the English mail contract. The captain explained that this did not pay, and therefore was not competed for by an English line.

A few days were spent at Zanzibar, where they had tea with the Sultan at Chukwani Palace on February 5th, 1909.

During the voyage up the African coast Rayleigh had occupied himself with observing the colour of the deep sea. He was inclined to think that it was mainly due to reflection of the blue sky. He made some mention of his observations later in a lecture at the Royal Institution (February 25th, 1910). Observations made in recent years by Raman and others seem to make it probable, however, that the scattering of light by the water molecules themselves intervenes to a material extent, the water having a blue of its own comparable with that of the sky, and due to similar causes.

They landed at Port Said, and found that there was no opportunity of getting on to Naples for several days. So, to avoid the stay in that uninviting spot, they took ship the same day for the Holy Land, where they stayed about a fortnight. They went on to Naples via Malta and Messina. The famous blue grotto at Capri was visited, and this was of interest in connection with the colour of sea water. They arrived back in England about the 15th of April, 1909.

Rayleigh did not return from the African trip in particularly good spirits. Perhaps this was the after-effect of his illness at Pretoria. He said he was too old for travelling, and expressed doubts whether he had any working power

[1] It was read in his absence.

left. "But," he said, "I think I shall have a look at the colour of water." This was to test the colour of water as seen by absorption in long tubes. The question had been suggested by his observation at sea. He was soon at work again on this and other problems.

From this time onwards for the rest of his life, Rayleigh was without mechanical assistance in the laboratory, except for occasional help from myself. The energy that was left to him after the many public calls upon his time had been discharged, was not enough to keep an assistant regularly busy; and to have one partly idle would have chafed him intolerably. Without an assistant experiments requiring elaborate preparation were out of the question. Accordingly, he devoted himself in the main to the mathematical side of his work, which, except during the climax of the argon period, had always claimed a considerable share of his energies. "I have the technique of that, and not of the other," he said. Sometimes he doubted whether the supply of problems of the kind that suited him would last out, but in the event they did. I was sometimes asked by scientific friends whether the material he was publishing so regularly was drawn from a store accumulated in earlier years. This was not the case. It was all red-hot from the anvil.

He did not give up experimenting entirely, and a number of experimental results are recorded in the sixth and last volume of his *Collected Papers*, which roughly covers this period. These, however, were all such as could be made with simple appliances, and little preparation. Most of them were concerned with optics, though a few dealt with capillarity and kindred subjects. They were chiefly carried out in the book-room, or in the first room of the laboratory adjoining it, known from former times as the school-room (p. 153). This was more adequately warmed then the rest of the laboratory, with the advantage that it was not necessary to go upstairs, while the progress of anything that took time could be conveniently watched in the intervals of reading or writing. The rest of the laboratory got into a state of considerable

confusion, as there was no one to wash up dirty vessels, or to put away things that were done with.

He was now sixty-seven years of age, and I think that during this part of his life he felt theoretically, at least, that his main work was done, and that he was free to work or not as he pleased. But in practice there was little if any slacking off. The truth is that his work was his chief pleasure and interest. His clearness of judgment was not in the least impaired to the end, though naturally he became less enterprising in entering new and unfamiliar fields. I once asked him what he thought of Huxley's remark that a man of science did more harm than good after the age of sixty. He said, "That may be, if he undertakes to criticize the work of younger men, but I do not see why it need be so if he sticks to the things he is conversant with." He rather deplored this tendency during his friend Lord Kelvin's later years. He thought that Kelvin would have done better to have left radioactivity and kindred subjects alone. "It does not do his reputation much good," he said. "He would do much better to work up the mathematical notes that he has for publication." [1]

Life went on at Terling much as before, in the years previous to the War. There was, however, a deep shadow cast over that period by the illness and death of Rayleigh's youngest son, William Maitland. He had been preparing for a career at the Bar, when he was struck down by spinal disease. Hope gradually faded and on November 22nd, 1912, he died. He had no scientific tastes, but during his last illness he wrote his *Musical Reminiscences*, which were privately printed after his death. Later on, in 1916, they were published at the suggestion of Sir Thomas Beecham and others. They are considered of interest by some who are well qualified to form an opinion. My youngest brother had a great capacity for friendship, and a wide circle of friends.

[1] I learn, however, from Sir Joseph Larmor, who acted as Lord Kelvin's scientific executor, that there was less unworked material of this kind than might have been expected.

After this Lady Rayleigh went abroad with friends for a rest and change. Rayleigh did not accompany her, but stayed in London at my house in Onslow Square (February, 1913). He had not been there long when he was attacked with pleurisy, and for a few hours there was some anxiety, as he realized himself. "I should like to live for another five years," he said. But a somewhat longer span than this was allotted to him.

He wrote to Sir Arthur Schuster to acknowledge the award of the Rumford Medal by the Royal Society (Terling, November 7th, 1914):—

"I am pleased that my optical work should be appreciated. It has been done perhaps more *con amore* than any other. But I hope I have not kept a good and younger man out."

At the beginning of the war period the Bedfordshire Yeomanry were quartered at Terling and in a neighbouring village. Many of the officers were personal friends, and were frequently entertained at Terling. Two or three of them were quartered in the house, and lived with the family for some months.

During this time the idea of a German invasion was very much in the popular mind, and our friends of the Yeomanry, some of whom had gone away for leave at Christmas, 1914, were suddenly recalled by telegram on Christmas Eve, presumably on account of a scare of this kind.

Terling seemed very much in the line of a possible German raid on London, and the question of hiding or removing one or two of the more valuable pictures was considered. But Rayleigh decided that on the whole it was safer to leave them where they were.

In subsequent summers during the War (1916–17) there was a large camp in Terling Village of some 5,000 men. The site was chosen chiefly for the good water supply. The tents were pitched on the cricket field and adjacent fields. The fall of a burning Zeppelin at Billericay, some 12 miles distant, was watched from the house, to the accompaniment of cheers

from the soldiers, who had been turned out from their camp into the park on the alarm being given. Viewed through the misty air, it had the fiery-red colour sometimes seen in the setting sun.

The guns on the Western battle front were often audible in the autumn of 1917, and the explosion of ammunition dumps, when the Germans were retiring in 1918, violently shook the window frames.

During this period Rayleigh's advice was constantly sought on scientific questions connected with the War, particularly the acoustical detection of aeroplanes and submarines. In Vol. VI of the *Scientific Papers* will be found memoranda on "The Cone as a Collector of Sound," "Propagation of Sound in Water," and "On the Possible Disturbance of a Range-finder by the Motion of the Ship which carries it." These were written in answer to appeals for his guidance.

In addition there was the business of the National Physical Laboratory and the Advisory Committee for Aeronautics, both of which were working at high pressure on War problems. One symptom of this activity was the large number of type-written reports and memoranda which overflowed the available space and were piled on the book-room floor. In the case of each of these committees, he was able to remain at the helm till the close of the War.

During the last period of the War life at Terling as elsewhere was a good deal hampered by lack of supplies of various kinds. To economize fuel and light it was necessary to live in one sitting-room (the dining-room) during the winter of 1918, though Rayleigh had a special allowance of coal that enabled him to use the book-room for his work. Other economies of various kinds seemed worth while. Sheep were tethered on the grass in front of the house. The flower-beds were filled up with potatoes and sugar-beets, and Rayleigh himself sawed up firewood and helped in making the hay grown on the unmown lawns. Owing to coal restrictions, Terling depended, in the main, on wood fuel, supplied by the windfalls of 1917, which had lain uncleared where they

fell for lack of labour. The logs from this source were sodden with wet and burned most uncheerfully.

In his later years, when he was not equal to much walking, Rayleigh was usually taken out for a drive by Lady Rayleigh, in a carriage drawn by two ponies, the lineal successors of those which used to drive him down to the Cavendish Laboratory in the early 'eighties. To get more variety than the roads in the neighbourhood afforded, they would often walk across the fields, or along some country footpath, sending the carriage round to meet them at the other end.

After coming in he usually had a nap in his arm-chair in the book-room. He had always been a good sleeper, and required more sleep than most men.

Rayleigh had long been interested in the problems of aeronautics. Kite-flying had been a favourite pursuit of his boyhood, and he amused himself with sending up a kite carrying a lighted lantern, or a ball of tow soaked in turpentine. This seems to have caused considerable alarm in the rustic mind. Thus his father wrote (date uncertain) :—

"Coming through Fuller Street from Braintree Markets yesterday the Steels saw some women looking at John's fiery kite and saying these must be the signs foretold in Scripture, and the end of all things must be at hand."

Possibly it was at this time that his first interest in aeronautics arose.

In 1897 he tried kite-flying again, with a box kite, which was tethered in the park and remained up all night. It considerably surprised a party of visitors from London when they saw it on their arrival. I think a wire was tried instead of a string, but with what success is not recorded.

In 1883 he discussed the question of the soaring flight of birds in a letter to *Nature*. Many naturalists had taken the view that they were sustained in the same way as a kite. But this analogy is altogether misleading because of the absence of a string giving connection with the ground. Without such a connection *uniform* motion of the air relative to the ground is not in point, and cannot help the bird in any

way. Rayleigh showed, however, that if the wind is more rapid at higher levels, the bird may take advantage of the fact by steering itself in an inclined circular path so as to go down an incline to leeward, and up an incline to windward. In this way energy may be gained from the wind, and the bird may maintain itself without doing muscular work. Later observations of naturalists seem to show that the albatross does in fact move in the way postulated.

The letter continued thus :—

"However the feat may be accomplished, if it be true that large birds can maintain and improve their levels without doing work, the prospect for human flight becomes less discouraging. Experimenters upon this subject would do well to limit their efforts for the present to the problem of gliding or sailing through the air. When a man can launch himself from an elevation, and glide long distances before reaching the ground, an important step will have been gained, and until this can be done, it is very improbable that any attempt to maintain the level by expenditure of work can be successful."

I believe this passage is important in the history of the conquest of the air.

A. Lieuentaal, who was one of the first successfully to glide long distances, wrote (18 Winchester Street, Acton, May 16th, 1895) :—

"Ever since the time your remarkable advices to experimenters on mechanical flight appeared in the columns of *Nature* (April, 1883) I have been constantly engaged in the solution of this very problem."

He went on to describe his experiments, in the course of which he had made gliding flights of several miles, and his attempts to apply a motor. Finally he added :—

"I think that with improvements thorough success might be possible. Still I wish most sincerely to have your kind opinion and advices on the subject. They will be again of the greatest value."

The brothers Wright also followed the course which Rayleigh had advocated. I have no definite evidence that they

were influenced by him, but the idea that they were so influenced has occurred to other minds. Thus Mr. Arnulf Mallock wrote (July, 1909) :—

"I have always thought that the Wright brothers must have read your 1883 paper with care, and that their success has been largely due to following the suggestions you made in the last paragraph."

In 1900 Rayleigh gave the Wilde lecture at Manchester on the Mechanical Principles of Flight. In this lecture he discusses the method of calculating the mechanical forces on a plane presented obliquely to a current of air, so far as this can be done. At the best, the calculation is very incomplete. He describes a simple method of experimenting on the forces at various angles by means of a kind of many-bladed propeller which could have the various blades set at different angles so as to neutralize the tendency to rotation when the propeller was carried forward.

He then considers the problem of the helicopter or flying machine maintained by a vertical screw, and shows the inherent superiority of an aeroplane as a means of obtaining lift.

It may perhaps be asked why, having gone so far into the theory of the subject, he did not put his views into practice. It can only be replied that there are diversities of gifts. He was not of an adventurous disposition, and disliked taking the responsible and anxious decisions which are necessary in large-scale experimenting, when a false step may take months or years to retrieve. He preferred to feel his way, without the need to carry prevision as far as it would go. Finally, he was very much the antithesis of the "man with a spanner." I remember that towards the close of the argon researches, it had been attempted to press into service a small gas engine of very antique pattern, installed many years before for blowing the organ. This was to drive a small alternator for consuming nitrogen on a small scale. Much trouble was experienced with it, and he finally said

in disgust, "That has beaten us more than anything," and "Who would have anything to do with machinery if they could help it?"

In 1909 the Government was becoming alive to the necessity of giving attention to problems of aeronautics, and Mr. Haldane, who was then Secretary of State for War, consulted Rayleigh on the possibility of research at the National Physical Laboratory on the problems involved.

A conference took place at the Admiralty. Mr. McKenna (First Lord), Mr. Haldane, Rayleigh, and the Director were present, and a scheme was outlined for the appointment of an advisory committee for Aeronautics, and for undertaking aeronautical research at the N.P.L.

The Committee was not primarily appointed to design, construct, or try aeroplanes. It was, in the words of Mr. Asquith, who expounded the scheme to the House of Commons, "for general advice on the scientific problems arising in connection with the work of the Admiralty and War Office in aerial construction and navigation."

It is true that the Wrights had flown at this time and thus no doubt the most important and difficult step had been taken. The thing was shown to be practicable, and public interest was aroused, so that it was possible to get to work without being considered a visionary, and without the serious trials which the pioneers had had to undergo, more particularly in the United States, from the persecutions of newspaper correspondents, constantly interfering with their work and disturbing their peace of mind. Rayleigh felt somewhat bitterly what his friend, S. P. Langley had suffered in this way.

On the other hand very little was known as to the principles of aeroplane design. It was impossible to say on existing knowledge what margin of strength there was in the various structural members of the machine, and whether or no it was safe to make them lighter. There was little basis on which to form a rational opinion as to what modifications of design were desirable or possible in order to get more speed or carrying power.

Lastly, the question of stability was untouched.[1]

The lack of all this knowledge made flying very dangerous, even under peace conditions. This was illustrated by the death of Wilbur Wright and many other pioneers.

Rayleigh was chairman of the Advisory Committee. The other members were: Dr. R. T. Glazebrook, Rear-Admiral R. H. Bacon, Mr. Horace Darwin, Sir George Greenhill, Major-General Sir C. F. Hadden, Mr. F. W. Lanchester, Mr. H. R. A. Mallock, Mr. Mervyn O'Gorman, Dr. W. N. Shaw, and Captain Murray F. Sueter. Mr. F. J. Selby was the Secretary.

The work of the Committee depended largely on the study of aeroplane models in a current of air. For experimental purposes it is more convenient, and amounts to the same thing, to move the air and keep the model at rest. Wind channels or tunnels are required for this, and the larger they are the better, so as to admit large models, not removed by too great a factor from the full scale. The forces on the model as a whole can be determined by suitable weighing arrangements, and the pressures on the various parts of the wings by attached pressure gauges.

There is an exaggerated notion in the popular mind as to what can be done in the way of calculating the resistance on objects of assigned shape in a current of air or water—this is perhaps the least satisfactory part of theoretical mechanics. It is however possible, if we know the behaviour of an exact model, to predict under certain limitations how the larger structure will behave. Rayleigh's chief personal contribution to the work of the advisory committee was to lay down exactly what these limitations are according to the theory of dynamical similarity. This theory had always been a favourite subject with him, and he now applied it to show exactly under what conditions we could determine

[1] A word of explanation is here desirable. An unstable aeroplane is not one which necessarily upsets. A bicycle, as opposed to a tricycle, is unstable in the sense used. The constant intervention of the rider, unconscious it may be, is necessary to avoid an upset.

the resistance of an aeroplane from that of the model. The discussion was limited to two short notes, but it may be regarded as the foundation stone of the Committee's work.

