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ADVANCING SCIENCE

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ADVANCING SCIENCE

being
Personal Reminiscences of
THE BRITISH ASSOCIATION
in the Nineteenth Century

by
Sir Oliver Lodge

London
ERNEST BENN LIMITED

First Published, September 1931

DEDICATED

TO THE MEMORY

OF

GEORGE FRANCIS FITZGERALD

PREFACE

These personal reminiscences of the British Association for the Advancement of Science were originally intended as part of an autobiography, showing the influence of various agencies on my life. But they appeared to possess a more general interest, since few people have memories going so far back as mine. And now that the British Association is celebrating its centenary, it seems appropriate to issue this portion separately, and in places to expand it a little, as a sort of contribution to the scientific history of our own time. I am not pretending to write serious history—I leave that to my brother Richard—but I have a vivid recollection of some incidents, and those I narrate as they appealed to me ; eking out my memory here and there by reference to the annual volumes of the British Association, so as to keep the dates right and to extract from Presidential Addresses passages which now seem of value in view of later developments.

The main object of this portion of my autobiography is to carry out a suggestion made in 1929 by

Dr. Allan Ferguson as Papers'-Secretary of the Physical Society of London, that I should record my notion of what the British Association and the big men associated with it accomplished during the latter third of the nineteenth century. In the present century its proceedings are more likely to be known to people now living ; but events of the past are not easily recoverable unless old people record their memories. My actual knowledge of a British Association meeting began at Bradford in 1873. Perhaps I ought to apologise for the personal reminiscences that are mingled with the scientific recollections, but I am assured that they too will be of interest. A few of these reminiscences will be reproduced in my Autobiography, to be published later in this same year.

OLIVER LODGE.

21st May, 1931.

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CHAPTER I

HUXLEY, TYNDALL, AND CLERK MAXWELL

Elsewhere I have written of the influence exerted on my life by the Royal Institution of Great Britain. I have now to deal with the effect of the British Association. I must begin with an apparent digression.

My father's business involved the selling of potters' materials, not only clays, but also various kinds of lead which were used in the glaze. At the time when my age was twenty-two, his health was such as to prevent him from taking his customary northern journey in the autumn, and I was sent instead. I visited the few potteries that existed in the neighbourhood of Glasgow and Edinburgh and in some of the towns of Yorkshire. While I was staying at an hotel in Leeds for this purpose, I read in the papers of a projected visit of the British Association to the neighbouring town of Bradford. I was nearing the end of my journey, and therefore wrote home for permission to stay another week in order to attend this meeting.

I had indeed read about previous meetings of the

Association, but had not expected to find one so close and accessible. I had heard echoes of Sylvester's remarkable address at Exeter, in 1869, when he was President of Section A, and when Stokes was President of the whole. In this address Sylvester eloquently depicts the soul of the pure mathematician, and vigorously contests the statement attributed to Huxley in "The Fortnightly Review" (though it might have been said by many people), that Mathematics "is that study which knows nothing of observation, nothing of induction, nothing of experiment, nothing of causation." Sylvester claims that no statement could have been more opposite to the facts of the case, and he gives examples to illustrate. For instance, he says that a single invariant was accidentally observed by Eisenstein "(the Quadrinvariant of a Binary Quartic) which he met with in the course of certain researches just as accidentally and unexpectedly as M. du Chaillu might meet a Gorilla." He goes on to recite an observation of his own :

"Were it not unbecoming to dilate on one's personal experience, I could tell a story of almost romantic interest about my own latest researches in a field where Geometry, Algebra, and the Theory of Numbers melt in a surprising manner into one another, like sunset tints or the colours of the dying dolphin, 'the last still loveliest' (a sketch of which has just appeared in the Proceedings of the London Mathematical Society), which would very strikingly illustrate how much observation, divination, induction, experimental trial, and verification, causation, too (if that means, as I suppose it must, mounting from phenomena to their reasons or causes of being), have to do with the work of the mathematician."

Speaking of the algebraical question of the invariantive criteria of the nature of an equation of the fifth degree, Professor Sylvester says :

" The first inquiry landed me in a new theory of polyhedra ; the latter found its perfect and only possible complete solution in the construction of a surface of the ninth order and the subdivision of its infinite content into three distinct natural regions."

This is too technical to be intelligible, but it illustrates his theme, and as he nears his conclusion he breaks into jubilant prose :

" The mathematician lives long and lives young ; the wings of his soul do not early drop off, nor do its pores become clogged with the earthy particles blown from the dusty highways of vulgar life."

So he would—

" rejoice to see mathematics taught with life and animation . . . and the mind of the student quickened and elevated and his faith awakened by early initiation into the ruling ideas of polarity, continuity, infinity, and familiarization with the doctrine of the imaginary and inconceivable."

Not only did Sylvester give this remarkable insight into the soul of the pure mathematician, it was at this same Exeter meeting, I find, that Clerk Maxwell gave an account of his experimental determination of what he called the relation of the electromagnetic to the electrostatic unit, in other words, his great determination of the value of v—the foundation of the electromagnetic theory of light. His paper even then was considered of sufficient importance to be included in full among the reports, just before the abstracts of the sectional transactions begin, and soon afterwards this research was to awaken the enthusiastic devotion of myself and Sir Ambrose Fleming—then, as it happened, students together in Professor Frankland's Chemical Laboratory at South Kensington. But at that time I had only heard echoes of the meeting at Exeter.

Liverpool, 1870

The meeting of 1870 was held in Liverpool. I read about it in the newspapers, and cut out and stuck in a press-cutting book all that I could gather of what went on there. The meeting is famous for the great pronouncement of Huxley on Biogenesis, a term which he himself invented. He sketched the attack which had been made on Spontaneous Generation by the Italian Redi, and the way it led up to the researches of Pasteur.

It is interesting to find that Redi had to defend himself against the charge of impugning the authority of the Scriptures, but " strong with the strength of demonstrable fact, [he] did splendid battle for Biogenesis." Many intermediate stages did the doctrine go through before it was finally established. Ultimately Pasteur, " in beautiful researches which will ever render his name famous," showed that something could be filtered out of air by cotton-wool which stopped any life appearing beyond the filter ; and then by examining the cotton-wool he found that germs had accumulated in it. It is now considered thoroughly established that when germs are excluded no life appears.

" But [continues Huxley] though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis [spontaneous generation] ever has taken place in the past, or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call ' vital ' may not, some day, be artificially brought together. All I feel justified in affirming is, that I see no reason for believing that the feat has been performed yet."

And he speaks of his expectation—though he has no right to call his opinion a belief—that such a process must have occurred in times long past. He goes on to speak of many other organisms, and refers to the work of Lister in advocating the antiseptic method of surgery, which was then beginning. He concludes with an eloquent peroration, some portions of which I will extract :

" Our survey has not taken us into very attractive regions ; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind.

" Nevertheless you will have observed, that before we had travelled very far upon our road, there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value—namely, money and life.

" The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling ; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalize its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn.

" And as to the equivalent of Redi's thought in life, how can we overestimate the value of that knowledge of the nature of epidemic and epizootic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced ? "

The concluding sentence of the address is interesting, as showing that Huxley was not under the illusion that the kind of science recognised by the British Association covered the whole of nature. He recognises that it is only half the field of science itself. This is the sentence :

" It is the justification and the glory of this great Meeting that it is gathered together for no other object than the advancement of the moiety of Science which deals with those phenomena of Nature which we call physical. May its endeavours be crowned with a full measure of success ! "

Section A at Liverpool in 1870

The President of Section A that year (1870) was no less a person than Clerk Maxwell. In his address he first expresses appreciation of Sylvester, and proceeds to deal with the interaction of Mathematics and Physics generally, making very little or no reference to his own great and recent achievements in that direction. Here is a paragraph which is appropriate to the condition of things to-day :

" There are men who, when any relation or law, however complex, is put before them in a symbolical form, can grasp its full meaning as a relation among abstract quantities. Such men sometimes treat with indifference the further statement that quantities actually exist in nature which fulfil this relation. The mental image of the concrete reality seems rather to disturb than to assist their contemplations.

"But the great majority of mankind are utterly unable, without long training, to retain in their minds the unembodied symbols of the pure mathematician, so that, if science is ever to become popular, and yet remain scientific, it must be by a profound study and a copious application of those principles of the mathematical classification of quantities which, as we have seen, lie at the root of every truly scientific illustration.

"There are, as I have said, some minds which can go on contemplating with satisfaction pure quantities, presented to the eye by symbols, and to the mind in a form which none but mathematicians can conceive.

"There are others who feel more enjoyment in following geometrical forms, which they draw on paper, or build up in the empty space before them.

"Others, again, are not content unless they can project their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort.

"To such men momentum, energy, mass, are not mere abstract expressions of the results of scientific inquiry. They are words of power, which stir their souls like the memories of childhood."

I remember feeling enthusiastically that that was exactly my own case.

Maxwell goes on [and it is remarkable as coming from a mathematician] :

"For the sake of persons of these different types, scientific truth should be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression."

Maxwell refers to Dr. Johnstone Stoney's earliest estimate of the size of atoms, in 1868, as well as to Sir William Thomson's extension of the idea in 1870.

The whole address is full of interest, but a paragraph seems specially appropriate now that the particle and the wave are being in a sense unified, and when old-fashioned dynamical considerations have rather fallen into the background :

"There are certain electrical phenomena [he says] which are connected together by relations of the same form as those which connect dynamical phenomena. To apply to these the phrases of dynamics . . . is an example of a metaphor of a bold kind . . . but it is a legitimate metaphor if it conveys a true idea of the electrical relations to those who have been already trained in dynamics."

He then speaks of problems in which two different explanations have been given of the same thing, and says :

" The most celebrated case of this kind is that of the corpuscular and the undulatory theories of light. Up to a certain point the phenomena of light are equally well explained by both."

He applies this to the Weber-Neumann theory of electricity, as compared with his own theory, which—

" denies action at a distance and attributes electric action to tensions and pressures in an all-pervading medium, these stresses being the same in kind with those familiar to engineers, and the medium being identical with that in which light is supposed to be propagated."

And he continues :

" That theories apparently so fundamentally opposed should have so large a field of truth common to both is a fact the philosophical importance of which we cannot fully appreciate till we have reached a scientific altitude from which the true relation between hypotheses so different can be seen."

The application of this to modern theories of Light is obvious to any physicist. The altitude from which we can fully reconcile the quantum expulsion of electrons with the long-known interference patterns of optics is still above us.

Edinburgh, 1871

There were subsequent meetings of the British Association in Edinburgh and Brighton. The Edinburgh meeting of 1871 was held under the Presidency of Lord Kelvin, then (and afterwards) Sir William Thomson, with Tait in the Chair of Section A ; and was made memorable by Thomson's speculation, not about the origin of life, as was often misrepresented, but about the possible origin of life on this planet. A few extracts on this point from his address may be selected.

"I confess to being deeply impressed by the evidence put before us by Professor Huxley, and I am ready to adopt, as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life.

"How, then, did life originate on the Earth? Tracing the physical history of the Earth backwards, on strict dynamical principles, we are brought to a red-hot melted globe on which no life could exist. Hence when the Earth was first fit for life, there was no living thing on it. . . . Science is bound, by the everlasting law of honour, to face fearlessly every problem which can fairly be presented to it. . . . When a volcanic island springs up from the sea, and after a few years is found clothed with vegetation, we do not hesitate to assume that seed has been wafted to it through the air, or floated to it on rafts. Is it not possible, and if possible, is it not probable, that the beginning of vegetable life on the Earth is to be similarly explained? . . . Should the time [ever arrive] when this Earth comes into collision with another body, comparable in dimensions to itself, [and should this happen] when it is still clothed as at present with vegetation, many great and small fragments carrying seed and living plants and animals would undoubtedly be scattered through space. Hence and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as probable in the highest degree that there are countless seed-bearing meteoric stones moving about through space. . . . The hypothesis that life originated on this Earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is that it is not unscientific."

This pronouncement of Lord Kelvin's attracted much attention from the Press, and was considerably jeered at. There was a recrudescence of jest about it after I had gone to Liverpool, ten years later, when I wrote an article pointing out that no attempt at the origin of life had been made, but only a suggestion as to a possible origin of the life on this planet. Incidentally I said that the utterance of a great man might be erroneous, and might be absurd, but not so absurd that any newspaper writer between ten o'clock and midnight could see through it, clean it out, and serve it up exposed for your breakfast edification. On this I received a comic protest from Charles Beard, a leading-article writer for "The Liverpool Post," which began, "Spare us, good Lodge, spare us!"

The rest of Lord Kelvin's address surveys many things—the connexion of comets with the orbit of meteors, worked out by "Neptune" Adams, among others. The death of Sir John Herschel in the spring of that same year was recorded; and the following footnote is of historical interest:

"* Frankland and Lockyer find the yellow prominences to give a very decided bright line not far from D, but hitherto not identified with any terrestrial flame. It seems to indicate a new substance, which they propose to call Helium."

That is the way in which discoveries that turn out of great importance are announced by the President at meetings of this kind.

Digression

The Brighton meeting seemed to me less interesting; and now in 1873 there was the chance of my actually attending a meeting in Bradford.

Perhaps I ought to say something about what preparation I had for attending such a meeting. I had matriculated at the London University by private study in the evenings and in the intervals of business. I had gone on to the first examination for the B.Sc. Degree, likewise by private study, and had taken First-class Honours in Physics. After that I had a winter at the Royal College of Science, South Kensington, with the permission and help of the Science and Art Department; and I was shortly going to complete the second examination for the B.Sc. Degree.

I did not attend the Association as what is now called a "student": there were no facilities in those days for a young man from the Universities to attend; and I was only externally connected with London University. I went entirely on my own; and the only thing that happened was that, seeing my enthusiasm at Section A, Professor H. J. S. Smith, the President of the Mathematical and Physical Section, spoke some kindly words to me after one of the meetings, and inquired about my mathematical knowledge, making me ashamed of my ignorance and determined if I could to rectify some of it, which I did at the beginning of 1874 by entering as a belated student at University College, London, and attending the second year and following courses of Henrici and Clifford for some four or five years, until I worked up into the higher senior and advanced classes along with Viriamu Jones and M. J. M. Hill—though it is true that Hill went on to a class all by himself as a born mathematician. Ultimately indeed he and I were appointed to take some of Clifford's lectures when he was invalided out to Madeira towards the end of his short life; but that was a good deal later, more like

1879 or 1880. I am now returning to 1873, and the British Association meeting at Bradford that I was allowed to attend.

Bradford, 1873

My plan was to go from Leeds every morning and return at night, spending the whole day in Bradford, and doing the meeting thoroughly. I had understood that Joule was to be President, but for some reason, possibly on account of health, this plan fell through, and the chemist A. W. Williamson took his place.

The opening meeting on the Wednesday evening was very impressive, for many of the great men of science, whose names were more or less known to me, were assembled on the platform to support the President; among them being James Clerk Maxwell. This was my first sight of him. Next day Section A met under the Presidency of H. J. S. Smith, that highly cultivated and learned Oxford mathematician; and in his honour had assembled what seemed to me nearly all the great mathematicians of the world, headed, so to speak, by Cayley and Sylvester, and attended also by Felix Klein, the German mathematician, and Stokes and W. K. Clifford and Maxwell, while the secretary of the Section was J. W. L. Glaisher.

There was also present his father, James Glaisher, the meteorologist, who some years before had made his sensational balloon ascent for the purpose of observing the conditions in the upper air, in which his life was only saved by the energy of Coxwell, the aeronaut, who pulled the rope with his teeth when his hands were numb, and so opened a valve and brought the balloon down again from a great height. Also J. D. Everett of Belfast, translator of Deschanel, William Huggins, Norman Lockyer, and Dr. Jansen, the French astronomer, a dignified-looking man with a limp, who had recently rivalled Lockyer in seeing the red flames round the margin of the sun without an eclipse, and who communicated several papers to the Section. Also William Spottiswoode, Sir Robert Ball with his theory of screws, George Forbes, Osborne Reynolds, and Balfour Stewart. The last-named now described his experiments, in conjunction with Tait, on ethereal friction by rapid rotation of a disk *in vacuo* ; one of the early though futile attempts to experiment on what to the senses and to all our instruments appeared like empty space.

Arthur Schuster was there too, and Lord Rayleigh, who communicated a paper on the Natural Limit to the Sharpness of Spectral Lines. Also Alexander Herschel, son of Sir John, who made a special report on Meteorites. W. F. Barrett spoke about the molecular changes accompanying the magnetisation of iron, nickel, and cobalt, and a year or two later showed the abnormal stretching behaviour of iron which he had discovered as it reached the critical temperature at which it lost its magnetic properties. G. M. Whipple and G. J. Symons and other meteorologists communicated papers, and there was one by Professor Zenger on the protection from electric discharges which could be obtained by a cage of wires surrounding any building.

The Rev. Robert Harley was a frequent attendant at the meetings about this date, and there was a paper by Charles Hermite on the irrationality of the base of hyperbolic logarithms, which now strikes one as very elementary as a communication to that Section. J. W. L. Glaisher communicated an interesting historical paper on the introduction of the decimal point into arithmetic, and also on the negative minima of the gamma function, together with other papers on the theory of numbers. H. J. S. Smith's address dealt mainly with mathematical education, advocating a broader instruction in geometry than could be obtained from Euclid. He spoke also of the unexpected services of pure mathematics to physics, saying that—

"without the treatises of the Greek geometers on the conic sections, there could have been no Kepler, without Kepler no Newton, and without Newton no science in our modern sense of the term, or at least no such conception of nature as now lies at the basis of all our science,—as subject in its smallest as well as in its greatest phenomena, to exact quantitative relations, and to definite numerical laws."

He also signalised the publication of the great treatise on Electricity and Magnetism by Professor Maxwell, which had appeared earlier that same year, saying :

"No mathematician can turn over the pages of these volumes without very speedily convincing himself that they contain the first outlines (and something more than the first outlines) of a theory which has already added largely to the methods and resources of pure mathematics, and which may one day render to that abstract science services no less than those which it owes to astronomy."

Needless to say, I soon invested my pocket money in those two volumes ; the bookseller deprecating and decrying them as " a product of the over-educated."

It may be recorded that H. J. S. Smith, as President of Section A, took the opportunity of advocating government assistance for the physical sciences, such as had already been given to mineralogy, botany, and zoology ; and said that " it need not cause alarm to the most sensitive Chancellor of the Exchequer if it should be determined to establish, at the national cost, institutions for the prosecution of these branches of knowledge (heat, light, and electricity) so vitally important to the progress of science as a whole " : thus foreshadowing vaguely what has now been installed as the National Physical Laboratory at Teddington. Some passages from these old addresses of the Presidents of Section A are well worthy of reproduction, and I suggest that a collection of them might be made, in view of the Centenary Meeting of the Association in 1931.

Professor H. J. S. Smith spoke also of the use of the meetings of the British Association, and called attention to the vast range of subjects which we " slur over rather than sum up " under the designation Section A. For instance, he says by way of contrast :

> " I have heard in this Section a discussion on spaces of five dimensions ; and we know that one of our Committees, a Committee which is of long standing and which has done much useful work, reports to us annually on the Rainfall of the British Isles."

The question of subdividing the Section had evidently already been raised, but on the whole he argued against it, instancing the danger of excessive specialisation, and claiming that the bond of union among the physical sciences is the mathematical spirit and the mathematical method which pervades them. He thought that " electricity now, like astronomy of old, has placed before the mathematician an entirely new set of questions, requiring the creation of entirely new methods for their solution." If this was true in 1873, how much truer it is now !

In fact, the Bradford meeting was a mathematical orgy, and the sittings lasted each day without interruption from eleven to three—a trying time for the internal economy. But I was young and adaptable and enthusiastic, and became imbued with the (probably grotesque) idea of trying to become a pure mathematician. On one occasion I remember Cayley folded a gummed strip of paper into a ring, in front of the Section, so as to make a sheet with only one surface. This was done by giving the paper a single twist before it was stuck together into a ring. He then cut it longitudinally right through without dividing it ; it remained a whole larger ring. In the discussion it was asked what would happen if the paper was cut again, and then and there the experiment was tried. The result, as I remember it, was two interlocked rings, curiously twisted together.

In those days there was no elaborate time-table ; the programme was made up each day, being printed overnight ; discussions were allowed to continue as long as they were lively, the Secretary or Secretaries frequently joined in, and any overflow papers were taken on the ensuing morning. This plan had its advantages ; for it enabled a fresh paper to be inserted in the programme with only a day's notice, and it allowed the discussion of an interesting point to be pursued to the end, without constant reference to the clock. No doubt it also had disadvantages, from the point of view of people who wanted to hear a particular paper. But that hardly applied to Section A. The papers were abstruse, and the general public rather wanted to see the people than to hear them talk about any particular subject ; and the absence of a prearranged programme gave no trouble to those who attended the sitting regularly throughout.

There were then no excursions till the Thursday after the meeting ; the business in hand was strictly attended to ; and though there was early closing on the Saturday, that morning and the final Wednesday morning were often prolific times. It was the custom to concentrate the pure mathematics of a more abstract kind on the Saturday, and to leave some experimental papers till the final Wednesday. The lantern was hardly ever used, but blackboards were much in evidence, and there was no hesitation about filling the board with equations, if occasion needed. One could not always follow, but one got a general notion of the methods used, and was stimulated to learn more about the detailed machinery of the process.

The Evening Lectures of the Association at this time were two in number, one on the Friday, the other on the Monday. They were both full-dress affairs, in analogy with the President's address ; and, like it, were held in the Town Hall, with the platform again filled with notables, the President being in the Chair. It was at a Bradford Evening Lecture that Clerk Maxwell gave that notable address on Molecules, which has been reprinted in his Collected Works (as well as I think in the current " Nature "). I listened to it with the most absorbed attention, and it aroused my utmost admiration. It was the last word of the theory of the atom as an indivisible speck. (Subsequently I heard his address to the Chemical Society of London on Van der Waals' identification of liquids and gases.

This was a further extension of the old atomic theory, delivered with the utmost power and clearness, and giving results which, though they have been added to and modified in many respects, remain essentially true.) The explanation of gaseous viscosity by diffusion, and indeed the whole of the essential features of the kinetic theory of gases, were expounded by Maxwell in that Evening Lecture at Bradford, or as much of it as could be assimilated in the time.

The other Evening Lecture was by W. C. Williamson, Professor of Botany at Manchester, on Coal Plants, in which subject he was an evident enthusiast. He showed a great number of slides, and described the process by which the sections were cut in order that he might examine them microscopically. The speaker who proposed the vote of thanks said that Williamson had cut innumerable slices of coal of microscopic thickness, and that it was not too much to say that his microscope had travelled over *acres* of them in order to get the results which he had expounded that night. I remember some chaff between the two Williamsons on the platform, W. C. and A. W., and about one having accepted a dinner intended for the other. But the botanist handsomely retorted that he had been the gainer by the confusion, for to him had been credited in some quarters all the learning and European celebrity of the distinguished chemical President.

The whole meeting was an experience never to be forgotten. At the end of it I wrote home an account, sitting up late in the hotel at Leeds to do so, wherein I expatiated on the magnitude of the men concerned ; and especially on the proceedings in Section A, where the President, H. J. S. Smith, and the Secretary, J. W. L. Glaisher, had specially aroused my youthful enthusiasm. This letter I regret to say has been destroyed, otherwise some of it might be worth reproducing here, for it was written at a white-heat.

After that experience, I never failed to attend a meeting of the Association, until it went to South Africa in 1905, and I heard many striking discourses, but never again was I so moved as by that first meeting at Bradford in 1873.

