LECTURE I

It is more than thirty years ago, that returning from a scientific expedition I hurriedly passed through India, and fascinated by its sunshine, the beauties of its mountain scenery and the mysteries of its national life, carried home with me the intense desire of a more leisurely visit. I then stood at the beginning of my scientific career and when now, nearing its end, my wish is fulfilled by being called upon to fill the honourable office of “Reader” to the University of Calcutta, I thought it might be interesting to you—as it is to me—if I discharge my duties by reviewing the progress of scientific thought in the interval.

It is my aim to trace the changes in our point of view, rather than to give an historical account of the sequence of the discoveries which make this period memorable. In taking this course I am aware of the risk incurred in an attempt, which to a great extent must be guided by subjective impressions; for though a man may see clearly the changes that have taken place in his own mind, he may be wrong in estimating the rise of the average level of scientific thought by the standard of his individual progress. According as
Lecture I

he has been taught by advanced or antiquated teachers, according as he ends his work ahead of, or behind his time, will he give a very different version of the progress which has been achieved. But rightly or wrongly I have chosen my course: I prefer to be frankly subjective, and warn you beforehand that my account will be fragmentary, and to a great extent reminiscent of those aspects which have come under my own personal view.

Let us briefly survey the general position of physical science at the time our story begins. The work of the previous twenty years was dominated by the gradual recognition of the Science of Energy founded experimentally on the work of Joule, and theoretically on that of Kelvin, Clausius and Helmholtz. Destined from the beginning to serve as a bond between all branches of physical knowledge, it found its first triumph in establishing the connexion between the science of heat and the theorems of dynamics. The first law of thermodynamics, which teaches us that heat is generated or destroyed in exact proportion to the amount of mechanical work lost or gained, was not only recognised as unassailable, but had truly saturated the soul of the scientific body. Such was not entirely the case (is it now?) with the second law, which regulates the conditions, under which heat can be changed into mechanical work. It is true that every one whose opinion counted, accepted the law, and no student was allowed to remain ignorant of Carnot’s cycle, and of the mathematical equations which can be derived from it; but the law was not a living and fertilising part of physical science; except in its special applications to
heat engines. In its more refined consequences dealing with entropy and available energy it had not yet become common property, and in its application to the decay of universal energy it was not sufficiently understood and appreciated.

The tendency to hide ignorance under the cover of a mathematical formula had already appeared but was not openly advocated; hence students were still taught to form definite ideas of the processes of nature. In the kinetic theory of gases ample material was found for a mental picture of the state of matter in its simplest form. The calculation of the average molecular velocities, the application of the theory to the processes of diffusion, conduction and viscosity had been accomplished and was taught at some, at any rate, of the Universities. I became acquainted with the theory of gases in lectures given by L. Soret at Geneva in 1869, and well remember how the possibility of determining the size of molecules and their number in a given volume seemed to open out before me, but though I attacked the problem I could make nothing of it, and it was not till some time later that I found it already solved by Johnstone Stoney and Loschmidt. Those who could follow the more intricate mathematical developments of the subject found in Maxwell's investigation on the law of distribution of molecular velocities a new revelation, which introduced for the first time the theory of probability into problems relating to the physical state of bodies. In the succession of new ideas that have influenced the progress of Physics this great step must always hold a pre-eminent position.

As regards light, the elastic solid theory held the
4

Lecture I

field, though serious difficulties, passed over too lightly in the period of its triumph, already foreshadowed its ultimate defeat.