The value of that work proved itself during the War. A detailed account of it hardly belongs to my subject, as Rayleigh's part in it was only one of general direction. Most of the initiative and hard work was supplied by younger men.

During the War the scope of the Committee's work became extended. Although the Royal Aircraft Factory was not controlled by it, yet in practice information was fully shared, and the reports of the Committee became a general collecting ground for the whole of British aeronautical information.

In all, there were 126 meetings of the Committee, during the eleven years of its existence. Of these meetings, Rayleigh presided at ninety-six. The last one that he attended was on May 15th, 1919, immediately before his last illness.

The Committee was dissolved about a year later, and replaced by a new one, which covered the same ground as the old one, with certain executive duties in addition.

Rayleigh's last appearance in public was to deliver his presidential address to the Society for Psychical Research. He had been a member of the Society from its foundation in 1882. In 1901 he was urged to accept the presidency. This had been the strongly expressed wish of Frederick Myers, one of the leading spirits of the Society, during his last illness a short time before; and it was also pressed by his friend Sir Oliver Lodge. But Rayleigh knew at that time that he still had useful scientific work before him, and felt unable to accept. "A man must decide for himself what he can do to the best advantage," he said.

In 1919, however, he accepted the presidency, and gave an address, summing up his experiences and attitude towards the subject. The attitude taken is a very balanced one, and no definite conclusion, positive or negative, is reached. I asked him whether he intended to reprint it in his *Scientific Papers*, and he characteristically replied that he had not

decided. The decision eventually fell to me, and I did reprint it there. Since, however, the address contains autobiographical matter, and will interest many who have no access to the *Scientific Papers*, I have reprinted it again as an appendix to this book (p. 379).

CHAPTER XX

RAYLEIGH'S WORK IN RELATION TO THE RECENT DEVELOPMENTS OF PHYSICS

The two great recent advances in physics are the theory of relativity and the quantum theory. Rayleigh's epoch of full activity closes about the time when they were coming into prominence. But it can be shown, I think, that he took no small part in building the foundations on which these advances were made, both by his own work and by the suggestions and encouragement which he gave to others. This will be the thesis of the present chapter.

On December 3rd, 1890, Rayleigh writes to Prof. Oliver Lodge :—

> "I wrote out my views on Aberration,[1] but in the end decided to omit everything speculative. . . . There are some references that might be useful (to you). Anyhow it is better than anything I could say now."

The article on aberration, here mentioned, includes a discussion of the various experiments which had been made at the time on the question of relative motion of the earth and the ether. It was eventually published in *Nature* in January, 1892, and is reprinted in the *Scientific Papers*, Vol. III.

He sums up the position by saying that, on the whole, Fresnel's hypothesis of a stationary ether appears more probable at the time of writing. He advocates the experiment of seeing whether the propagation of light in air is affected by the rapid motion of heavy masses parallel to, and in the immediate neighbourhood of, the ray. This

[1] For the article "Wave Theory" in the *Encyclopædia Britannica.*

experiment, as is well known, was admirably carried out a little later by Oliver Lodge, with negative results.

The chief argument against a stationary ether, i.e. a motion of the ether relative to the earth, was the celebrated experiment of Michelson, which seemed to show that the velocity of light was the same along and across the direction of the earth's motion through space. At that time the experiment had not been carried out well enough to give a perfectly unambiguous answer, yes or no, and Rayleigh urged in his article the importance of doing this. He wrote at the same time to Prof. Michelson on the subject. It is to be regretted that this letter is not extant, but the answer was as follows :—

CLEVELAND, *March 6th/87.*

MY DEAR LORD RAYLEIGH,—

I have never been fully satisfied with the results of my Potsdam experiment, even taking into account the correction which Lorentz points out.

All that may properly be concluded from it is that (supposing the ether were really stationary) the motion of the earth thro' space cannot be very much greater than its velocity in its orbit.

Lorentz' correction is undoubtedly true, I had an indistinct recollection of mentioning it either to yourself or to Sir W. Thomson when you were in Baltimore.

It was first pointed out in a general way by M. A. Potier of Paris, who however was of the opinion that the correction would entirely annul any difference in the two paths ; but I afterwards showed that the effect would be to make it one half the value I assigned, and this he accepted as correct. I have not yet seen Lorentz' paper and fear I could hardly make it out when it does appear.

I have repeatedly tried to interest my scientific friends in this experiment without avail, and the reason for my never publishing the correction was (I am ashamed to confess it) that I was discouraged at the slight attention the work received, and did not think it worth while.

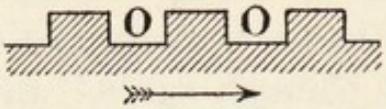
Your letter has however once more fired my enthusiasm and it has decided me to begin the work at once.

If it should give a definite negative result then I think your very valuable suggestion concerning a possible influence of the vicinity

of a rapidly moving body should be put to the test of experiment; but I too think the result here would be negative.

But is there not another alternative?

Suppose for example that the irregularity of the earth's surface be crudely represented by a figure like this—

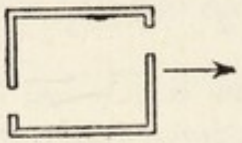


If the earth's surface were in motion in the direction of the arrow, would not the ether in O O be carried with it?

This supposes of course, contrary to Fresnel's hypothesis, that the ether does not penetrate the opaque portions, or if it does so penetrate, then it is held prisoner. Fizeau's experiment holds good for transparent bodies only, and I hardly think we have a right to extend the conclusions to opaque bodies.

If this be so and the ether for such slow motions be regarded as a frictionless fluid—it must be carried with the earth in the depression.

Would this not be partly true say in a room of this shape?



If this is all correct then it seems to me the only alternative would be to make the experiment at the summit of some considerable height, where the view is unobstructed at least in the direction of the earth's motion.

The Potsdam experiment was tried *in a cellar*, so that if there is any foundation for the above reasoning, there could be no possibility of obtaining a positive result.

I should be very glad to have your views on this point.

I shall adopt your suggestion concerning the use of tubes for the arms, and for further improvements shall float the whole arrangement in mercury; and will increase the theoretical displacement by making the arms longer, and doubling or trebling the number of reflections so that the displacement should be at least half a fringe.

I shall look forward with great pleasure to your article on "Wave Theory" (hoping however that you will not make it too difficult for me to follow).

I can hardly say yet whether I shall cross the pond next summer. There is a possibility of it, and should it come to pass I shall certainly do myself the honour of paying you a visit.

Present my kind regards to Lady Rayleigh and tell her how highly complimented I feel that she should remember me.

Hoping soon to be able to renew our pleasant association, and thanking you for your kind and encouraging letter,

I am,

Faithfully yours,

ALBERT A. MICHELSON.

A few months later, Prof. Michelson wrote to announce his result :—

NEW YORK, *Aug.* 17*th*, 1887.

MY DEAR LORD RAYLEIGH,—

The experiments on relative motion of earth and ether have been completed and the result decidedly negative. The expected deviation of the interference fringes of the zero should have been ·40 of a fringe—the maximum displacement was ·02 and the average much less than ·01—and then not in the right place.

As displacement is proportional to squares of the relative velocities it follows that if the ether does slip past the earth the relative velocity is less than one-sixth of earth's velocity.

I enclose a poor photograph of the apparatus—which consists of a stone five feet square and one foot thick which floats on mercury and which holds the optical parts. Light from an argand lamp falls on *a*, part going to *bcbcbcbaf* and part to *dedededaf*.

I hope to be able to send you a copy of the paper within a month.

With kind regards to Lady Rayleigh,

Very sincerely yours,

ALBERT A. MICHELSON.

Prof. Oliver Lodge wrote (February 3rd, 1891) :—

"It will be first rate if your article is published shortly before mine. I see you mention in it the desirability of experiment on Ether Motion near matter.

"I intend trying not only steel discs but copper, etc. also. . . .

". . . I fully expect some optical difficulties with the Michelson semi-silvered plate, etc. etc. and if so I'm afraid I shall bother you occasionally. But I will have a struggle myself first. Can't do anything much till the Easter Holidays.

"If I do it properly, even though the result is negative, it should be the jam to make my aberration paper go down. I shouldn't wonder though if Michelson will step in before me, as you have suggested it to him."

When the article on "Aberration" eventually appeared in *Nature*, Prof. H. A. Lorentz wrote (August 18th, 1892):—

"I have read this note with much interest and I gather from it that we agree completely as to the position of the case. Fresnel's hypothesis, taken conjointly with his coefficient $1 - \frac{1}{n^2}$, would serve admirably to account for all the observed phenomena were it not for the interferential experiment of Mr. Michelson, which has, as you know, been repeated after I published my remarks on its original form, and which seems decidedly to contradict Fresnel's views. I am totally at a loss to clear away this contradiction, and yet I believe that if we were to abandon Fresnel's theory, we should have no adequate theory at all, the conditions which Mr. Stokes [1] has imposed on the movement of the æther being irreconcilable to each other.

"Can there be some point in the theory of Mr. Michelson's experiment which has as yet been overlooked?"

We must take up the story after the lapse of about ten years, occupied with argon and many other matters.

In spite of the negative results of earlier laboratory tests to detect the earth's motion through space, it was still thought doubtful in 1902 whether or not the rotation of the plane of polarization by quartz would be affected by it. H. A. Lorentz was at that time of opinion that according to electro-magnetic theory there should be an effect: Sir Joseph Larmor thought not. The experiment had been tried thirty years before, by Mascart, but the accuracy he was able to attain was hardly enough to detect a change amounting to a ten-thousandth part of the whole rotation, representing the relative velocity of the earth in its orbit in comparison with the velocity of light. Rayleigh determined to re-examine the question. I remember asking him why he undertook it, as (I thought) it was sure to give a negative result. He said, "Because it seems to me so extraordinary that the ether should be streaming through the laboratory at 19 miles a second, and yet that it should be impossible to detect it in any way."

[1] Sir George Stokes.

The test was carried out with the necessary accuracy to make it certain that no effect existed of the magnitude to be expected. The rotation of a length of 10 inches of quartz, amounting to fifteen complete revolutions, was the same within $\frac{1}{25}$ of a degree, when the path of the light was reversed, so that the ether drift was first with the direction of propagation, and then against it.

Shortly afterwards he tried another experiment of the same class. It was suggested by Fitzgerald's hypothesis of a contraction of solid bodies in the direction of the motion, made in order to explain Michelson and Morley's negative result. If a piece of glass is squeezed by screw pressure so as to contract in one direction only, it is found to possess the property of double refraction for light. It seemed possible that the contraction produced by motion might have the same effect.

Suppose that at noon a beam of light directed from north to south passes through a slab of glass. The earth's motion round the sun in the east-west line, and therefore the Fitzgerald contraction, will be in this direction. In the vertical direction there will be no contraction. If, therefore, the north and south beam of light is polarized, and the direction of vibration is at 45 degrees to the horizontal, the contraction will have a chance of showing itself by a double refraction which will disappear when the whole apparatus including the source of light is rotated, so that the light is along the same line as the motion. In the latter position the motion could not differentiate between vertical and horizontal vibrations, since each would be perpendicular to it.

It is clear that in this case a very exacting test for double refraction is required, if a negative result is to have any significance. The contraction itself is only 1 part in 200,000,000, depending as it does on the square of the ratio of the earth's velocity to that of light. It was necessary to look for a double refraction of the same order, i.e. the difference of velocity for the two vibrations (if any) would be some such fraction of the whole velocity. It might seem hopeless to look for so small an effect, but in fact it is not so, for an extraordin-

arily minute double refraction can be detected by using a sufficiently bright source of light, and observing the restoration of light between crossed Nicol's prisms.

Rayleigh used an ingenious method for setting a limit to the double refraction which might be present. He used a strip of glass, resting on two supports and loaded with weights outside the supports, tending to bend it up into an arch. The bottom edge of the strip was in compression, the top in tension, and between them was an unstressed region called the neutral axis. This glass strip was between the nicols, in front of the long length of glass, and it showed a dark band at the neutral axis. Any change in the double refraction of the glass behind would compensate part of the double refraction of the loaded strip, and cause the neutral dark band to shift, when the whole apparatus was turned through a right-angle.

There is a mystery about who invented this device. Prof. Zeeman wrote to Rayleigh some years later (1911) :—

"I have always greatly admired the extremely delicate method you have made use of in your investigation concerning possible double refraction caused by motion through the æther. The slightly loaded glass bar works indeed admirably. . . ."

Rayleigh wrote in reply :—

"I am glad you have found the loaded strip useful, but I don't think I was the originator of it. Perhaps it was Kerr. Anyhow I agree with you in thinking it very convenient."

Prof. Zeeman tells me, however, that he has searched for it in vain in Kerr's writings and it is quite possible that Rayleigh did invent it after all.

The experiment was carried out both with glass and with bisulphide of carbon, most satisfactorily with the latter : but in either case it was clear that there was no effect of the magnitude to be expected.

I have described these two experiments from the point of view from which they were undertaken. It was shown that the motion of the earth through the ether showed no effect

of the first order on rotatory polarization, and no effect of the second order on double refraction. The results are now regarded from a somewhat different standpoint and they form a not unimportant part of the material for Einstein's induction of 1905, known as the restricted principle of relativity: this principle may be stated in the form that *it is impossible by any experiment to detect uniform motion relative to the ether.*

Rayleigh himself took no part in working out the consequences of this principle, which so dominates the thoughts of the younger generation of physicists. He was, however, keenly interested in it, and often discussed the earlier writings upon it in conversation with me. He found it very difficult to adapt his mind to so strange a train of thought, and expressed surprise at so many of the younger generation professing, at least, to be at home in it.

The more general principle of relativity had hardly come into notice before the time of his death, and I do not remember his making any reference to it.

I come now to another subject—the difficulties that beset the application of Newtonian mechanics to explain the behaviour of molecules and their relation to radiation. It is of course one thing to emphasize a difficulty and another to solve it: but the definite recognition that there is a contradiction to be cleared up is often a long step on the road. In the present case, the end of the journey has by no means been reached: and how far off the goal may be no man can say.

Apart from some slight hints given long before by Maxwell, Rayleigh was, I believe, the first to emphasize these difficulties in all the chief cases where they have now been admitted to be insuperable. Taking these in the order in which he first dealt with them, we have the series of lines in the spectrum, specific heats, and the distribution of energy in the spectrum of a black body.

The spectrum of a gas consists of a series of bright lines, often very fine and sharp, each denoting vibrations of the

atom in a very definite period. As soon as this had been recognized, in the late 'sixties, it seemed as if a clue was within reach which could hardly fail in competent hands to reveal the secret of atomic structure. Maxwell, for instance, wrote (1875): "An intelligent student armed with the calculus and the spectroscope can hardly fail to discover some important facts about the internal constitution of a molecule."

Rayleigh felt the same. "It gives such a tremendous numerical handle," he said.