The Belfast Meeting, 1874

Next year the Association met at Belfast, under the Presidency of Professor Tyndall, and it was here he delivered that famous Belfast address which aroused so much interest and opposition. It was the chief pronouncement of the materialism of the nineteenth century. He had taken a great deal of trouble over it, had written much of it in Switzerland, and delivered it with eloquence. His delivery occupied nearly two hours, but everybody sat it out till the end, and I remember feeling the atmosphere grow more and more sulphurous as the materialistic utterances went on in that strongly Protestant atmosphere of northern Ireland. On the Sunday all the pulpits fulminated their anathemas. And indeed they had a double reason for this, for one of the Evening Lectures was by Professor Huxley on Animal Automata, wherein he promulgated Descartes' view of animal automatism, and extended it to man. This lecture by Huxley was a brilliant *tour de force.* He stood there with his hands unoccupied, and with hardly a movement of the body recited his address straight away; the manner of his utterance being the admiration of all.

The other Evening Lecture was by Sir John Lubbock, with illustrations by his daughter, over which (or whom) Tyndall in returning thanks waxed enthusiastic and emotional.

During and before this meeting there had been a strike of operatives in Belfast, and the President exerted himself to bring the parties together. At the concluding meeting he was able to say that the efforts had been successful, that a compromise had been effected, and that the men would be returning to work " tomorrow morning." Whether the Association had really anything to do with this result I do not know, but the climax was dramatic.

The President of Section A in 1874 was the Rev. Professor Jellett, of Dublin, father-in-law of G. F. FitzGerald, and the concluding paragraphs of his address may here be quoted :

" Let none presume to fix the bounds of Science.

'Hitherto shalt thou come, but no further'—that sentence is not for man. Not by our own powers, not by the powers of our generation, not even by our conceptions of possibility, may we limit the march of scientific discovery. To us, labourers in that great field, it is given to see but a few steps in advance. And when at times a thicker darkness has seemed to gather before them, men have recoiled as from an impassable barrier, and for a while that path has been closed. But only for a while. Some happy accident, some more daring adventurer, it may be time itself, has shown that the darkness was but a cloud. The light of Science has pierced it; the march of Science has left it behind; and the impossibility of one generation is for the next but a record of a new triumph.

"If seeming plausibility could give to man the right to draw across any path of scientific discovery an impassable line, surely Comte might be justified in the line which he drew across the path of chemistry. Fifty years ago it might seem no unjust restriction to say to the chemist, Your field of discovery lies within the bounds of our own earth. You must not hope to place in your laboratory the distant planet or the scarce-visible nebula. You must not hope to determine the constituents of their atmospheres as you would analyse the air which is around your own door; and you never will do it. Fifty years ago no chemist would have complained that chemical discovery was unjustly limited by such a sentence; perhaps no chemist would have refused to join in the prediction. Yet even those who heard it uttered have lived to see the prediction falsified. They have seen the barrier of distance vanish before the chemist, as it has long since vanished before the astronomer. They have seen the chemist, like the astronomer, penetrate the vast abyss of space and bring back tidings from the worlds beyond. Comte might well think it impossible. We know it to be true."

At this meeting it was that Professor Feddersen of Leipzig demonstrated the alternating discharge of a Leyden jar, which had been predicted by Sir William Thomson and Professor Kirchhoff. Arthur Schuster raised the question of the accuracy or otherwise of Ohm's law. Sir William Thomson made several communications; and Professor G. Wiedemann, Dr. Johnstone Stoney, Captain Abney, and Professor Frederick Guthrie also read papers.

The Bristol Meeting, 1875

The next meeting was at Bristol in 1875, under the Presidency of Sir John Hawkshaw, the civil engineer. As far as I remember, the Victoria Rooms were used as a reception room, and after the President's address in the Colston Hall, some of the evening meetings took place in what I can only describe as sheds near the railway; so that the proceedings were more or less interrupted by the noise of the shunting engines, which, after all, seemed appropriate to an engineering occasion. Hawkshaw reviewed the improvements in transport, and spoke about a project for a canal joining the Mediterranean with the Red Sea.

Sir Frederick Bramwell was in great form at this meeting, and in his Evening Lecture on " Railway Safety Appliances " sometimes reduced the audience to shouts of laughter.

Professor Balfour Stewart presided over Section A, and in connexion with terrestrial magnetism referred to a doctrine of Sir William Thomson's that motions of conducting layers of the atmosphere across the earth's magnetic field might be responsible for some magnetic storms. Which is not unlike something that has been urged lately by meteorologists, and referred to by the President of Section A (Sir F. E. Smith) at the recent meeting at Bristol (1930).

The most remarkable thing at the Bristol meeting of 1875 was a communication from William Froude, who was President of Section G. He had rigged up a hydraulic model illustrating the pressure of liquids as they flowed through tubes of varying section, the principle being that where the velocity was greatest the pressure was least, thus giving rise to apparently paradoxical results : the liquid often flowing from a place at low pressure to one of high. Water can easily flow uphill for a time ; but of course with decreasing velocity. He also illustrated the stream-lines of ships, which was the beginning of the " Froude tank " employed in naval designing, and still in full use at the National Physical Laboratory. One of his experiments, arranged in an adjoining room, consisted in an endless chain hanging from a pulley, which when rotated rapidly made the chain acquire a sort of rigidity, so that any dent in it persisted ; though the deformation travelled slowly downwards because of the weight of the chain. When the chain was knocked off the pulley it stood up on end as long as the motion continued. This kind of experiment had already suggested to Sir William Thomson an essential part of his theory of vortex atoms ; and Froude mentioned this as a striking theoretical outcome of his experiments, which were intended only to have an engineering application. They showed that a fluid in motion was capable of attaining the rigidity of a solid, a fact the full consequences of which have not even yet been worked out. Many curious things about the flow of fluids, though now well known, are given or emphasised for the first time with illustrative plates in Froude's address to the Mechanical Section.

Professor Cayley read a Paper at this meeting of 1875 " On the Analytical Forms called Trees " with special reference to the number of isomers corresponding to a chemical formula. He dealt with ramifications, not with loops or closed chains of atoms. His Paper is printed in the Annual Volume in full, pp. 257–305. In the discussion Professor W. K. Clifford made a striking remark : he said that no Paraffin (of the form C_nH_{2n+2}) could possibly form a loop, but must be a simple ramification; and that in a hydrocarbon of the form $C_nH_{2n+2-2x}$ there must be x cycles or loops. For instance, in Benzol, C_6H_6, there are four cycles, the three double bonds and the whole ring. This led me to enter on the subject of nodes and loops in chemical compounds, and I published my results in the "Philosophical Magazine" of November 1875. Professor Sylvester had also joined in the discussion, and I had the pleasure of a letter from him about my Paper. Incidentally, I was led to a shorthand method of writing compounds in Organic Chemistry using only the valency to denominate and discriminate between the four commonly occurring elements, C, H, O, N; and this method of drawing graphic formulæ, with no redundant strokes, I found very helpful in remembering the constitution of organic compounds as ascertained and recorded by chemists. I drew every formula in Roscoe and Schorlemmer, and remember most of them to this day. I still believe that this easy pictorial method of representing a graphic formula would be helpful to students.

Glasgow, 1876

The meeting at Glasgow in 1876 was presided over by Dr. Andrews of Belfast, well known for his experiments on the continuity of the liquid and gaseous states.

Sir William Thomson was President of Section A, and his address was mainly concerned with terrestrial phenomena, such as the rigidity of the earth, and other facts deduced from tidal theory.

The interior of the earth cannot be molten and plastic, else the tides would be other than they are. The tides represent a yielding of the liquid ocean greater than that of the solid earth below.

Sir Wyville Thomson gave an Evening Lecture on the voyage of " The Challenger," which had recently terminated, in which many biological and hydrographical facts had been discovered. Professor Tait gave the other lecture, on Force, in which he poured scorn and sarcasm on those who had used the term " force " when they meant " energy," giving telling illustrations of the confusion ; though really the error in nomenclature was pardonable, since it was not very long ago that the term " energy " had a definite significance attached to it for the first time; and Helmholtz's early and powerful memoir on the conservation of energy had the title " Die Erhaltung der Kraft." I expect that when Tait's address was printed subsequently in " Nature," the sting was taken out of some of the paragraphs, or they may have been omitted altogether, but his language at the time was strong. One illustration of the danger of a little knowledge remains in my memory as a quotation which he made from some writer (was it Lord Brougham ?), that a porter carries a load on his shoulder instead of in his hands, because the weight of a body varies inversely with the square of its distance from the earth. This last, needless to say, would have been a true statement if the term " earth " signified the centre (4,000 miles away) instead of the surface : only then it would make no appreciable difference to the porter.

Dr. Kerr was referred to by the President as having discovered an optical effect of electrified glass or other substance subjected to electrostatic strain ; and he now gave an account of his discovery of the rotation of the plane of polarisation by reflection from a magnetic pole. Dr. Kerr described all his experiments beautifully, in a manner that reminded me of the experimental part of Newton's Optics.

It appears that I now gave my mechanical illustrations of " Electric Induction and Conduction, according to Maxwell's Theory," and one on " Thermo-electric Phenomena," which were among my first published contributions to science. In connexion with these papers, when published in " The Philosophical Magazine," I received an interesting and humorous and quite long letter from Clerk Maxwell himself ! A letter which, I regret to say, through " moving accidents by flood and field," has vanished into oblivion. I know that in it he suggested " lubricating " the beads with canada balsam, of all strange substances.

Alfred Russel Wallace that year was President of the Biological Section D, which was not then subdivided into the numerous branches of the present day. The sections of the Association ranged from A to G; there was no H or I or K, still less L or M, or J. It may be of interest to mention that the great chemist Henry Perkin presided over the Chemical Section, and devoted his address chiefly to aniline dyes.

Stokes attended this meeting, and gave a paper on Metallic Reflection. James Thomson illustrated by a model his now well-known theory of the origin of the windings of rivers in alluvial plains. And Crookes described the radiometer. Willoughby Smith's discovery of the curious property of selenium, now so much used in television experiments, was referred to by the President as having been recent; also Tait's discovery of neutral points in a thermoelectric circuit.

DISTRACTIONS

Of the meeting at Plymouth in 1877 I appear to have very little recollection, although my friend Carey Foster was President of Section A. It may be that I could not attend properly, because the meeting began on Wednesday, August 15th, and I was due to return home into Staffordshire and be married on Wednesday, August 22nd. My eldest son was born on August 11th next year, and on August 14th the Dublin meeting began.

Dublin, 1878

The meeting of the year after, in Dublin, is memorable to me because there it was that I made the acquaintance of my great friend, G. F. FitzGerald, and also saw something of the great men in Trinity College, Dublin; having breakfast with the Provost, Dr. Salmon, and likewise being invited by Professor Townsend. What struck me about the Senior Fellows of Trinity was that they seemed to spend their time in telling comic tales, each one capping the other, and not having much serious conversation. Professors Haughton and Mahaffy were conspicuous at that breakfast, and it seemed early in the day for so much sparkle.

The President of the Association that year was the eminent mathematician William Spottiswoode, who made a survey of recent advances; while Professor Salmon presided over Section A. One would have expected Salmon to be President of the whole, but it was against the tradition of the B.A. to select a President from the place visited. A procedure sometimes difficult to avoid.

It was at this meeting that I began my long tenure of the office of Secretary of Section A, overlapping J. W. L. Glaisher, and receiving much kindly instruction from him, so that old traditions might be carried on.

Professor Stanley Jevons gave a paper in Section A citing the Brownian movement to explain the cleansing action of soap. Several papers were communicated by Dr. Jannssen and by Dr. Johnstone Stoney. James Thomson had some cautionary remarks on units ; and Dr. Haughton was much in evidence. George Darwin spoke on the precession of a viscous spheroid, and was evidently beginning his work on the tides, which led to his theory of the birth of the moon ; and he referred to Asaph Hall's recent discovery of the two satellites of Mars as tending to confirm his theory. A paper " On Unilateral Conductivity in Tourmaline Crystals," by myself and Silvanus Thompson, is abstracted in the report ; while Mr. J. Brown's experiments on contact electricity are also described.

That exuberant genius W. K. Clifford was also in Dublin, and I remember encountering him on the Sunday as I was leaving the church in which Professor Jellett had been holding forth. Clifford was on the other side of the street, and he shouted across, " Hullo, Lodge, have you been to Section Hell ? "

Another joke about the lettering of the sections was when a Physiological Section was started as Section I. It was said to be appropriate to W. B. Carpenter, the well-known physiologist.

CHAPTER II

CROOKES ON RADIANT MATTER

SHEFFIELD, 1879

The Sheffield meeting of 1879 is famous for the evening discourse of Sir William Crookes on Radiant Matter. In this lecture he had a remarkable series of specially designed vacuum tubes. He emphasised the newly discovered second dark space near the cathode, through which a torrent of cathode rays, which he called Radiant Matter, was passing; passing freely till it struck either the glass walls or some mineral exposed to the bombardment, thus exciting phosphorescence. He arranged a longitudinal piece of mica inside the tube smeared over with phosphorescent powder, so that the course of the otherwise invisible cathode rays could be traced, and showed that they were deflected by a magnet, thus proving that they constituted an electric current. He had windmills inside the tube propelled by the rays from the cathode; showing that a current of this kind had momentum. In fact, he said that the tube was full of "matter in a fourth state." Hitherto we had been familiar with solids, liquids, and gases, and now a further step was taken, the atom of matter was disintegrated and its electrons driven along ; that is how we express it now, but in 1879 this discovery had not been made. The phenomena were exhibited with surprising clearness, and all the old experimenting with vacuum tubes was superseded by Crookes's skill, assisted as he was by a skilled glass-blowing assistant, Mr. Gimingham. His facts were mainly novel, partly because he was attaining vacua superior to anything that had been employed up to that time, so that there was very little residual undisintegrated matter left in the tube ; though even so he had not reached the perfection of vacua such as are now deemed commonplace. For one of his experiments consisted in focusing the rays from a concave cathode on to a piece of platinum, so that it got red hot, which it would not do if the vacuum had been high enough : it would emit X-rays instead. A tube of this very kind was subsequently employed by Sir Herbert Jackson for the production of X-rays. The focus constituted a point source, thus giving the shadows of bones and objects far better definition than Röntgen had been able to get. But this was in 1896, or 1897, some seventeen years later.

Crookes failed to discover the Röntgen rays, but his cathode-ray experiments were the foundation on which Sir J. J. Thomson worked when, like Schuster, he examined their magnetic deflection metrically ; but Thomson added other refinements, derived from C. T. R. Wilson, and thus really discovered and isolated the electron as the universal constituent of all matter. He at first called them corpuscles, because " electron " was the name given by Johnstone Stoney long ago to the disembodied unit of electricity dealt with in electrolysis, and he did not wish to assume that he had got as far as discovering and isolating that : though he had. He was content to establish that corpuscles, all of the same kind, were knocked by electrical means out of every kind of atom, and he proved that this universal corpuscle was far lighter than any atom of matter, lighter as an ounce is lighter than a hundredweight or (in some cases of heavy atoms) lighter than a ton ; but that was in 1897–1899.

Those experiments of Sir William Crookes in 1879, then demonstrated at an evening meeting of the B.A., were the prelude and starting-ground of all the brilliant researches which have led to twentieth-century physics. Will it be believed that I did not hear this lecture, though I saw and repeated the experiments afterwards ; for it was delivered on August 22nd, the anniversary of my wedding day, and we were staying with friends (the Ashtons) in Sheffield, who persuaded me to stay with them at the last moment, so that I abstained from the drive to the Firth College, or wherever the lecture was given.

The second Evening Lecture was by Sir Edwin Ray Lankester on Degeneration, when he was careful to point out that evolution did not always mean progress upward ; there might be degradation as well.

Professor Ayrton gave a lecture to the operative classes on electrical transmission of power, and strongly advocated the utilisation of power from the high-level reservoirs constructed by the Corporation of Sheffield as sources for the supply of water to the town. He pointed out that the water would not be diminished in quantity, and that power might be distributed to many handicraft cottagers who had been wont to carry on their trade by tapping the hydraulic power of mountain streams. The lecture excited much interest; but, doubtless for good reasons, the project of utilising the power of the descent of the reservoir water from the hills was not taken up in that form: nor have similar reservoirs elsewhere been used for power. But the subject was then new, and everyone now knows how extensively this method has been applied to the electric utilisation of water power in Scandinavian countries and in Switzerland.

It was in this year that Hughes brought out his Induction Balance. I wrote a paper on the theory of it in "The Philosophical Magazine" for February 1880; and Lord Rayleigh expressed the essential parts of the theory more neatly, in a paper to the B.A. that same year at Swansea (p. 472 of the Report for 1880).

By the end of the year 1879 Clerk Maxwell, alas, died at the early age of 48.

Swansea, 1880

It was at the Swansea meeting that Professor H. B. Dixon startled the chemical world by announcing that perfectly dry carbon monoxide could not burn in perfectly dry oxygen, and that the two gases would not explode under the influence of an electric spark unless water was present. This effect of dryness in withholding chemical action has since been extended to other gases, such as hydrogen and chlorine, although in presence of a trace of moisture these gases can easily be made to combine explosively by ultra-violet light—a fact which Sir Henry Roscoe was fond of using for various optical demonstrations. And, most extraordinary of all, even hydrogen and oxygen will not combine if perfectly dry. The need for a trace of water to initiate any chemical action, and the importance which the formation of water plays in all chemical reactions, where it had usually been treated as a bye-product of no importance, has now been emphasised, chiefly by Professor Henry E. Armstrong, and the subject was followed up with great skill by " his dryness " Professor H. F. Baker ; but so far as I know it was Dixon at Swansea who started the hare.

Swansea saw the first establishment of the Secretarium, where the secretaries of all the sections were housed together and entertained by the City, thus saving their scantily filled pockets, and promoting interchange of information among the various sections ; which, by the way, still stopped at G. I am sure that this informal conference and friendliness among the secretaries was a good plan.

YORK, 1881

Eighteen hundred and eighty-one marked the Jubilee of the Association, and accordingly * the meeting was held in York under the Presidency of Sir John Lubbock, with Sir William Thomson once more President of Section A. It was at this meeting that he introduced the term " dynamo " instead of the more cumbrous " dynamo electric machine." He had put in a paper for the Friday of Section A at the last moment, the title of which ended with what, as Secretary (whose business it was to get the programme printed for next morning by visiting the printers and correcting the type), I thought was the mere written abbreviation " dynamo." But instead of altering the unusual term I consulted Sir William about it, and he said, " No, leave it dynamo, since I consider that it is now time to have a handy term, and I want to introduce this one." So there it stands as the first public use of what is now an exceedingly familiar name.

The Secretarium that year was in the buildings of a

* Because the first meeting was held at York in 1831.

girls' school lent for the occasion during the vacation. So the secretaries had to behave properly.

SOUTHAMPTON

The Southampton meeting of 1882 had C. W. Siemens for President, and Lord Rayleigh for President of Section A. The latter made many communications to the sections, but my chief recollection of this Southampton meeting was walking to the Ordnance Survey Office arm-in-arm with Professor H. J. S. Smith, who kindly took my arm as we went from the section. A proud youngster was in Southampton that day.

The Report for this year is noteworthy for containing a communication on the " proposed " Forth Bridge by Sir Benjamin Baker (see pp. 419–433) with a detailed plate. There is also a report on the Channel Tunnel by J. C. Hawkshaw (pp. 404–419) with three plates. One of these engineering exploits has now become a well-known success ; the other is still in haze.

In passing, I see a calculation of Lord Rayleigh's on p. 446, of the duration of free electric currents in a conducting cylinder, which might be of renewed interest in these days of super conductivity.

SOUTHPORT, AND THE ORIGIN OF ETHER WAVES

It was at the 1883 meeting that G. F. FitzGerald gave two papers whose titles alone are now sufficient to attract attention, one "On the Energy lost by Radiation from Alternating Electric Currents," when he gave an expression for the radiation in terms of the wave-length, or rather of the time period of the oscillation, showing how much more effective in radiation were short waves compared with long ones. The result he got has been adopted by everyone since: at least after such radiation was discovered experimentally.

His result may be put thus: Let a current alternate in a circular ring of radius a, with amplitude i_0 and period T; it may be said to have a magnetic moment $\pi a^2 i$, which we might generalise and call M. Then a certain portion of energy will be lost to the circuit and will not return; this radiated portion per unit time was reckoned by FitzGerald as—

$$\frac{8\,\mu\pi^4}{3\,T^4c^3}\cdot(\pi a^2 i_0)^2\;;$$

or what is the same thing—

$$10^{-29}n^4\,M_0^2,$$

n being the frequency.

Or, again, the radiating power might be expressed as

$$8\times 10^{12}\cdot\frac{M^2}{\lambda^4}$$

The second paper is "On a Method of producing Electromagnetic Disturbances of Comparatively Short Wave-lengths": the whole of this paper may be copied here as reported in the annual volume—it consists of three lines:

"This is by utilising the alternating currents produced when an accumulator is discharged through a small resistance. It would be possible to produce waves of as little as 10 metres wave-length, or even less."

These papers were the sequel to a previous paper of his, and to many discussions with me, on the possibility of producing Maxwell's waves artificially. The title only appears in a previous volume, that of 1880. It runs: "On the Possibility of originating Wave Disturbances in the Ether by Electromagnetic Forces." This was an extremely general mathematical paper, reproduced in his Collected Works, and its original title had been "On the *Im*possibility . . . ," for it was a doubtful point then whether ether waves could be generated directly by electrical means; though by 1883 (as we have seen) he was satisfied, not only that ether waves could be produced, but exactly what power they would have. It was a subject constantly in his mind, and it made him very receptive of the discovery made by Hertz, four or five years later, in 1887 and 1888.

The method suggested by FitzGerald in 1883 is exactly the way they are produced to this day. The fact is that it turns out to be ridiculously easy to produce the waves. They are produced whenever a condenser or even any conductor is discharged. The trouble was to detect them. The eye is a splendid instrument for detecting waves of infra-microscopic length, but these other waves have a much bigger length, easily calculable but not readily detectable. We have no kind of sense for them. We knew about electric oscillations, for Lord Kelvin had given their theory so long ago as 1853, and we were accustomed to demonstrate the oscillating character of the Leyden-jar spark in various ways ; e.g. by magnetising a knitting-needle in strata, and by a revolving mirror ; but only one or two thought of the emission of waves.

The meeting was held at Southport rather through the machinations of the combined secretariat, who had been visited in their den at Swansea and elsewhere by a Southport gentleman of extreme popularity, Dr. Barron, I think. He had persuaded us that Southport was *the* place, so that when competitive invitations came up for discussion before a large meeting of the General Committee, Southport was narrowly carried by reason of unanimity among the sectional secretaries, in face of some disapproval in high quarters among the officials. H. B. Dixon would remember this episode, but most are gone.

For Southport Professor Cayley was elected President, at an advanced age, and his address was mainly inaudible. As Southport is so near Liverpool my wife attended this meeting ; we were hospitably entertained by a banker, and enjoyed the meeting. Section A was presided over by my old teacher, Professor Henrici, who dealt in his address with Modern Geometry.

Lord Rayleigh described some anomalous results which he got with a galvanometer subjected to momentary transient currents.

> "If the contacts are properly made, the integral current through the galvanometer at each operation is rigorously zero, but in the experiments that I have made no one could infer the fact from the behaviour of the galvanometer needle."

It was this kind of thing which led him humorously to remark, "The Uniformity of Nature may be axiomatic, but it would never have been discovered in a laboratory."

So now we come to the first journey of the B.A. to the Dominions across the seas, initiated by the Montreal meeting of 1884.