Stokes by his brilliant investigations had placed the laws of transmission of vibrations through elastic solids on a firm basis. He had shewn how a complete theory must take account of two waves, the condensational wave and the distortional one. The first of these is important in the theory of sound, but no optical phenomenon indicates its existence and hence arises a certain difficulty. When a single homogeneous medium is considered, this difficulty is most easily overcome by assuming the condensational wave to be transmitted with infinite velocity, but Lord Kelvin shewed that consistent results are obtained equally well, by introducing the condition that these waves are propagated with a velocity which is infinitely small. This however was at a date considerably later than the one to which I am at present referring. Condensational waves were then generally ignored, but another stumbling block worried our minds. The laws of transmission of light through different bodies could, up to a certain point, be equally well explained by assuming the elasticity of the aether to be the same everywhere, and its density to be different in different bodies, or by specifying that the density is constant, while the elasticity changes. An increased velocity inside a body could accordingly either be due to an increased elasticity or to a diminished density. The relationship between the direction of vibration in a beam of light and what is called its plane of polarisation depends on which of the two alternatives we adopt. If the density be variable, the direction of
Elastic Solid Theory of Light

vibration must be at right angles to the plane of polarisation, while parallelism between the direction of vibration and the plane of polarisation would signify that the elasticity differs. The more refined consequences of the theories gave rise to the hope, that experiments might be devised to decide between the two alternatives, but unfortunately the verdict of experiment was not uniformly on the same side. When Fresnel’s formula for the intensity of a ray reflected from a transparent body, which had been verified experimentally by Brewster, was closely criticised by Lord Rayleigh, its theoretical foundations were only found to be sound, on the supposition of variable density. This meant that the vibration was at right angles to the plane of polarisation, a result which was also supported by the polarisation observed in a wave scattered by small particles.

But the law of double refraction in crystalline media led to the opposite conclusion. Here again Fresnel was the pioneer, who gave us the equation of the wave surface by specifying that elasticity in a crystal depends on the direction of displacement. Physicists were now placed in the dilemma, that in different parts of optics two mutually destructive theories on the properties of the æther were supported by experiment. For though Rayleigh shewed that we may obtain a surface not differing very much from Fresnel’s, by assuming a variable inertia according to the direction of vibration, the difference was sufficient to admit of the question being decided by experiment, which, performed by Glazebrook gave its verdict emphatically in favour of Fresnel. These difficulties and discrepancies, though
Lecture I

not considered fatal, prepared men of science for the great revolution, which was soon to come and sweep away the whole elastic solid theory.

The state of electrical science in 1870 must present itself somewhat differently to the scrutiny of a historian who derives his information from the records of published papers, and to the recollection of a student, who received his impressions from the teachers at the time. Maxwell’s great paper “A Dynamical Theory of the Electromagnetic Field” appeared in 1864, but I doubt whether the younger generation of physicists had their attention drawn to, or seriously arrested by it, before the publication in 1872 of the two volumes *Electricity and Magnetism*. I believe that the first systematic course of lectures based on Maxwell’s theory was given by myself at the Owens College during the session 1875–76. (Sir Joseph Thomson was one of the three students attending the course.) At the time Maxwell’s volume appeared, the teaching of electricity centred round the calculation of coefficients of induction and futile discussions on the laws of action of so-called current elements. A wider and more philosophic aspect was now brought before us, and though Maxwell’s treatise was essentially mathematical, it put an end to the university tradition of looking upon electricity as part of applied mathematics to the neglect of its physical aspect. Hence Maxwell’s work changed the whole point of view of the study of electrical science.

While the above indicates in brief outline the general condition of the main branches of Physics at the period of which we are speaking, we must now turn our attention to the important question as to how
Training of Students

students were prepared by teaching and example to take an active part in advancing their subject.

I think I interpret correctly the recollection of those who passed through their scientific education at the time, when I say that the general impression they received was that, apart from theoretical work, a reputation could only be secured by improved methods of measurement which would extend the numerical accuracy of the determination of physical constants. In many cases the student was led to believe that the main facts of nature were all known, that the chances of any great discovery being made by experiment were vanishingly small, and that therefore the experimentalists work consisted in deciding between rival theories, or in finding some small residual effect, which might add a more or less important detail to the theory. There were no doubt great differences of opinion depending on the temperament of the teacher, as to how far increased accuracy of measurement was an object desirable in itself or only a means to an end, and in this connexion a passage taken from Clerk Maxwell’s Introductory Lecture in Experimental Physics deserves to be quoted:

“This characteristic of modern experiments—that they consist principally of measurements—is so prominent, that the opinion seems to have got abroad, that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science

1 Collected Works, II. p. 241. No date is attached to this lecture in the published volume, but according to the information given in Maxwell's Life it was delivered in October, 1871.
Lecture I

will be to carry on these measurements to another place of decimals.