The problem, as it then presented itself, was to specify a mechanical system, or set of systems, which could give a series of periods similar, for instance, to those which had been found in the spectrum of Sirius by Huggins, and in the spectrum of terrestrial hydrogen by Cornu. The series of frequencies thus found is represented with extreme accuracy by substituting the ordinal numbers in a formula given by Balmer. These frequencies converge to a definite end at an accessible point in the ultra-violet spectrum, when the lines of the series crowd together, and beyond which none of them extend. This at once rules out many of the obvious analogies of vibrating bodies. An ideal stretched string, for instance, has a series of frequencies converging towards a value infinitely great. However, some rather far-fetched mechanical analogies can be found for this particular feature. The crucial difficulty is set forth in the following passage, which I quote in full on account of its fundamental importance. It is the conclusion of a paper dated October, 1897, "On the Propagation of Waves along a Connected System of Similar Bodies."

"There is one circumstance which suggests doubts whether the analogue of radiating bodies is to be sought at all in ordinary mechanical or acoustical systems vibrating about equilibrium. For the latter, even when gyratory terms are admitted, give rise to equations involving the square of the frequency; and it is only in certain exceptional cases, e.g. (31), that the frequency itself can be simply expressed. On the other hand, the formulæ and laws derived from obser-

vation of the spectrum appear to introduce more naturally the *first* power of the frequency. For example, this is the case with Balmer's formula. Again, when the spectrum of a body shows several doublets, the intervals between the components correspond closely to a constant difference of frequency, and could not be simply expressed in terms of squares of frequency. Further, the remarkable law, discovered independently by Rydberg and by Schuster, connecting the convergence frequencies of different series belonging to the same substance, points in the same direction.

"What particular conclusion follows from this consideration, even if force be allowed to it, may be difficult to say. The occurrence of the first power of the frequency seems suggestive rather of kinematic relations [1] than of those of dynamics."

To illustrate this argument, let us consider, as a simple example, a pendulum consisting of a heavy mass, hung up by a light chain. The number of links of the chain may be anything from one upwards. It is known to all students of dynamics that the frequency of vibration of a simple pendulum of this kind is inversely proportional to the square root of the length, i.e. to the square root of the number of links in use. Thus by adding successive links we get a series of frequencies in the ratio $\frac{1}{\sqrt{1}}, \frac{1}{\sqrt{2}}, \frac{1}{\sqrt{3}}, \frac{1}{\sqrt{4}}, \frac{1}{\sqrt{5}}$, etc.

We may, for simplicity, adopt such a unit of time that these become the actual frequencies. Now in the study of experimental spectra we get relations of importance by subtracting some of the frequencies which appear in the spectrum from others. The square roots in the above series are intractable for this purpose. For instance, $\frac{1}{\sqrt{21}} - \frac{1}{\sqrt{22}}$ is an expression which does not admit of simplification in itself and which does not bear any simple relation to any other member of the series, or to any other interval. Nothing can in general be done with the members of such a series by way of sub-

[1] E.g. as in the phases of the moon. (Rayleigh's note.)

traction, yet the square roots always appear in such cases where the frequencies are determined by the laws of motion, as laid down by Galileo and by Newton.

In the passage above quoted we find, I believe, the first suggestion of what is now universally accepted, that these laws are not adequate to the purpose.

We have seen in a former chapter something of Rayleigh's controversy with Kelvin on the question of the Maxwell-Boltzmann doctrine of the partition of kinetic energy among the various degrees of freedom of a mechanical system. This doctrine asserts that the kinetic energy will be equally shared among all the degrees of freedom. Kelvin considered the doctrine unproved, and probably untrue. Rayleigh held that Maxwell's arguments were valid and showed that Lord Kelvin's appeal to certain special cases in disproof was unsuccessful. He felt, however, that the application of the doctrine to the molecules of a gas presented grave difficulties. These are set out in the following passage from his paper of 1900.[1]

"The difficulties connected with the application of the law of equipartition to actual gases have long been felt. In the case of argon and helium and mercury vapour the ratio of specific heats (1·67) limits the degrees of freedom of each molecule to the three required for translatory motion. The value (1·4) applicable to the principal diatomic gases gives room for the three kinds of translation and for two kinds of rotation. Nothing is left for rotation round the line joining the atoms, nor for relative motion of the atoms in this line. Even if we regard the atoms as mere points, whose rotation means nothing, there must still exist energy of the last-mentioned kind, and its amount, according to the law, should not be inferior.

"We are here brought face to face with a fundamental difficulty, relating not to the theory of gases merely, but also to general dynamics. In most questions of dynamics a condition whose violation involves a large amount of potential energy may be treated as a *constraint*. It is on this principle

[1] *Scientific Papers*, Vol. IV, p. 451.

that solids are regarded as rigid, strings as inextensible, and so on. And it is upon the recognition of such constraint that Lagrange's method is founded. But the law of equal partition disregards potential energy. However great may be the energy required to alter the distance of the two atoms in a diatomic molecule, practical rigidity is never secured, and the kinetic energy of the relative motion in the line of junction is the same as if the tie were of the feeblest. The two atoms, however related, remain two atoms, and the degrees of freedom remain six in number.

"What would appear to be wanted is some escape from the destructive simplicity of the general conclusion relating to partition of kinetic energy, whereby the energy of motions involving larger amounts of potential energy should be allowed to be diminished in consequence. If the argument, as above set forth after Maxwell, be valid, such escape must involve a repudiation of Maxwell's fundamental postulate as practically applicable to systems with an immense number of degrees of freedom."

Lord Kelvin quoted part of the above at the end of his lecture on "Nineteenth Century Clouds over the Dynamical Theory of Heat and Light," and he said that the simplest way to escape from the destructive simplicity of the general conclusion was to *deny the conclusion*. There is, however, another course open: and that is to deny the premises from which the conclusion is deduced rather than the logic of the deduction. Kelvin condemned the latter. But recent science takes the other alternative, and escapes from the difficulties which Rayleigh felt by denying that Newtonian Mechanics can be applied without reserve to individual atoms and molecules regarded as mechanical systems.

Rayleigh came up against other difficulties of the same class in applying the Maxwell-Boltzmann doctrine to the problem of heat radiation. It will be remembered that, according to the demonstration of Balfour Stewart and Kirchhoff, the radiation from the inside of a closed oven at a definite temperature, is of determinate quality, depending on the

temperature and wave length, and not at all on what the walls of the oven are made of. A lamp-blacked surface in the open gives very nearly the same radiation as escapes from a small hole in the walls of the oven. In either case the spectrum at a given temperature has a definite distribution of energy, the energy rising to a maximum at a particular wave length. This wave length of maximum energy moves towards the blue end of the spectrum as the temperature rises.

Since this spectrum is independent of the properties of any particular substance, its exact constitution is evidently a matter very fundamental in the nature of things, and the question arises: Can the energy distribution at any given temperature be foreseen by any dynamical or thermo-dynamical considerations?

Rayleigh began as usual by illustrating his argument by a simple case. Suppose we take a stretched string, like a piano wire. If this is set into vibration in the most general manner, it vibrates in a number of different modes, giving rise to the fundamental vibration, the octave, and higher harmonies. In this mode of vibration the string is divided into 1, 2, 3, 4, etc., segments, and so on *ad infinitum*. This applies to an ideal string which is to be regarded as having mass, but no finite diameter.[1]

It is possible to deal on similar lines with the vibrations of a cubical mass of air. In this case there is even more tendency for the higher frequencies to cluster closely together. In the case of air there would be a limit when we come to subdivisions of the volume which are so small as only to contain a few molecules. Thus the possible number of frequencies would be finite, and a Maxwell-Boltzmann distribution of energy between these frequencies presents no obvious paradox.

Consider, however, the case of luminous vibrations instead of sonorous ones. For this application it will be necessary to imagine a cubical box with perfectly reflecting walls, which

[1] In a real string the law will begin to fail for subdivisions of the string which are not a large multiple of the diameter.

will act the same part towards light that the rigid walls of the former case did towards sound. Suppose that there is a small hot object in the box. This will give off radiation into the enclosed space, which will be reflected to and fro between the walls, and back to the hot object, and since the radiation is propagated with the finite speed of light, there must at any time be some radiant energy in the space. Suppose, now, that things have settled down to a steady state. If the Maxwell-Boltzmann doctrine is applicable, this energy in the space must be equally distributed between all the modes of vibration. Now comes the difficulty. The luminiferous ether conveying the vibration has not a grained, or molecular structure, like air. We suppose it to be without structure, and therefore there is no limit to the smallness of the subdivision of the box. The number of possible frequencies is without limit, and the higher they are, the more numerous they become, and the more crowded on a scale of frequencies. The doctrine requires us to assign an equal energy to each, when the steady state has been attained. So that if we examine the radiation which comes out of a small hole, the energy ought to increase without limit, as we go to the region of small frequencies, i.e. towards the violet end of the spectrum.

If the longer waves have any energy at all, then the whole spectrum must have an unlimited amount. Even if this difficulty could be escaped, we should still have the fact that the *relative* energies in the different parts of the spectrum are not at all what the doctrine would indicate. If we take the case of a red-hot oven, there is found to be a maximum of energy for waves very many times longer than the shortest of which we have experimental knowledge, indeed, for waves which are too long to be detected by the eye.

"It seems to me," Rayleigh wrote, "that we must admit the failure of the law of equipartition in these extreme cases. If that is so, it is obviously of great importance to determine the reason."

He made no constructive suggestion as to the solution of

these difficulties, and he was ill at ease with the methods by which others were seeking a solution. In October 1911 the first Solvay Conference on these problems was held at Brussels, and he was asked to take part in it. He did not attend the conference—precisely why I do not know. I should guess that he was disinclined for the effort, and that he felt that his age (sixty-nine) justified him in doing what he liked. In his written contribution,[1] which took the form of a letter to Prof. Nernst, he made reference to his earlier papers, and re-emphasized the difficulty about the stiffness of a molecule along the lines of junction of two atoms, and added :—

" Perhaps this failure might be invoked in support of the views of Planck and his school that the laws of dynamics (as hitherto understood) cannot be applied to the smallest parts of bodies. But I must confess that I do not like this solution of the puzzle. Of course I have nothing to say against following out the consequences of the quantum theory of energy, a procedure which has already, in the hands of able men, led to some interesting conclusions. But I have a difficulty in accepting it as a picture of what actually takes place."

Prof. Nernst, in acknowledging this communication, wrote :[2]

" It has especially interested me to observe from what you have been so kind as to send me, that already eleven years ago, in your paper on the law of the partition of kinetic energy, you, so to speak, were the first to place your finger on the open wound in theoretical physics, with the healing of which we are concerned in Brussels."

I will add some recollections of what Rayleigh said privately on the same subject.

" I saw some of these difficulties myself as soon as anyone, but I doubt if I should have had the enterprise to go in for a quantum theory."

Again, we had been discussing the history of the wave theory of light, and the difficulties that used to be felt about the " loss of half an undulation " indicated by the black centre of Newton's rings. " I think the difficulties we are up against now are far more serious, and what dissatisfies me is

[1] *Scientific Papers*, VI, p. 45. [2] I translate from the German.

that I can hardly expect to see them solved in my time."

I asked him if he had seen Bohr's first paper on the hydrogen spectrum which had then recently appeared. In this paper, it will be remembered, Bohr introduces for the first time the idea of restricting the application of classical mechanics to certain "stationary states" of the atom. The states themselves, and the transitions between them, are governed by arbitrarily assumed rules depending on Planck's conception of the quantum of vibrational energy.

He replied, "Yes, I have looked at it, but I saw it was no use to me. I do not say that discoveries may not be made in that sort of way. I think very likely they may be. But it does not suit me."

I have quoted these words exactly as they were spoken. It must be remembered that they were used in casual and intimate conversation and before the scientific world had pronounced at all on the merits of the theory or scheme, whichever it is to be called. I think that younger men, including no doubt the distinguished author of the theory himself, feel essentially the same difficulties and dissatisfaction, though they rightly think it good policy to note them, and pass on, with a view to returning later to make good. For a man of nearly seventy years old, this policy was not practicable.

CHAPTER XXI

GENERAL CHARACTERISTICS. DEATH AND FUNERAL

Rayleigh's bias was strongly towards seeing the best in people. For instance I remember discussing with him a scientific man whose attempts at original investigations I had often heard belittled in scientific circles at Cambridge. I repeated and endorsed what I had heard. "Yes, no doubt there is that side, but there is the other too," he said. "You must remember how good he is as a writer and expounder." I can only recall one or two instances of his expressing a general contempt for anyone. One of these was a well-known journalist, whom he always mentioned as "that wretched creature ——." But this was altogether exceptional. It was much more typical of him to defend people who were attacked.

Again, he was very slow to dismiss any widely held belief as wholly devoid of foundation. His capacity for suspending his judgment was greater than I have seen in anyone else. The curious incident of the N-rays will be remembered in scientific circles. A well-known French experimenter persuaded himself that he had observed a new kind of rays, which were emitted by solids in a state of strain, e.g. tense human muscles, or pieces of hardened steel such as an ordinary file. The test for emission of these rays was a supposed brightening of a screen of phosphorescent paint, already faintly luminous, when the source of "N-rays" was brought near. These experiments could be tried with very simple means, and many experimenters made the attempt. A few reported success, but the majority failed, and scepticism grew. Finally, it was shown that the original observer could not comply with

objective tests, and scepticism was merged in general and complete incredulity. Rayleigh, with his assistant George Gordon, tried the experiments before scepticism had raised its head, but he had not the shadow of a suspicion of a positive result. I was present, and can vouch for the fact. After this experience most men would have accepted the failure of others with alacrity; but, even when every one else had dismissed the subject as a myth, Rayleigh was still disposed to think there might be something in it. It was easy to arouse his interest in dubious phenomena, but very difficult to get his final verdict either for or against them.

On the much-debated question of water-divining with a hazel rod: it would have been impossible to extort from him an admission that he believed in it; but it was equally impossible to extract a declaration in the contrary sense. "If I had a difficulty in finding water I should certainly employ a water-finder," he said. I remember pointing out certain difficulties which I thought would discredit the belief if they were generally understood: he agreed that this would be so; at the same time he tacitly reserved judgment himself.

He had a deep sense of the mysteries of existence, and totally disagreed with those who thought that science was approaching to a solution of them. A remark comes back to me, made one evening as he and I were leaving the dining-room, after a conversation on (I think) some of the questions of relativity which were then beginning to excite interest. "It is a strange world," he said, "and perhaps the strangest thing of all is that we are here to discuss it."

Referring to a discussion at the British Association on the "Origin of Life," he wrote (1912): "It seems to be agreed that no one can say what life is, so that it seems rather premature to discuss how it began."

Again at an earlier date (1889): "I am reading Lecky's *History of Rationalism*. The book is interesting, but I have not a clear idea of what rationalism is."

Again, in answering a correspondent who was collecting religious beliefs of scientists he wrote (1910): "I may say

that in my opinion true science and true religion neither are nor could be opposed."

The rather evasive attitude towards some burning questions which is revealed in these extracts is not uncharacteristic. No one could be readier to face a difficulty in physical science if he thought it was being slurred over by smooth phrases. In other fields where there seemed a definite chance of getting knowledge by enquiry, as in the case of psychical research, he was keenly interested, and anxious to take part in the investigation, or, at the least, to encourage what others were doing. But dialectical scepticism about traditional beliefs was repugnant to him. No doubt he felt the same kind of difficulties as other thinking men, but he was not readily drawn into a discussion of them, and if the attempt was made, he would take refuge in the kind of evasion which I have tried to exemplify.