CHAPTER III

THE FIRST DOMINION MEETING—DEPOSITION OF SMOKE AND FUME

Montreal, 1884

Thus we come to the first meeting in the Dominions beyond the seas, the series initiated by the great Montreal meeting of 1884. This departure, though it was implicitly provided for by the Statutes drafted by the Founders of the Association, was not undertaken without much searching of heart and many qualms. In fact there were not lacking advisers, in the Press and elsewhere, to advocate that after the Jubilee meeting of 1881, the Association might terminate its existence, that its work was accomplished, and that it had better now cease to be. It is not too much to say that the success of the Montreal meeting silenced these prophets of ill-omen, and helped to renew the vitality of the Association, so that it has now another fifty years behind it.

For the Presidency at this meeting Lord Rayleigh was unanimously selected ; for he was a man of position who had written his name large on the mathematical physics of his time. As the Hon. J. W. Strutt he had been a brilliant Senior Wrangler at Cambridge, and was appointed to succeed Clerk Maxwell in the Chair of Physics. There, with Glazebrook and Shaw as demonstrators, he had set the teaching of experimental physics at the University on a firm foundation, as well as conducting many researches of his own.

About the Presidency of Section A at Montreal there was much debate ; and at last it was decided to ask Sir William Thomson once more to occupy the Chair, which accordingly he did.

I was selected to give one of the Evening Lectures, the other being given by Dr. Dallinger. I selected " Dust " as my subject ; for I had been engaged for a year in experimental researches on dusty air, along with my friend and assistant, Mr. J. W. Clark. I had been started on this investigation by a reprint, in " Nature " for June 7th, 1883, of a Royal Society paper of Lord Rayleigh's on the dark spaces observed and called attention to by Tyndall in the beam of an electric light below which a flame or other hot body was placed. Tyndall knew that the dark spaces were not smoke but the absence of smoke, and he surmised that the dust had been burnt up or volatilised by the flame. Lord Rayleigh repeated the experiment with care and precision, and had shown that a dust-free plane arose from rods or wires of quite moderate temperature ; whereby the idea of combustion was rendered absurd. He suggested several explanations, but was not satisfied with any of them.

Clark and I found not only a dust-free plane ascending from the body, but a dust-free coat surrounding it. I wrote a rather interesting letter about it in the same volume of "Nature" (xxviii), p. 297. One special feature noticed by Rayleigh is that the stream-lines are symmetrical above and below the hot wire; a fact which, as he says, is due to the motive force originating in the wire itself: if the stream had been propelled against it there would have been no symmetry, and eddies would have been produced in the wake. We measured the thickness of the dust-free coat at various temperatures and pressures, and ultimately (though not at first) came to the conclusion that it was due to an effect of molecular bombardment, somewhat akin to Crookes's radiometer, the dust particles being small enough to feel the extra bombardment of the atoms on one side, and to be driven away from a hot body to a remarkably uniform extent. We thus found that hot surfaces tended to keep themselves clear of dust, whereas cold surfaces had dust out of the air bombarded upon them; thus explaining the black patch that used to be seen on the wall near a stove or on the ceiling above a gas-jet, or even above an electric-light globe, where no question of smoke could arise. This aspect of the subject was well developed by Mr. John Aitken, and has many applications. Our long paper was published in "The Philosophical Magazine" for March 1884 and occupied twenty-six pages.

Before arriving at any conclusion, however, and in the course of experiments, we electrified the rod to see what effect that would have on the dust-free coat. The method used was to have a long rod or thick wire, projecting straight into a cigar-box with glazed sides, illuminated laterally by a strong light (in those days lime rendered incandescent by an oxyhydrogen jet), and looked at longitudinally through a microscope. The temperature was regulated by applications of heat to the portion of the rod outside the box, and the box was filled with smoke.

On now electrifying the rod gently, the dust-free coat seen round it thickened, contorted itself, and ultimately extended all through the box. In fact, we found that anything like a brush discharge cleared a confined space of smoke or dust completely. The particles first congealed into flakes and then clung to the sides of the box. We began trying that, on what we thought was a large scale,using a bell-jar with a pointed rod passing into it, and connected with one terminal of a Voss machine. The smoke used in the cigar-box had been tobacco smoke, blown into it by the indefatigable smoker J. W. Clark. For the larger experiment we used the smoke of burning magnesium, which is clean and easy to produce ; but we used also ammonium chloride or sulphide, produced by generating ammonia and either distilling hydrochloric acid or burning sulphur into the same space. We also cleared a cellar of turpentine smoke, looking at it from outside, thereby making a horrid mess ; but we usually employed a bell-jar. On gently turning the handle of the Voss machine, the first effect was a great agitation of dust, then the formation of snow flakes or streamers, and then total disappearance, and clearness of the air, though the walls were all dusted over. It was a striking experiment, and I intended to show it for the first time in Montreal.

The rest of the lecture was to deal with dust in various forms, with a great number of experiments, some of them due to Tyndall, who had shown that a beam of light was invisible unless the air was dusty, and had made some long big tubes for the purpose. These tubes, and a quantity of other apparatus, together with one of Tyndall's assistants, Mr. Cotterill, were lent by the Royal Institution for the purposes of this lecture. Cotterill had travelled across the Atlantic with a lot of packing-cases, and had unpacked them and begun to set them up in the lecture-room before I arrived on the scene. I didn't arrive even on the morning of the day that the lecture was to be delivered ; for I had an important controversy with Sir William Thomson, on the seat of the electromotive force in the voltaic cell, in Section A that same morning. The discussion lasted two hours, and he and I took opposite sides. Whether there is anyone living who now remembers it, I don't know. I think Donald MacAlister might ; and I know that the Oxford mathematician E. B. Elliott was much impressed, and wrote enthusiastically to my brother Alfred about it. It was a subject that I had studied with considerable thoroughness, and I did not hesitate to oppose the distinguished President, though I always held him in the greatest veneration.

After this tournament I went to the lecture-room in the afternoon to see how Cotterill was getting on. I found him in a state of despair and ready to wash his hands of the whole business. He complained that there had been no rehearsal, and said that Dr. Tyndall would have had a complete rehearsal of all the experiments the previous day. He had rigged up the optical tube of Tyndall and the other experiments that he was familiar with, though he kept saying that the lecture would be a failure. I worked away, erecting my own apparatus (I suppose I got some food), till the people began to come in. I remember going to the top of the stairs as Sir William Thomson came up, and saying, "I am afraid it is going to be a failure." The atmosphere was very damp, and the Voss machine declined to work, so that the chief experiment seemed bound to fail. However, as it happened, the Canadian climate took a sudden turn for the better, and after I had done successfully the early part of the lecture, I turned with some trepidation to my chief experiment of the bell-jar. To my surprise the atmosphere was now dry, the Voss machine insulated perfectly and was in excellent order. The experiment accordingly was a complete success, and Sir William, dear old man, showed his enthusiasm by getting out of his seat and going to the bell-jar, clapping his hands as he went, to inspect the artificial snowstorm more narrowly. Lord Rayleigh proposed a vote of thanks, and I am bound to say that the lecture was a tremendous success. It is reported for the most part in " Nature " for January 22nd, 1885, vol. 31, p. 265. Most of the experimental work that led up to it was reported by myself and J. W. Clark in " The Philosophical Magazine " for March 1884.

I took occasion to caution the Canadians against allowing their beautiful city to be submerged with smoke, for I had seen a chimney near the docks behaving badly. But the caution was not well received by the Press. They said there was plenty of fresh air in their continent, and they wanted some signs of industry. I was told of two schoolboys, one pointing out a smoking chimney to the other, saying, " Ah, ha ! your father's chimney doesn't smoke like ours does." Well, there are always different points of view. Pittsburgh used natural gas then, and was clean. Imperfect combustion is not a thing to be proud of.

The smoke experiment was to us and to the scientific world in general absolutely new ; though someone discovered a paragraph in an " English Mechanic " of 1851 that something of the kind had been observed by a Mr. Guitard, and there briefly recorded, but it had attracted no attention. This experiment at Montreal attracted considerable attention, and was supposed capable of depositing the smoke caused by careless firing in cities ; whereas the expense of making smoke ought to be saved. For metallic fume, however, the method should be applicable, and a first application in that direction was attempted by Mr. Alfred Walker of the firm of Walkers-Parker at Chester, who built a big Wimshurst machine in order to produce the electricity, and tried to apply it to deposit the dust from his lead-smelting furnaces at Bagillt in North Wales. A lot of metallic ore from the furnace went off in the smoke, and as it was valuable many attempts had been made to save it. The chimney was put at the top of a hill some half a mile away; and the flue which led from the furnace to the chimney up the side of the hill collected some of the dust; but still a lot escaped from the chimney, and was deleterious to the countryside. This early attempt to generate electricity by a Wimshurst machine and to lead it into the flue, where a torrent of hot gas was rushing past, was a task of considerable difficulty, and it was not overcome. The insulation was defective, and many times the machine refused to act. A better and more engineering method of providing the high-tension electricity was needed, such as the rectified potential from a high-tension transformer excited by an alternating dynamo. So for a time the attempt was abandoned.

Later one of my sons (Lionel) was impressed with the possibility of generating high-tension electricity on a large scale, and revived the idea of fume deposition in works. He made many experiments in my laboratory as to the best form of electrodes, alternately positive and negative, for the collection of dust, as the smoky air passed over the electrified surfaces even at a high rate through a chamber. We had a fan to propel it. At last he managed to overcome the difficulties, and put the apparatus into a practicable large-scale working form, and with his brother Noel started the Lodge Fume Depositing Company, at 51 Great Charles Street, Birmingham.

Meanwhile in America a Dr. Cottrell had done something of the same sort, and had got the method actually adopted by some firms. He was very friendly about it, fully acknowledging the source of the notion, and offered to co-operate with Lionel and Noel, and post them information, dividing the areas. This accordingly in due time they did, changing the firm's name to Lodge-Cottrell both here and in America. Later a German firm also combined and co-operated. The whole thing now got out of my hands, and became a large commercial process, applied on a tremendous scale, with railway trucks running under the great depositing chambers to receive and carry away the deposited dust. The high-tension electrical plant had to run night and day continuously, with three relays of workmen, and was only stopped once a year. Anything up to a hundred thousand volts was used, and the deposition of dust in smelting operations and in blast furnaces attained 99 per cent. Each successful installation led to others, so that there was no need to advertise or make the process known. Consequently it has not attracted general attention ; but if anyone thinks it easy to design and run a high-tension installation like this, night and day, without accident and without getting choked with its own deposit, he is mistaken.

This is a digression from the meeting at Montreal. The Dust Lecture was on the Friday, actually in the evening of the same day on which the seat of the E.M.F. had been discussed in Section A ; and the day was not over then, for that same night we all got on the steamer to travel on a B.A. excursion back to Quebec. FitzGerald, and, I think, Silvanus Thompson, were there, and I remember that instead of going to bed early, we sat on a bench outside the cabins, and continued the discussion on the seat of the E.M.F. To our surprise a window behind us opened, and Sir William Thomson looked out, and joined in the discussion. He referred to the discussion in the section, and presently said, " Good heavens, it was this morning ! "

My paper " On the Seat of the Electromotive Force in a Voltaic Cell " was ordered by the General Committee to be printed *in extenso* in the annual volume, and there it will be found between pp. 464 and 529. Later it was amplified still farther and reproduced in " The Philosophical Magazine," and attracted considerable attention in Germany.

At Quebec we were received by the Governor, Lord Lansdowne, on the terraces above the St. Lawrence, and next evening there was dancing before we took the boat back to Montreal. I remember having a long talk with Lord Lansdowne, in a way which must have impressed him ; for Lord Rayleigh told me that he had spoken to him about it afterwards. How I got through all that work in those days without breaking down I don't know ; but so far from breaking down, I was thoroughly alive. My age at the time was thirty-three, and at that age one seems to be able to carry off any amount of responsibility and hard work without turning a hair.

The Yellowstone Park

Now let me say a word or two about what happened before the meeting. I had arranged to go from Liverpool with my friend, Isaac Cooke Thompson, a well-known microscopist and drug manufacturer of that city, who later worked with Herdman at his marine biological station, specialising on the copepoda. He took our passage in the Dominion liner " Vancouver," some little time before the date of the meeting, in order to see something of the Continent beforehand. We landed at Quebec, and were surprised at its old-fashioned appearance, its streets paved by longitudinal planks of wood, and its language being mainly French. We stayed a night at Quebec before going on to Montreal, and visited the falls of Montmorency, which are remarkable for having no visible tail water. The river disappeared, and was supposed to come up again somewhere in the St. Lawrence. We also visited a lunatic asylum, the chief officer of which was a friend of Isaac Thompson's brother. The methods seemed backward as compared with similar institutions in England. Then we went on for a day in Montreal, and thence started on our western pilgrimage. We went on what we understood was the first train of the Canadian Pacific Railway to go as far as Calgary, except that there was a long break at Lake Superior, which had to be negotiated by steamer.

We had intended to get food at Toronto, but the train was late, and we never went into Toronto ; so when we arrived on the shores of Lake Huron and boarded the steamer, the passengers were all famished, and felt like murdering the nigger attendants, who declined to supply anything before the proper time. The steamer went through the locks at Sault Ste Marie, and after a whole day or more on Lake Superior we arrived at Port Arthur, and again took train. The train was very leisurely, and used to stop every now and then, while the passengers got out and gathered blackberries ; at the sound of a bell they all remounted, and went on, at about twenty miles an hour. We got as far as Winnipeg, and then were driven in a bus to the hotel, along the worst street that ever I encountered—the most extraordinary drive. I thought if we survived that, I should never be nervous in a carriage again. Winnipeg was in a very early state ; and many of the things we went to see were " not built yet." There were old shanties left against modern stone buildings, and the roads had not yet been changed from prairie tracks with great holes in them. I saw Winnipeg again in 1920, and it had altered beyond recognition in the intervening thirty-six years. Everything was by that time in a state of prosperous organisation.

From Winnipeg we trained south to a junction on the Northern Pacific. There was some trouble at the frontier, for I had a small package of things left behind by my brother when he emigrated to Chicago, which I wanted to send to him. They asked me to sign a form saying that I wished to become an American citizen, and naturally I declined. We boarded a train, and went on for days through the cornfields of North Dakota and over the " bad lands " of Montana, till we got near to Idaho and the foothills of the Rocky Mountains, and thence towards Wyoming and the Yellowstone Park, which had just then been opened. This too was in a primitive condition : the main hotel at Mammoth Springs was a wooden structure, with cow-boys in their picturesque accoutrements grouped about a great iron stove in the central hall, under the glare of an electric arc-light, which was run by a threshing-machine in the yard.

We chartered horses and a guide, and went two or three days' journey in the Park to see the geysers. Some of the " hotels " were wooden shanties, others were tents. It was all very primitive, and there were odd adventures. For instance, the partitions between the so-called " rooms " in one of the hotels didn't reach up to the ceiling : consequently everything that went on in any of them was audible in the rest. Most of the occupants realised this and were quiet. But in one room there was evidently a couple who had had a disagreement, and for a long time the lady was remonstrating with her husband, saying, " Oh, George, it's cruel of you not to kiss me good night ! " After many requests of this kind had been listened to, a loud male voice ejaculated, " For goodness sake, kiss her, George, and let's all get to sleep." After which there was silence.

In the so-called " Park," or Reservation, which is as big as Wales, there were no roads—we rode along water-courses and through scrub, occasionally cantering on a stretch of prairie, where we were lucky if the horse didn't put his foot down a rabbit-hole. We saw " Old Faithful " among the geysers, erupting regularly every hour, as I suppose it is doing still : we passed many " paint pots " of bright-coloured hot mud, and a path where there was boiling water on one side and a fish pond on the other, and saw many strange sights in that radioactive or volcanic district, though we knew nothing about radioactivity then. After falling down on the accumulated deposit of the Castle geyser, and getting my knee badly bruised, we began our long return to the railway ; and so started on the return journey for Montreal, reading paper-backed novels by the way, which were brought round by a train attendant. When after some days we reached Chicago, we felt as if we were half-way home. We were getting quite used to sleeping on the train by this time. How we entered Canada I don't remember, possibly by Detroit and Windsor (Ontario), but anyhow we took the quickest route, as we had no time to spare. Ultimately we arrived in Montreal on the Wednesday of the initial meeting of the Association, half an hour before the President began his address. We scrambled into dress clothes, and were there just as Lord Rayleigh began to speak. We had travelled five days and nights from the Yellowstone Park, and hit it off with great precision. It was a near thing !

After the meeting we went to Toronto by the lovely thousand isles, descending the Lachine rapids with an Indian pilot ; and so to Niagara by steamer. I remember an absurd feeling of hurry as we steamed on Lake Ontario for fear all the water should be over the falls before we arrived to see it. I was keen on the utilisation of water-power. The river as we approached was most impressive, and we had the advantage of having our first glimpse of the falls from the wooded country on the Canadian side, and were duly impressed. It was the only waterfall I had seen over which I did not want to pour some more water : I felt that if the Thames were added it would make no appreciable difference. We walked all about the shores of the Niagara River, and continually were stopped and asked for payment, but we had encountered an acquaintance on the steamer who was writing an article on "The Extortions of Niagara," and so the more we were "extorted" the better we were pleased. We walked by the rapids, and found Captain Webb's widow installed in a hut selling photographs. It seems that Captain Webb lost his life through an imperfect acquaintance with physics. He had experienced the waves in the Channel, and knew them to be superficial disturbances : he probably thought that the waves of the rapids were of a similar nature, but really they were the outward and visible sign of a violent turmoil beneath, and in that violent commotion the gallant sailor lost his life. The calmness of the water for some distance immediately below the falls is striking. A steamer, "The Maid of the Mist," plied there, and my friend F. W. H. Myers had swum across in the same place, as he had also swum the Hellespont in rivalry to Leander and Byron.

We returned home from New York, and Isaac Thompson had taken our passages by the "Austral," a steamer built for the Pacific, with roomy airy cabins, much better than the close dens of the "Vancouver." Its owners were now running it on the Atlantic to demonstrate its seaworthiness, for it had had the misfortune to sink while moored in Sydney harbour : after being unloaded of coal on one side, it had turned turtle in the night ; and they wanted to show that nevertheless it was trustworthy for passengers. Accordingly we had a venturesome voyage in her. The captain was a good and enthusiastic sailor, willing to run risks if anything was to be gained thereby. As we went out of New York harbour and past the Statue of Liberty, we saw the " Arizona " behind us, just starting on her voyage an hour or two later. The " Arizona " and the " Alaska " were fine ships of the Guyon line. The " Alaska " had recently beaten the record, and was known in Liverpool as the greyhound of the Atlantic. The " Arizona," though not so quick as the " Alaska," was much quicker than the " Austral." Our captain determined to beat her if he could, and accordingly laid our course straight for the Fastnet. This was unusual, as it took us right over the Newfoundland banks, by what is called the overland route, so that we went right through the fishing fleet afterwards glorified by Kipling in " Captains Courageous." They seemed astonished at our passing. Indeed, we might easily have run aground, but we didn't. Every breath of wind was used to help the engines. Sails were rigged up and tautened continually to catch every breath of gale. Part of the voyage was fairly rough, and the rolling powers of the " Austral " were abundantly demonstrated. Passengers were not allowed on deck during this part of the time, for the guard rail went under water at every roll. We stayed in our cabins, and heard crockery crashing in all directions, and amused the stewards by calling for steak and onions or other violent food, for we were quite well. The navigation was perfect. We didn't have to alter our course an iota : the " Fastnet " lighthouse lay right on our track, and so on full speed to Liverpool. In the Mersey we had to pull up, as our company's tenders were not ready. Looking back we caught sight of the " Arizona " again ; it was steaming up the Mersey behind us and overhauling us rapidly, and to our chagrin and the fury of our captain, though we had won the race, it was allowed to begin to land and discharge its passengers first. In those days the ships hovered in mid river and did not tie up at the landing-stage ; there was no riverside station—you took a cab to Lime Street. But we didn't want a train, we chartered a cab, and drove off to our adjacent homes in Waverley Road, where our wives were joyfully awaiting us after nine weeks' absence.

CHAPTER IV

KELVIN'S VORTEX THEORY AND ELECTROLYSIS

Aberdeen, 1885

In 1885 the meeting was in Aberdeen under the Presidency of Sir Lyon Playfair. This chemist was a well-known member of the House of Commons, and his Address deals largely with scientific teaching from the national point of view. Incidentally he says that when he was a boy—

" the only way of obtaining a light was by the tinder-box, with its quadruple materials, flint and steel, burnt rags or tinder, and a sulphur-match. If everything went well . . . a light could be obtained in two minutes."

Phosphorus matches, he says, were introduced in 1833. By 1845 red phosphorus had been applied, and the safety match introduced.

The President of Section A was George Chrystal, the Professor of Mathematics in Edinburgh. He was famous for having written the articles " Electricity " and " Magnetism " for the Ninth Edition of the

"Encyclopædia Britannica," showing a grasp of the subject far superior to that of most other professors at the time.

It was at this meeting that Osborne Reynolds gave his paper on what he called Dilatancy, a curious phenomenon exhibited by a granular medium, that is, a medium consisting of solid particles. He showed that in such a medium the application of external pressure, instead of driving the particles closer together, causes the spaces between them to enlarge. That is illustrated by the sand on the sea-shore :

"As the foot presses upon the sand when the falling tide leaves it firm, that portion of it immediately surrounding the foot becomes momentarily dry. When this happens the sand is filled, completely up to its surface, with water raised by capillary attraction. The pressure of the foot causes dilatation of the sand, and so more water is required. . . . On raising the foot we generally see that the sand under and around it becomes wet for a little time. This is because the sand contracts when the distorting forces are removed, and the excess of water escapes at the surface."

He goes on to say that this—

"places a hitherto unknown mechanical contrivance at the command of those who would explain the fundamental arrangement of the universe. [He proceeded to explain, in a general way,] how bodies in such a medium would—in virtue of the dilatation caused in the medium—attract each other at a distance.

. . . [While in a region close to the body] the density varies several times from maximum to minimum . . . a condition which seems to account for cohesion and observed molecular force far better than any previous hypothesis."

He suggests that this phenomenon might explain statical electricity, and with a little addition electro-dynamic and magnetic phenomena also.

In other words, his paper constituted the beginnings of a granular theory of the Ether, in which the particles of matter embedded in it are represented by the hollows between the granules ; an idea not unlike that of the electron as a hollow in an otherwise massive medium, which a few writers advocate to-day. That a granular constitution for the ether is ever likely to be a satisfactory explanation of the universe seems to me extremely unlikely. But Osborne Reynolds was a genius whose ideas are not to be despised, and until we know more about the ether it is just as well to bear this heroic speculation in mind.

The annual volume contains two great reports, one by Sir J. J. Thomson on Electrical Theories, in which he deals with the German theories and compares them with Maxwell ; the other by Sir Richard Glazebrook on Optical Theories, including not only Fresnel and MacCullagh, but Stokes and Kelvin and Helmholtz, and many others, Kelvin's being obtained from his Baltimore Lectures delivered in the year previous.

The success of the discussion on the seat of the

E.M.F. at Montreal led to my being asked to open a discussion on Electrolysis at a joint meeting of A and B, the request being conveyed to me by Professor Henry E. Armstrong, who was then President of Section B. My introduction took the form of an elaborate paper, which was ordered by the General Committee to be printed *in extenso* among the Reports (see pp. 723–772). It concludes with a number of questions or suggestions for future research, and a Committee of the Association was appointed to carry them out as far as possible. I would call special attention to a long discussion of ionic velocity and other chemical matters from the electric point of view; beginning with a section headed "Atomic Idea of Electricity: Electrostatic Theory of Chemistry," and continuing on pp. 743–763 of the Aberdeen volume.