"If this is really the state of things to which we are approaching, our Laboratory may perhaps become celebrated as a place of conscientious labour and consummate skill, but it will be out of place in the University, and ought rather to be classed with the other great workshops of our country, where equal ability is directed to more useful ends.

"But we have no right to think thus of the unsearchable riches of creation, or of the untried fertility of those fresh minds into which these riches will continue to be poured. It may possibly be true that, in some of those fields of discovery which lie open to such rough observations as can be made without artificial methods, the great explorers of former times have appropriated most of what is valuable, and that the gleanings which remain are sought after, rather for their abstruseness, than for their intrinsic worth. But the history of science shews that even during the phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of the new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers. I might bring forward instances gathered from every branch of science, shewing how the labour of careful measurement has been rewarded by the discovery of new fields of research, and by the development of new scientific ideas. But the history of the science of terrestrial magnetism affords us a
Maxwell on Laboratory Instruction

sufficient example of what may be done by experiments in concert, such as we hope some day to perform in our Laboratory."

Another interesting passage throws light on Maxwell’s idea of the proper function of a laboratory.

“Our principal work, however, in the laboratory must be to acquaint ourselves with all kinds of scientific methods, to compare them, and to estimate their value. It will, I think, be a result worthy of our University, and more likely to be accomplished here than in any private laboratory, if, by the free and full discussion of the relative value of different scientific procedures, we succeed in forming a school of scientific criticism, and in assisting the development of the doctrine of method.”

Maxwell’s view was that of the most enlightened and progressive philosopher then living, but his optimism was not shared by others who at the time enjoyed an equal or greater reputation. Gustav Kirchhoff, for instance, was by no means a man who despised experimental enquiry; I have heard him speak with appreciation of men who without much theoretical knowledge tried to upset theory by experiment; but the sole advantage he expected to accrue from their labours was the mending of the theory and he did not anticipate new facts being discovered leading to a revision of fundamental conceptions. When I told him of the discovery then made in England, that light falling on the surface of a bar of selenium altered its electrical conductivity, he remarked: “I am surprised that so curious a phenomenon should have remained undiscovered till now.” This represents
Lecture I

the attitude of mind not only of Kirchhoff but of the great majority of physicists at the time. Looking back now on this period, when Roentgen rays and radio-activity were undreamt of, we may well learn to be cautious in our own predictions of the future.

It will be noticed that though Maxwell evidently looked forward to further discoveries, he expected them to appear as residual effects rather than by a direct experimental attack deliberately made to break new ground. A typical example of a great discovery brought about by the method indicated by Maxwell is furnished by Rayleigh’s measurements, which culminated in the discovery of argon through a research undertaken to determine with accuracy the density of gases, and shewing unexpectedly a marked discrepancy between two samples of apparently pure nitrogen. The gas derived from air was found to be heavier than that generated chemically, because as was ultimately shewn, it contained small quantities of a denser and new gas. But though this may be quoted as an example of the orthodox method of discovery, and also fully recognising that a teacher is almost bound to point to this as the soundest method, it is nevertheless indisputable that the greatest discoveries, both formerly and in recent years have not originated in the hunt of residuals. Altogether I am doubtful whether any great discovery has ever been made by anyone who has only aimed at recording a number of facts. I do not believe, in spite of what is sometimes asserted, that Schwabe was led to the discovery of the periodicity of sunspots simply by sitting down to record without ulterior motive the number of spots he could see; and it is to me unthink-