CHAPTER II

Early Pioneering Work in Ether Waves

NOW that electric waves are so easy to produce, and so absurdly easy to detect, it is amazing to think how long it took to discover them, and how many times they hinted at their existence before they were properly recognized and appreciated. From time to time in laboratories little residual phenomena appeared, which were noted as rather interesting and surprising but were not seriously followed up. They were sporadic phenomena, to which the observers had no clue, of which therefore they did not understand the meaning, and which accordingly were relegated for the most part to the subsidiary and unimportant rôle of accidental disturbances, a sort of *poltergeist* phenomenon, which could not be brought to book and which seemed rather lawless and capricious. Occasionally and for a short time some man of genius had a suspicion that the odd little sparks and things which he observed must have a meaning, which might be of importance, if it could be ferreted out; but meanwhile the lack of a clue or any guiding

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theory caused them to be forgotten again and not followed up either by the observer or his contemporaries.

Early pioneering work is too often overlooked and forgotten in the rush of a brilliant new generation, and amid the interest of fresh and surprising developments. The early stages of any discovery have, however, an interest and fascination of their own, and teachers would do well to immerse themselves in the atmosphere of those earlier times, in order to realize more clearly the difficulties which had to be overcome, and by what steps the new knowledge had to be dovetailed in with the old. Moreover, for beginners, the nascent stages of a discovery are sometimes more easily assimilated than the finished product. Beginners need not, indeed, be led through all the controversies which naturally accompany the introduction of anything new; but some familiarity with these controversies and discussions on the part of the teacher is desirable if he is to apprehend the students' probable difficulties. For though he does not himself feel them now, the human race did feel them at their first introduction; and the individual is liable to recapitulate, or repeat quickly, the experience of the race.

A large number of people now interested in the most modern developments of wireless have but little idea—perhaps none at all—of the early work, in apparently diverse directions, which preceded and made such developments possible. No one, indeed, can subsequently feel in touch with the history so closely as those who have lived through the period covered by it. Only those who have survived the puzzled and preliminary stages of a discovery can appreciate fully the contrast with subsequent enlightenment. It may suffice to say that the term "inductance" or "self-induction," which we now use so glibly, did not at first exist; and that so late as 1888 Sir William Preece still spoke of it as "a bug-a-boo": whereas it is the absolute essential to tuning, and even to electric oscillation.

Lord Kelvin, who first introduced it in 1853 as a mathematical coefficient, without any explanation, called it "electro-dynamic capacity." The name self-induction was given to it by Maxwell, though it was long before it was understood or utilized; and the name "inductance" is a nomenclature of Heaviside.

I wish here to say little about anything to do with wireless later than 1896. What I have to deal with is the early pioneering work apart from practical developments. Let me say at once, to avoid misunderstanding, that without the energy, ability, and enterprise of Signor

Marconi, what is now called "wireless" would not have been established commercially, would not have covered the earth with its radio stations, and would not have taken the hold it has upon the public imagination. Before 1896 the public knew nothing of its possibilities: and for some time after 1896, in spite of the eloquence of Sir William Preece and the demonstrations by Signor Marconi, the public thought it mysterious and almost incredible; and still knew nothing about the early stages. Indeed, I scarcely suppose that Signor Marconi himself really knew very much about them. He had plenty to do with the present; he felt that the future was in his hands; and he could afford to overlook the past.

It may be doubted whether the younger generation, who are so enthusiastically utilizing and perhaps improving the latest inventions, will care much about the past either. Incidentally, however, I want to say two things to those who are occupied with the subject to-day First, do not hesitate to speak and think of the *ether of space* as the continuous reality which connects us all up, and which welds not only us but all the planets into a coherent system. Do not be misled by any misapprehensions of the Theory of Relativity into supposing that that theory dispenses with the ether merely because it succeeds in ignoring it. You can ignore a thing without putting it out of existence: and the leaders in that theory are well aware that for anything like a physical explanation of light or electricity or magnetism or cohesion or gravitation, the ether is indispensable. The ether has all these functions, and many more. We are utilizing it every day of our lives; and it would be ungrateful as well as benighted if we failed to render due homage to its omnipresent reality and highly efficient properties. It lies at the origin of all electrical developments, and forms the basis for this new and broadcast method of communication.