Birmingham, 1886

One result of the appointment of an Electrolysis Committee at Aberdeen was to stimulate me to fresh exertions, and accordingly, in 1886, at Birmingham, there is another long communication drawn up by me on Electrolysis, with contributions by others, and with abstracts and translations (see pp. 308–389), to which Arrhenius and Willard Gibbs contributed. My criticism of Arrhenius was perhaps not quite fair to that distinguished man. He objected that I had been more polite to Bouty and Kohlrausch than to him. I agree; but I translated his paper and said:

"So far as I am able to judge, and making allowance for possible inadequacy of data and somewhat hasty generalisation, the paper seems to me to be a distinct step towards a mathematical theory of Chemistry. The title affixed to it is 'Chemical Theory of Electrolysis,' but it is a bigger thing than that: it really is an attempt at an *electrolytic theory of Chemistry.*"

Then follows my own paper "On the Migration of Ions and an Experimental Determination of Absolute Ionic Velocity," which occupies the report from pp. 389–412. This particular research is referred to in my Autobiography. Its results are to verify Kohlrausch's theory (cited in the Aberdeen Report, pp. 753–757) of the migration of ions and the mechanism of electrolytic conduction generally. In particular the velocity of a hydrogen ion under a gradient of 1 volt per centimetre applied to an electrolyte came out from the first experimental measurement ·0029 centimetre per second; whereas Kohlrausch's theoretical number deduced from conductivity and migration data had been ·003 centimetre per second. A very striking agreement: undoubted as regards order of magnitude.

The whole Birmingham meeting in 1886 was presided over by Sir William Dawson, a Canadian geologist, with George Darwin as President of Section A.

At this meeting Crookes was President of Section B, and his address is remarkable for his suggestion about atomic weights, namely that any atomic weight determined by chemical methods might be a kind of average; and he reviews some arguments which had been suggested for the evolution or gradual falling together of the constituents of atoms, so as to produce a regular series, and leave behind the known elements in its course. Crookes in his conclusion surmises that the elements are either compounds or mixtures, and that they have been evolved from one primordial form. He asks investigators not necessarily either to accept or reject his hypothesis of chemical evolution, but to treat it as a provisional hypothesis. It is needless to say that these speculations, about chemically determined atomic weights being an average, have been abundantly justified by the discovery of isotopes, and indeed by many other now known facts.

Manchester, 1887

The Manchester meeting of 1887 was to have been presided over by Dr. J. P. Joule: at the last moment Sir Henry Roscoe had to take his place, and incidentally exhibited a pathetic kind of hero-worship in his reference to Joule. He adopted words of a previous President in Manchester in 1842 referring to John Dalton, and applied them to Joule : " I would gladly have served as a doorkeeper in any house where Joule, the father of science in Manchester, was enjoying his just pre-eminence."

Sir Robert Ball was President of Section A, and gave a curious dynamical parable illustrating his theory of screws, and the great advantage that that theory had over Cartesian methods in stating certain kinds of motion.

Professor Schuster described his then new and beautiful experiments on the ionisation of rarefied air. By sparking air he renders it conducting, so that a single-cell battery can maintain a current through it and deflect a galvanometer. In fact, ionised air becomes an electrolytic conductor.

There was also a paper by Professor Horace Lamb on Helmholtz's investigation of Electric Endosmose, which I refer to in a contemporary number of the " Electrical Review," where I comment on the meeting, and incidentally proceed as follows :

" Immediately after this paper occurred the event of the meeting, the thing for which in all future times the visit of the Association to Manchester will probably be famous. Not that it made any sensation at the time ; it scarcely took up 10 minutes of the time of the section ; the President made no remark upon it, and only by two or three individuals who happened to have been thinking in the same direction was its real significance understood. It was a short paper by Sir

William Thomson, entitled, 'On the Vortex-Theory of the Luminiferous Ether.' To understand the meaning of it one must go back a little.

"It is well-known that several years ago Sir William Thomson suggested a vortex theory of matter, based on the observed behaviour of a vortex ring and on the properties of vortices in general as investigated mathematically by Helmholtz. The 'vortex-atom,' as it is called, is a favourite subject of speculation, and Prof. J. J. Thomson is attacking the problems of chemical combination by its means. But although this remarkable theory of the nature of matter was before the world, and its consequences being traced, yet of the nature of the ether we had no such theory. It was perceived that many of the properties of the ether must be those of a perfect fluid, else how could planetary bodies move unresistedly through it? But, on the other hand, some of its properties must be those of a solid, else were it incapable of transmitting waves of light. It was likened, therefore, to a jelly, and to pitch, which in some respects are semi-fluid, in others semi-solid; it was pointed out that bullets could sink through apparently solid pitch if time enough were given them; and the difference between this slow creep and the rush of a planet was asserted to be only one of degree. Yet this was felt to be not quite satisfactory, and the contradictory properties possessed by the ether remained paradoxical.

"Then Prof. FitzGerald, of Dublin, made a brilliant guess: the properties of the ether could only be explained, he conjectured, by an assemblage of minute vortices in a perfect fluid—a ' vortex sponge,' as Sir William Thomson had previously called such a structure. If the ether consisted of a perfect fluid, everywhere completely enwrapped in utterly minute vortices, much more minute than the comparatively gross vortex rings constituting the atoms of matter, then would it permit the passage of bodies through it with perfect ease. It would also act as an electric and magnetic medium in the way needed by the Faraday-Maxwell theory ; and in all probability, so he surmised, it would be found capable of transmitting the transverse waves of light. But to prove this last property deductively from the theory of vortices seemed a task of supreme difficulty, from which he shrank.

" Actuated possibly by this suggestion, perhaps quite independently of it, it must have occurred to Sir William Thomson to consider this problem as a mathematical exercitation during some hours of leisure. However it happened, he did consider it, and did solve it ; apparently, too, without very much difficulty, or much consciousness of the feat he had achieved. At Manchester he scribbled the proof in a few lines on the black-board : and it was thenceforth known that a vortex sponge of perfect fluid could do everything that the luminiferous ether was known to do. Among the audience was Prof. W. M. Hicks, who probably knows as much of vortex mathematics as any man living, and he expressed himself afterwards as fully satisfied with the proof, and conscious of the greatness of the discovery.

“ Prof. FitzGerald, in proposing afterwards at the committee meeting that the paper be printed *in extenso* among the reports, characterised it as the greatest step towards the comprehension of the intrinsic structure of the universe which had been made since the time of Newton.

“ It is perhaps rash to wax enthusiastic over the greatness of an infant at the moment of birth : a man must commonly live, and must die, before his right magnitude becomes really known ; nevertheless, when a discovery has been watched and hoped for, it is surely not only natural but seemly to herald its advent in not too dull and prosaic a manner.

“ It is not to be supposed that the discovery is complete and finished ; a great deal more remains to be done to get it into shape and work it out, but a splendid start has been given.”

Whether this contemporary enthusiasm (about a paper which will be found in the annual volume for 1887, pp. 486–495, or in Vol. IV of Sir William Thomson’s Collected Papers, pp. 308–320) was justified or not has become doubtful in view of the discredit poured upon any attempts at explaining the ether dynamically, in recent years ; and I see that Sir William himself was not quite satisfied and concludes the printed version, with Scottish caution, as “ not proven.” But I still expect that posterity will recognise some inklings of truth in these hydrodynamical speculations. My report of the meeting went on :

“ Towards the end of the sitting Prof. Ewing described his recent remarkable researches in magnetism.

"It is perhaps known that the Ampère-Weber-Maxwell theory of magnetism requires that for some very high magnetising forces the magnetisation produced shall cease to increase and begin to diminish ; in other words, that the susceptibility of a substance to magnetism shall decrease and even become negative for sufficiently high magnetising force.* Thus then iron, if enormously over-magnetised, might actually become diamagnetic like bismuth.

"To verify this prediction, the writer of this article has himself made some attempts, and is able to show a body which is paramagnetic in a weak field and diamagnetic in a strong one ; but then such a body is not simple, pure, and homogeneous, it is purposely built up of a magnetic and a diamagnetic substance. Or even when not purposely built up, who is to say that some trace of impurity is not at the bottom of the phenomenon ?

"Professor Ewing has proceeded in a different way. He has pushed the magnetism of iron far beyond hitherto recorded limits, and believes that he finds indications that the decrease of susceptibility has begun.

* "Weber's theory of diamagnetism depends on the induction of currents in conducting molecular channels, while paramagnetism requires the facing round of already existing molecular currents. A paragraph in § 844 in Vol. II of Maxwell's 'Electricity and Magnetism' ends thus :

"If it should ever be experimentally proved that the temporary magnetisation of any substance first increases, and then diminishes as the magnetizing force is continually increased, the evidence of the existence of these molecular currents would, I think, be raised almost to the rank of a demonstration."

Whether this be so or not, there is no doubt of the tremendous magnetic inductions he has obtained. On the same scale as that on which the earth's horizontal intensity is ·18 units, Professor Rowland of Baltimore had obtained a field in iron as high as 17,500 units; and had given reasons for believing that this was about the maximum possible. Prof. Kundt, however, soon overstepped this limit; while Prof. Ewing afterwards managed to get it up to 32,000. And now, with the great magnet of Edinburgh University excited by a storage battery, putting a sharply sloped double-coned piece of Lowmoor iron with very narrow neck between its large poles, he has succeeded in beating his own record and forcing the magnetic induction in iron up to 45,350. Cast iron has gone up to 31,270; and even in air he has measured a field of 25,620 units."

These figures may be compared with the enormous values which Dr. Kapitza, at Cambridge, has recently been able to obtain in air.

CHAPTER V

THE DISCOVERY OF ELECTRIC WAVES

BATH, 1888

The success of the Montreal meeting of the B.A. in 1884 may be said to have given the B.A. a new lease of life; and since then it has never looked back. Incidentally, the meeting improved my own scientific position: I became more widely known. I had only been a Professor at Liverpool for eight years, and yet when in 1889 I took some experiments up to London with my assistants, great crowds were attracted; and I remember Professor Rucker saying: "It isn't often a young man comes up from the provinces and sets all scientific London agog." The experiments then shown were connected with high-tension discharges and the consequent production of what are now universally known as etheric or electric waves.

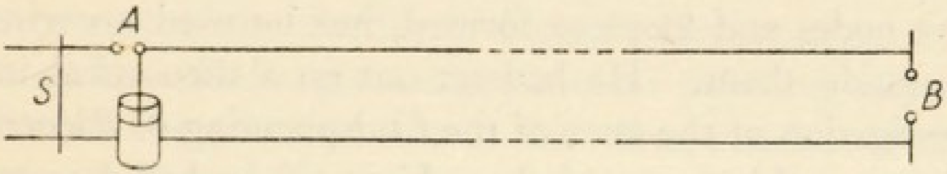
Although we knew all about electric oscillation, no one but G. F. FitzGerald in 1883 had thought of their being competent to emit waves into the ether, as a tuning-fork emits them into the air. It is surprising now to realise our blindness in this respect, but at the time it seemed a question of some difficulty whether such waves could be generated electrically. We had only recently learnt that light was an electromagnetic oscillation, and we had no idea of the mechanism of the process of radiation. There was no atomic theory of radiation, nor did we know anything about electrons. Zeeman was the first to show that an electron was the optical radiator, by means of a magnetic field applied to the source, but that was not till 1895 or 1896.

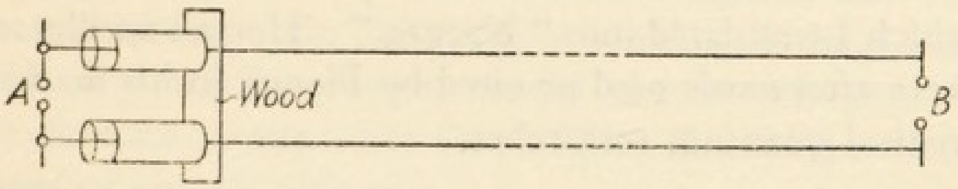
When Hertz tried to examine the result of his sparking conductors he found to his surprise that when faced by other conductors, even in another room, they emitted an influence strong enough to cause small sparks. The radiating power was tremendous while it lasted, amounting to hundreds of horse-power, though only for a minute fraction of a second. This was strikingly demonstrated during the lecture I gave to the Royal Institution in 1889, when the metal wallpaper sparkled vigorously, and diverted the attention of the whole audience. Sparks also occurred in the gilt key-pattern high up in the hall of the Electrical Engineers, from the discharge of Leyden jars on the lecture table round a thick wire circuit, during a demonstration which I gave later that same year.

These minute sparks or scintillæ of Hertz constituted the first mode of detecting the waves. Then in 1889 I hit on the principle of the coherer, though I only applied it to conductors and to the overflow of Leyden jars; and later M. Branly discovered the curious fluctuation of resistance in metal powder smeared on paper, under the influence of electric sparks, and so was led to his filings-tube wave-detector. In early days I used to employ the single-point coherer along with a telephone to listen for the waves ; but after the Marconi demonstration of the possibility of commercial telegraphy by their aid, Alexander Muirhead was anxious to get them recorded upon tape by one of the siphon recorders which his firm manufactured for the Eastern Telegraph Company ; and that diverted us to the improved wheel coherer, designed largely by E. E. Robinson, which enabled us to get admirably clear messages upon tape. But that was later.

In the history of the subject it is interesting to realise the part played by my great friend G. F. FitzGerald. He first showed that it was possible to produce electric waves artificially by mathematical reasoning on Maxwell's principles. Then he suggested that the discharge of a condenser through a current of low resistance would emit such waves (see Southport, 1883) : and the wonder is that we neither of us followed this up with vigour and determination. I did indeed in my experiments in connexion with lightning detect such waves running along wire appendages attached to a Leyden jar, as in the diagram on page 91.

Waves ran along the wires, and were reflected at the far end, so that they became stationary waves. If the wires were attuned to the circuit, the recoil kick at the end of the wires was considerable. And in one form of the experiment a subsidiary spark at B at the end of long leads was accordingly much longer than the initiating spark at A. The circuit was tuned by the slider S, and thus the condition for maximum recoil kick at B was ascertained. Subsequently I exhibited to the Physical Society of London much longer wires, with a glow upon them in alternate long loops and





nodes, thus demonstrating the waves to the eye, after the manner of Melde's experiment with a stretched thread attached to the prong of a large tuning-fork.

The best plan was to use two jars standing on the table or otherwise connected by a poor conductor so that they could be charged : the pair of long wires stretching from their outer coatings. Then with long and properly adjusted thick wires the B spark could be got sometimes of extraordinary length, and if really long wires were used they became luminous in visible portions with intervening dark nodes.

I worked out the theory of these wire-guided waves (see "The Philosophical Magazine" for August 1888, p. 217), but I had not faith enough to look for them in free space without guidance. Hertz did that, being led up to it by a curious series of experiments. He reflected the waves into stationary waves, and detected the nodes and loops so formed, but he used no wires to guide them. He had set out on a theoretical investigation of the laws of the Outspreading of Electric Force, and he pursued the subject till he had detected the waves in space. This constituted the apotheosis of Maxwell's theory; and Hertz went on to study the electric mechanism of radiation by oscillators, in papers which I translated into "Nature." Hertz's oscillators were afterwards used or cited by Planck in his mathematical quantum researches.

Theory of Discharge modified by Self-induction

The origin of the above series of experiments in my case was that in 1887 I was told by Sir Trueman Wood, Secretary of the Society of Arts, that a course of lectures on protection from Lightning had been endowed by Sir Robert Mann, from South Africa, where the storms were frequent and violent ; and I was asked whether I would give this course of lectures. I agreed, and in preparation for them I began investigating lightning discharges, with the aid of large Leyden jars of a good pattern, procured from Chemnitz in Saxony.

I soon found that the problem of protection was much more complicated than had hitherto been thought. Everybody, including Sir William Preece, the head of the British Telegraph Department, who must have had a great experience of lightning protection on the practical side at that time, considered that it was all a question of comparison of paths. They thought that lightning would take the easiest path provided, and that accordingly if a perfectly conducting copper rod protruded above the building, and was well earthed below, the lightning in making its way from the sky to the ground, would choose this path in preference to any other that might offer itself, over an area of considerable size.

In those days the peculiar character of transient currents, and the effects of electromagnetic inertia which Faraday called extra currents and which Maxwell had christened self-induction, was not rightly understood by electrical engineers ; and the effort of an occasional physicist to draw attention to the importance of inductance was stigmatised by Preece as "what Americans called a bug," a sort of red herring thrown across the trail, an unnecessary complication.

About the same time, or soon afterwards, Oliver Heaviside was emphasising the importance of self-induction in connexion with telegraph cables, and made the amazing discovery that not only was it no detriment to the cable to have plenty of self-induction, but that the more it had the better, inasmuch as the transmission of signals would be more distinct. He showed that Kelvin's theory, which had treated the flow of electricity as similar to the flow of heat, a kind of diffusion through the cable, though partially true and a great step forward on what had previously been taught by practical men, was incomplete ; and that, by satisfying certain conditions involving adequate self-induction, true waves could be got to travel through the insulator of the cable, and that these waves would be guided by the wire to their destination, however far off, without distortion. He gave, in fact, the modern theory of the distortionless cable. It was not understood by telegraph engineers, and his name was rather scouted by them as a faddist, until Lord Kelvin perceived the truth of his contention, and dragged Oliver Heaviside into the limelight at a Presidential Address to the Electrical Engineers in 1889. Since then the progress made in this direction in the use of loaded lines for practical telephony, especially in America, has been extensive and astonishing.

Something of the same sort went on in connexion with my Mann Lectures at the Society of Arts. I think there were four of them, illustrated by experiments. They showed that an electric discharge was not a simple rush in one direction along a single path, whose property depended only on its conductance, but that the best conductor offered considerable opposition, so that when struck by a flash it did not convey the flash easily to the ground. Sparks were liable to fly from it in all directions, because of its inductance or inertia, and an inferior conductor was nearly as liable to be struck as a good one under certain circumstances; while a column of hot air afforded an easier path than a thick copper rod.

The question of "impedance" rose to the front, a thing which, though measured in ohms, was quite different from resistance in some cases. Its value depended on the frequency or rate of change in the current, so that under certain circumstances a copper rod a yard long and an inch thick might have an impedance of 100 ohms, whereas its resistance could hardly be more than a microhm. So that when an alternative path was provided, one through the common air, say one-eighth inch, and the other through a thick copper conductor, the momentary discharge sometimes chose the air, not instead of, but as well as, the copper conductor—a thing which seemed absurd to the old electricians.

The novelty of these results excited a good deal of interest, mingled with some scepticism, and at the Bath meeting in 1888, a formal discussion on the subject was set down on the programme of Section A, the protagonists being Sir William Preece and myself. I had the support of Lord Rayleigh and others, and was not annoyed to find that the technical journals considered that Preece had won. A cartoon was drawn representing him as a gladiator standing over my prostrate body, wielding a trident. (The rival contentions are referred to in "Nature," vol. 47, p. 536.)

I was well satisfied with the result of the discussion, however. It was felt that the old rules for lightning protection had to be overhauled, and a fresh set drafted. The work of a previous committee on the subject had become obsolete.

I made some mistakes, however, in questions of meteorological fact. I treated a cloud as a conductor, and assumed that the discharge was usually oscillatory. I knew by Lord Kelvin's theory of 1853 that a discharge might be oscillatory or might be a mere leak, and in some of my experiments it had become intermittent, a succession of small discharges all in the same direction. It is now known that a cloud is not a conductor—it may have different potentials in different parts—and that a discharge is usually of the intermittent character : so that a flash need not be as instantaneous as I had thought it was.

All this introduces minor modifications into the theory. At that time the origin of a thunderstorm was unknown, and the curious modern theory, supported and elaborated by Dr. G. C. Simpson, that the electrification is due to the rain (or rather to rain drops which are blown up by the air), had not then been mooted. I was working not so much at lightning itself, which is not easy to experiment with and whose conditions require a meteorologist to observe, but at electric discharges in general, that is to say, the laws of momentary rush in conductors when an accumulator or condenser is discharged. These discharges were truly oscillatory ; the electricity surged backwards and forwards ; and the electric oscillations generated waves. I gradually realised that there were periodic things, travelling out on the conducting wires from a discharge, and that these could be observed by reflecting them at the free terminal of a wire, thus converting them into stationary waves with nodes and loops ; which accordingly I proceeded to demonstrate in various ways.

In the same year, 1887, Professor Hertz at Carlsruhe, a brilliant disciple of Helmholtz, was investigating what he called the "Outspreading of Electric Force." He was not then, like me, imbued with Maxwell's doctrine, and was not on the look-out for waves ; but he pursued the subject with unexampled skill, and presently he found that his results could all be understood on Maxwell's principles ; so that he rapidly became an enthusiastic disciple, the first, I think, in Germany. He wrote to me afterwards about the difficulty he had in gaining the acquiescence of the old professors. Hertz published this work early in 1888 ; FitzGerald was going to be President of Section A that year. He pounced on Hertz's work as a subject for his Presidential Address. He was well prepared to understand it and receive it forthwith, for the experiments supported his own theories.

I communicated my results, or some of them, as an outcome and further development of the Mann Lectures, to the same meeting of the B.A. Only the titles are recorded in the volume ; they are as follows : " On the Measurement of the Length of Electromagnetic Waves," " On the Impedance of Conductors to Leyden-jar Discharges." Hertz's papers were translated next year into English by D. E. Jones of Cardiff, and Lord Kelvin suggested as the title of the book " Electric Waves."

I remember at the Bath meeting Lord Kelvin, who had hitherto been hostile to Maxwell's theory, began to be shaken in his opposition to it, and went about with the second volume under his arm, every now and then appealing to FitzGerald to explain a passage. The theory hardly fitted in with his own conceptions, and I should say that he was never an enthusiastic admirer of Maxwell, who was a younger man than he, and had admittedly assimilated much knowledge from his senior. Tait, however, though he seldom came to B.A. meetings, was more personally enthusiastic about Maxwell ; for I remember an utterance of his about the improvements that had been effected in the Cambridge Tripos by the introduction of real physical questions " due to the originality and learning of

Dr. James Clerk Maxwell of Trinity, formerly of Peterhouse."

FitzGerald and Lord Kelvin had, in fact, a sort of battle or impromptu discussion in Section A about the laws of propagation of electric force and the speed at which it moved according to Maxwell; also as to whether some of his symbols were helpful or necessary, or an artificial complication. (See under Leeds, 1890, further on, pp. 134-7.)

It may be well to quote here a pronouncement on the subject from Oliver Heaviside:

"According to Maxwell's theory of electric displacement, disturbances in the electric displacement and magnetic induction are propagated in a non-conducting dielectric after the manner of motions in an incompressible solid. The subject is somewhat obscured in Maxwell's treatise by his equations of propagation containing A, Ψ, J, all of which are functions considerably remote from the vectors which represent the state of the field, viz., the electric and magnetic forces, and by some dubious reasoning concerning Ψ and J. There is, however, no doubt about the statement with which I commenced, as it becomes immediately evident when we ignore the potentials and use E or H instead, the electric or the magnetic force.

"The analogy has been made use of in more ways than one, and can be used in very many ways. The easiest of all is to assume that the magnetic force is the velocity of the medium, magnetic induction the momentum, and so on, as is done by Prof. Lodge (Appendix to 'Modern Views of Electricity'). I have also used this method for private purposes, on account of the facility with which electromagnetic problems may be made elastic-solid problems. I have shown that when impressed electric force acts, it is the curl or rotation of the electric force which is to be considered as the source of the resulting disturbances. Now, on the assumption that the magnetic force is the velocity in the elastic solid, we find that the curl of the impressed electric force is represented simply by impressed mechanical force of the ordinary Newtonian type. This is very convenient.