As a very young man, I pressed him on the subject, and he said, " When I was your age I expected to attain much greater certainty than I now think can be attained. I do not think the most religious among scientific men, say Faraday or Maxwell, would pretend to certainty—perhaps Faraday would have though," he added.

As regards the externals of religious observance, his views were what would now be considered old-fashioned. Attendance of the family and household at prayers and at Sunday morning church was expected, though he was careful to avoid pressure on visitors. Lawn tennis on Sunday was long resisted, though in the end the concession was made. Unless it was unavoidable, no orders were given to the stables on that day. I think that in these external things his attitude was more the result of early training and conservatism than of anything that went deeper. Certainly there was nothing of puritan rigour about it. He had no objection to a joke, even what might be considered a childish one, which turned on Biblical subjects. Or again, if anyone complained of being bored by a sermon, he said that it showed a great lack of mental resource if they could not amuse themselves with their own thoughts.

With ritualism he had no sympathy, and was rather inclined to make a joke of it than otherwise. He disliked "the dissidence of dissent," though he would attend a Presbyterian service in Scotland, or a Roman Catholic one in France.

On the fundamental question of theism he remarked to me once, "Suppose I discover a new mathematical theorem. Has it never been apprehended by any mind before? To me that is inconceivable."

In some moods at least he had a strongly devotional turn of mind. Witness, for instance, the motto from the Psalms with which he prefaced the first volume of his *Collected Papers*, which has already been mentioned in another connection (p. 307), and another from the Book of Wisdom prefixed to his fifth volume: "Thou hast ordered all things in measure and number and weight."

The following letter, which was written on the occasion of a family bereavement (1911), will perhaps most clearly bring out his religious attitude:—

"You know I have a constitutional dislike to saying more than I feel, and this may lead to my sometimes saying less.

"I have never thought the materialist view possible, and I look to a power beyond what we see, and to a life in which we may at least hope to take part. What is more, I think that Christ and indeed other spiritually gifted men see further and truer than I do, and I wish to follow them as far as I can.

"But the great thing is to pray, even if it be in a vague and inarticulate fashion. Surely we can ask a blessing on those we love and (in the words of the Collect) that the Holy Spirit may in all things direct and rule our hearts. The vaguest attitude of aspiration and resignation seems better than no attitude at all."

On questions of sexual morality his views would be accounted severe. He was not the man to pay much attention to irresponsible gossip, but he would not countenance anyone socially if there was something definite against them.

Referring to a work of fiction which treated such matters with what he considered undue levity, he said, "It is not fit to leave about the house."

Again, after a conversation on this subject: "Perhaps we have left the religious side of it too much out of account. If there is anything the New Testament is strong about it is that."

As already mentioned (Chapter XIV), Rayleigh was an assiduous student of the newspapers, which, during the more active part of his life, formed his chief reading by way of recreation. During the palmy days of the *Westminster Gazette* as an evening paper, his habit was to study it at breakfast. His comments were usually limited to a denunciation of the political views expressed, which would be demolished by some simple *reductio ad absurdum*. He would complain that a phrase was coined—"Ireland a Nation," for instance—and the argument was then based on the assumption that the phrase had a meaning, when in fact it had none. Ireland was not a homogeneous whole. The *Times* only arrived when breakfast was half over, and would be reserved for the afternoon and evening.

He would study not only the political news, but also the correspondence. Readers who have followed me so far will perhaps be surprised to hear that he was a good deal interested in the periodic outbreaks of the "Bacon Shakespere controversy." I think he rather enjoyed dallying with this heresy, and he remarked with relish that so far he thought the Baconians had had the best of it. He told us of a conversation with Sir Norman Lockyer on this subject.

Rayleigh: It rather amuses me to think that perhaps all the Americans who have been down to Stratford-on-Avon have gone on a fool's errand.

Lockyer: But *I've* been down to Stratford-on-Avon.

To prevent any misconception, it may be well to say that I do not think Rayleigh was altogether serious on this subject.

Apart from the newspapers, biographies were perhaps his favourite reading. He used to say that one of the few advantages of growing old was that it gave the opportunity of reading the biographies of men that he had known. He remarked too that in a large proportion of those which he read there

was some incident of a supernormal kind—a premonition, an appearance at the moment of death, or something of the sort; and he was disposed to think this significant of the reality of these things. I am sure, however, that he would not have committed himself to a definitive conclusion on the strength of any amount of evidence of this kind. It was very easy to impress him, but very difficult to convince him.

Once for amusement I pressed him as to what evidence from others would convince him of the supposed physical phenomena of spiritualism. I made the hypothetical case very good. He said he would be impressed. I then strengthened it largely. The medium was to float in the open air in broad daylight in the presence of several of his most trusted scientific colleagues, who were to have full liberty of examination, and to have the performance repeated day after day. This would impress him still more. But I could not get him to admit that he would unreservedly accept it even then. He doubted if he would ever be able to do that without seeing for himself.

He was a moderate reader of novels, chiefly classical ones. Miss Austen, George Eliot, Anthony Trollope and Thomas Hardy were among his favourites. Occasionally he read history, but only if it was exceptionally readable, e.g. Macaulay's *History* or Froude's *Lectures on the Council of Trent.* The only poetry I personally remember to have seen in his hands was Pope's *Homer*, but I do not think that he persisted with it for long. This was towards the close of his life. He had professed the intention of reading poetry in his old age, but when the time came, this intention was not made good.

He was a diligent student of *Punch*, not only the current number of the time being, but of the bound volumes as well. This brings me to a salient characteristic, his love of funny stories. He had a large repertoire collected from his early manhood onwards, and he was very ready to bring them in when they were apposite. Some of them had not much substance, but any deficiency in this way was made up by the dramatic art with which they were told. His own intense

enjoyment of them was an important element in their success. Towards the end of his life, when, owing to increasing deafness, he could not hear general conversation, it was difficult to find opportunity for telling them, and, perhaps as a partial substitute, he amused himself by making a collection in a manuscript book, writing down a word or two in his pocket book as a reminder whenever anything recalled a favourite story to his mind.

This collection was begun in 1911. It was not strictly limited to funny stories. Incidents of interest in his life or remarks from well-known people which had struck him were recorded as well, and use has been made of these in the appropriate place in this book. A selection from the remainder will be found in Appendix III.

He appreciated very much an original *bon mot* thrown off in conversation, and sometimes contributed one himself. Here is an example that I can call to mind. To me at least it was amusing :—

The occasion was when during the war or post-war period some labour delegates from Australia were refused permission to land in Ireland. They had no alternative therefore but to come in to an English port, and as a protest said they would not land. They afterwards modified this by saying that they would make no public appearance.

Self : I suppose what it amounts to is that they will not have tea with the Labour Members on the terrace of the House of Commons.

Rayleigh : Or at all events, if they do, they won't have any sugar in it !

It was noticed in a former chapter than when young he had been a good rifle shot, but had no success with a shot gun. I always thought this would serve as a parable to illustrate where his intellectual strength lay. It was in the sureness of his deliberate judgment. When quick decision or action were required he was not in his element. He knew this very well, and was not easily induced to give a hasty decision. When pressed for one, he has been known to complain of having

a pistol held at his head. This was particularly the way when his sanction was sought for any necessary building work on the estate. Indeed, so difficult was it to get his consent that desirable alteration was sometimes postponed indefinitely on this ground. In one or two cases he was faced with a small *fait accompli*, but, kindly and good-humoured as he was, no one would have ventured to go very far in this direction.

When he *had* come to a decision, even on a matter which he did not consider important, he was not prepared to alter it to suit the fancy or caprice of anyone else.

He had an almost morbid dislike to committing himself publicly, or indeed privately, when he had not had the opportunity of forming a matured opinion based on personal knowledge. He was sometimes asked to appear as an expert witness, but always refused to do so. In the incident described on p. 185 his neglect to appear before the magistrate as a witness was partly due to this dislike.

On one occasion (March 5th, 1907), as President of the Royal Society, he had to appear before a Royal Commission which was considering the question of experiments on animals, to present a memorial from the Council. "I was drawn into saying more than I intended," he said. First they asked me how many members of the Council were physiologists. I could not refuse to answer that, as it was relevant to the memorial I had presented. I told them that only two or three of them were so. Then they rather appealed to me for my opinion because the evidence on the anti-vivisection side had been so utterly rotten. It has been represented to us, they said, that the necessary information could be got by taking advantage of such opportunities of observation as may arise, without special experiment. "Well," I said, "I think you may take it that that is nonsense. Physiology must be very unlike any science that I have had to do with, if it could get on without experiment."[1]

[1] This account includes things which are not in the shorthand report. Probably these were "asides."

In spite of the extreme moderation of his character, I have known him on occasion advocate drastic action. For instance, on a case being mentioned of some one being appointed a high sheriff in Ireland, who could ill afford the expense, and who was hampered thereby in providing education for his children :—

Rayleigh : He would do well to refuse, and go to prison, so as to get the law altered.

Again, the subject of injured husbands coming under discussion, I remember his saying it would be an excellent public example if the injured party more often shot the betrayer dead, and took the consequences, whatever they might be.

Although his own career gave no definite occasion for it, my impression is that he would have been capable of strong action if he had been clear that it was called for. His attitude during the Parliament Bill crisis in 1911 was uncompromising. He thought the Bill a bad one, and he ranged himself with the "diehards" who voted against it be the consequences what they might. In this connection it will be remembered that the words read out by Lord Morley of Blackburn were generally taken to mean that in the event of a defeat of the Bill the Government could and would swamp the opposition by the creation of a sufficient number of Peers. Rayleigh was not convinced that the words would bear no other interpretation : a conversation with Morley of some years later which was repeated to him confirmed him in these doubts.[1]

After boyhood Rayleigh never sought pleasure by the acquisition of new possessions, whether territorial or otherwise. He utterly repudiated the idea of "rounding off the property," though on the advice of his brother some farms were bought near home which, it was thought, could be farmed with profit. On one occasion a small farm forming an "island" in his own property came into the market. The

[1] It may be questioned, however, whether Rayleigh's view of this matter is really tenable. It certainly cannot be reconciled with the account in Morley's *Recollections*.

sitting tenant pressed him to buy, fearing that a chance comer might do so and give him notice to quit. "I don't like the idea of extinguishing an independent property," Rayleigh said. He preferred to let the tenant buy, lending him the money for this purpose. The loan was slowly paid off in the course of years, and Rayleigh wrote to congratulate the new owner on becoming an independent proprietor.

He has been known to buy a picture of family interest under pressure from his mother, but his general philosophy about buying things was "It does not do you much good." It was very rarely that anything was bought for the laboratory that was not absolutely and immediately necessary for the purpose in hand.

His economical instincts have often been referred to. It was an annoyance to him to see guests leave food uneaten on their plates, and his children were not allowed to do it. Again, some wooden lumber from the laboratory was to be got rid of, and he burnt it as fuel on his own study fire. "If I don't burn it up myself, I can't feel sure that anyone else will take the trouble to do it," he said.

Again he would say, with obvious satisfaction, on returning late from a day's work in London that he had dined at Liverpool Street station for 6*d.* on a bun and a glass of milk.

During the time that I can remember the only decorative improvement in which he was the mover was an alteration in the chancel of Terling Church, about the year 1899. An ugly ceiling was removed, so as to expose the oak beams of the roof.

The economical instinct appeared in matters of dress as well. A suit of clothes would be worn, at all events in domestic life, till it was quite threadbare, or even till it came through at the elbows. In person he was neat and most scrupulously clean, but for fashion he had no instinct whatever. I once tried to point out the enormity of going up to London for the day in a black tail-coat with brown shoes. "Well, there may be something in it," he said dubiously. New fashions he positively disliked, and I remember his saying, perhaps

half-seriously, that he defeated his tailor in this matter by keeping new suits of clothes for several years before wearing them.

His instinct for economy had absolutely nothing of the element of meanness in it. Indeed, this was amusingly apparent in one instance which became a family joke. He was presented with a valuable fur coat which had been bought as a surprise; the surprise proved too effective, and he was betrayed into saying, "You should not have sprung this upon me." However, when he saw that this attitude caused disappointment he was sincerely repentant, and indeed went so far as to recommend his brother-in-law Henry Sidgwick to provide himself similarly. The latter remarked, "I was impressed, because it is the only time I can remember hearing John suggest any expenditure that was not strictly necessary." "Are you going to do it?" I asked. "No," he replied, "but I was impressed."

This remark about the fur coat became classical. After the discovery of argon, several large prizes of money were awarded to him by various scientific academies. He opened the letter announcing one of these awards at the breakfast table, and remarked, "I think they are overdoing it. I don't know what I am to say in acknowledgment."

Lady Rayleigh and I answered in the same breath, "Tell them, 'You should not have sprung this upon me.'"

Ceremonial was on the whole irksome to him. Thus he attended the Coronation of King Edward, as in duty bound, but he remarked that he thought that the occasion would have been a good opportunity for having the influenza! Village coronation festivities were held at Terling and at the suggestion of the Vicar he allowed his coronet and robes to be sent for, and under a little pressure, he good-naturedly put them on, to satisfy the curiosity of the villagers.

I add an appreciation by Mrs. Sidgwick: "The more one thinks about him the more one realizes what a remarkable man, under a very quiet exterior, he was—combining so much goodness—desire to do right, I mean—and sympathetic kind-

ness and gentleness, with such great abilities and power of work."

I take the opportunity of collecting here a few miscellaneous dicta and opinions on scientific matters which are worth preserving, and for which no place has been found elsewhere in the book.

On the method of least squares : " I don't think it is much use. I don't know that I think very much even of the process of taking arithmetic means—but perhaps it is going rather too far to say that."

It will perhaps appear on consideration that in most cases of physical measurement the real object of taking a large number of readings is rather to judge of their consistency among themselves than to obtain increased accuracy by combining them. An exception must doubtless be made in such a case as stellar parallax, when the inevitable unsystematic errors are comparable with the whole quantity to be measured.

Again, on the theory of errors of observation : " It is a good thing to read up, and when you have done that you may as well forget it."

Rayleigh of course recognized fully Faraday's great qualities as an experimentalist.

" He could think in front of his apparatus. I cannot do that," he said. This was in fact very noticeable. When a difficulty occurred in the course of the evening's experimenting, he generally preferred to sleep on it, turning to something else in the meanwhile.

But to return to his opinion of Faraday. As a philosopher and thinker he did not place him so high as some other competent judges have done. He admired like everyone else the famous researches on induction of electric currents, but some of Faraday's theorizing about electrolysis appeared to him lacking in distinctness of ideas.

On the subject of the reality of molecules :—

" I never felt any further doubt about it after I read Maxwell's paper on the viscosity of gases. I always thought that

those who doubted or denied it simply showed that they did not understand."

From a letter to Prof. Oliver Lodge (1885) :—

"Pray do not hesitate to consult me on any theoretical difficulties, but I am afraid an off-hand opinion is generally worth little, unless one can speak from memory of conclusions arrived at after attentive consideration."

Again, to the same, in connection with an article which he was writing for the *National Review* on Rayleigh's Scientific Work (1902) :—

"I wonder you have let yourself in for this botheration, but I feel grateful."

Both of these are very characteristic sentences.

On the question of comparing the intensities of lights widely differing in colour :—

Rayleigh : I do not think any definite meaning can be given to it. I think it might be as easy to compare a light with a sound.

Self : I do not understand what you mean.