That is one thing: the second is to congratulate all those whose wonderful and rapid advances have rendered possible the astonishing feat of, in any sense and by whatever means, carrying the human voice across the Atlantic. When Signor Marconi succeeded in sending the letter "s" by Morse signals from Cornwall or Ireland to Newfoundland, it constituted an epoch in human history, on its physical side, and was itself an astonishing and remarkable feat. The present achievement of changing over from Morse signals to ordinary speech, made possible by the valves of Prof. Fleming and Dr. Lee de Forest and others, is a natural though still surprising outcome and development of long-distance transmission, and must lead to further advances, of which at present we can probably form but a very imperfect conception.

Well, now I must go back to early times. In or about the year 1875 Mr. Edison observed something, which at that time could by no means be understood, about the possibility of drawing sparks from insulated objects in the neighbourhood of an electrical discharge. He did not pursue the matter, for the time was not ripe; but he called it "Etheric Force"—a name which rather perhaps set our teeth on edge; and we none of us thought it of much importance. Silvanus Thompson, however, took up the matter in a half-hearted sort of way, and gave a demonstration to the Physical Society of London in, I believe, June, 1876—a paper which I have had a little difficulty in finding in the Proceedings of that Society. Nothing much came of it, however, though his argument tended to show that the sparks could be accounted for on known principles. The value of this is merely that it must have rendered Thompson susceptible to methods of detecting real electric waves when they were discovered later.

Colonel Crompton informs me that these Edison sparks were also examined by Prof.

Elihu Thomson in the Central High School Laboratory, Philadelphia, in 1875 or 1876. Thomson used an ordinary Ruhmkorff coil, one terminal to earth—a water-pipe; the other terminal being connected by a few feet of wire to a large tin vessel mounted on a glass jar. Whenever the coil sparked, he found that small sparks could be drawn from knobs and pipes in many other parts of the building, even up on the sixth floor, 100 feet away from his apparatus. He claimed that the reason Edison's etheric force did not affect galvanometers and electroscopes was because it was oscillatory; the alternations of current being far more rapid than any instrument could respond to.

In fact, he was dealing with high frequencies before the word frequency was in electrical or optical use: the apparent lack of polarity exhibited in Edison's effect was accounted for. But Elihu Thomson did not follow up the observation: he did not recognize their Maxwellian significance. In fact, not till 1887 and 1888, when I independently stumbled on the same phenomenon, was it recognized that there —or rather in the extensions or expansions of that phenomenon—was something which could be detected and measured, running along wires with a definite wave-length, and that these impulses were truly Clerk Maxwell's waves;

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which were thus introduced to science on wires at about the same time as Hertz was detecting them in space. (See latter half of my paper in *Philosophical Magazine* for August, 1888.)

It was found afterwards that Joseph Henry, at the Smithsonian Institution in Washington, had observed something of the same kind so early as 1842. He seems to have had an intuition of the possible importance and far-reaching consequences of his observation, for he speaks as follows (I quote a passage cited in my lecture "On the Discharge of a Leyden Jar," printed at the end of my 1889 book, "Modern Views of Electricity"):—

"It would appear that a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400 feet of capacity, and . . . it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from flint and steel in the case of light."

That is to say, so early as 1842 Joseph Henry had the genius to surmise that there was some similarity between the etherial disturbance caused by the discharge of a conductor and the light emitted from an ordinary high temperature source.

In the light of our modern knowledge, and Clerk Maxwell's theory, we now know that the similarity is very near akin to identity. Both sources emit ether waves, though prodigiously differing in length.

Subsequent to these early stray observations an amazingly suggestive observation, of a partially similar kind, was made by that singular genius and brilliant experimenter David Hughes, the inventor of the microphone or telephonic transmitter, and of the Hughes Printing Telegraph still used in France. He was a man who " thought with his fingers," and worked with the simplest home-made apparatus —made of match-boxes and bits of wood and metal, stuck together with cobbler's wax and sealing-wax. Such a man, constantly working, is sure to come across phenomena inexplicable by orthodox science. As a matter of fact, Hughes unknowingly was very nearly on the trail of what was subsequently discovered, in a so much more enlightened manner, by Hertz. Hughes, too, got sparks in the course of his experiments, but he also got something very like coherer action by means of his microphone detectors. They enabled him to get actual galvanometer deflexions—such as Hertz never got.

All this was early in the 'eighties and before either Hertz or me. Hughes was a telegraphist, and though he would never have worked out the subject mathematically as Hertz did, and would not have been interested in matters of theory, he might well have stumbled, even at that early date, on something like a rudimentary system of wireless signalling had he been encouraged. But he was not encouraged. He showed his results to that great and splendid mathematical physicist Sir George Stokes; and Stokes, alas, turned them down, considering that they were explicable either by leakage or some other known kind of fact.