"But the difficulties in the way of a complete and satisfactory representation of electromagnetic phenomena by an elastic-solid ether are insuperable. Recognising this, Sir W. Thomson has recently brought out a new ether; a rotational ether. It is incompressible, and has no true rigidity, but possesses a quasi-rigidity arising from elastic resistance to absolute rotation."

("Electromagnetic Theory," vol. i, p. 127.)

[See also references in this present book, pp. 84, 85, and 171.]

Report of the Bath Meeting of Section A

I had been asked to report on the doings at the Bath meeting of 1888 by the then editor of "The Electrician," W. H. Snell, and parts of this report may be reproduced here. I began with a reference to my friend G. F. FitzGerald, Professor of Physics at Trinity College, Dublin, who had been chosen to preside over the section devoted to Mathematics and Physics at the Bath meeting of the British Association.

"First, then, the section was happy in having for its president a man exhibiting an altogether exceptional combination of qualities—profound knowledge, great originality, mathematical and experimental skill both of the first order, quickness of comprehension, and a ready wit. The address with which he opened the business of the section was not unworthy of the man. Its subject was that greatest of all physical subjects, 'The Ether,' and it largely consisted in a glorification of the quite recent experiments of Dr. Hertz in Berlin, whereby Prof. FitzGerald shows that the existence of an ether is raised out of the rank of a very probable hypothesis, which it has long been, into the domain of demonstrated facts. He hints also that certain outstanding problems concerning the structure and properties of the ether can most probably be answered by a further discussion and repetition of experiments like those of Hertz. Our engineering friends ought not to sneer at the prominence given to such a refined abstraction or hypothetical figment as the ether may seem to some of them to be, for it is by its means they drive all electric motors, it is it they use in dynamos and transformers of all sorts; it is the ether which transmits their signals, whether telegraphic or telephonic; and, though this may appeal to them less forcibly, it is through the action of the ether that mankind is enabled to see. Not to mention the as yet incompletely verified hypothesis that it is of ether that Engineers and all other material substances are composed.

" *Rowland Grating*

" On Friday morning the serious electrical business of the section began, and Prof. Rowland exhibited his most recent photographs of the solar spectrum, obtained by a yet more nearly perfect concave grating which he had just completed. The photographs exhibited at a former meeting by Prof. Rowland had far eclipsed anything previously accomplished ; but those now shown are still better, and represent an extraordinary stage of perfection. They are by far the most magnificent things of the sort ever seen. The special appliances in Prof. Rowland's laboratory at Baltimore for this work are so perfect and convenient, that the whole solar spectrum can be photographed on an immense scale in the course of a morning. Terrestrial spectra, such as that of iron, can be photographed for purposes of comparison at the same time on the same strips of glass.

" It may be hastily thought that such a subject belongs primarily to optics, but it is not so. Ingenious mechanical devices and optical theorems are made use of in obtaining the result, but the influence of the result is more felt in other sciences. The existence of such a standard of wave-lengths and permanent measureable records of atomic vibration reacts upon several of the sciences. It reacts upon astronomy by furnishing a statement of the present condition of sun and stars, with which future observations may be readily compared; upon chemistry by exhibiting the relations among the vibration numbers of the elements, whereby hypotheses as to the construction and compound nature of our elements may be tested; and it reacts upon electricity by affording us some knowledge of the precise way in which the ether is disturbed by electric oscillations in the substance of the atoms, as well as by the motions of electrically charged atoms themselves.

"*Rayleigh Experiment*

"Lord Rayleigh followed, with an account of an exceedingly delicate and difficult experiment on the question whether an electric current through a liquid influences the velocity of light therein. It is well known that when an electric current flows through an electrolyte an actual transfer of matter accompanies it—two opposite transfers, in fact, as evidenced by the continuous appearance and escape of the travellers, one at each electrode. It is also known by a refined experiment of Fizeau, confirmed by Michelson, that when a beam of light travels down a stream of moving water its speed is slightly increased, whereas if light travels against a stream of water it is slightly retarded. These things being so, it may be held as probable that whenever the two ions taking part in an electrolytic current differ in momentum a slight effect may be exerted on the velocity of light travelling with or against the current. But then, according to the calculations of Kohlrausch, confirmed by some experiments of the present writer,* the speed of the electrolytic ions is extremely small, the quickest being 30 microns per second, or about 4 inches an hour, for an applied slope of potential of 1 volt per centimetre.

"The effect of such a creep as this was not what Lord Rayleigh looked for. It was quite within the range of possibility that the existence of an electric current in an electrolyte could so disturb the ether inside it as to produce quite a notable change in its index of refraction. Were such an effect discovered it would be a distinctly new fact, not taken account of or even rendered probable by existing theories, and it is very well to have the question experimentally examined, and to a certain extent set at rest.

"The method adopted was a beautiful interference arrangement of Michelson, whereby a beam of light is split up into two halves, which are sent along a certain route, or circular tour, in opposite directions, are then recombined into one again at the point whence they originally split off, and are examined by a magnifying eye-piece. The result is a set of interference bands more or less well defined. Tubes containing dilute sulphuric acid supplied with an electric current are then placed along the route taken by the two half beams of light, so that one half the beam will be helped and the other half hindered by the current, if it produce any effect at all. The thing looked for is to see if the

* "Birmingham British Association Report, 1886, p. 393."

interference fringes shift along microscopically when the current is supplied, stopped, or reversed. The result is negative, and by considering carefully how much of an effect could have been certainly perceived if it had existed, the definite statement is made that a current of intensity 1 ampere per square centimetre through dilute sulphuric acid does not affect the velocity of light in its own direction by so much as one part in thirteen millions, or by 15 metres per second.

"There are two ways of experimenting. One is to order expensive and highly polished optical banks and other apparatus from an instrument maker ; the other, to rig up an arrangement with odd bits of glass, cement and portions of telescopes. The born experimentalist adopts the latter plan ; considering beforehand where exactness and steadiness is essential and where it is unnecessary, and accommodating his means to his end with forethought and localised precision. It is by this latter mode of experimenting that the just quoted result has been obtained.

"*Electric Wave Experiments*

"A couple of Papers by the present writer followed in due course. One, 'On the Measurement of Electromagnetic Wave-Lengths,' the other, 'On the Impedance of Conductors to Leyden Jar Discharges.' The point of the first Paper was to bring forward a case of electric resonance whereby pulses rushing to and fro along a couple of long insulated wires of certain length, synchronised with the known oscillations or alternations in a discharging Leyden jar circuit connected to them. The experiment is thus analogous to an acoustic experiment known by the name of Melde, wherein a tuning fork attached to a long stretched silk string throws it into synchronous vibrations when everything is properly adjusted. And just as the length of the sympathetic string is half the wave length of the sound emitted by the vibrating fork, so the length of each wire in the electrical analogue is half the length of the ethereal waves emitted by the discharging Leyden jar : these waves being essentially light, except that they are much too long to affect the retina. If Hertz had not simultaneously been experimenting in very similar directions, the interest of these experiments might have been greater, but as it is they are perhaps mainly interesting as affording a confirmation of the theory of Kirchhoff, Thomson, and Heaviside, regarding the rate of propagation of telegraphic impulses along a wire. These pulses travel at a speed always a trifle under the velocity of light—under certain perfectly known conditions very much under ; but in the most favourable case of two thin parallel wires, transmitting exceedingly rapid oscillatory disturbances (the case of the above described experiment), the velocity of the pulses is, according to theory, exactly the velocity of light ; and so it comes out in the experiment. The length of the waves emitted by the Leyden jar circuit can be calculated, directly one knows the capacity of the jar and the self-induction of its circuit ; the length of the waves sent along the wires can also be observed experimentally ; the two determinations are found to be in close agreement."

The following data may be of interest :

" A microfarad condenser, discharging through a good conducting coil of 1 secohm, gives a current alternating 159 times a second, and emits ether waves 1,200 miles long.

" A gallon Leyden jar, discharging through a stout wire suspended round an ordinary sized room, emits waves between three or four hundred yards in length, its current alternating at the rate of a million per second.

" A pint Leyden jar, sparking through an ordinary pair of discharging tongs, gives a current of 15 million alternations per second, with ether waves some 20 yards in length.

" An ordinary electrostatic charge on a conducting sphere two feet in diameter, if disturbed in any way, will surge to and fro at the rate of 300 million vibrations a second, emitting waves a metre long.

" Electric charges on bodies of atomic dimensions, if able to oscillate at all, would vibrate thousands of billions of times a second, and produce ultra-violet light.

" The maximum amplitudes of current given in these several cases can also be stated if the initial difference of potential be given. For instance, the microfarad case charged to a volt will have a current amplitude of one milliampere ; the pint jar case charged to give an inch spark will give a discharge current of maximum strength 2,500 amperes.

" The second Paper by Prof. Lodge, 'On Impedance of Conductors,' has to do with the obstruction offered by conductors of different sizes and materials to sudden rushes of electricity. A series of experiments on the E.M.F. needed to force the contents of a Leyden jar, under given circumstances, through a wire of measured length and thickness arranged in the form of a circle (a single turn about a yard and a half in diameter being usually employed) are briefly described. The numerical results obtained are then compared with the theory of Clerk Maxwell and Lord Rayleigh, and are found to be in very satisfactory agreement.

"The noteworthy thing about these sudden rushes, or violently alternating currents, is that they keep wholly to the outer surface of a conductor, and accordingly the obstruction they meet with has next to nothing to do with either the specific resistance or the magnetic properties of the material. A number of quantitative determinations have been made which will be published elsewhere.

"The reason why these alternating or rapidly-varying currents keep to the outside of a conductor is merely because the outside affords the easiest path. If they penetrated its substance, as steady currents do, it would have to be magnetised and demagnetised in concentric cylinders by them, and this would cause delay and obstruction. By keeping to the outside they avoid having to do this. It is true that they thereby throttle the conducting area open to them very considerably. They would meet with much less frictional resistance if they permeated the whole sectional area of the wire, but then, for these very rapid variations, frictional resistance is of far less consequence than the inertia-like obstruction of varying magnetisation ; and, on the whole, they find it better to escape the inertia by keeping to the outside, even at the expense of a throttled and highly resisting path. The whole thing is a compromise ; currents of slower rates of alternation will penetrate deeper, always taking the course which reduces total obstruction to a minimum, until we come to steady or slowly-changing currents, to which the inertia effect of magnetisation matters next to nothing, while friction matters just as much to them as to any of the others ; these therefore will flow through the whole cross section of the wire with complete uniformity. Regret was expressed both by Lord Rayleigh and by the President that Mr. Oliver Heaviside, who had done so much in the theory of these matters, was not present to join in the discussion.

"*Lord Kelvin's Papers*

"Next came three Papers by Sir William Thomson, the third of which, as connected with the foregoing subject, it is convenient to take first. Its title is 'Five Applications of Fourier's Law of Diffusion, illustrated by a Diagram of Curves, with Absolute Numerical Values,' and its subject matter is the accentuation of the analogy and complete mathematical equivalence existing between the different kinds of diffusion, so that the method suitable for treating one enables problems in the others to be solved. The best-known cases of diffusion are : (*a*) *The diffusion of substance*, as when a heavy salt solution creeps up from the bottom of a vessel of water until it becomes, in a century or two, thoroughly mixed ; or when a scent diffuses in the air ; (*b*) *the diffusion of momentum*, as when motion is transmitted from one part of a viscous fluid to the rest, this being the process by which tea can be stirred, and by which winds and whirlpools are brought to rest some time after the exciting cause has ceased ; (*c*) *the diffusion of energy*, better known as the conduction of heat, a frequently quoted consequence of its laws being the gradual penetration of an alternating disturbance deep and deeper into the substance of a conductor—as, for instance, the diurnal and annual fluctuation of temperature finding their way down into the crust of the earth, so that at a certain depth one could find evidence of heat waves six months or more behindhand ; (*d*) *the diffusion of electric potential* in the conductor of a submarine cable, worked out long ago by Sir William Thomson as one of the problems to which Atlantic telegraphy gave rise, and by the solution and complete understanding of which rapid submarine telegraphy became practicable ; and (*e*) *the diffusion of electric currents* in the substance of a homogeneous conductor.

" It is this last kind of diffusion which is evidently most interesting Sir William at the present time, and he does justice to Mr. Oliver Heaviside's labours in connection with it."

For a further discussion at this Bath meeting see pages 134-7 below.

Supplementary Remarks on Waves

I will also reproduce the concluding part of my paper published in "The Philosophical Magazine" in that same month, August 1888, though it ought to have appeared in July. It represented work done earlier in the year, before that which was communicated to the Association. All this sounds rather complicated, but in those days months were important. I was thoroughly imbued with the notion of electric waves from the beginning. Hertz was not. But Hertz had splendid opportunities for experiment, in the large premises at Carlsruhe, and, moreover, he had great mathematical facility, besides a strong scientific instinct and quickness of apprehension. Accordingly, when he did begin to assimilate the theory, he carried it farther than I did, and his results were hailed by FitzGerald with enthusiasm as a new and important departure. In that everybody concurred, and when Hertz was invited over to England next season by Professor Ayrton and entertained at the Langham Hotel, he received a cordial reception from English physicists, including myself. We exchanged several friendly letters, and his premature death, soon after, under an operation, was a great blow. He spoke of the difficulty he had in getting his ideas accepted in Germany, where the professors were working under the theory of Weber, Neumann, and others, and did not understand Maxwell.

The discovery of the electron has since that time recalled attention to these old theories, and rather justified their discontinuous treatment, as opposed to the continuity of Maxwell. This is a subject with which the scientific historian will have to deal ; but the time is not ripe for its discussion. It only shows, as so often happens, that there is an element of truth on both sides, and that no one theory is sufficient to cover all the facts.

Hertz and Lodge

With reference to the part played by my experiments, and those made by Hertz, it is instructive to hear what a great expert on the subject said.

On p. 489 of his "Electrical Papers," vol. ii, Oliver Heaviside refers to FitzGerald's address at the Bath meeting and to this paper of mine :

"On this question of waves I take the opportunity of referring to a point mentioned at the Bath meeting by Prof. FitzGerald. That physicist, in directing attention to Hertz's recent experiments, considered that they demonstrated the truth of the propagation of waves in time through the ether ; but that, on the other hand, the waves sent along a circuit did not do so, because they might be explained by action at a distance.

"It seems to me, however, that the more closely we look at the matter the less distinction there is between the two cases, and that to an unbiased mind the experiments of Prof. Lodge, sending waves of short length into a miniature telegraph circuit, with consequent 'resonance' effects, are equally conclusive to those of Hertz on the point named ; in one respect, perhaps, more so, because their theory is simpler, and can be more closely followed."

Here is another passage from Oliver Heaviside, "Electromagnetic Theory," vol. i, p. 398 :

"In one respect, however, a formerly very strange anomaly has been cleared up satisfactorily. When Hertz opened people's eyes and made them see the reality of Maxwell's ether as a medium propagating electromagnetic disturbances at the speed of light, by showing their transmission across a room and reflection by a metallic screen, the full acceptation of Maxwell's theory was considerably hindered for a time by his finding that the speed of waves sent along wires was much less than that of free waves. The discrepancy was a large one, and gave support apparently to the old view regarding the function of wires, which made the wires the primary seat of transmission, and effects outside secondary, due to the wires. And it came to pass that people, whilst admitting the truth of Maxwell's theory, yet made a distinction between waves in free space and 'in wires.' This was thoroughly out of harmony with Maxwell's theory, which makes out that the wires, though of great importance as guides, are nevertheless only secondary. On the other hand, it should be mentioned that Lodge found no such large departure from the speed of light in his experiments. But the matter has been explained by the discovery that an erroneous estimate was made of the permittance of the oscillator in the experiments which apparently showed that the speed was largely reduced. When corrected, there is not left any notable difference between the speed of a free wave and of one guided by wires."

Here is the portion referred to of the "Philosophical Magazine" paper. It was submerged under the title "Theory of Lightning Conductors," which was indeed the subject of the first part of the paper, but it went on to the production and detection of electromagnetic waves, and ran as follows:

Discovery of Waves along Wires

Refer to the diagram on p. 91.

"The jar discharges at A in the ordinary way, and simultaneously a longer spark is observed to pass at B at the far end of two long leads. Or if the B ends of the wire are too far apart to allow of a spark, the wires glow and spit off brushes every time a discharge occurs at A.

"The theory of the effect seems to be that oscillations occur in the A circuit according to equation (3′) with a period

$$T = 2\pi\sqrt{(LS)},$$

where L is the inductance of the A circuit, and S is the capacity of the jar. These oscillations disturb the surrounding medium and send out radiations, of the precise nature of light, whose wave-length is obtainable by multiplying the above period by the velocity of propagation.

"This velocity is known to be

$$v = \frac{1}{(\sqrt{\mu K})};$$

so the wave-length is

$$\lambda = vT = 2\pi \sqrt{\left\{\frac{L}{\mu} \cdot \frac{S}{K}\right\}} \quad . \quad . \quad . \quad (15)$$

"Now $\frac{L}{\mu}$ is the electromagnetic measure of inductance, and $\frac{S}{K}$ the electrostatic measure of capacity. Each of these quantities is of the dimension of a length, and the wave-length of the radiation is 2π times their geometric mean.

"The propagation of these oscillatory disturbances along the wires towards B goes on according to the following laws:—

"Let l_1 be the inductance per unit length of the wires; let s_1 be their capacity, or permittance as Mr. Heaviside calls it, per unit length; and let r_1 be their resistance per unit length.

"Then, for the slope of potential along them, we have

$$-\frac{dV}{dx} = l_1 \frac{dC}{dt} + r_1 C, \quad . \quad . \quad . \quad (16)$$

and for the accumulation of charge, or rise of potential with time,

$$-\frac{dV}{dt}=\frac{1}{s_1}\cdot\frac{dC}{dx} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (17)$$

"These are equations to wave-propagation, and will give stationary waves in finite wires of suitable length, supplied with an alternating impressed E.M.F.

"The solution for a long wire, for the case when r_1 is small and the frequency big,* is

$$V = V_0\, e^{-\frac{m_1 x}{n_1}} \cos n\left(t-\frac{x}{n_1}\right) \quad . \quad . \quad . \quad (18)$$

where

$$m_1=\frac{r_1}{2l_1}, \text{ and } n_1=\frac{1}{\sqrt{(l_1 s_1)}}.$$

The velocity of propagation is therefore n_1, and the wave-length is $\frac{2\pi n_1}{n}$.

"Now, for two parallel wires as in the figure,

$$l_1 = 4\mu \log\frac{b}{a}+\frac{r_1}{n}$$

and

$$s_1=\frac{K}{4\log\frac{b}{a}}$$

while r_1 = the geometric mean between its ordinary value and $\frac{1}{2}\, n\mu_0$;

where the μ and K refer to the space outside the substance of the wires, μ_0 refers to their substance, a is their sectional radius, and b their distance apart.

"* Mr. Heaviside has treated the problem in a much more general manner, see 'Phil. Mag.,' 1888, especially February 1888, p. 146."

"The second term of l_1 is, we have seen, practically zero for these high frequencies. Hence (n_1) the velocity of propagation of condenser-discharges along two parallel wires is simply the velocity of light, the same as in general space ; because $l_1 s_1 = \mu K$.

"The pulses rush along the surface of the wires, with a certain amount of dissipation, and are reflected at the distant ends ; producing the observed recoil kick at B. They continue to oscillate to and fro until damped out of existence by the exponential term in (18). The best effect should be observed when each wire is half a wave-length, or some multiple of half a wave-length, long. The natural period of oscillation in the wires will then agree with the oscillation-period of the discharging circuit, and the two will vibrate in unison, like a string or column of air resounding to a reed.

"Hence we have here a means of determining experimentally the wave-length of a given discharging circuit. Either vary the size of the A circuit, or adjust the length of the B wires, until the recoil spark B is as long as possible. Then measure, and see whether the length of each wire is not equal to
$\pi \sqrt{\left(\frac{L}{\mu} \cdot \frac{S}{K}\right)}$.

"I hope to communicate some numerical results of observations made in this way to the British Association meeting at Bath. [And accordingly I did.]

"It is interesting to see how short it is practically possible to make waves of this kind. A coated pane can be constructed of say two centimetres electrostatic capacity, and, by letting it overflow its edge, a discharge circuit may be provided of only a few centimetres electromagnetic inductance. Under these circumstances the radiated waves will be only some 20 or 30 centimetres long, corresponding to a thousand million alternations per second. [The difficulty was how to detect such waves. I could have calculated their power by FitzGerald's theory, but I did not, and had no idea they would be strong enough to cause sparks. No one had imagined light waves of this strength. So I go on :] Some beautiful diffraction experiments have been described by Lord Rayleigh in a recent Friday evening discourse to the Royal Institution (reprinted in 'Nature,' June 1888), and some of these might be used to concentrate the electromagnetic radiation upon some sensitive detector—possibly one of Mr. Boys's radiomicrometers, more likely some chemical detector—some precipitate or other that can be shaken out of solution by the impact of long waves, or some of Captain Abney's photographic agents.

"Certainly the damping-coefficient R/2L is high, and the radiation has a very infinitesimal duration ; but a rapid succession of discharges can be kept up by connexion with a machine.

"No doubt much shorter waves still may be obtained by discarding the use of any so-called condenser, and by causing the charge in a sphere or cylinder to oscillate to and fro between its ends, as might be done by giving it a succession of sparks. These oscillations, it is to be feared, however, would have too small energy to be detected by ordinary means. [That is a complete mistake : they have lots of energy, for an instant comparable to a millionth of a second.] If they could be made quick enough to affect the retina, no doubt we could detect them with the greatest ease ; but it is manifest that this can only be done by reducing the circuit to a size less than the wave-length of light. The wave-length of the electrical radiation is six times the mean of the inductance and capacity, and each of these quantities is very comparable with the linear dimensions of the conductor concerned. By setting up electric oscillations in a body as small as a molecule, no doubt they would be rapid enough to give ordinary light-waves ; but the probability is that this is precisely what light-waves are.

"Either the atoms are made to vibrate relatively to the æther, by the effect of heat, and so to produce radiation ; or else electrical oscillations are set up in comparatively quiescent atoms, not by heat, but by the impact of radiation from other sources, or by some organic process set in play by living protoplasm.

"It is thus I would seek to explain phosphorescence and other direct production of light from cold sources.

"This direct production of light we have not yet learned artificially to accomplish ; we can only heat bodies and trust to their emitting light in some unknown manner as a secondary result ; but the direct process has been learnt by glowworms and Noctilucæ, and it is for us, I believe, one of the problems of the immediate future.

"University College, Liverpool,
"*July* 7, 1888.

"*Postscript.*—Since writing the above I have seen in the current July number of Wiedemann's *Annalen* an article by Dr. Hertz, wherein he establishes the existence and measures the length of æther waves excited by coil discharges ; converting them into stationary waves, not by reflexion of pulses transmitted along a wire and reflected at its free end, as I have done, but by reflexion of waves in free space at the surface of a conducting wall.

" My friend Mr. Chattock has also written to me about a recent experiment exhibited to the Physical Society [by a Dr. Cook], which shows that the same discharge as can excite æther waves a kilometre long can excite air waves of one millimetre. The whole subject of electrical radiation seems working itself out splendidly.

" CORTINA, TYROL,
" *July* 24, 1888."