Rayleigh : I do not know what I mean myself. But I think it might make as definite a basis of measurement as the other.

His notion may perhaps have been that the minimum perceptible in each case could be compared ; or possibly the intensity which began to be painful. I have only recorded this conversation with hesitation. I have no doubt that it is here recorded correctly : but at the time it struck me that he had "let himself go" to a most unusual extent in saying it.

To John Aitken (1917) :—

"I recommend you not to be too modest ! A good instinct and a little mathematics is often better than a lot of calculations."

Again, a conversation with myself on the same subject at a much earlier date :—

Rayleigh : A thorough appreciation of the rule of three will go a long way.

Self : I should have thought that was pretty common.

Rayleigh : On the contrary, I think it is extremely rare.

Again, I was looking at a very elementary introduction to the differential calculus, which had been sent him by the author, and I said that I thought a demonstration inconclusive.

Rayleigh : I daresay it is quite conclusive enough.

If he was satisfied that a mathematical argument was substantially justified, in the application he wished to make, he did not take much interest in a close criticism of it.

Writing to me a letter of encouragement on some early attempts at scientific work (1899) :—

" On the whole there is probably no better life than a scientific one."

In 1918 there were signs that Rayleigh's health was failing. He complained during the winter of 1917-18 of cold feet, which had never troubled him before, and in the late summer of 1918 he had a two months' attack of jaundice, and there was some talk of an operation.

I think he realized at this time that he would not live to an extreme age. Still, he continued at work, publishing five scientific contributions in 1918, and seven in 1919. On December 27th, 1918, he dined at Buckingham Palace, at the banquet in honour of President Wilson's visit.

On January 21st, 1919, he attended the Executive Committee of the National Physical Laboratory, and opened his intention of resigning, but was strongly pressed to defer it, and agreed to do so. It may have been on the same day that he came for the last time to my laboratory at the Imperial College, and found me working with a brilliant mercury vapour lamp. When he saw it, he was reminded of a project he had entertained for some time to produce optical gratings by photographing interference fringes. This could not well be done at Terling, for want of an electric supply. I suggested that we might do it together when he came to stay in London a little later. He seemed pleased with the idea, and a few preliminaries were discussed.

On his way home by the Tube Railway he fainted in the lift. He was supported by the bystanders, and no immediate harm resulted. But he felt it was a warning, and decided that he must resign the heavier part of his London work, the chairmanship of the National Physical Laboratory Committee. Some of his other committee work was continued however. He stayed at 4 Carlton Gardens, for the last time from April 3rd to 14th, 1919, and gave his Presidential Address to the Society for Psychical Research (see Appendix II). He returned to Terling, and his last visit to London was on May 14th, for the Advisory Council of the Research Department. He continued to work at his theoretical investigations until Sunday, May 18th. After reading the lessons in church as usual, he felt faint, and left before the end of the service. He lay on the sofa all the afternoon, and did not get up from bed the next morning.

He struggled downstairs to have tea out of doors in the sunshine on May 27th and 28th, but after that he never left his room again. I saw him for the last time during the week-end of June 1st. Some business matters were mentioned, and he said something implying that he could not expect to live long. He added, "I do not know that I much care for myself, except that there are one or two things that I should like to finish." I asked whether he would like me to make arrangements to depute my duties in London, so that I could stay at Terling, but he did not think that any immediate change was impending.

He was not in pain, and not too ill for conversation, and he liked to see friends and relations in his bedroom. Sometimes he was able to sit up, and amuse himself with novel reading and playing patience, though I think he only did the latter when he had a companion.

On June 25th he dictated to Lady Rayleigh the closing paragraphs of a paper on "The Travelling Cyclone" which was practically complete, and it was sent off to the *Philosophical Magazine*.

On June 30th and the preceding days he seemed to be

somewhat better. He had had dinner, and was reading a novel, Miss Austen's *Emma*, an old favourite with him. The servant who came to remove the tray found him in a heart attack, and had just time to fetch Lady Rayleigh before all was over.

The funeral was at Terling. The chief mourners were the widow and the two surviving sons. The King sent a representative, and the principal officers of the University of Cambridge and of the Royal Society were present. Many scientific men attended, including several who had come over from the United States on War Service. The labourers on the estate lined the pathway to the church door. The service was conducted by the Bishop of Chelmsford, the Bishop of Sheffield (first cousin), and the Rev. C. Boutflower, whom Rayleigh had appointed Vicar of Terling forty years before, and who was about to retire.

The grave is in the quiet corner of the churchyard reserved for the family, adjoining the garden of Terling Place, with access from the latter by a small iron gate.

A simple monument of red sandstone is placed over it, with the words, "For now we see in a glass darkly, but then face to face."

Some months after the funeral the suggestion was made of erecting a Memorial in Westminster Abbey. The Dean readily gave his consent. A meeting was convened by the Chancellor of the University of Cambridge and the President of the Royal Society, and a subscription list opened. A memorial tablet, with a side-face medallion portrait above, was executed by Mr. Derwent Wood, R.A. It is placed in the north-east corner of the north transept, symmetrically with the memorial of Thomas Young. This association was thought to be specially appropriate in view of the intellectual affinity between the two men. The unveiling ceremony was performed by Sir J. J. Thomson, on November 30th, 1921, and he delivered an address.

The inscription runs :—

JOHN WILLIAM STRUTT, O.M., P.C.

3rd Baron Rayleigh

Chancellor of the University of Cambridge, 1908–1919

President of the Royal Society, 1905–1908

An Unerring Leader in the Advancement of Natural Knowledge

Appendix I

LIST OF HONORARY DISTINCTIONS

UNIVERSITIES

Sheepshanks Exhibitioner, Trinity College, Cambridge	1864
Bachelor of Arts, Cambridge University	1865
Master of Arts, Cambridge University	1868
Fellow of Trinity College, Cambridge, 1866. Honorary Fellow	1881
Honorary Doctor of Science, Cambridge University	1888
Chancellor of Cambridge University	1908
Honorary Doctor of Civil Law, Oxford University	1883
Honorary Doctor of Laws, McGill University, Montreal	1884
Honorary Doctor of Laws, Edinburgh University	1884
Honorary Doctor of Philosophy, Heidelberg University	1886
Honorary Doctor of Laws, Glasgow University	1888
Honorary Doctor of Science, Dublin University	1892
Honorary Doctor of Philosophy, University of Erlangen	1893
Honorary Doctor of Science, Victoria University, Manchester	1900
Honorary Doctor of Mathematics, Christiania University	1902
Honorary Doctor of Science, Birmingham University	1909
Honorary Doctor of Science, Leeds University	1910
Honorary Doctor of Laws, Durham University	1913

GOVERNMENTS

Officier Lég. Hon., France	1896
Order of Merit	1902
Foreign Knight of the Prussian Order Pour le Mérite	1903
Privy Councillor of Great Britain	1905
Grand Star of Honour and Merit Red Cross of Spain	1910

LEARNED SOCIETIES AND ACADEMIES

Royal Institution of Great Britain:	
Member	1867
Professor of Natural Philosophy	1887–1903
Honorary Professor	1905

Royal Astronomical Society, London :
Fellow 1867
Royal Society of London
Fellow 1873
Royal Medallist 1882
Secretary 1885–96
Copley Medallist 1899
President 1905–8
Rumford Medallist 1914
Institution of Mechanical Engineers, London :
Honorary Member 1878
Cambridge Philosophical Society :
Member 1880
Manchester Literary and Philosophical Society :
Honorary Member 1886
Wilde Medal 1899
American Philosophical Society, Philadelphia :
Member 1886
Königliche Gesellschaft der Wissenschaften (Göttingen) :
Correspondent 1886
Foreign Member 1906
Royal Society of Edinburgh :
Fellow 1886
American Academy of Arts and Sciences, Boston :
Foreign Honorary Member 1888
London Mathematical Society :
De Morgan Medal 1890
Royal Bavarian Academy, Munich :
Associate 1890
Académie des Sciences, Paris :
Correspondent 1890
Prix Leconte 1895
Foreign Associate 1910
Institution of Civil Engineers, London :
Honorary Member 1891
Reale Academia dei Lincei, Rome :
Corresponding Member 1891
Royal Society of Sciences, Upsala :
Ordinary Member 1891
Junior Institution of Engineers, London : . . .
Honorary Member 1892
Smithsonian Institution, Washington :
Hodgkins Prize 1895

Società Italiana delle Scienze, Rome:
Matteuci Medal 1895
Foreign Member 1898
Reale Academia delle Scienze, Turin:
Corresponding Member 1895
Bressa Prize 1896
Société Hollandaise des Sciences, Haarlem:
Foreign Member 1895
Academie Royale des Sciences, Amsterdam:
Member 1895
United States.—National Academy of Sciences, Washington:
Barnard Medal 1895
Foreign Associate 1898
Chemical Society of London:
Faraday Medal 1895
Königliche Akademie der Wissenschaften, Berlin:
Corresponding Member 1896
Société Scientifique "Antonio Alzati," Mexico:
Honorary Member 1896
Royal Danish Academy of Sciences, Copenhagen:
Ordinary Member 1896
Royal Swedish Academy, Stockholm:
Foreign Member 1897
Société Helvetique des Sciences Naturelles, Geneva:
Honorary Member 1897
Società Italiana delle Scienze, Rome:
Foreign Member 1898
Deutsche Chemische Gesellschaft, Berlin:
Honorary Member 1899
Académie Impérial Militaire de Médecine, St. Petersburg:
Honorary Member 1899
New York Academy of Sciences:
Honorary Member 1899
Kaiserliche Akademie der Wissenschaften, Vienna:
Foreign Corresponding Member 1902
Société Batave de Philosophie Experimentale (Rotterdam):
Corresponding Member 1904
Nobel Institute, Stockholm:
Nobel Prize for Physics 1904
Royal Society of Arts, London:
Albert Medal 1905
Société Impériale des Amis des Sciences Naturelles, Moscow:
Honorary Member 1905

Society of Engineers, London :
Honorary Member 1905
Royal Medical and Chirurgical Society, London :
Honorary Member 1905
Société Française de Physique :
Honorary Member 1906
Reale Academia de Ciencias, Madrid :
Foreign Correspondent 1906
Scientific Society of Christiania :
Foreign Member 1908
Imperial University of Moscow :
Honorary Member 1909
Imperial Society of Natural Science, Moscow :
Honorary Associate 1909
American Chemical Society, Boston :
Honorary Member 1909
Regia Scientiarum Academia Borussica, Berlin :
Foreign Member 1910
Institution of Naval Architects, London :
Honorary Member 1911
Franklin Institute, Philadelphia :
Elliot Cresson Gold Medal 1914
Academia Romana, Bucharest :
Honorary Member 1914
Académie Hongraise des Sciences, Budapest :
Foreign Corresponding Member 1914
Imperial University of Kazan :
Honorary Member 1916

Appendix II

PRESIDENTIAL ADDRESS TO THE SOCIETY FOR PSYCHICAL RESEARCH

DELIVERED ON APRIL 11TH, 1919

Before entering upon the matters that I had intended to lay before you, it is fitting that I should refer to the loss we have sustained within the last few days in the death of Sir William Crookes, a former President of the Society during several years from 1896–1899, and a man of world-wide scientific reputation. During his long and active life he made many discoveries in Physics and Chemistry of the first importance. In quite early days his attention was attracted by an unknown and brilliant green line in the spectrum, which he succeeded in tracing to a new element named Thallium, after its appearance. Later he was able so to improve vacua as to open up fresh lines of enquiry with remarkable results in more than one direction. The radiometer, a little instrument in which light, even candle-light, or ordinary day-light, causes the rotation of delicately suspended vanes, presents problems even yet only partially solved. And his discoveries relating to electric discharge in high vacua lie near the foundation of the modern theories of electricity as due to minute charged particles called electrons, capable of separation from ordinary chemical atoms, and of moving with speeds of the order of the speed of light. One is struck not only by the technical skill displayed in experiments more difficult at the time they were made than the younger generation of workers can easily understand, but also by the extraordinary instinct which directed Crookes' choice of subjects. In several cases their importance was hardly realized at the time, and only later became apparent.

I shall have occasion presently to notice in some little detail his early "Notes on Phenomena called Spiritual." It was these that attracted my own attention to the subject. In 1889 he published further "Notes of Séances with D. D. Home" in Vol. VI.

of our *Proceedings*. I fancy that he was disappointed with the reception that his views met with, having been sanguine enough to expect that he would obtain the same credence when he wrote on psychical matters as when he was dealing with Physics or Chemistry. In later years I understand he did not often introduce the subject, but when questioned was firm that he had nothing to retract. One would give much to know whether this attitude is still maintained.

Any hesitation that I may have felt in undertaking the honourable office to which you have called me was largely due to the fact that I have no definite conclusions to announce, and that such experiences as I have had were long ago, and can hardly now carry weight as evidence to anyone but myself. But I have always taken an interest in questions such as those considered by the Society, and I may perhaps as well give a short account of what I have seen, for it will at any rate help to explain my attitude and serve as a foundation for comment.

I may begin with what is now called hypnotism. This is an old story; but many have forgotten, or never realized, the disbelief which was general in the 'fifties of the last century both on the part of the public and of medical men. As to the former, reference may be made to *Punch*,[1] and as to the latter I suppose there can be no doubt, although of course there were distinguished exceptions. At the present day orthodox medical opinion has so far shifted its ground as to claim for the profession control of what was formerly dismissed as impossible and absurd—certainly a less unreasonable position.

It was some ten or eleven years from the date of *Punch's* cartoon that I witnessed in a friend's rooms at Cambridge an exhibition of the powers of Madame Card. I think eight or ten of us were tried, including myself. We were made to gaze for a time at a "magnetic" disc; afterwards she made passes over our closed eyes, and finally defied us to open them. I and some others experienced no difficulty; and naturally she discarded us and developed her powers over those—about half the sitters—who had failed or found difficulty. Among the latter were personal friends of my own and two well-known University athletes. One was told that he could not give his name, another that he would have to cross

[1] Vol. XXIV, p. 120 (1853). *Lecturer on Electro-biology:* "Now, Sir! you can't jump over that stick." *Subject:* "Jump? Eh! Ugh! Lor bless me, jump? No, I know I can't—never could jump—Ugh!" (Thunders of applause from the gentlemen in the cane-bottom chairs —[i.e. believers].)

the room towards her when she beckoned, and so on. In spite of obvious efforts to resist her influence they had to obey. In conversation afterwards they assured me that they could not help it; and indeed they made such fools of themselves that I had no difficulty in believing them. From that evening I have never felt any doubt as to the possibility of influencing unwilling minds by suggestion; and I have often wished that on other occasions, where dubious phenomena were in question, some of which I shall presently refer to, conviction one way or the other had followed this precedent. I ought to add that, although stories were afloat to that effect, I never saw the influence of Madame Card conveyed otherwise than by word or gesture.

After this experience I was not disinclined to believe that what was, or at any rate had recently been, orthodox opinion might be quite wrong, and accordingly became interested in what I heard from friends of the doings of Home and other so-called mediums. Some of the stories could, as it seemed, be explained away only on the supposition of barefaced lying, or more charitably as the result of hallucination, whether self-induced, or due to the suggestion and influence of others. The possibility of the latter view cannot be left out of account, but I have never seen anything to show that it has the remotest application to my own experience or that of the friends with whom I have co-operated.