That is the danger of too great knowledge; it looks askance at anything lying beyond or beneath its extensive scope; whereas an experimenter operating at first hand on Nature may quite well occasionally stumble on a fact which lies outside the purview of contemporary science, and which accordingly neither he nor anyone else at the time understands. Crookes himself had a similar experience. In his pertinacious and systematic way he explored many unfamiliar and untrodden regions; and he also invited the attention of Stokes to a simple and easily investigated case of abnormal movement; who, however, perceiving that such motion was physically impossible, declined to take any interest in it or even to see it. His reason told him (and the reason he gave was) that on recognized principles the asserted phenomenon could not happen. But that

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was precisely its point of interest, and that was why Crookes with his instinctive sagacity conceived that such things held within them the germ of a great science of the future.

In Crookes's case the germ still remains unfructified by orthodox science. In Hughes's case the germ was rediscovered and has borne fruit a million-fold. But this is to anticipate. Suffice it now to direct attention to the collection of Hughes apparatus now unearthed by the energy and piety of Mr. Campbell Swinton, and exhibited in the Museum of Science at South Kensington. And let us try to avoid imitating the mistakes of our revered scientific ancestors: though I admit it is a difficult task. So much rubbish is brought to our notice that we are bound to run the risk of neglecting a jewel amid the chaff.

THEORIES OF LIGHT

These spasmodic observations, however, are not exactly discoveries: they were more akin to vague intuitions. The first and gigantic step in the real discovery was made by Clerk Maxwell, in or about 1865: and he made it in mathematical form, not in experimental actuality, by one of those superhuman achievements which are only possible to our greatest

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mathematical physicists. He did not discover either the way to generate those ether waves, or to detect them ; but he did give their laws ; he legislated for them before they were born. He knew the velocity with which they must move, and gave implicitly, though without elaboration, the complete theory of their nature.

Up to his time the nature of light was unknown. All the other theories of light had attempted to explain it on mechanical principles, like the vibrations of an elastic solid. Light was known to consist of transverse waves : the wave-length and the frequency of oscillation could be determined. But no one knew what was oscillating, nor what the mechanism of propagation was. With extraordinary genius Fresnel and MacCullagh had explained the phenomena of light in all detail as regards reflection, refraction, diffraction, interference, and polarization. But the nature of the waves was unknown ; and the elastic solid theory, though fascinating, was felt by those who dived most deeply into it to contain some flaw, and to be, strictly speaking, unworkable. Light did not seem explicable on dynamical principles—the principles which were so fruitfully devised by Galileo and Newton for dealing with ordinary matter.

MacCullagh's theory indeed was not dynamical, and in that respect had some advantage. But it was also vaguer and less definite on that account; though, being thus indefinite and yet enabling results to be achieved, it was less liable to be upset and replaced by future discovery.

To Clerk Maxwell we owe the epoch-making discovery that light was not a mechanical oscillation at all, that the ordinary mechanical properties of matter did not apply to it, but that it was explicable solely and wholly in terms of electricity and magnetism. It is impossible to sum up his discovery in a few words; but roughly we may say that the most obvious outcome was:

(1) That if electric waves could ever be generated they would travel with the velocity of light.

(2) That light was essentially an electromagnetic and not a mechanical phenomenon.

(3) That the refractive index of a substance was intimately related to the dielectric coefficient.

(4) That conductors of electricity must be opaque to light.

Maxwell showed further, though he did not then express it in language of this character, that the ether had two great and characteristic

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constants, of value utterly unknown to this day, though guessed at by a few speculators like myself—one of them the electric constant of Faraday called "K"; the other the magnetic constant of Kelvin called "μ." It was impossible then, and it is impossible now—though it is not likely always to remain impossible—to determine the value or even the nature of either of these constants. But Maxwell did perceive a way of measuring their product; and he was the first to measure it. Their product is known; and it is equal—as he showed it must be—to the reciprocal of the square of the velocity of light.

Well now, this great discovery aroused in us young physicists the greatest enthusiasm. In the early seventies of last century—I think about 1871 or 1872—I remember discussing it with the man we all now know and honour, J. A. Fleming, who at that time was a fellow-student with me in Prof. Frankland's advanced chemical laboratory at the brand-new College of Science, South Kensington. A year or two later, at Heidelberg, I studied Maxwell's treatise pretty thoroughly, and formed the desire to devote my life if possible to the production and detection of Maxwell's electric waves.