SEQUEL TO THE BATH MEETING AND BEGINNINGS OF RADIO TELEGRAPHY

The postal address given just above at the end of my " Philosophical Magazine " paper shows where I was at the time. I had been invited by my literary and philosophic colleague Andrew C. Bradley to join him in a holiday trip to Tyrol, where he proposed to go this year instead of Switzerland. We had a wonderful time and many talks. He tried to explain to me something of the Hegelian philosophy. We went first to Innsbruck, where we stayed a day, thence by train to Toblach, and after a night there walked to Cortina, where we put up at the Stella d'Oro, an inn in the village which has now probably ceased to exist. It was very near the campanile, and the bell at about 4 a.m. intended to wake up the valley certainly woke up me through the open window.

We did the usual tramping excursions, past Misurina See—no hotel there then—and round by Landro. We also drove one day to Pieve di Cadore, associated with Titian, and half thought of going on to Venice. We stayed a week or two at Cortina, and then tramped across a flower-laden land to Caprile, thence by the Marmolada to Botzen, then by rail to Trient, the capital of what was then Italia Irridenta ; thence early in the morning by coach to Madonna di Campiglio, a large hotel hidden in the woods, and not then easily accessible. After staying another week, we were going to tramp north, with mules to carry the baggage ; but after getting up at five, and hanging about in flannels for a long time, no mule was available ; they were wanted for the harvest, and ultimately we had to charter an open carriage. I got a chill, with a pain in the back, so that every jolt of the carriage was painful. The driver managed to negotiate with another one about halfway, so that he himself could return. We didn't carry out our original scheme, but went down the Mendel Pass to Botzen, and there joined the train.

So I got home to the neglected waves, but did not follow them up as I ought to have done. I left them to Hertz mainly ; though I did devise a coherer method of detecting them, so that even the smallest spark, as from a gas-lighter, gave very appreciable results on an electroscope all across the quadrangle. Lord Rayleigh, in congratulating me on this, said, " If you follow that up, there is a life-work in it," and he was quite right ; but I didn't follow it up effectively ; though I did exhibit at a Soirée of the Royal Society in June 1894 a compact Hertz receiver complete, see "Nature," vol. 50, p. 182. I took it up again after the death of Hertz, and after Branly had discovered the filings tube, and in 1894 showed many experiments, many of them new, which in a manner were dedicated to the memory of Hertz.

My friend Alexander Muirhead advocated their extension to telegraphy ; and on this also I made many experiments, demonstrating the possibility at Oxford that same year. That lecture excited a good deal of interest ; but unfortunately it didn't come to the knowledge of Mr. Preece, head of the Telegraph Department, who was interested in efforts at space telegraphy by means of induction and other methods. So that when, two years later, in 1896, Mr. Marconi arrived in England with the same sort of procedure, extended however by the use of a large vertical aerial and earth connexion, Preece considered it something quite new, lent the resources of his staff to its development, went about the country discoursing on it, so that the waves, their generation and modes of detection, began to be known.

It so happened that the British Association met in Liverpool in 1896; Preece came there, and told us in Section A of the remarkable invention which had been brought over from Italy. It was stale news to me and FitzGerald and to Lord Kelvin and to a few others; but whereas we had been satisfied with the knowledge that it could be done, Mr. Marconi went on enthusiastically and persistently till he made it a practical success. He employed a high aerial and an earth connexion. In the achievement of actual telegraphy the earth connexion was an assistance; but in my experiments on the demonstration of the waves I had avoided earth connexion as giving an unfair advantage from the point of view of theory. If a disturbance was detected through the earth, that wasn't the same thing as detecting it through waves in space. But for practical telegraphy any and every method was legitimate; and by this time no one had any serious doubt about the waves.

Hertz had devised a theory as to how they were generated, and had drawn diagrams of the lines of force in different stages of electric oscillation (see my translation in "Nature" for February 21st, 1889, vol. xxxix, p. 402, and especially p. 451). In true waves the electric and the magnetic disturbance are in the same phase at the same place, and wherever this occurs, by Poynting's theorem, the pulse must advance with the speed of light. But close to an electric oscillator the phases of the opposite disturbances differ by a quarter period, like the piston and slide-valve of a steam engine ; and in that case the pulse sometimes advances, sometimes recedes, so that, though there is a strong vibration, there is no true wave advancing steadily in one direction, until you get to a distance of about a quarter wave-length from the oscillator. There the energy breaks off and is lost. When the oscillations are very rapid, and the wave very short, the place where the energy breaks off is fairly close to the oscillator, and accordingly the power of the radiation is very great. When a slow or ordinary oscillator is used, it radiates much less, because the place where the energy breaks off, being a quarter wave-length away, may be at quite a considerable distance. Hence, other things being equal, short-wave radiation is far more intense than long-wave. This, indeed, followed at once from FitzGerald's theory of the loss of energy by an alternator (see p. 54). Every alternating dynamo radiates some energy. But for a frequency of one hundred a second, the wave is two thousand miles long ; consequently the place of breaking off the energy is five hundred miles away, say in the Hebrides.

To get effective radiation in space you don't want a closed circuit. You want a great deal of what engineers call leakage. The electrostatic field must spread out in space, and so must the magnetic. This was achieved unintentionally by the Hertz radiator, which might be regarded as a Leyden-jar circuit with the two coats of the jar separated as far as possible from each other ; so that the discharge " circuit " became linear, and all the lines of the field spread out into space, the electrostatic lines reaching from plate to plate, and the magnetic lines surrounding the discharge wire. In about a quarter period, the two kinds of disturbance have caught each other up, and thereafter proceed with equal energy, at right angles to each other and in the same phase ; so that they can be detected at a distance, either by a straight wire which detects the electric disturbance, or by a coil which detects the magnetic component, the latter method being used to-day in portable sets. Faraday would be pleased to find his lines of force, thus forming closed loops, detached and rushing across the country. But in those days I, for one, had not the smallest idea of general broadcasting. We thought of telegraphy in terms of the Morse code. I thought perhaps that this method of telegraphy might be used for public work, that it would do for newspapers or for political speeches. We thought that speeches by politicians, in or out of the House of Commons, might be thus transmitted to all the newspapers of the country, and that ships at sea might receive tidings of any important event. But I had no idea that in twenty or thirty years the invention would become thoroughly domesticated.

After Marconi had seriously begun the telegraphic application of the waves, I did indeed realise that, as sending stations multiplied, there would have to be a method of discriminating between them ; and further that the principle of resonance would give one much greater sensitiveness than could otherwise be obtained : and so in 1897 I took out a patent for Tuning, which was subsequently adjudged by Lord Parker to be the fundamental one, and was extended on that ground to twenty-one years. I could not patent the general telegraphic use of the waves in this country, for everything had been published. But in America the law was different, and I had only to prove that my 1894 lecture had got into the States to be awarded a fundamental wireless patent antedating Marconi's of 1896. I was advised to oppose the granting of Marconi's first patent, which was certainly weak ; but I am glad we did nothing to increase his difficulties at that early stage.

But still we were dependent on the Spark system of telegraphy, which Muirhead and I, working together, got recorded on the tape of a siphon recorder in a very clear manner, as shown by the journals of the time. The coherer got modified first into a wheel coherer and then into the crystal detector, and the obvious telephone was found after all the best thing to use with it in practice. Marconi extended the system to tuned closed-circuit reception by his 7777 Patent of 1900.

Then came Fleming's perception that the unilateral conductivity of the residual air in a vacuum constituted a much better detector than any coherer or blindfold mineral or crystal device. The electron carriers conveyed a negative current only, so the current was rectified. Electrons had only just been born ; they were almost immediately harnessed. Then when De Forest found that the traffic of electron-carriers could be regulated by an intermediate electrode or grid, placed where it could help or hinder their transit in accordance with the charge it itself received from the anode of a previous valve, the possibility of reinforcing the power to any extent became apparent ; and the electrons were so docile that the magnification could be pushed very far without introducing perceptible distortion. Indeed, by back action, using a local source of energy, the original detector can be reacted upon to such an extent that the whole circuit is almost (sometimes quite) self-generating, so that its effective resistance becomes reduced to nearly zero * ; and thus a very minute trace of energy received from a thousand miles off can be reinforced by local energy, guided and directed accurately by the feeble trace from the distant station, until a loud-speaker can be operated to any desired loudness. The whole became something like an organ, where the energy is generated by an engine applied to the bellows, but where it is guided and directed and converted into music by the touches of the operator on a series of light keys at a distance. In the wireless case the operator is at Daventry or some such station, controlling and sending out energy

* If ever its resistance becomes negative, through too much reaction, it howls.

which gradually spreads out, attenuates, and gets weaker, till at a great distance it is almost infinitesimal. But the ether is so perfect that although thus enfeebled the waves retain their character perfectly; every oscillation travels at the same rate so that the shape of the waves is preserved; wherefore if we have the means of magnification, we can use this residual faint stimulus to stimulate the local energy, so as to reproduce the original vibration precisely; the magnitude only being enormously enhanced, and the quality preserved.

But now came a really surprising and remarkable discovery. The waves did not spread simply out into space as had been thought, but for the most part kept near the earth. This was discovered when Mr Marconi got the letter *S* transmitted from Cornwall to Newfoundland, on a memorable day in December 1901, and thus initiated the possibility of Atlantic telegraphy. Signalling was in due time extended to New Zealand, mainly by the efforts of amateurs working with short waves. The reason for this astonishing limitation of the wave to the neighbourhood of the earth was not by any means obvious. The explanation was suggested by Mr. Oliver Heaviside, as due to an upper conducting ionised layer of the air. Mathematicians tried many other explanations; but this idea of a kind of refracting or reflecting layer in the upper air has survived. Eccles and Larmor are both responsible for it. Waves can penetrate it if they strike it normally, but a vertical aerial radiates horizontally, and the waves mostly strike the layer very obliquely ; so that they get bent round to an extent roughly corresponding with the curvature of the earth when the waves are of suitable length. Other reflecting material has recently been found in space, far beyond the confines of the earth's atmosphere, so that waves can now be detected returning or echoed back after having travelled away from the earth more than a million miles. We have here an additional means of perceiving what goes on in so-called empty space, to which we can attach no limit. The amount of information already obtained by examining the radiation from distant stars and nebulæ far exceeds anything that could have been anticipated. Indeed, radiation is the chief newsagency in the universe, and tends to weld it all together into an intelligible whole. Modern astronomers have speculated extensively about the consequences of Einstein's theory of the connexion between light and gravitation ; one of the suggestions being that the universe is finite, and another, that it is not only finite, but expanding, like a bubble of gas liberated in a perfect vacuum. Another view is that it is contracting, or else alternately expanding and contracting, in other words pulsating. I express no opinion on these apparently wild surmises, which, however, lend themselves to mathematical calculation. But I do think it a great achievement to have been able for the first time in the history of the world to produce and detect radiation, not by a blindfold method of mere heating, but with a real and intimate knowledge of what we are doing. The whole suggestion or discovery that etheric waves are in their nature essentially electro-magnetic is to be traced back to the middle of the nineteenth century and to the genius of Clerk Maxwell.

CHAPTER VI

DISCUSSIONS AND NATIONAL PHYSICAL LABORATORY

NEWCASTLE, 1889

Of the meeting of 1889 in Newcastle my recollections are meagre. I was staying at the time at Tynemouth, and was rather ill. It was a big meeting, at which Flower was President, with Abney at Section A.

Sir Richard Glazebrook and I gave an account of our determination of "v," the velocity of electromagnetic radiation (now often called "c,") by photographing the spark from an electrostatically measured condenser discharged through a coil of measured self-induction, and receiving the image of the spark on a sensitive plate revolving at a measured rapid speed in its own plane. The plates were kindly supplied by Sir Joseph Swan for the purpose, and the beaded bands into which the sparks were drawn out were measured micrometrically (many of them by Mr. J. W. Capstick) with a special graduated circle provided for the purpose, holding a travelling microscope. The result was to confirm with great accuracy the theory of those oscillations as originally promulgated by Lord Kelvin in

1853, and to verify incidentally the assumption that ether waves some miles in length travelled through space at the same speed as light. The paper, which is quantitative and rather elaborate, was published in the "Stokes Memorial Volume" of 1899, issued by the Cambridge Philosophical Society, and the C.U. Press. It is reported on (along with other papers) in "The Electrician" for October 4th, 1889, vol. xxiii, p. 544.

Heaviside's term "impedance" was adopted by the Electrical Standards Committee in addition to the dissipative term "resistance," from which it differs enormously in the case of rapidly alternating currents.

Sir Arthur Schuster had much to say about Crookes's Cathode rays and their magnetic deflexion, and he made measurements of their speed in that way.

Leeds, 1890

The meeting of 1890 was held in Leeds. This meeting was chiefly remarkable in my memory for great controversies in the Chemical Section, some of them within the section room, some outside, on Ionisation. They continued into a garden party on the Sunday. Professor Ostwald and G. F. FitzGerald took a leading part in these discussions. Arrhenius and Van't Hoff were there too; and so I think was Henry E. Armstrong, and certainly Professor Smithells. The last will have a more vivid recollection of these live discussions than I have myself. Professor Thorpe, the President of Section B, was going to report the discussion, but I do not find that he did.

At Leeds Professor Poulton gave one of the Evening Lectures on "Mimicry," illustrated by innumerable slides of butterflies. And Vernon Boys gave the other, on "Quartz Fibres and their Applications," in which he took occasion to refer in an interesting and instructive manner to the habits and peculiarities of spiders. It is unnecessary to emphasise the importance of the quartz fibre, as thus introduced and manufactured of excessive fineness by the ingenuity and great experimental skill of Vernon Boys.

The Leeds meeting of Section A was favoured by the climax of Professor Ewing's long study of magnetic hysteresis; for it was here that he brought his molecular magnetic model, consisting of a flock of pivoted compass needles, which to everyone's surprise (including, I think, his own), went through all the contortions necessary to illustrate, and in a manner explain, the three main stages of a hysteresis curve. The manner in which the compass needles behaved, first deflecting and then toppling over, as the applied magnetic force gradually increased was very interesting. Their mutual action sufficed to hold them in a more or less saturated position until they were released.

In my record of the Bath meeting two years previously I have omitted to refer to a remarkable discussion, largely between Kelvin and FitzGerald and Rowland, in which others joined, about the propagation of electrostatic potential. It arose out of the work of Hertz, and the discussion turned upon Maxwell's theory. When a body is suddenly charged, how does its electrostatic potential reach a distant electroscope? Is the force felt instantaneously, or does it travel through space? In a report of the Bath meeting I made for "The Electrician" for September 21st, 1888, vol. xxi, I refer to the discussion thus:

"It is not often that the public are admitted to the genesis of scientific ideas, or are allowed to witness a development of opinion going on before them. But in the presence of Sir William Thomson this is no new thing. . . .

"The main point under discussion relates to the mode and rate of propagation of electrostatic potential as compared with electromagnetic potential. Electromagnetic disturbances correspond to shearing strains in an elastic solid; they travel through the ether with the velocity of light—the lines of magnetic force spreading broadside on—and about them there is no difficulty. Difficulties begin with the propagation of electrostatic displacements, which correspond to the disturbances possible in an ordinary fluid, longitudinal not transverse disturbances, having to do with incompressibility instead of rigidity, disturbances which in ordinary matter we call *sound*. . . .

"This electrostatic potential is denoted in Maxwell's equations by the symbol Ψ, and so the attempt to get rid of it, and do without it in the general equations of an electromagnetic field, was referred to by the President as the 'murder of Ψ.' Many deaths, and many bringings to life, this unfortunate symbol suffered in the course of the week, and I would not like to say that its fate is even yet decided. On the last day of the meeting Rowland and FitzGerald seemed to come to an understanding, but whether Sir William will coincide with it, or will upset the whole thing once more, remains to be seen. As they put it the matter is now like this: The propagation of electrostatic potential does not go on by end thrust at all; it is not really analogous to a pulse of longitudinal compression, though it is apparently and superficially so; and accordingly its rate of propagation depends not at all on the compressibility or incompressibility of the ether, a question on which it has nothing to say one way or the other. An electrostatic field is not developed *sui generis*, but is always the consequence of a previously existing electromagnetic one, which, on subsiding, leaves it as its permanent record. . . .

"Generating it in this way, all distinction between rate of propagation of electrostatic and electromagnetic potential vanishes, they both travel together with the velocity of light; or rather, the thing which travels is the magnetic potential, and its permanent effect *in situ* is the electrostatic potential.

"Thus, once more, the difficulty of a longitudinal or pressural wave disappears from the electrical theory of light, into which it had seemed to intrude itself, and Ψ is left to enjoy 'a long and useful career,' though it is not permitted an infinite or any other rate of propagation in its own proper nature. . . . If it be asked at what rate electrostatic potential travels, the answer is that it does not travel, but is generated *in situ* by the subsidence of a magnetic potential which travels with the velocity of light." (Refer back to p. 99.)

There the matter rested, but now at Leeds in 1890 Professor FitzGerald revives and closes the subject by a paper entitled " An Episode in the Life of J." And this I summarised in " The Electrician " for September 26th, 1890, vol. xxv, as follows :

" As already known, Maxwell's equations appear to leave the question of the propagation of electrostatic potential vague, as something depending on ethereal volume elasticity [or incompressibility] a quantity about which nothing definite is at present known. Maxwell seemed to suppose it infinite ; Thomson has recently suggested that it may be zero ; while Helmholtz provided opportunity for everything by calling it k, and making the propagation of electrostatic potential explicitly depend upon it.

" FitzGerald and others have argued, however, that electrostatic potential is not propagated *per se* at all, but that it is the consequence and residue of a subsident electro-magnetic field ; and hence, if it can be said to be a travelling entity or to have any velocity of propagation, it must be said to travel at the same rate as an electro-magnetic disturbance, viz. with the velocity of light ; but that the best mode of regarding the matter is not to attend explicitly to the electrostatic potential (Maxwell's function Ψ) until everything has settled down and become static. That electrostatic ideas, in fact, are not appropriate to the conception of kinetic phenomena. The discussers at Bath were led to perceive that Maxwell's occasionally introduced function J, which is connected with Ψ in a definite manner, was much more convenient, and should be explicitly employed in preference to it. FitzGerald is now able to point out the reason why Maxwell dropped J at an early stage in considering periodic disturbances, and to put his finger on the mistake, viz., that the linear relation for it $\frac{d^2J}{dt^2} = 0$ is erroneous. and that the true relation is $\mu K\frac{d^2J}{dt^2} = \nabla^2 J$, which at once gives a wave propagation travelling with the velocity of light, and eliminates from the subject every question connected with the compressibility of the ether and Helmholtz's constant k. Hertz has virtually independently revived the J function under the name Π, and has shown that it is most useful in working out his oscillator results."

With this conclusion Sir William Thomson expressed practical agreement. The question is still asked by intelligent students, so I hope that this résumé may be a help to them.

Cardiff, 1891

The year 1891 was memorable as far as I was concerned by my being President of Section A, while Huggins presided over the whole meeting.

Dr. Johnstone Stoney gave a paper "On the Cause of Double Lines in Spectra," in which he dealt with the elliptical revolution of electrons inside an atom and the apsidal motion of their orbits ; a subject so greatly developed afterward by Professor Bohr.

In my own Presidential Address I advocated at some length the establishment of a National Physical Laboratory. Incidentally I referred to the recent discovery by spectroscopic means of a rapidly revolving twin-star, Beta-Aurigæ, by Professor Pickering and the staff connected with the Draper Memorial ; the components revolve round each other in four days, but are not visible in any telescope. I referred also to the twinning of chlorate of potash crystals investigated by the late Lord Rayleigh, and the possible pure spectrum which might be got out of a series of regularly spaced faintly reflecting surfaces ; a method which was used long afterwards with great skill by Moseley for the spectra of X-rays, and ultimately for counting the electrons in a number of elementary atoms.

The connexion between life and matter had begun to attract my attention, for even at that date I find this sentence in my Address to the section.

" A vulnerable spot on our side seems to be the connection between life and energy. The conservation of energy has been so long established as to have become a commonplace. The relation of life to energy is not understood. Life is not energy, and the death of an animal affects the amount of energy no whit ; yet a live animal exerts control over energy which a dead one cannot. Life is a guiding or directing principle, disturbing to the physical world but not yet given a place in the scheme of physics."

And then, after discussing the nature of " time," I felt bound to refer to obscure psychic phenomena not recognised by the Association ; which I did in a careful and inoffensive manner, though probably some people did take offence at it.

I see that Sir William Huggins in his address referred also, more authoritatively, to the discovery of Beta-Aurigæ, since it was an application of a determination of velocity in the line of sight, a kind of measurement in which Huggins himself was a pioneer. His address is mainly concerned with spectroscopy and its application to astronomy, and to the constitution of the stars ; with some speculation as to their classification and different ages, and as to the temperature of their atmosphere at different depths. The idea of there being both young and old red stars, one on the way up, the other past the maximum a good way down, was already in the ascendant ; and he refers to a view of Johnstone Stoney's so long ago as 1867.

There is also a paper by Professor H. A. Newton published in full, " On the Capture of Comets by Planets " (see pp. 511–532), a theory which I understand is now regarded as established. I extract the following from a report I made at the time for " The Electrician " :

"He has been mathematically investigating the action of large planets, especially of Jupiter, on these small bodies as they pass within its range of attraction. A certain percentage are diverted from their natural parabolic orbit round the sun and swung into an elliptic orbit so that they become permanent members of the solar system, always thereafter revolving round the sun and receding from him to about the distance of Jupiter. A few, however, are affected still more seriously ; they go so near Jupiter as to be caught and checked. Their prodigious rush, of a dozen or so miles a second, is taken out of them, and they are left quiescent in space ; so that there is no help for them, they simply drop into the sun. All large planets thus feed the sun with meteors, but the number thus struck dead and dropped is not great ; and only the larger planets are able to do it. The earth, for instance, is not able. By the time a meteorite reaches the earth it is flying with a speed of 26 miles a second, and in order that the earth may be able to wipe out this velocity by gravitative attraction, its centre must approach the meteor as close as 3,000 miles. Now it is impossible for the earth to get as near a body as this, for its own radius is 4,000 miles, hence the earth cannot act in the way just described. It may perturb a few, and cause them to move in elliptic orbits, but its action is insignificant when compared with that of the giant planets Jupiter, Saturn, Uranus, and Neptune. All these planets have been the means of introducing a number of comets or meteoric streams into the solar system, which thereafter circle in elliptic orbits whose greatest distance (or aphelion) is usually something not very different from the distance of the planet which introduced them, though in a few cases it may be less.

"Prof. George Forbes mentioned that he had already called attention to the fact that a number of cometary orbits ended at about the distance of a planet, and had shown that the position of Neptune could have been roughly estimated by this very method. He has also predicted the existence of two trans-Neptunian planets, by considering the aphelion of other cometary orbits, and has endeavoured to calculate the present position of one of those hypothetical planets next beyond Neptune. Since its distance is assumed to be about double Neptune, it must be an excessively faint object, but modern processes of stellar photography ought to be able to detect it."

And here is another extract, referring to this same Cardiff meeting :

"Mr. W. Barlow showed some remarkably beautiful models of coloured beads, strung so as to imitate the arrangement of the atoms in various crystalline minerals. The physical characteristics of particular crystals were shown to be brought out by the atomic grouping necessitated by simple principles of symmetry, regarding each atom as a mere centre of force. Thus zinc blende showed its structural appearance. Quartz exhibited a spiral structure, which might be either right or left handed, and which was known to be competent to explain its rotation of the plane of polarisation of light. Iceland spar had a complex structure, which had a twin or image of itself in a mirror, into which it could be forced over by appropriate displacement. It appears probable that, by long study of these matters, Mr. Barlow has been able to penetrate pretty far into the secrets of crystalline form and arrangement."

This may be regarded as a prelude to the more complete researches for which the Braggs are now famous.