The interest that I felt was greatly stimulated by the appearance of Sir W. Crookes' "Notes of an Enquiry into the Phenomena called Spiritual during the years 1870–73."[1] I was acquainted with some of the author's scientific work, and knew that he was a skilful experimenter and likely to be alive to the precautions required in order to guard against sense illusions. Presumably also he would feel the difficulty of accepting conclusions so much out of harmony with ordinary and laboratory experience. If heavy tables in a dining-room can leave the floor, how is it that in the laboratory our balances can be trusted to deal with a tenth of a milligram?

I have lately read over again Sir W. Crookes' article, and I do not wonder at the impression it produced upon me. I am tempted to quote one or two passages against which I find my old pencil marks. Under the heading—the Appearance of Hands, either Self-luminous or visible by Ordinary Light, he writes, "I have retained one of these hands in my own, firmly resolved not to let it escape. There was no struggle or effort made to get loose, but it gradually seemed to resolve itself into vapour, and faded in that

[1] *Quarterly Journal of Science*, January, 1874.

manner from my grasp." I believe that the rationalistic explanation is that the hand was an inflated glove, like a rubber balloon, from which the air gradually leaked away, but I gave Sir W. Crookes credit for being able to retain the rubber.

Another incident of an entirely different character is thus described. "A lady was writing automatically by means of the planchette. I was trying to devise a means of proving that what she wrote was not due to 'unconscious cerebration.' The planchette, as it always does, insisted that, although it was moved by the hand and arm of the lady, the *intelligence* was that of an invisible being who was playing on her brain as on a musical instrument, and thus moving her muscles. I therefore said to this intelligence, 'Can you see the contents of this room?' 'Yes,' wrote the planchette. 'Can you see to read this newspaper?' said I, putting my finger on a copy of the *Times*, which was on the table behind me, but without looking at it. 'Yes,' was the reply of the planchette. 'Well,' I said, 'if you can see that, write the word which is now covered by my finger, and I will believe you.' The planchette commenced to move. Slowly and with great difficulty, the word 'however' was written. I turned round, and saw the word 'however' was covered by the tip of my finger.

"I had purposely avoided looking at the newspaper when I tried this experiment, and it was impossible for the lady, had she tried, to have seen any of the printed words, for she was sitting at one table, and the paper was on another table behind, my body intervening."

The two mediums whose names are mentioned in the article, and with whom most of the observations were made, are Home and Miss Fox, afterwards Mrs. Jencken. A highly desirable characteristic of Home's mediumship was the unusual opportunity allowed to the sense of sight. Home always objected to darkness at his séances. "Indeed," says Sir William Crookes, "except on two occasions . . . everything that I have witnessed with him has taken place in the light."

I found (and indeed still find) it difficult to accept what one may call the "knave and fool theory" of these occurrences; but failing that, it would seem to follow that one must admit the possibility of much that contrasts strongly with ordinary experience, and I was naturally anxious to obtain first-hand information on which I could form an independent judgment. Home was no longer available, but I was able to obtain the co-operation of Mrs. Jencken, who stayed in my country house as guest during two or three visits extending altogether, I suppose, over fourteen days or so. She was accompanied by a nurse and baby, and for a small part of

the time by Mr. Jencken, who seemed curiously slow to understand that we had to regard him as well as his wife with suspicion, when I explained that we could not attach importance to séances when both were present. It may be well to add that they received nothing beyond the usual courtesy and entertainment due to guests.

The results were upon the whole disappointing, and certainly far short of those described by Sir W. Crookes. Nevertheless there was a good deal not easy to explain away. Very little of importance occurred in a good light. It is true that at any hour of the day Mrs. Jencken was able to get raps upon a door by merely placing her fingers upon it. The listener, hearing them for the first time, felt sure there was some one on the other side, but it was not so. The closest scrutiny revealed no movement of her fingers, but there seemed nothing to exclude the possibility of bone-cracking with the door acting as sounding-board. However, on one or two occasions loud thumps were heard, such as one would hardly like to make with one's knee. With the exception of her fingers Mrs. Jencken seemed always to stand quite clear, and the light was good.

On the other hand, during séances the light was usually bad —gas turned very low. But in some other respects the conditions may be considered good. Before commencing, the room was searched and the doors locked. Besides Mrs. Jencken, the sitters were usually only Lady Rayleigh and myself. Sometimes a brother or a friend came. We sat close together at a small, but rather heavy, pedestal table ; and when anything appeared to be doing we held Mrs. Jencken's hand, with a good attempt to control her feet also with ours ; but it was impracticable to maintain this full control during all the long time occupied by the séances. In contrast to some other mediums, Mrs. Jencken was not observed to fidget or to try to release her limbs.

As I have said, the results were disappointing ; but I do not mean that very little happened or what did happen was always easy to explain. But most of the happenings were trifling, and not such as to preclude the idea of trickery. One's coat-tails would be pulled, paper cutters, etc., would fly about, knocks would shake our chairs, and so on. I do not count messages, usually of no interest, which were spelt out alphabetically by raps that seemed to come from the neighbourhood of the medium's feet. Perhaps what struck us most were lights which on one or two occasions floated about. They were real enough, but rather difficult to locate, though I do not think they were ever more than six or eight feet away from us. Like some of those described by Sir W. Crookes, they might be imitated by phosphorus enclosed in

cotton wool; but how Mrs. Jencken could manipulate them with her hands and feet held, and it would seem with only her mouth at liberty, is a difficulty.

Another incident hard to explain occurred at the close of a séance after we had all stood up. The table at which we had been sitting gradually tipped over until the circular top nearly touched the floor, and then slowly rose again into the normal position. Mrs. Jencken, as well as ourselves, was apparently standing quite clear of it. I have often tried since to make the table perform a similar evolution. Holding the top with both hands, I can make some, though a bad, approximation; but it was impossible that Mrs. Jencken could have worked it thus. Possibly something better could be done with the aid of an apparatus of hooks and wires; but Mrs. Jencken was a small woman, without much apparent muscular development, and the table for its size is heavy. It must be admitted that the light was poor, but our eyes were then young, and we had been for a long time in the semi-darkness.

In common, I suppose, with most witnesses of such things, I repudiate altogether the idea of hallucination as an explanation. The incidents were almost always unexpected, and our impressions of them agreed. They were either tricks of the nature of conjuring tricks, or else happenings of a kind very remote from ordinary experience.

A discouraging feature was that attempts to improve the conditions usually led to nothing. As an example, I may mention that after writing, supposed to be spirit writing, had appeared, I arranged pencils and paper inside a large glass retort, of which the neck was then hermetically sealed. For safety this was placed in a wooden box, and stood under the table during several séances. The intention was to give opportunity for evidence that would be independent of close watching during the semi-darkness. It is perhaps unnecessary to say that though scribbling appeared on the box, there was nothing inside the retort. Possibly this was too much to expect. I may add that on recently inspecting the retort I find that the opportunity has remained neglected for forty-five years.

During all this time I have been in doubt what interpretation to put upon these experiences. In my judgment the incidents were not good enough, or under good enough conditions, to establish occult influences; but yet I have always felt difficulty in accepting the only alternative explanation. Some circumstances, if of secondary importance, are also worthy of mention. Unlike some other mediums that I have known, Mrs. Jencken never tried to divert one's attention, nor did she herself seem to be observant or

watching for opportunities. I have often said that on the unfavourable hypothesis her acting was as wonderful as her conjuring. Seldom, or never, during the long hours we were together at meals or séances did she make an intelligent remark. Her interests seemed to be limited to the spirits and her baby.

Mr. Jencken is another difficulty. He, an intelligent man, was a spiritualist, and, I have no reason to doubt, an honest one, before he married his wife. Could she have continued to deceive him ? It seems almost impossible. He bore eye-witness to the baby—at the age of three months I think it was—taking a pencil and writing a spirit message, of which we saw what purported to be a photograph. If, on the other hand, he had found her out, would he have permitted her to continue her deceptions ?

After the death of Home and Mrs. Jencken, so-called physical manifestations of a well attested kind seem rather to have fallen into abeyance, except in the case of Eusapia Paladino. Although I attended one or two of her séances at Cambridge and saw a few curious things, other members of the Society have had so much better opportunities that I pass them by. There is no doubt that she practised deception, but that is not the last word.

One of the difficulties which beset our inquiry is the provoking attitude of many people who might render assistance. Some see nothing out of the way in the most marvellous occurrences, and accordingly take no pains over the details of evidence on which everything depends. Others attribute all these things to the devil, and refuse to have anything to say to them. I have sometimes pointed out that if during the long hours of séances we could keep the devil occupied in so comparatively harmless a manner we deserved well of our neighbours.

A real obstacle to a decision arises from the sporadic character of the phenomena, which cannot be reproduced at pleasure and submitted to systematic experimental control. The difficulty is not limited to questions where occult influences may be involved. This is a point which is often misunderstood, and it may be worth while to illustrate it by examples taken from the history of science.

An interesting case is that of meteorites, discussed by Sir L. Fletcher, formerly Keeper of Minerals in the British Museum, from whose official pamphlet (published in 1896) some extracts may be quoted :—" 1. Till the beginning of the present [i.e. 19th] century, the fall of stones from the sky was an event, the actuality of which neither men of science nor the mass of the people could be brought to believe in. Yet such falls have been recorded from the earliest times, and the records have occasionally been received as authentic by a whole nation. In general, however, the witnesses

of such an event have been treated with the disrespect usually shown to reporters of the extraordinary, and have been laughed at for their supposed delusions; this is less to be wondered at when we remember that the witnesses of a fall have usually been few in number, unaccustomed to exact observation, frightened by what they both saw and heard, and have had a common tendency towards exaggeration and superstition."

After mention of some early stones, he continues :—

"3. These falls from the sky, when credited at all, have been deemed prodigies or miracles, and the stones have been regarded as objects for reverence and worship. It has even been conjectured that the worship of such stones was the earliest form of idolatry. . . . The Diana of the Ephesians, 'which fell down from Jupiter,' and the image of Venus at Cyprus, appear to have been, not statues, but conical or pyramidal stones."

"5. Three French Academicians, one of whom was the afterwards renowned chemist Lavoisier, presented to the Academy in 1772 a report on the analysis of a stone said to have been seen to fall at Lucé on September 13, 1768. As the identity of lightning with the electric spark had been recently established by Franklin, they were in advance convinced that 'thunder-stones' existed only in the imagination; and never dreaming of the existence of a 'sky-stone' which had no relation to a 'thunder-stone,' they somewhat easily assured both themselves and the Academy that there was nothing unusual in the mineralogical characters of the Lucé specimen, their verdict being that the stone was an ordinary one which had been struck by lightning."

"6. In 1794 the German philosopher Chladni, famed for his researches into the laws of sound, brought together numerous accounts of the fall of bodies from the sky, and called the attention of the scientific world to the fact that several masses of iron, of which he specially considers two, had in all probability come from outer space to this planet."

In 1802 Edward Howard read a paper before the Royal Society of London giving an account of the comparative results of a chemical and mineralogical investigation of four stones which had fallen in different places. He found from the similarity of their component parts "very strong evidence in favour of the assertion that they had fallen on our globe. They have been found at places very remote from each other, and at periods also sufficiently distant. The mineralogists who have examined them agree that they have no resemblance to mineral substances properly so called, nor have they been described by mineralogical authors." After this quotation from Howard, Fletcher continues :—

" 13. This paper aroused much interest in the scientific world, and, though Chladni's theory that such stones come from outer space was still not accepted in France, it was there deemed more worthy of consideration after Poisson (following Laplace) had shown that a body shot from the moon in the direction of the earth, with an initial velocity of 7,592 feet a second, would not fall back upon the moon, but would actually, after a journey of sixty-four hours, reach the earth, upon which, neglecting the resistance of the air, it would fall with a velocity of about 31,508 feet a second.

" 14. Whilst the minds of the scientific men of France were in this unsettled condition, there came a report that another shower of stones had fallen, this time . . . within easy reach of Paris. To settle the matter finally, if possible, the physicist Biot was directed by the Minister of the Interior to inquire into the event on the spot. After a careful examination . . . Biot was convinced that on Tuesday, April 26, 1803, about 1 p.m., there was a violent *explosion* in the neighbourhood of l'Aigle . . . that some moments before . . . a *fire ball* in quick motion was seen . . . that on the same day many stones fell in the neighbourhood of l'Aigle. Biot estimated the number of the stones at two or three thousand. . . . With the exception of a few little clouds of ordinary character, the sky was quite clear. The exhaustive report of Biot, and the conclusive nature of his proofs, compelled the whole of the scientific world to recognize the fall of stones on the earth from outer space as an undoubted fact."

I commend this history to the notice of those scientific men who are so sure that they understand the character of Nature's operations as to feel justified in rejecting without examination reports of occurrences which seem to conflict with ordinary experience. Every tiro now knows that the stones to be seen in most museums had an origin thought impossible by some of the leading and most instructed men of about a century ago.

Other cases of strange occurrences, the nature or reality of which is, I suppose, still in doubt, are " Globe lightning " and " Will of the wisp." The evidence for globe lightning is fairly substantial, but in the judgment of many scientific men is outweighed by the absence of support in laboratory experience. At one time I was more disposed to believe in it than I am now, in view of the great extension of electrical experimenting during the last thirty years. Kelvin thought it might be explained as an ocular illusion. By a lightning flash the retina is powerfully impressed, it may be excentrically, with the formation of a prolonged positive " spectrum " or image which, as the eye tries to follow it, appears to sail slowly along. Some seconds later, the arrival of the sound

of thunder causes a shock, under which the luminous globe disappears and is thought to have burst explosively. I think this explanation, which would save the good faith and to some extent the good sense of the observers, deserves attention.

Then again the Will of the wisp, for which I take it there used to be plenty of evidence. I have been told by the Duke of Argyle —the friend and colleague of Gladstone—that in his youth it was common at Inveraray, but had been less seen latterly, owing, he thought, to drainage operations. Chemists will not readily believe in the spontaneous inflammation of "marsh gas," but I have heard the suggestion made of phosphoric gases arising from the remains of a dead sheep that had got entangled.

The truth is that we are ill equipped for the investigation of phenomena which cannot be reproduced at pleasure under good conditions. And a clue is often necessary before much progress can be made. Men had every motive for trying to understand malaria. Exposure at night on low ground was known to be bad; and it had even been suggested that mosquito nets served as a protection; but before Pasteur, and indeed for some years after, it seems never to have occurred to anyone that the mosquito itself was the vehicle. Sir A. Geikie has remarked that until recent times the study of the lower forms of life was regarded with something like contempt. Verily, the microbes have had their revenge.

But when all this has been said we must not forget that the situation is much worse when it is complicated by the attempts of our neighbours to mislead us, as indeed occasionally happens in other matters of scientific interest where money is involved. Here also the questions before this Society differ from most of those dealt with by scientific men, and may often need a different kind of criticism.

Such criticism it has been the constant aim of the Society to exercise, as must be admitted by all who have studied carefully our published matter. If my words could reach them, I would appeal to serious inquirers to give more attention to the work of this Society, conducted by experienced men and women, including several of a sceptical turn of mind, and not to indulge in hasty conclusions on the basis of reports in the less responsible newspaper press or on the careless gossip of ill-informed acquaintances. Many of our members are quite as much alive to *a priori* difficulties as any outsider can be.