At Cardiff the Marquis of Bute was very friendly to the Association, and invited all the Sectional Presidents to a gorgeous banquet, on gold plates, in Cardiff Castle. Subsequently I had many talks with him, walking round the grounds of the Castle, so that we were both late for our appointments afterwards.

CHAPTER VII

OLD SECTIONAL TRADITIONS

It strikes me that it would be worth while letting the present generation know about the methods adopted in the sections, certainly in Section A, forty or fifty years ago, so that they may be compared with the procedure now. There has been a great advance in organisation; everything is now prearranged and carried out in a clockwork manner; whereas previously it was rather haphazard and living from hand to mouth. Superficially the modern method is an obvious improvement; but deep down, and considering the objects of the meeting, I feel that the old plan had some advantages.

When I took on the Secretaryship of Section A, which I held for seven or eight years, I received the traditions from my predecessor, J. W. L. Glaisher, in 1878, and I tried to pass them on to my brother Alfred when he succeeded me as Secretary.

The colloguing of the secretaries of the different sections, by grouping them in a Secretarium during the meeting, was only begun in my time. The organising committee only met on the first day, and then handed over its labours to the Sectional Committee. There was no intersectional discussion, and indeed no set or organised discussions were arranged, which seems a pity ; but those set discussions are very apt to be not discussions, but a series of papers, each writer dealing with his own branch of the subject ; so that a discussion on the first paper is almost impossible by the time the last one has been read ; and indeed there is usually very little discussion at all. In the old days there were sometimes real discussions, especially when Sir William Thomson or FitzGerald was present. They nearly always had something of value to say, or some hare to start, or they spotted a point of interest which had escaped others, and which drew further remarks from the members present, as well as from the author of the paper.

Moreover, the papers as a whole seemed more intelligible to the general public than they are now ; so that sometimes a local man was allowed to air his knowledge or contribute some item of information. This, of course, left it open for the crank to disport himself, and occasionally he was difficult to repress, but that didn't happen often, and the risk was worth running. The worst kind of crank was not found among the local inhabitants, but in one who attended the meetings year after year, and was anxious to display the bee in his bonnet. But a man of that stamp very soon became known to the officers, and precautions could be taken against him. A man from the locality who wanted to air his opinions ought to be encouraged, within moderation ; for surely part of the object of the meeting is to arouse scientific interest in the localities. And as colleges were not so numerous then, any private worker or thinker had very little other opportunity of ventilating his views.

The papers, though roughly sifted beforehand, were not so stringently scrutinised as they must be now. Papers of no value were sometimes admitted, perhaps inadvertently : and occasionally it happened that some doubtful papers turned up trumps. Notably this was the case about eighty years ago, when a Mr. Joule of Manchester introduced heterodox views about the nature of heat, and described experiments which ran contrary to the accepted Carnot theory of the time ; since he made out that heat was liable to go out of existence and to reappear in other forms, the amount of work done by an engine being the exact equivalent of the heat destroyed, as far as he could judge by experiment. Fortunately, Professor William Thomson was present, and, although full of the orthodox theory, he spotted the merit of the experiments, and entered into conversation with the young reader, whose similar work had been already turned down by the Royal Society. The result of the ultimate acceptance of Joule's work was an alteration of ideas about the nature of heat, a recognition of it as one of the forms which energy took, and an establishment of the first law of thermodynamics. Only by reading Carnot now is it possible to gain some idea of the difficulty of apprehending this apparently simple fact. I think this discussion must have happened at Ipswich in 1851, for a year later we find W. Thomson and J. P. Joule working together at the flow of gases through small orifices, a procedure which now has great applications in the liquefaction of air and all gases.

This is only an example to emphasise what I mean about the difficulty of always secretarially assessing the value of an experimental communication. The very novelty of a result which makes it important also makes it difficult to apprehend and fit into the scheme of organised knowledge. To return to the procedure.

The Organising Committee of the sections sat at 10 a.m. on the morning of the first Wednesday, and continued for several hours. It consisted only of the secretaries and of such past Presidents as choose to attend. The papers were reviewed and roughly mapped out as to the days when they could be heard. It then reported to the Sectional Committee meeting the same afternoon. The plan was to arrange that on Thursday, after the President's address, there should be taken a few other papers of a dignified character, about which there could be no doubt, and some from distinguished foreigners. Before the Sectional Committee was over, the members had begun to arrive in the town, and some of them had fresh papers in their pockets to communicate. Papers which seemed likely to be of novelty or to be in the line of advance of the physics of the day were taken on Friday, a day devoted to pure physics. Mathematical papers, that is, pure mathematics, were arranged for Saturday. Meteorology and astronomy were generally given Monday to themselves. Tuesday was devoted again to pure physics, and contained the overflow of the previous days. The section sat again on Wednesday morning, and made a clearance of the rest of the papers. A rough classification of this sort was all that was attempted, and each morning the Sectional Committee sat at 10 a.m. and decided the list of papers for the next day. Then the secretaries had to transmit that list to the printers, later in the evening to correct the proof, and have it all out next morning, where it appeared in the local Press, and also on fly-sheets which could be obtained at the reception room and on the breakfast tables of the more knowing of the citizens. Each day the Sectional Committee sat again at ten o'clock, and arranged the papers for the following day, and also began the discussion of the appointment or reappointment of committees.

The section itself began its meetings at eleven. The President's address began the proceedings on the first day, and it was customary to invite distinguished foreigners and any leaders who were present to occupy seats on the platform and support the President, instead of leaving him solitary with only a secretary and a glass of water. All the secretaries sat at the table throughout the meeting. One of them attended to the blackboard, while another had the duty of writing up in the entrance the paper which was going on, so that visitors to the section could decide whether they should enter or not. Sometimes it happened that the section was fairly deserted owing to some attraction elsewhere ; for the townspeople migrated from one section to another, anxious to see the celebrities and to pick out any particular person they wanted to hear.

After a paper was read, a discussion was invited, and if it showed signs of life and interest, was allowed to continue, without much regard to the clock ; there was no time-table, and the next paper was called on when the discussion ceased. Occasionally these discussions lasted quite a long time, and they were sometimes the best part of the meeting. Lord Kelvin in particular had plenty to say, felt that he had time available, and was occasionally discursive ; but most of the audience were delighted, and in this informal manner knowledge was advanced.

All this threw a lot of labour on the secretaries. They had no time for entertainments or garden parties, or anything of that sort ; they had to work, sometimes till late at night, when they visited the printing office and corrected the proofs. To an active secretary a meeting was a very serious thing, but though laborious his activity was rewarded by the liveliness of the meeting and the value of the impromptu discussions. That seemed to be the chief object of a meeting. It was not an opportunity for a series of formal papers, such as might equally well be given at one of the London societies, but it was a method of extracting conversation from the great men present, and of enabling foreigners to have their say and to contribute their point of view, sometimes in a foreign language.

Intelligible or not, the townspeople liked to see the great men whose names they knew and to have a chance of hearing them. Moreover, the Presidents of the section, or perhaps one of the secretaries, took it upon himself to remember the lay audience who were attracted to the meeting, and to give some idea of what the paper was about in ordinary language which could be followed ; and sometimes the President summed up the discussion at the end.

The work was hard, but it seemed worth while. We lived from hand to mouth, much more than we do now, and, at first by messenger boy and afterwards by telephone, some intercommunion was kept up between the sections ; so that the papers in other sections could be written up on a board for the benefit of visitors, and for the information of such members as wanted to go elsewhere. It must be admitted that the papers were not so numerous as they are now ; but there seemed really more migratory intercommunion among the sections than is probable now. A famous man like Professor Huxley always attracted a big audience ; for the general public wisely concentrated more on the personalities of the speakers than on what they happened to be talking about. A lantern was seldom used, for there was no electric light, and so we seldom had to sit in the dark. A small well-illuminated screen was sometimes used, which could be seen by daylight sufficiently without much darkening of the room ; but as a rule diagrams were much in evidence, and one of the secretaries was busy pinning up to the diagram-frame illustrations which each speaker brought, while two or three blackboards were continually being filled. Diagrams are much more trouble to produce than lantern slides, but they have the advantage of being permanent, and they are not replaced by another before their full meaning is understood. People then took the trouble to prepare good diagrams beforehand ; and often during the course of a meeting a fresh paper not on the preliminary programme was introduced and set down for a future day. There was a rule that papers had to be sent in before the meeting, but it was frequently broken when a paper was forthcoming from one of the leaders. There was an elasticity about the proceedings which had both advantages and disadvantages. There were no meetings or organised committees in Burlington House beforehand ; everything had to be done in the week of the meeting, and, as I say, it was a strenuous time.

CHAPTER VIII

THE RED LION CLUB, ARGON, AND THE BEGINNINGS OF WIRELESS

Edinburgh, 1892

In 1892 the Association met again in Edinburgh, this time under the genial Presidency of Sir Archibald Geikie, the geologist. Arthur Schuster presided over the Physical Section; and Michelson, in a paper "On Interference Methods applied to Spectroscopy," mooted his idea of the varying visibility of the interference bands, which has resulted in such splendid results when applied to determine the actual diameter of stars. His paper, with many plates, will be found on pp. 170–185 of the annual volume.

A committee had now been appointed to consider the establishment of a National Physical Laboratory, as an outcome of the Cardiff meeting, and there was a formal discussion on the subject, opened by myself. Several papers of mine are included in the report, but need not be specially referred to.

Nottingham, 1893

In Nottingham in 1893 the President was Burdon

Sanderson, while Glazebrook presided over Section A. Smithells gave a lecture on Flame, and Victor Horsley one on the Physiology of the Nervous System. I had known him as a brilliant medical student at University College.

With reference to inter-sectional procedure, and the regrettable bifurcation of the sciences, one of the causes which renders intercommunion difficult, and tends to separate the sciences into detached A and B groups, is the technical language and queer terms used I suppose by us all, and needlessly so at Meetings of the British Association. I would call attention to a long letter of mine written after the Nottingham Meeting of 1893, which appears in "Nature," vol. 48, pp. 564–6. In it I refer to the peculiar technical language used by biologists ; so that whereas, while wondering what is the precise mechanism accompanying a process, a physicist like Clerk Maxwell could ask " What is the particular go of it ? " or more explicitly " What makes the white corpuscles attack and kill the disease germs ? " a physiologist like Burdon Sanderson might be represented as asking (oratorically) " What supplies the chemiotactic stimulus impelling the leucocytes towards the morbific microbes which they are devitalising ? "

Professor Burdon Sanderson was good enough to understand and answer this letter genially on p. 613 of the same volume of " Nature."

Oxford, 1894

In 1894 the Association met in Oxford under the Presidency of Lord Salisbury. It was a great and important meeting, and I have much to say about it. But the portion relating to myself is perhaps best treated in my Autobiography. Suffice it to say here that the possibility of wireless telegraphy was experimentally foreshadowed before a large and distinguished audience in the lecture-theatre of the Museum.

At the Oxford meeting the Red Lion Club, which has not hitherto been mentioned, but which had been having its humorous orgy each year, was taken in hand by H. B. Dixon and Chandler Roberts, and made rather a feature. A large gathering assembled ; and from my supposed likeness to Lord Salisbury at that time, I was selected as Lion King. I got my brother Richard to concoct for me a suitable Latin oration, which I learned and declaimed from the top of a pair of steps representing the Chancellor's seat at which, during the inaugural meeting in the Sheldonian Theatre, Lord Salisbury had sat before he was inducted to the Chair. Then I was marched up the hall in a ridiculous gown by two bedels carrying literal pokers (and tongs), and so took my place at the High Table. After dinner I made a parody of Lord Salisbury's address, which was well received, though some of it was perhaps in questionable taste. As Rücker said afterwards, however, the proceedings altogether were of such a length that it was just as well to have them diversified by breadth.

A good deal has been written about the Red Lion Club in one form or another. At one time it was remarkably good, but in my view its best days are over. The war may have been responsible, but I think it had begun to fail before that. It was the custom to parody not only the President's address, but one or other of the most popular of the Evening Lectures. They were the only things that were attended by the whole Association : the sectional proceedings, though they might occasionally lend themselves to parody, would not have been understood by the majority of the audience ; but the others were public property. The fun was good-natured enough, and comic experiments were performed, recalling and, of course, guying the actual experiments which had been sometimes brilliantly shown at the real lecture. A great deal depended on the personality of the two members of the Association who called themselves " jackals," and saw to the details of the feast, whether of food or of fun. In later years they were not always competent, these jackals, and the fun had to be rather forced. Perhaps it was that we were all getting older. Anyhow, these remarks must be taken as no more than my recollections. Still older members of the Association than myself would remember what were considered the palmy days of the Red Lion Club, when Professor Rankine was in great form and sang a number of his own jovial songs on the occasion, his appearance being rather " Jove-ial " too.

Lord Salisbury's address is memorable for a rather sideways attack on Darwinism, which I fancy roused Thomas Henry Huxley to silent fury, and also for a characteristically ingenious gibe at the Ether, that it was invented in order to yield a " nominative case to the verb to undulate." It was that certainly ; but a great deal more was known about the ether, from the electromagnetic point of view, than Lord Salisbury was aware of : and though it is out of fashion just now, I predict that it will come into its own again, perhaps while I am still on the planet—though things hardly move as fast as that, even in these days. At the conclusion of the address Professor Huxley was called upon to move a vote of thanks. His venerable figure, standing there in a place of many memories, was the signal for a spontaneous ovation which must have given him pleasure. His speech was dignified and worthy of the occasion ; and, as far as I know, this was the last public appearance of the great biologist.

The Evening Lectures that year were by J. W. Gregory on "African Exploration," and by Professor Shield Nicholson on " Historical Progress and Ideal Socialism."

Transactions of Section A at Oxford

A discussion on Flight was opened by Hiram S. Maxim, whose method of preliminary flying at that time took the form of a sort of locomotive running on rails, with an inclined plane which at a certain speed raised it from the ground by reaction against the air ; thus bringing its guiding wheels into contact with a pair of upper rails, which were intended to prevent the flight from becoming extensive and dangerous ! Passengers in this curiously lumbersome vehicle seem to have found the sensation of rising even a few inches from the ground exhilarating. The late Lord Rayleigh (who had worked mathematically at the elevating power of an air-plane, an essential feature in all flying mechanism) went on one of these trips, but was humorously urged not to risk his valuable life as ballast, on so dangerous an expedition, but to send a copy of his Works, which would be sufficiently heavy for the purpose ! The remarkable progress made in the art of flying since then needs no comment.

Beautiful photographs of spiral nebulæ were shown by Dr. Isaac Roberts, the spiral character of the Andromeda nebula having been thus recently discovered ; and in his contribution the author claims :

(1) That the existence of spiral nebulæ is a physical reality ;

(2) That the convolutions of the spirals proceed symmetrically from a centre. This indeed is completely in accord with the powerful mathematical investigations of Dr. J. H. Jeans on the behaviour of an immense rotating mass of gas, and the consequent evolution of stellar systems.

One fact which makes the 1894 Oxford meeting of the British Association memorable is that during that meeting the announcement of Lord Rayleigh's discovery of argon was made to the Chemical Section. The detection of a new gas as a constituent of the atmosphere was announced by Rayleigh and Ramsay, and was received with a good deal of scepticism. Some chemists doubted the discovery of a new gas altogether; while others denied that it was present in the atmosphere. One distinguished chemist complained of the properties of the new element, implying that as it had no chemical activity, but was entirely inert, it hardly deserved to be considered a chemical substance at all. This led Lord Rayleigh humorously to apologise for its sluggish behaviour. But the statement that the air contained nearly 1 per cent. of a non-oxidisable gas, heavier than nitrogen in the proportion 20 : 7, was quite true, and has been abundantly confirmed since. It was the first of the inert gases, of which Ramsay proceeded to discover the whole family; and argon became of fundamental importance as representing the first discovered member of a wholly overlooked column or keynote series in the classification of the elements. All the members of this series possess a compact and self-satisfied constitution, which has now been elaborated by the notable genius of Bohr.

I have been asked to refer specially to the communication which I made, to a joint sectional meeting, in illustration of Clerk Maxwell's theory and the production and utilisation of Hertzian waves, which is often referred to as the beginning of radio telegraphy. I have referred to this before, but the subject may be of sufficient importance to justify reference to some other points connected with it. Certainly the demonstration excited great interest, and was made before a crowded audience in the large lecture-theatre of the Oxford Museum. Among those present was the great mathematical physicist Ludwig Boltzman, then Professor in an Austrian University. The meeting was a joint meeting of several sections to discuss a theory of vision. We were honoured by the presence of physiologists, including Dr. Burdon Sanderson, and an extract may be made from a report of the meeting by some writer unknown to me, taken from "The Electrician" of date August 17th, 1894, vol. xxxiii, p. 458:

"The climax of electrical interest was undoubtedly reached on Tuesday, when a joint discussion on theories of vision, by the physical and physiological sections, was inaugurated by some brilliant experiments and two memorable Papers by Prof. Lodge. The large lecture-theatre of the museum was crowded to overflowing, among the audience there being a large number of distinguished physicists and physiologists. The proceedings commenced with a Paper by Prof. Lodge in connection with some 'Experiments illustrating Clerk Maxwell's Theory of Light,' in which the quasi-optical properties of electromagnetic radiation were very beautifully, very carefully, and very convincingly demonstrated. This Paper was really the prologue to the Paper on 'An Electrical Theory of Vision,' which immediately followed, and its direct purpose was to initiate the physiologists into the researches of Hertz, Lodge, and others on this quasi-optical behaviour of electromagnetic waves. Prof. Lodge, in his second Paper, disclaimed the intention of advancing a 'theory' of vision, and urged that his communication was rather in the nature of a question put to physiologists as to whether it is possible that anything resembling the action of electro-magnetic waves on a coherer could be supposed to take place in the vertebrate eye. It was difficult to know which to admire most, whether the carefully logical manner in which Prof. Lodge arranged the facts, or the modesty with which he advanced his hypothetical deductions. The gist of Prof. Lodge's hypothesis is, that a simple electric circuit consisting of a coherer contact and an otherwise closed circuit—with or without a seat of electromotive force—may be taken as an analogue, and may, *ex hypothesi,* be an enlarged model of the mechanism of vision. Just as a galvanometer in the coherer circuit will, if there is an electromotive force, be affected by electromagnetic stimuli acting on the coherer, so the mechanism of vision may be supposed to be affected by the stimuli of optical rays. Each element of the retina—each rod and each cone—must, on this assumption, be taken as consisting of a coherer circuit which can transmit its individual impulse to the brain. The author then showed that there were two positive but opposite stimuli required, if the analogy was to be made complete. Not only was an optical stimulus necessary in order to enable the eye to perceive the element of light which falls on it ; but a cancelling stimulus from within the organism was required in order to enable the eye *not* to see the visual impression when the luminous source was removed from its gaze. A coherer circuit once affected by an electromagnetic wave has a tendency to persist in its lessened resistance and increased current ; and it requires some mechanical stimulus to jerk the coherer contact back to its normal state of badness. Prof. Lodge showed by an experiment that it was quite possible to limit the 'persistence of vision' of the coherer circuit by arranging that the coherer should be mechanically tapped in rapid succession. The conclusion of Prof. Lodge's Paper was received with hearty and prolonged applause. In the discussion which followed, Lord Rayleigh, after congratulating Prof. Lodge on the ingenuity of his hypothesis, made the remark to the Physiological Section, that physicists could object neither to the Young-Helmholtz theory nor to that of Henry, provided that three *independent* variables in the system of equations were granted. There might be six variables in all, three dependent and three independent ; but whatever hypothesis might be advanced, no explanation of vision could be recognised which did not postulate these three independent variables.

"Prof. Burdon-Sanderson made an excellent speech, in which he said that physiologists could perceive no inherent objection to the hypothesis of Dr. Lodge ; but, he said, it was useless to advance any theory of the action of the complex stimuli of vision which did not also apply to the more simple stimuli met with in other organs and in tissue of plants and animals generally. There were a number of 'touch and go' stimuli, and the stimulus of the retina by mechanical means was one, where the action was functionally precisely the same as that of light on the retina. Prof. Schäfer pointed out that the vertebrate eye had a number of filamentary processes which might possibly represent a coherer; but, he added, the phenomena of vision were not confined to the vertebrate eye, but were also found in invertebrates, where a complicated retina and quasi-coherer processes were not to be found. A number of other leading physiologists added their support to the view that the morphology of the vertebrate eye very strikingly fell in with Prof. Lodge's hypothesis of a coherer circuit. Prof. Armstrong supposed that the true hypothesis of vision must account for the chemical actions which occur when light falls on the retina, and suggested that the chemical actions were the primary, and the electrical actions the secondary, cause of vision."

Since then, the energetic expulsion of electrons by ether waves, or vibrations of sufficient frequency, has been added to our knowledge, and is generally accepted as containing a clue to the manner in which the retinal nerves are stimulated, though the details have still to be accommodated to the three independent colour perceptions insisted on as above by Lord Rayleigh.

Thus it would seem as if the major part of the section's attention was held by the optical rather than by the telegraphic portion of my paper. For the date was still two years before Marconi commenced his great and most successful enterprise. I was chiefly interested in emphasising the fundamental work of Clerk Maxwell and the brilliant experimental discoveries of the recently deceased Heinrich Hertz. I am afraid I went beyond demonstration, saying that the waves ought to be called Maxwellian waves, and at the same time speaking of Hertz as " no ordinary German." Professor Boltzmann was moved to controvert both these statements. He rose and objected, with some warmth, to my manner of dealing with the great men of his nation ; partly perhaps because he rather misunderstood the trend of what I was saying, and partly because I may have been led by comparatively youthful enthusiasm to speak in an insufficiently balanced and well-guarded manner. Comparisons are always objectionable ; and the emphatic recognition of Hertz's work in this country had not been entirely pleasing to the older physicists of his own land. To most of them at that time Maxwell's theory was hardly known : they had their own theories and their own experiments. In letters to me Hertz sometimes referred to the difficulty which he had in getting his results understood by the orthodox professors of his own country, always excepting his quondam chief, von Helmholtz. And Boltzmann felt rather indignant at the picking out of one worker for special laudation. " For me," he said in the course of his speech, " I am an ordinary German ! " His meaning was clear enough, but his developed theory of the equipartition of energy, his work on the kinetic theory of gases, and the many other researches for which he was famous, showed that Boltzmann was no ordinary man, whether of his own or any other nation : and I trust that the incipient conflagration was speedily damped down by an appreciative reply.

The wave experiments shown at that meeting were a development of those I had previously expanded, as a memorial to Hertz, two months earlier on June 1st, in the theatre of the Royal Institution, London. (Neither of these 1894 occasions was the one at which the wallpaper sparkled. That was five years earlier, in a lecture of mine on the Leyden jar in 1889. See " Modern Views of Electricity," first edition, p. 401.) The reflection, refraction, polarisation, and other properties of the waves were demonstrated (in 1894) in what may now be considered the usual manner : the detection being accomplished, not by point coherer and telephone, as was the simplest plan in the laboratory, but by Branly's method with a tube of iron filings and a galvanometer. The possibility of actual signalling by this method was insisted on at Oxford : the iron filings being provided with a tapping-back arrangement, which restored them to sensitiveness directly the wave had subsided ; the potential collected from the wave having the power of increasing the conductivity of the filings enormously, while a mechanical tap reduced the tube to its previous high resistance.

The sending instrument was a Hertz vibrator actuated by an ordinary induction coil set in action by a morse key. This apparatus was in another room, and was worked by an assistant.