Of late years the published work of the Society has dealt rather with questions of another sort, involving telepathy, whether from living or other intelligences, and some of the most experienced and cautious investigators are of opinion that a case has been made

out. Certainly some of the cross-correspondences established are very remarkable. Their evaluation, however, requires close attention and sometimes a background of information, classical and other, not at the disposal of all of us. In this department I often find my estimate of probabilities differing from that of my friends. I have more difficulty than they feel over telepathy between the living, but if I had no doubts there I should feel less difficulty than many do in going further. I think emphasis should be laid upon the fact that the majority of scientific men do not believe in telepathy, or even that it is possible. We are very largely the creatures of our sense-organs. Only those physicists and physiologists who have studied the subject realize what wonderful instruments these are. The eye, the ear, and the nose—even the human nose—are hard to beat, and within their proper range are more sensitive than anything we can make in the laboratory. It is true that with long exposures we can photograph objects in the heavens that the eye cannot detect ; but the fairer comparison is between what we can see and what can be photographed in say 1/10th second—all that the eye requires. These sense-organs, shared with the higher animals, must have taken a long time to build up, and one would suppose that much development in other directions must have been sacrificed or postponed in that interest. Why was not telepathy developed until there could be no question about it ? Think of an antelope in danger from a lion about to spring upon him, and gloating over the anticipation of his dinner. The antelope is largely protected by the acuteness of his senses and his high speed when alarmed. But would it not have been simpler if he could know something telepathically of the lion's intention, even if it were no more than vague apprehension warning him to be on the move ?

By telepathy is to be understood something more than is implied in the derivation of the word, the conveying of feeling or information otherwise than by use of the senses, or at any rate the known senses. Distance comes into the question mainly because it may exclude their ordinary operation. Some appear to think that all difficulty is obviated by the supposition of an unknown physical agency capable of propagating effects from one brain to another, acting like the transmitter and receiver in wireless telegraphy or telephony. On a physical theory of this kind one must expect a rapid attenuation with distance, not suggested by the records. If distance is an important consideration, one might expect husbands and wives with their heads within two or three feet of one another to share their dreams habitually. But there is a more fundamental objection. Specific information is, and can only be, conveyed in this manner

by means of a *code*. People seem to forget that all speaking and writing depend upon a code, and that even the voluntary or involuntary indications of feeling by facial expression or gestures involves something of the same nature. It will hardly be argued that telepathy acts by means of the usual code of common language, as written or spoken.

The conclusion that I draw is that no pains should be spared to establish the reality of telepathy on such sure ground that it must be generally admitted by all serious inquirers. It is quite natural that those who have already reached this position should be more interested in the question of communications from the dead. To my mind telepathy with the dead would present comparatively little difficulty when it is admitted as regards the living. If the apparatus of the senses is not used in one case, why should it be needed in the other ?

I do not underrate the difficulties of the investigation. Very special conditions must be satisfied if we are to be independent of the good faith of the persons primarily concerned. The performance of the Zanzigs may be recalled. When there could be no question of confederates, answers respecting objects suddenly exhibited were given with such amazing rapidity that secret codes seemed almost excluded. But when a party in which I was included attempted to get a repetition under stricter conditions, there was an almost entire failure. Our requirement was simply that the husband should not speak *after* he had seen the object that was to be described by the wife. But I must add the inevitable qualification. Towards the end of the evening cards were correctly told several times, when we were unable to detect anything that could serve as audible signals.

I have dwelt upon the difficulties besetting the acceptance of telepathy, but I fully recognize that a strong case has been made out for it. I hope that more members of the Society will experiment in this direction. It is work that can be done at home, at odd times, and without the help of mediums, professional or other. Some very interesting experiences of this kind have been recorded by a former President, Prof. Gilbert Murray. With perhaps an excess of caution, he abstained from formulating conclusions that must have seemed to most readers to follow from the facts detailed. I trust we may hear still more from him.

It is hardly necessary to emphasize that in evaluating evidence it is quality rather than quantity with which we are concerned. No one can doubt the existence of apparently trustworthy reports of many occult phenomena. For this there must be a reason, and our object is to find it. But whatever it may be, whether

reality of the phenomena, or the stupidity or carelessness or worse of the narrators, a larger sweep is sure to add to the material. However, we may hope that such additions will occasionally afford clues, or at least suggestions for further inquiry. And if the phenomena, or any of them, are really due to supernormal causes, further solid evidence of this will emerge. I feel that I ought to apologize for giving utterance to what must seem platitudes to the more experienced working members of the Society.

Some of the narratives that I have read suggest the possibility of prophecy. This is very difficult ground. But we live in times which are revolutionary in science as well as in politics. Perhaps some of those who accept extreme "relativity" views, reducing time to merely one of the dimensions of a four-dimensional manifold, may regard the future as differing from the past no more than north differs from south. But here I am nearly out of my depth, and had better stop.

I fear that my attitude, or want of attitude, will be disappointing to some members of the Society who have outstripped me on the road to conviction, but this I cannot help. Scientific men should not rush to conclusions, but keep their minds open for such time as may be necessary. And what was at first a policy may become a habit. After forty-five years of hesitation it may require some personal experience of a compelling kind to break the crust. Some of those who know me best think that I ought to be more convinced than I am. Perhaps they are right.

However this may be, I have never felt any doubt as to the importance of the work carried on by the Society over many years, and I speak as one who has examined not a few of the interesting and careful papers that have been published in the *Proceedings*. Several of the founders of the Society were personal friends, and since they have gone the same spirit has guided us. Our goal is the truth, whatever it may turn out to be, and our efforts to attain it should have the sympathy of all, and I would add especially of scientific men.

Appendix III

COLLECTION OF JESTS AND ANECDOTES

[SELECTION]

A large crowd were assembled in the West for an execution. The sheriff asked the prisoner whether he wished to address them. "No," said he, "I don't care to." On which another man got up saying that he would like to speak.

Sheriff to prisoner: "This is your show. Have you any objection?" No," said the prisoner, "I don't object; but hang me first."

A lady asked a visitor whether he cared much for Botticelli. "I don't care about it," he said; "in fact I like Chianti better." After they had left, his friend said to him: "You made a precious fool of yourself over Botticelli. Don't you know that Chianti is a wine and Botticelli a cheese?"

Priest (to Irishman, who had stolen a pig from Widow Maloney, at confession): "Ah, Paddy, think of the time when you meet Widow M. and the pig at the day of judgment."—"And will the pig be there, your reverence?"—"Yes, Paddy, and what will you have to say to Widow M. then?"—"I'll say—Widow Maloney, there's your pig."

A Dublin carman conveying an English sightseer pointed out the Post Office with its four statues outside. "And what are the four figures?"—"The Apostles, your honour." "But I always thought there were twelve Apostles." "Sure, the other eight are inside, sorting the epistles."

Lefanu tells of a child who called for his mother. "Come quick, here is Tom up to his ankles in the bog." "That won't hurt him," was the reply. "But he is the wrong way up, mother."

Saying attributed to Edison.

Genius is ten per cent. inspiration, and the rest perspiration.

A young man who had been a patient met Andrew Clark at dinner. "How is this, Dr. Clark, after what you said to me? I see you drinking champagne." "Oh," said he, "doctors are one thing and patients are another. Besides, when I get home to-night I shall probably find fifty letters waiting for me." This silenced the young man for the moment, but he soon recovered himself. —"Do you mean to say, Dr. Clark, that you will answer those letters before you go to bed?"—"I didn't say that, but when I have had the champagne I don't care a damn whether they are answered or not."

A party of American lynchers had made a mistake and killed the wrong man. They felt that some explanation and apology was due to the widow, and a deputation was appointed for the purpose. The leader addressed the widow in these terms:—"We must own, marm, that you've the laugh of us this time."

Mrs. Steel's Story.

The night before the Colonel of a Pathan regiment started for a holiday, the drummer-boy came to ask for a certain cuckoo-clock. "Oh no," said the Colonel, "I promised it you when I go home. Now I am going out on a shooting expedition, I shall soon be back." "I don't know about that," said the boy, "and I wish you would give it me now."

A few days after the start he recognized in the bazaar a man whom he had dismissed from the regiment for misconduct. Knowing the revengeful character of these people, he took the precaution of placing a bolster to represent him in bed, while he slept outside. In the morning there were dagger cuts in the bolster. A little later he met the man in the bazaar, who seemed surprised to see him. Going up he said, "You are just the man I was looking for. I am going for three weeks' shooting, and I want some one with me as a servant. Will you come?" The man looked at him and hesitated, but at last said, "I will, huzzoor." "And then," said Mrs. Steel, "he knew he was safe."

Commanding Officer visiting an out-of-the-way station: "Have you anything to complain of?" "Nothing special; only that it's terribly dull." "Pray, sir, have you no resources in yourself? Can't you smoke a cigar, or drink brandy and water?"

A couple were celebrating their golden wedding. In the morning there was a thanksgiving service in church. As they came out, an

old villager was heard to mutter: "Well, I'm glad he has made an honest woman of her at last."

There has been a good deal of talk about adapting education to the future calling of the scholars. A schoolmistress, under examination upon the subject, said she had taken pains to ascertain the predilections of her pupils. One intended to be a soldier and most of the remainder hoped to be pirates.

Hairdresser to a customer: "A big head is a fine thing. It gives room for brains. Brains is the best thing you can 'ave. It nourishes the roots of the 'air."

An Irishman in his dream met Queen Victoria. After various inquiries had passed, she suggested a drink and asked whether he would have it hot or cold. Seeing that she had a tin kettle in her hand, he said he would take it hot; but while the kettle was heating, he awoke. "Pat," said I to myself, "you were a thundering fool you didn't take it cold."

Omnibus conductor to a Frenchman. "Twopence, please!" "Je ne comprends pas." "Your fare is two-pence," and so on for some time, after which the conductor puts his head into the bus: "Can any lady or gentleman tell me the French for 'You're a bloody fool'?"

At ——, a lady's maid who was late in answering the bell excused herself by saying that she forgot herself as she was so much interested in a discussion going on below as to whether we were all descended from Darwin.

A curious man in the train to a fellow-traveller with a basket: "I hope you won't think me impertinent, but may I inquire what you have in that basket?"—"Oh, a mongoose."—"Would it be too much if I asked what the mongoose was for?"—"Not at all; the fact is an aunt of mine is under the delusion that she is infested with snakes, and I am taking down the mongoose to tackle the snakes."—"But didn't I understand you to say that the snakes were imaginary?"—"Yes, so is the mongoose!"

McMahon had the reputation of saying stupid things. He was reviewing a regiment in which there was a negro, and they suggested to him to say something to the man. He began, "Vous êtes le nègre?" And when the man pleaded guilty: "Continuez."

Andrew Clark told me of a patient who came to consult him looking a miserable object. A.C. asked as to his habits and diet. "I come down to breakfast at nine and I have a cup of coffee and a poached egg. Afterwards I go into the garden and then come back and read the paper. At lunch I have a mutton-chop, etc."—"If you go on like that I don't wonder that you are in a bad state. At breakfast drink tea and have your egg plain-boiled. Afterwards give your digestion a chance. Read the paper first and walk in the garden later. Then at lunch you must have a beef-steak; a mutton-chop is poison to a man like you. And come and see me again in three months." At the end of the three months he walked in looking another creature and said, "How could you tell, Dr. Clark, that I was doing the wrong things?"

An Irishman explaining a black eye to a friend. "He spoke disrespectfully of my sister." "I didn't know you had a sister." "Neither I have, but it's the principle of the thing."

A child's version of an incident in the New Testament. He said, "Bring me the tribute money" and they brought unto him a penny. "Whose miserable subscription is this?"

A certain doctor practised curing by suggestion. One day a man came complaining that his nights and even his days were disturbed by a sort of nightmare which took the form of a woman attended by imps. He could not get rid of it and feared he should go mad. The doctor operated and after a few weeks the man said he was better. The forms were less persistent. The improvement continued, the forms becoming more shadowy, and the patient began to hope that the treatment would effect a cure. About this time a lady called on the doctor saying she believed her husband had been consulting him, and that she had become very anxious about him. He now took hardly any notice of her, and yesterday he had sat down upon a chair already occupied by one of the children, just as if he hadn't seen it.

Judge: "Now tell me exactly what he said, and use his own words." Witness: "He said he stole the pig." Judge: "He did not use the third person." Witness: "There was no third person." Judge: "Tut! I suppose he really said, 'I stole the pig.'" "No, he never mentioned your lordship at all."

Pepper, lecturing before Queen Victoria, is reported to have said:

"The oxygen and hydrogen will now have the honour of combining before your Majesty."

Charles Darwin to his family: "What is this place 'Wean' (Wien) where they seem to publish important books?"

A boy whose father had recently died was asked as to his father's last words. "He didn't have any. Mother was with him until the end."

A man going to a fancy-dress ball as the devil had as tail an eel in an umbrella case.

Uncle ——, reproved by some one for want of consideration for his mother: "How can you behave so to the mother that bore you?" replied "The mother that bores me, you mean."

Judge to convicted prisoner: "The Almighty has given you health and strength, instead of which you go about the country stealing hens."

A man with an uncertain ailment, not content with his local doctor, called in Sir Reginald from London. After the examination it occurred to him that what it most concerned him to know was what they said to one another, rather than the story they would concoct for him, so he directed his servant to follow them to the other room and to listen at the keyhole. When the servant came back: "Now then, tell me exactly what they said." "Our Mr. Brown here asks Sir Reginald to have a glass of sherry." "I don't care about that. Go on." "Mr. Brown says he doesn't know what's the matter with you. And Sir Reginald says nor he either, and he doubted whether anyone will till the inquest."

In an Aberdeen society there was a debate and division on the existence of the Deity. Some one reporting the result said, "I am glad to say He got a working majority."

During the early days of the Home Rule movement, Herbert Spencer said to me in the Athenæum, "I can only congratulate myself on leaving no descendants"; such was his dislike. Not bad for the prophet of evolution.

An American on presentation to the Pope: "Glad to make your acquaintance, sir. I knew your father, the late Pope."

Fisher remarked: "It is a mistake to be off speaking terms with a man: you lose opportunities for saying nasty things."

Chamberlain on Speaker Brand in illustration of the importance of wig and robes. An Irish member was disputing the ruling of the Speaker and in his (Chamberlain's) opinion made out so good a case that he was curious to see how Brand would deal with it. Brand rose slowly, wrapped his robe about him, and said, "The hon. member must know that he is trifling with the House."

In illustration of different voices in different parts of United States. A Southerner was in love with a Boston girl, who died prematurely. For years afterwards the scratch of a slate pencil brought tears into his eyes.

Bramwell in witness box was explaining that ten seconds were allowed between two movements of railway signals for a decision. The lawyer against him was trying to prove that ten seconds was not long enough. "Do you maintain that ten seconds is enough?" No reply. "Have I not made myself plain?" No reply. "Surely you must understand me." And after a fourth pause, "Yes, but I was waiting for the ten seconds."