The receiving apparatus was a filings tube in a copper hat, in circuit with a battery, actuating either a morse recorder on a tape, or, for better demonstration to an audience, a Kelvin marine galvanometer, as first used for Atlantic telegraphy, before the siphon recorder replaced it. The instrument was lent me by Dr. Alexander Muirhead, whose firm habitually constructed a number of cable instruments. The little mirror in this speaking galvanometer is enclosed in a flat box so constricted as to damp its motion rapidly by the viscosity of the contained air. Thus it responded to signals sharply, in a dead-beat manner, without confusing oscillations.

When the morse key at the sending end was held down, the rapid trembler of the coil maintained the wave production, and the deflected spot of light at the receiving end remained in its deflected position so long as the key was down ; but when the key was only momentarily depressed, a short series of waves was emitted, and the spot of light then suffered a momentary deflection. These long and short signals obviously corresponded to the dashes and dots of the morse code ; and thus it was easy to demonstrate the signalling of some letters of the alphabet, so that they could be read by any telegraphist in the audience—some of whom may even now remember that they did so.

Truly, it was a very infantile kind of radio telegraphy ; but we found that distance was comparatively immaterial ; and at Liverpool, where I was then working, the dots and dashes were received with ease across the quadrangle, or from any reasonable distance.

My often quoted estimate of half a mile as a possible distance relates to earlier and still more primitive experiments, when the waves were produced by minute sparks to or from a sphere only about six inches in diameter ; these short and highly damped waves, about nine inches long, gave remarkably powerful effects at the coherer in the detecting room of the laboratory, considering the smallness of the energy involved in their production. The fact is that a spark lasts so infinitesimal a time, less than the millionth of a second, that almost any energy when divided by the time gives a very large horse-power. A small Hertz radiator (two plates connected by a bar) emits energy, as I reckoned, at the rate of 100 horse-power ; the radiant energy, other things being equal, varying inversely with the fourth power of the wave-length, in accordance with the theory of G. F. FitzGerald (see p. 54).

Hence I had hopes that by the use of short waves, signalling across considerable distances of space could by this means be accomplished ; and the late Lord Rayleigh urged me to take this matter up and develop it. I did not follow his advice, being fully occupied in teaching, and I had not the perceptive faculty shown about the same time by Sir William Crookes, who anticipated that such a method of telegraphy would be or might become of international importance.

How extraordinarily this incipient demonstration has been extended, remodelled, and altogether revolutionised by the skill, energy, and enterprise of Senator Marconi and his co-workers is known to the whole world. Real selective tuning became possible through my patent of 1897, and now the further development of the Fleming vacuum valve, which has made possible telephony or speech transmission, is known to every listener-in. Certainly it is a surprising development to have taken place in so comparatively short a time ; and the perfection of the transmitters now in use by the B.B.C. casts unlimited credit on the engineers and physicists responsible for all these vast improvements.

Marconi's great discovery that the waves travel all round the earth instead of advancing in straight lines has already been referred to. The ionisation of the atmosphere in its upper portion, called the Heaviside layer, can be regarded as a bonus on the part of Providence to those who were working for international conversation and co-operation.

It may be convenient at this stage to give a list of the more important dates connected with the discovery of Ether waves and their utilisation on an extensive scale.

Salient Dates about Ether Waves and Their Application

1864. Clerk Maxwell's paper in the "Philosophical Transactions Royal Society," in which he formulated Faraday's ideas in mathematical equations, and elaborated from them a wave equation, with an expression for the speed "v" with which those waves would advance. Appealing to experiment, he convinced himself that this theoretical speed was practically identical with the velocity of light. Hence followed a new revolutionary theory about the nature of light.

1873. Publication of Maxwell's two-volume treatise on "Electricity and Magnetism"; which was mentioned at the British Association Meeting that year at Bradford.

1879. Death of James Clerk Maxwell, at the age of 48.

1880. Mathematical discussion by G. F. FitzGerald as to whether Maxwell's electromagnetic waves could or could not be generated experimentally.

1883. G. F. FitzGerald suggested that the oscillatory disturbance of a condenser would suffice to emit waves of known or calculable wave-length; and he reckoned the energy of such waves.

1888. Lodge's experiments on waves, so generated, running along wires. Also announcement of

Hertz's discovery of such waves in free space, and several optical experiments made with them. Detection by little sparks or scintillæ.

1889. Lecture on the discharge of a Leyden jar at the Royal Institution, and further demonstration of Hertz's waves.

1890. Resonance Jar experiment, the foundation of tuning and wave measurement, described by Lodge in "Nature" February 20th.

1892. Experiments by Hughes, and Article by Crookes foreshadowing wireless telegraphy.

1894. January 1st. Death of Heinrich Hertz, aged 36.

1894. June 1st. Memorial to the work of Hertz by Lodge in a lecture to the Royal Institution, using a coherer method of detection and Branly filings tube, with full demonstration of refraction, interference, diffraction, and polarisation shown by the waves. See "Nature," Vol. 50, pp. 133–9.

1894. August 14th, at Oxford. First public demonstration that the waves could be used for telegraphic signalling in the Morse code.

1896. Sir W. H. Preece, knowing nothing of any of the above (but who had been interested for some years in possible methods of utilising leakage and inductive effects from telegraph circuits, as picked up by the telephone, for space telegraphy) announced to Section A at Liverpool that "an Italian had come with a box giving a quite new system of space telegraphy."

Date of Marconi's first patent, June 1896.

1897. Patent for selective tuning by use of self-induction taken out by Lodge, before publication of Marconi's patent.

1899. The Marconi Co. signalled across the channel, from Dover to Wimereux.

1901 February. Death of George Francis Fitz-Gerald, aged 49.

December. First ether waves across the Atlantic, when Mr. Marconi in Newfoundland heard the letter S (3 dots) transmitted from Cornwall.

1904. Lodge-Muirhead system established by the Indian Government between Burmah and the Andaman Islands. Also used in steamers round the coast.

Fleming's patent for use of an electric valve as detector of waves.

1908. Lee de Forest's improvement by introducing a grid into Fleming's valves, thus making magnification possible.

1911 Validity of Lodge's tuning patent of 1897 upheld by Mr. Justice Parker, extended for seven years, and purchased by the Marconi Company on condition that the Lodge-Muirhead Syndicate ceased their activities.

1922. Establishment of the B.B.C.

CHAPTER IX

THE DISCOVERY OF THE ELECTRON

Ipswich, 1895

The President, Sir Douglas Galton, took the opportunity of further advocating the establishment of a National Physical Laboratory, which I had mooted at some length in Section A at Cardiff in 1891. He read a paper on the Reichsanstalt at Charlottenburg, Berlin, and was evidently much impressed with the importance of the work it had aimed at and accomplished. The result was that the idea of a National Physical Laboratory for this country was taken up in earnest by the Association and a committee was appointed to report on the matter.

Sir Douglas unfortunately was getting near the end of his life, and in the course of his address had a seizure of some kind, and, to the horror of everybody, fell backwards on the floor. He soon recovered, however, but the rest of his address had to be read by someone else, and I am afraid did not receive the attention it deserved.

W. M. Hicks presided over Section A, and devoted his address to the subject of a rotational ether, a subject with which he was eminently qualified to deal. His argument was based on Lord Kelvin's old demonstration that a fluid in motion could acquire the kind of elasticity known as rigidity, one of the characteristics of an ordinary solid, and could therefore behave in a way which without a rotational or gyrostatic property would be impossible (p. 100). His phrase was "elasticity as a mode of motion," and he constructed a spring balance of spinning tops. I am glad of this opportunity of recalling attention to this subject, though he himself was inclined to mistrust it, and perhaps it is still rather premature. I must confess that such an ether is out of fashion at the present time, but I feel convinced that to some variety of it we shall have to return sooner or later. As a type of ether which he thought most likely to contain an element of truth, Hicks instances the mathematical rotational ether of MacCullagh, where—

> "the energy of the medium when disturbed depends only on the twists produced in it. This ether has recently been mathematically discussed by Dr. Larmor, who has shown that it is adequate to explain all the various phenomena of light, electricity, and magnetism."

Lord Kelvin's gyrostatic model, and also a vortex sponge ether, belong to this type. The velocity of propagation of light in a vortex-sponge ether, as used by Lord Kelvin, is two-thirds or ·47 times the root-mean-square velocity of the intrinsic gyrating motion of the medium—the speed which I believe to be the fundamental and universal velocity known as *c*.

Hicks refers to his report to the British Association in 1885 on the constitution of the luminiferous ether on the vortex-atom theory, which, though doubtless superseded, may contain some element of truth. At the Bath Meeting, in 1888, he had sketched a vortex analogue of static electricity. Then he refers to Larmor's great memoir in the "Philosophical Transactions" of 1894 on a dynamical theory of the electric luminiferous medium, where the conclusion is that a magnetic field is to be regarded as a flow of the ether, probably with the necessary accompaniment of rotational elements in it. Hicks, however, though he admits that states of motion are produced in the medium by electric and magnetic fields, declines to admit bodily flow. At least, says he,

"a magnetic field cannot be due to an irrotational flow of the ether alone. A field may possibly demand a flow of the ether, but, if so, it must carry in it some structure oriented at each point to the direction of flow."

Hicks is full of admiration for the work of MacCullagh and Larmor, and his address probably represents what some modern physicists would regard as the last dying kick of any dynamical treatment of the Ether of Space, but which I consider contains the germs of its resuscitation. The elastic solid theory is quite hopeless, but we are hardly yet acquainted with all the properties potentially belonging to an absolutely perfect continuous fluid. Such a thing has never been encountered experimentally. It would, indeed, decline to give any result; and that is just what the ether does.

In 1897 I reported on a preliminary experiment made to detect the etheric flow first suggested by Euler along lines of magnetic force; and though the power required turns out to be tremendous, since the flow is so slow in any ordinary field, I have hopes now that the genius of Dr. Kapitza will some day be able to grapple with the problem and attain a positive result. Such a result would be of the greatest interest: it might run counter to a postulate of the theory of relativity as originally formulated, but that is no fatal defect. We should not be debarred from experimenting on the ground of a theoretical assumption, even though that assumption has been justified by many striking consequences.

Liverpool, 1896

The meeting at Liverpool was presided over by Sir Joseph Lister, before he became Lord Lister. Two years later, the University College of Liverpool, recently made a constituent of the Victoria University, asked Lord Lister to open its new physiological and pathological Thompson-Yates laboratories, and took advantage of the occasion to confer upon him their first honorary degree; which accordingly was done in the presence of an exceptional number of distinguished men, including Lord Kelvin, Michael Foster, Burdon Sanderson, and Professor Virchow, the anthropologist, antiquarian, and pathologist ; the Vice-Chancellor of Cambridge, the Presidents of the College of Physicians, both of London and Edinburgh, Sir J. Crichton Browne, Mr. R. B. Haldane, Professor Schäfer, and, of course, the Professors Sherrington and Boyce ; also the Earl of Derby and the Chancellor, Earl Spencer. Lord Lister delivered a short address, in which he referred in a human and dignified manner to experiments on animals, concluding this part of his speech thus :

"While I deeply respect the humane feelings of those who object to this class of inquiry, I would assure them that, if they knew the truth, they would commend and not condemn them."

Conspicuous among those present was Professor Virchow from Germany, a great admirer of Lister, and we had the pleasure of seeing the two great men almost embracing on the platform.

Sir J. J. Thomson presided over Section A at Liverpool ; and I had the privilege of welcoming the section in my lecture-theatre and laboratory. Thomson referred to the discovery "at the end of last year by Professor Röntgen of a new kind of radiation." Lenard, whose work had led up to this, was present at the meeting, and received an ovation ; for it so happened that my lectures on the subject had prepared the mind of the public to appreciate Lenard's work.

Thomson seems to have thought that a varying magnetic field might set the ether in motion, and so affect the velocity of light, and described experiments to test that view.

A committee reported on the establishment of a National Physical Laboratory, which was very soon to become effective.

Dr. Isaac Roberts, who hitherto had been an amateur devoted to Astronomy in the north of Liverpool, but who soon became well known for his admirable photographs of nebulæ, had a paper "On the Evolution of Stellar Systems, and on the Spiral Nebulæ."

Many of the papers were naturally concerned with the newly discovered Röntgen rays, which were also referred to by Ludwig Mond, as President of Section B.

My brother Alfred's Table of Bessel Functions appears between pp. 98 and 149 of the annual volume.

Toronto, 1897

In 1897 the Association once more visited the Dominions, the meeting at Toronto being presided over by Sir John Evans. I went across in the same ship as Lord Lister and his party. His brother (an old man) used to go on deck every morning and be douched with the ice-cold water of the Arctic current. "Very refreshing," was his response to the surprise of much younger men. Professor Goldwin Smith was a great man at Toronto, and one of the garden parties was at his house.

Professor Forsyth, presiding over Section A, said :

" One of the most important events of the past year, connected with the affairs of this Section, has been the reception by the Prime Minister, Lord Salisbury, of a deputation to represent the need for the establishment of a National Physical Laboratory to carry out investigations of certain definite types."

So the idea, advocated at Cardiff in 1891, was then growing close to fruition. Soon afterwards the Government found a site for it in Bushey Park, the Royal Society undertook its management, and the beginnings of what has become a great and growing institution were speedily set on foot.

At this meeting I missed an opportunity which I have always regretted. The Dominion had placed a special train to run on the Canadian Pacific, and take a party to Vancouver and Victoria, B.C., stopping on the way at special points in the Rocky Mountains and elsewhere. But for some reason or other I felt I had to return home. I saw the party off from the station in a saloon car. There was my friend FitzGerald ; there were William Ramsay and his wife, and many another attractive person. And there in the saloon car was an arm-chair reserved for me. The group had a delightful trip, which I often heard them speak about afterwards, but, alas, I was not one of the party !

Bristol, 1898

In 1898 the Association met in Bristol, and Sir Wm. Crookes gave his address on Wheat, which is still often quoted. He predicted a world shortage unless nitrogen were extracted from the air ; which fortunately has been done both by the electric arc and by Haber's process. He also referred to the beginnings of practical wireless telegraphy, on pp. 21 and 22 of his Presidential Address as printed in the annual volume ; and then Crookes finished his address by a reference to his own psychic researches, which he abbreviated to three pages and a half, out of deference to the feeling of the Association against such subjects. Accordingly he enters into no detail, but he sticks to his guns. He says his experiments are well known, and goes on :

"Perhaps among my audience some may feel curious as to whether I shall speak out or be silent. I elect to speak, although briefly. To enter at length on a still debatable subject would be unduly to insist on a topic which—as Wallace, Lodge, and Barrett have already shown—though not unfitted for discussion at these meetings, does not yet enlist the interest of the majority of my scientific brethren. To ignore the subject would be an act of cowardice—an act of cowardice I feel no temptation to commit.

"To stop short in any research that bids fair to widen the gates of knowledge, to recoil from fear of difficulty or adverse criticism, is to bring reproach on Science. There is nothing for the investigator to do but to go straight on, 'to explore up and down, inch by inch, with the taper his reason' ; to follow the light wherever it may lead, even should it at times resemble a will-o'-the-wisp. I have nothing to retract. I adhere to my already published statements. Indeed, I might add much thereto. I regret only a certain crudity in those early expositions which, no doubt justly, militated against their acceptance by the scientific world. My own knowledge at that time scarcely extended beyond the fact that certain phenomena new to science had assuredly occurred, and were attested by my own sober senses, and, better still, by automatic record. I was like some two-dimensional being who might stand at the singular point of a Riemann surface, and thus find himself in infinitesimal and inexplicable contact with a plane of existence not his own."

The reticence of Crookes in not insisting on the importance of this subject more strongly has been used unfairly to imply that he had lost interest in it. That is perfectly absurd, as those know who knew him well up to the end of his life. As a matter of fact, he was President of the Society for Psychical Research that very year, and had many opportunities for announcing his views on the facts to a more sympathetic audience.

Sir Herbert Jackson gave one of the Evening Lectures (on Phosphorescence), brilliantly illustrated by experiments ; and Professor Sollas gave the other.

Dover, 1899

The 1899 meeting took place at Dover, and is famous in many respects. A simultaneous meeting of the French Association took place in Boulogne, and they were invited to send delegates over to be entertained at a meeting on Saturday. We chaffed our President, Michael Foster, warning him that he would have to kiss the French President on his arrival. He braced himself up to it, and did it manfully. I remember the meeting well. We gave our visitors lunch, and at the small table at which I presided they had put bottles of Rhine wine. I was going to remonstrate, and say that we ought to have had French wine ; but the Frenchmen were satisfied and said jokingly that they would "drink the wine of future France," which accordingly was done, though not very seriously.

Before this, that same morning, a contingent of the members had visited Section A, and been entertained by an entrancing exposition by Sir J. J. Thomson about a particle of matter smaller and lighter than the atom. It really constituted the first international publication of his discovery of the electron. I was to follow him with a communication on a still controverted subject, "The Seat of the E.M.F. in a Voltaic Pile," but I was so enamoured of Sir J. J. Thomson's brilliant discovery that for the first quarter of an hour I could speak of nothing else ; and, indeed, that was the feature of the meeting.

George Wyndham was Member of Parliament for Dover, and he had brought a party over from Clouds, whom I was glad to be able to attend to.

Poynting was President of Section A, and gave a sensible address on the true mode of regarding what are called Laws of Nature ; but he was somewhat overshadowed by the great discovery of Sir J. J. Thomson, whose paper was subsequently published in the "Philosophical Magazine" for December 1899 (pp. 547–567).

BRADFORD, 1900

In 1900 the Association met once more in Bradford, and as far as I am concerned the cycle is complete. I do not propose to continue my reminiscences into the twentieth century. Some will say that by including 1900 I have already entered the twentieth century. That would be in accordance with the view of Lord Kelvin, but I contend that it was an utter mistake. It would be hard lines to withdraw the year 1900 from the nineteenth century, to which it had all along given the name. The twentieth century began with January 1st, 1901. The first year of the century was then born; it was not completed for twelve months, but it certainly was the first year of the twentieth century, as the year 1900 was the last year of the nineteenth century, which ended on December 31st, 1900. Lord Kelvin's mistake was intelligible; it was due to the method of counting in the laboratory the ticks of a clock or other periodic disturbances. The tick at the start of an event is not called 1 but 0, and subsequent ticks are counted regularly from 0 onwards. This may lead to confusion when applied to things of long duration like years, but 0 only represents the starting-point; it has no duration. The first second is still 1, but it is not named till the end ; a year lasts so long, it requires to be named all through. A century starts with the year 1 ; there is no year 0, though there is always a century year like 1900, which completes the nineteenth century when it ends. This may be taken as a reminiscence of the controversy which went on more or less all through the year, and will probably be repeated when the twentieth century comes to an end.

Sir William Turner, the anatomist, presided over the second Bradford meeting ; and Sir Joseph Larmor over Section A. It was not so striking a meeting as the first had been. Larmor was still old-fashioned enough to refer to the dynamics of the ether, and, in fact, to devote his address to it. He finds himself up against the difficulty of the equipartition of energy. He says :

" In the words of Maxwell, when he first discovered in 1860, to his great surprise, that in a system of colliding rigid atoms the energy would always be equally divided between translatory and rotatory motions, it is only necessary to assume, in order to evade this unwelcome conclusion, that 'something essential to the complete statement of the physical theory of molecular encounters must have hitherto escaped us.' "

Otherwise the energy would all be frittered away among innumerable degrees of freedom, and the molecular activity of the universe would come to a speedy end.

Well, we know now what had escaped us, when discussing full equipartition; it was nothing less than the "quantum," to regulate the interchange of energy between matter and ether. Planck introduced the quantum into the theory of radiation in this very year; it soon encroached on other departments, and thus inaugurated the revolutionary science of the twentieth century. Einstein was heard of five years later. The clouds which Lord Kelvin had lamented as hanging over the science of the nineteenth century were getting ready to disperse. The revolution had begun. But it was a supplementary, not a destructive, revolution; no one should suppose that it denied and falsified all that had been taught before. There was much truth still in those old contentions, and though they have now entered the shadow, they will return in due time, strengthened, enlarged, and revivified by the illumination which has been poured on them during the present eventful years.

Let no one imagine that the ether is extinct. It may be fibrous, it may be vortical, it may be composed of lines of force, but a universal connecting medium is absolutely essential for the philosophic understanding of even the simplest behaviour of matter. Matter can be moved; that is all we can do to it. When a spring is bent it is the connecting mechanism that is strained; the particles of matter are merely altered in position. The stored potential energy does not belong to them; their behaviour only demonstrates its existence. Material particles are separate discrete isolated units, perfectly inert, taking the path of least resistance, and having no spontaneity, no energy even, of their own. They require something to weld them into a cosmos and to bring about all the phenomena that we observe. The whole of that activity, the whole of the energy, is to be sought in the properties of the connecting medium in what we call empty space ; and this seeking is the problem of the twentieth century. Already a recondite initial theory of the ether can be traced in the assimilation now detected between a particle and a wave. Matter itself is one of the forms which the energy of the ether can take ; radiation is the other form. The two are going to be interchangeable, and the interchange constitutes all the activity that surrounds us.

Matter and ether : we are only directly aware of one of them. Hence arise nearly all our controversies ; and there lies the basis of our philosophic ignorance. The psychical and the physical worlds seem to belong to different realms ; yet they are conspicuously interlocked, and one controls the other. The etheric aspect of the universe only manifests itself to our senses when it interacts with matter. When it ceases to do that, it disappears from our ken. The movements of matter are all that our senses tell us of, but they are never primary, never self-induced ; they all have to be accounted for. The old ambition of Newton and Newtonian physicists was to explain everything in terms of dynamics. Now it is perceived that dynamics itself requires explanation ; and thus a wave theory of dynamics has begun. By the end of the century it will be perceived that the properties and constitution of the ether are fundamental, and that in those alone is there any hope of tackling the essential nature of guidance, and the physical side of the other problems presented by Life and Mind. Matter when animated behaves oddly, but *qua* matter its properties are not in the least changed ; it is exactly like any other matter, but it is otherwise controlled. Etheric control is known to account for electric and magnetic and, indeed, for all inorganic behaviour : can it equally account for vitality ? Modern physics has begun seriously to attend to the ether, or what it calls " space." Already we are dealing with a multitude of things that elude our senses, from an electron upwards ; may we not hope that we shall soon begin to attend to, and so gradually make the scientific discovery of, a spiritual world ? From another aspect this spirit world has always been known to humanity—witness the churches throughout all lands—but it has seemed alien to scientific inquiry and to resent critical examination. An infantile demonstration of its reality and actual presence has been heralded by weird and unwelcome and incredible experiences, attended to under protest by very few trained observers ; but when the discovery is complete, all other of our advances into the unknown will fade into comparative insignificance.

APPENDIX

SALIENT DATES ABOUT ETHER WAVES AND THEIR APPLICATION

The List of Salient Wireless Dates at the end of Chapter VIII is incomplete. The following may be added:

1893. FitzGerald's theory of the travel of electric waves round the globe, assuming an upper conducting layer.

1901. Fessenden patented his alternator for emission of continuous waves.

1903. Poulsen invented a high-frequency arc for generating continuous waves.

1903. Wehnelt patented the thermionic valve for rectifying.

1904. Fleming's patent for use of an electric valve as detector of waves.

1905. Fessenden devised the heterodyne method of receiving continuous waves.

1907. Lee de Forest introduced a grid into a valve, thus making magnification possible.

1913. Valve method of generating continuous waves invented independently by de Forest, Armstrong, and Meissner.

1917. Eccles constrained a mechanical vibrator to emit continuous waves, of high and exceedingly constant frequency, by the use of a tuning-fork and quartz-plate oscillator sustained by valves; subsequently applying this plan to the Rugby station.

1920. Broadcasting started in the U.S.A.

1922. October. Establishment of the B.B.C.

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Printed and Made in Great Britain by
Hazell, Watson & Viney, Ltd., London and Aylesbury.