INDEX

Abel, Sir Frederick, 292
Aberration, 167, 342
Abney, Sir William, 176
Acland, Sir Arthur, 177
Aeronautics, 335, 338
Agricultural experiments, 77
Airey, Sir G. B., letter from, 84
Ampere, determination of the, 120
Ancestry, 1
Anecdotes, Collection of, 392
Angström, A. J., 87
Argon, 200
  article on, 168
  concentration by diffusion, 210
  monatomic nature, 217
  spectrum, 215, 219
Argyll, Duke of, 129, 221
Armstrong, Major-General John, 8
Arnold, Matthew, 44
Ashburton, Louisa Lady, 182
Asquith, H. H., 281, 283
Aston, F. W., 225
Atmosphere, new constituent of, 199, 219
  transparency of, 300

Balfour, Arthur, 40, 55, 139, 181
Balfour, Lady Blanche, 55, 59
Balfour, Eleanor, 55
  marriage, 108
  work with Lord Rayleigh, 108, 116, 119, 120, 259
Balfour, Evelyn (Lady Rayleigh), 55, 116, 258
Balfour, Frank, 129, 131
Balfour, Gerald, 129, 293
Balfour, James Maitland, 55, 77
Balfours, peculiar colour vision of, 175
Balmoral, visit to, 180
Baltimore Lectures, 145
Barker, Prof. G. F., 146
Barry, Sir John Wolfe, 297
Bath, visit to, 136
Bernard, E. R., 16
Berthelot, 222
"Bird call," 237
Birds, flight of, 335
Birkeland, Prof., 223
Birth, 8
Blood, absorption spectrum of, 32
Board of Trade Committee on electrical standards, 126, 244
  on gas standards, 294
  on vibration from the Tube Railway, 297
Bohr, Prof. Ch., 314
Bohr, Prof. N., 357
Bose, J. C., 127
Boyle, Robert, 8
Boys, Prof. C. V., 195
Bramwell, Sir Frederick, 230
British Association at Aberdeen, 179
  at Bath, 31
  at Bristol, 70
  at Edinburgh, 57, 126
  at Liverpool, 44, 55
  at Montreal, 138
  at Norwich, 46
  at Oxford, 205, 226
  at Southampton, 132
  President of, 138
British Association Committee on electrical standards, 112
Bubbles from peaty water, 182
Buckingham Palace, Dinner at, 371
Buoyancy correction, 162
Burne-Jones, Sir Philip, 151
Bushy House, Teddington, 279

Cambridge, Books read at, 29
  Cavendish Professorship, 99
  Chancellor of the University, 321
  Commissioner to Colleges, 70
  examiner at, 74
  gift of Nobel Prize money to, 314
  laboratory organization at, 106
  lectures at, 106
  life at, 25, 128
  opening of physiological laboratory, 328
  Senior Wrangler, 34
  Sheepshanks Exhibitioner, 32
  Smith's Prizeman, 36
  undergraduate life at, 25
Camphor, movements on water, 260

Canadian tour, 143
Capillary action, article on, 168
  work on, 182
Carnegie, Andrew, 324
Cavendish, 194
Cavendish Laboratory, 106
Central London Tube, vibration from, 297
Chamberlain, Joseph, 262, 263
Chief Gas Examiner, 294
Clark cell, 122
Clausius, 52
Cleveland, Duke of, 70
Clifford, W. K., 79
Cockburn, Sir Alexander, 70
Colour, Royal Institution lectures, 80
Colour-blindness, 175
Colour vision, 44, 46, 174
Coming of age, 30
Copley Medal, 172
Cordite, 292
Crookes, Lady, 265
Crookes, Sir William, 65, 68, 215, 265
  death, 379
  letter from, 197
  spectrum of argon, 219
Cruelty to Animals Bill, 69

Dairy-farming, 184
Darwin, Charles, 45
  centenary, 326
Darwin, George, 40, 127
Darwin, Horace, 109, 127
Death of Lord Rayleigh, 373
Devonshire, Duke of, letter from, 100
Dewar, Sir James, 193, 212, 231
  letters from and to, 212, 213
Diffraction, article on, 168
  gratings, 60, 86
Diffusion experiments, 210
Donkin, W. F., 50

Edison, 147
Education, views on, 141
Electric lamps, argon in bulbs of, 224
Electric Lighting Act, speech on amendment to, 179
Electric locomotives, 297
Electrical Standards, Committee on, 126, 244
  redetermination of, 109
Elliot, Arthur, 40
Encyclopædia Britannica, 166, 168
Enock, J. C., 391
Essex County Cottage Nursing Association, 269
Eton, school at, 13
Ewing, Prof. J. A., 297
Explosives, stability of, in tropical regions, 293
Explosives Committee, 291

Faraday centenary, 234
Featherstonehaugh, Sir Matthew, 1
Firearms, care with, 257
Fiscal Question, views on, 264
Fisher, H. A. L., 285
Fitzgerald, Lady Charlotte, 4
Fitzgerald, Lord Edward, 4
Flame, sensitive, modification of, 260
Fletcher, Sir L., 385
Fluorescence, 32
Fog-signals, 272, 274
Foster, Sir Michael, 133, 168, 171, 315
Fraunhofer, 87
Frere, Sir Bartle, 77
Froude, William, 71, 79

Galton, Sir Douglas, 277
Galton, Francis, 303
Galvanometer, soft iron, 45
Garnett, William, 105
Gas legislation, Committee on, 294
Gases, densities of, 158, 161
  liquefaction of, 231
  refractivity of, 151
Gelatine, bichromatized for copying gratings, 89
Gibbs, Willard, 172
Gladstone, W. E., 56
Glazebrook, Sir Richard, 105, 126, 279
Globe lightning, 387
Goodday, Anne, 4
Gordon, George, 104, 160, 231, 301
Grenfell, Field-Marshal Lord, 18
Guns, erosion of, 292

H-cell, 122
Haldane, Lord, 291, 338
Harker, J. A., 223
Harrow, school at, 16
Harvard, visit to, 147
Heat, dynamical equivalent of, 117
Heat radiation, laws of, 353
Heidelberg, visit to, 135
Helium, discovery of, 224
Helmholtz, 84, 126
  visit to Cambridge, 130
Herkomer, Sir Hubert, 27, 323
Herschel, Sir John, 31
Herschell, Lord, 268
Hicks-Beach, Sir Michael, 277
Holland, John, 40
Holmes, Oliver Wendell, 147
Homburg, visit to, 134
Honorary distinctions, 375
Huggins, Sir William, 316
Hunt, A. R., 18, 79
Huxley, T. H., 227

Hydrogen, ratio of densities of oxygen and, 165
Hyndman, H. M., 19
Hypnotism, 380

Illnesses, 58, 133, 333
India, visit to, 286
Interferometer, Michelson, 157
International conferences on electrical standards, 126, 127
Congress of Mathematicians, 328
Irish Land Bill, 131
Isotopes, 225
Isted, Thomas, 75
Italy, trip to, 37

Jefferson Physical Laboratory, 147
Jencken, Mrs., 66, 382
Jenner, Sir William, 58
Joule, James P., letter from, 117
meeting with, 180

Keely, 146
Kelvin, Lord. *See* Thomson, Sir William.
Keppel, Admiral, 3
Kew Observatory, 278

Laboratory at Terling, 71, 149
at Tofts, 59
Langford Grove, Maldon, 8
Larmor, Sir Joseph, obituary notice by, 310
Lectures at Cambridge, 106
at Royal Institution, 80, 221, 233
Lieuentaal, A., 336
Light, polarization of by reflection, 151
scattering of, 52, 599
wave theory of, 166
Lightfoot, J. B., 25
Lighthouses, lightning conductors for, 273
lights for, 273
Lister, Lord, 173, 220
Literature, taste in, 362
Liveing, Professor G. D., 38
Livesey, Sir George, 295
Lockyer, Sir Norman, 91, 225, 362
Lodge, Sir Oliver, 276, 345
Lord Lieutenant of Essex, 267
Lords, House of, Speeches in, 69, 178, 327
Lorentz, Prof. H. A., 346
Lubbock, Sir John, 44
Lyell, Sir Charles, 31
Lytton, Lady Constance, 209
Lytton, Lord, 265

McConnel, J. C., 127
Madan, H. G., 205
Madeira, trip to, 77
Magnesium, combination of nitrogen with, 195
Mallock, Arnulph, 72, 297
Manures, artificial, 77
Marriage, 57
Matthews, Thomas, 275
Maxwell, Clerk, 44
death, 99
letters from and to, 46, 47, 48, 49, 59, 80
Maxwell-Boltzmann doctrine, 249, 352
Mayer, Prof. A. M., 79, 199
Meteorites, 383
Michelson, A. A., 125, 145
Echelon grating of, 90
letters from, 343
Molecules, size of, 236
Mond, Dr. Ludwig, 206
Moore, Mrs. Bloomfield, 146
Moulton, J. Fletcher, 123
Mowatt, Sir Francis, 277
Murray, George, 13

N-rays, 358
National Physical Laboratory, 276
department of aero-dynamics, 282
financial arrangements, 283
vice-chairman of, 279
work on electrical standards, 282
Nile, trip up the, 60
Nitrogen, atmospheric and chemical, densities of, 193
combination of magnesium with, 195
industry in Norway, 223
oxidation of, 223
weighing of, 187
Nobel Prize, 313
Nobert gratings, 87
Noble, Sir Andrew, 291
Norway, nitrogen oxidation in, 223

Ohm, determination of the, 110
Oil films on water, 246, 250
Olszewski, K., 219
Optics, articles on, 166
Order of Merit, 312
Oxygen, ratio of densities of hydrogen and, 165

Paladino, Eusapia, 228, 385
Paley, Mrs. John, 9, 58
Parentage, 7
Paris, conference at, 123
Photography, 22, 154
of diffraction gratings, 86
Physical Society, 91
Playfair, Sir Lyon, 180

Political views, 43, 131
Porter, Dr., 195
Portrait by Burne-Jones, 151
by Herkomer, 323
by Sir George Reid, 174, 184
Poynting, J. H., letter from, 137
Prince of Wales, H.R.H., 30, 234
Prisms, resolving power of, 97
Property, administration of, 74

Quantum theory, 356
Quincke, Professor G., 135

Radiation in relation to molecules, 349
Radioactive disintegration, 225
Ramsay, Sir William, 188, 281
discovery of helium, 224
discovery of neon, 225
isolation of argon, 199
letters from and to, 189, 200, 201, 202, 203, 204
Nobel Prize, 313
Rayleigh Horn, 274
Rayleigh, Lady. *See* Balfour, Evelyn.
Rayleigh, Lord. *See* Strutt.
Rayleigh Memorial, 373
Rayleigh Prize, 324
Refractivity of gases, 151
Reid, Sir George, 174, 184
Relativity, theory of, 349
Religious views, 360
Resonance, 49
Reynolds, Osborne, 246, 250
Richardson, Charles, 7, 76
Rix, H., 168, 176
Roberts-Austen, Sir William, 291
Roscoe, Sir Henry, 195
Rotatory polarization, 346
Routh, Dr. E. J., 27, 83
Rowland, H. A., 90, 112, 136
Royal Commission on Explosions from Coal Dust, 262
Royal Institution centenary, 235
lectures, 80, 221, 233
Professor at, 230
Royal Society, Committee on colour vision, 175
Fellow of, 65
paper on argon, 219
paper on density of nitrogen, 196
presidential address, 317
Secretaryship, 168
Rücker, Sir Arthur, 315
Rumford, Count, 230
Rumford Medal, 333
Rutherford, Sir Ernest, 225
Rutherford, L. M., 91, 145

Salisbury, Lord, 49, 70, 74, 102, 180, 226, 267
Saponine, 183
School life, 13
Schuster, Sir Arthur, 109, 215
Scientific and Industrial Research, Department of, 284
Scientific Papers, republication of, 396
Sea water, colour of, 330
Shaw, Sir Napier, 105, 126
Sidgwick, Henry, 65
death, 266
letter to, 66
marriage to Eleanor Balfour, 108
Sidgwick, Mrs. *See* Balfour, Eleanor.
Siemens, Werner, 123
Siemens, Sir William, 138, 141
Silberrad, Dr. O., 292
Sky, article on, 168
blue colour of, 51, 299
Slade, 68
Smithsonian Institute prize, 217
Society for Psychical Research, presidential address, 67, 229, 340, 379
Soddy, Prof. Frederick, 225
Sorby, H. C., 90
Soret, 88
Sound, direction of, 302
interference and diffraction of, 236
theory of reciprocity, 82
treatise on, 62, 80, 83, 228
work on, 50
South Africa, visit to, 329
South Metropolitan Gas Co., 295
Spectral series, 350
Spectroscopes, auto-collimating, 150
resolving power of, 90
Spiritualism, researches in, 65, 228, 382
Spottiswoode, William, 65
Steel, Mrs. F. A., 289, 393
Stockholm, visit to, 313
Stokes, Sir George, 32, 37, 133, 168
Stornoway Castle, 182
Strathcona, Lord, 144
Strathmore, Lord, 134
Strutt, Arthur, 79
Strutt, Edward, 77
Strutt, Emily Anne, 11, 33
Strutt, John, 1, 3, 4
Strutt, John James, Lord Rayleigh, 6, 7, 64
Strutt, Joseph, 4
Strutt, Joseph Holden, 4, 5
Strutt, Julian, 129
Strutt, Robert John, Lord Rayleigh, 70, 301, 303, 371
Strutt, Major-General William Goodday, 8
Strutt, William Maitland, 181, 332
Stuart, James, 38, 103

Tait, Prof. P. G., 169
Taylor, Colonel, letter to, 42
Taylor, H. M., 34, 36, 83
Telepathy, 388
Terling Place, 1, 41
  agricultural depression at, 183
  laboratories at, 149
  life at, 254
  typhoid at, 41
  visitors' book, 261
  war conditions at, 333
Thompson, Frederick, 24
Thomson, Elihu, 235
Thomson, Sir J. J., 127, 225
  memorial address on Lord Rayleigh, 309, 373
Thomson, Sir William, Lord Kelvin, 58
  Baltimore lectures, 145
  death, 252
  determination of the ohm, 111
  friendship with Lord Rayleigh, 239
  letters from, 78, 99, 124, 130, 246
  president, Royal Society, 172
Tobacco smoke, blue light scattered from, 52
Tofts, laboratory at, 59
Torquay, school at, 17
Tower, Beauchamp, 71
Travers, M. W., 225
Trevelyan, Sir George, 16
Trevose Head, foghorn at, 276
Trinitrotoluene (T.N.T.), 293
Trinity College, 38, 130
Trinity House, scientific adviser to, 271
Tyndall, John, 52, 230
  acoustical experiments of, 81
  letters from, 54, 82, 232

United States, tours in, 40, 145

Vibration recorder, 297
Vicars, Clara Elizabeth La Touche, 7
Vivyan, Sir George, 275
Volt, determination of the, 121
Voltaic cells, standard, 121

Warner, Rev. G. T., 17
Water, cohesion of, 235
  experiments on flow of, 73
Waterston, George, 170
Waterston, J. J., 169
Weber, H., 112
Wellington, Duke of, 11
Westminster Abbey, Memorial in, 373
Wilde Lecture, 337
Will of the Wisp, 387
Williamson, Prof. A. W., 294
Wimbledon, school at, 13
Wood, Derwent, 373
Wood, R. W., 88
Wools, Holmgreen's, 178

Yellow, subjective, experiments on, 174
Young, C. A., 92
Young, Thomas, 234, 235

Zeeman, Prof. P., 348
Zone plates, 88