CHAPTER III

The Discovery of the Waves

I USED to discuss the possibility of producing these waves with my great friend, G. F. FitzGerald, whose acquaintance I made at the meeting of the British Association in Dublin in the year 1878; and he wrote some mathematical papers discussing the possibility of producing such waves experimentally. I myself also spoke at the British Association about them, in 1879, 1880, and again in 1882, at the Royal Dublin Society. FitzGerald, as I say, mathematically examined what then seemed the abstruse question of electric wave production; and after some hesitation came to the conclusion that direct artificial generation of waves was really possible on Maxwell's theory, in spite of certain recondite difficulties which at first led him to doubt it. (See "Scientific Writings" of FitzGerald, edited by Larmor, pp. 90–101.) Indeed, one of his papers on the subject was originally entitled "On the Impossibility of Originating Wave Disturbances in the Ether by Means of Electric Forces." The prefix "im" was subsequently dropped;

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although his first, or 1897, paper concluded thus:

> "However these [displacement currents] may be produced, by any system of fixed or movable conductors charged in any way, and discharging themselves amongst one another, they will never be so distributed as to originate wave-disturbances propagated through space outside the system."

In 1882 FitzGerald corrected this erroneous conclusion, and referred to some early attempts of mine at producing the waves. ("Scientific Writings," p. 100.) I state all this in order to emphasize the difficulty which in those early days surrounded the subject on its theoretical as well as on its practical side.

In 1883, at the Southport meeting of the British Association, FitzGerald took a further step and surmised that one mode of attaining the desired result would be by utilizing the oscillatory discharge of a Leyden jar, if only we had the means of detecting such waves when they were generated.

Inspired by FitzGerald's views, I and A. P. Chattock, now of Bristol, then working at Liverpool, did succeed in getting clear evidence of the existence of these waves, running along wires: and we were able to measure and express their length in metres or fractions of a kilometre, by the nodes and loops which accompanied the stationary waves caused by their reflection at the far end of the wire, and which displayed themselves to the eye in the dark by spaced-out brush-discharge luminosities, as well as in other more metrical ways. The arrangement was afterwards known by the name of Lecher, who made many measurements on this plan with the aid of vacuum tubes, as also did Dr. Dragoumis in my laboratory. So I summarize as follows :—

"In 1887 and 1888 I was working at the oscillatory discharge of Leyden jars (initially in connexion with the phenomena of lightning), and I then found that the waves could be not only produced but also detected, and the wavelength measured, by getting them to go along guiding wires adjusted so as to be of the right length for sympathetic resonance. Thus I obtained the phenomenon of electric nodes and loops, due to the production of stationary waves by reflection at the distant end, and in my own mind thus verified Maxwell's theory."

Transmission along wires popularly sounds different from transmission in free space, but it was well known to me that the process was the same, and that the waves travel at the same speed, being only guided by the wires, much as sound is guided in a speaking-tube, without the velocity of transmission being to any important extent altered. The theory is given near the end of my paper—an important one as I think, and as Silvanus Thompson agreed—in the *Philosophical Magazine* for August, 1888, where the experimental production of much shorter waves is also foreshadowed.

The beginning of my experiments was reported to the Society of Arts in April 1888; they are recorded in the *Phil. Mag.* for August 1888, and they were more completely described orally at the British Association at Bath that year. (See the *Electrician*, vol. 21, pp. 607–8, September, 1888.)

In that year, also, I heard for the first time of Hertz's brilliant series of experiments, where, by the use of an open-circuit oscillator, he had obtained waves in free space, and by reflection had also converted them into stationary waves and observed the phenomena of nodes and loops, and measured the wave-length.

Attention was directed to these experiments of Hertz by FitzGerald in his presidential address to Section A of the British Association meeting at Bath in 1888. No wonder they interested him; for they showed that his method of utilizing the oscillatory discharge of a Leyden jar was effective; and, to the surprise of all of us, including Hertz himself, that the waves from an opened-out condenser (or early insulated aerial) had sufficient power to generate sparks in an insulated conductor upon which they impinged; the detecting conductor, as generally used by Hertz, being in the form of a nearly closed circle with a minute spark gap at which the scintilla appeared. The radiating power of even a small Hertz oscillator was calculated by me in a subsequent paper (*Phil. Mag.* for July 1889, p. 54), and was found to be 100 horse-power, while it lasted. The duration was excessively short, for, at that date, practically all the energy was expended in a single swing (about the 100-millionth of a second), but its property of exciting little sparks was amply explained by its remarkable radiating power.

This work of Hertz was splendid. He was then professor at Carlsruhe, still quite a young man. He had been trained under Helmholtz; and I had made his personal acquaintance in Berlin when I went to call on Helmholtz in 1881, on a tour of the universities of the Continent. He was then Helmholtz's demonstrator, and was thought highly of by that great master. He could speak English, and was very friendly. I did not see him again till some time after the publication of his great discovery.

Hertz was not at that time fully acquainted with Maxwell's theory, though indeed he knew his equations better than any other German except Helmholtz. Maxwell had not then made any serious impression on the Continent. Even Hertz does not seem at first fully to have realized what he was doing, and did not use the words "electric waves." That title was attached to his subsequently translated book at the suggestion of Lord Kelvin. He spoke about "the out-spreading of electric force"; somewhat as Joseph Henry had done. That was the title of his book. He worked out the phenomena he observed with extraordinary skill, both experimentally and mathematically, rapidly perceiving that Maxwell's theory could be applied to it and that it might be elaborated in detail so as to include the whole of his phenomena. He it was who drew those accurate diagrams of the genesis of the waves, showing what is happening near the oscillator at every phase—diagrams which now appear in most text-books and of which the upper half is represented as scouring across the country when an aerial is earthed. He knew that true waves were not emitted till beyond a quarter-wave length from the source. He knew how they were polarized, and how their intensity differed in the equatorial and polar directions, and how it varied with

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what may be called latitude. In fact, he rapidly came to know all about these waves.

As to us, we knew not which to admire most — his experimental skill when working with a tiresome and irritating mode of detection—for he had nothing but a scintilla seen in the dark, as a detector—or his mathematical thoroughness in ascertaining the laws of the waves' generation and propagation. A synopsis of his equations will be found clearly cited in Preston's "Theory of Light," as well as in other books. I translated some of his papers into *Nature*. Never was there the smallest iota of jealousy between us, or anything but cordial and frank appreciation. Maxwell and Hertz are the essential founders of the whole system of wireless. That is to say, they constructed the foundations solidly and well. Of the superstructure—splendid as it is now—we are as yet far from seeing the completion.

In March 1889 I lectured to the Royal Institution on "The Oscillatory Discharge of a Leyden Jar," and incidentally exhibited many of the effects of waves, both on wires and in free space, with overflow and recoil effects. But there was nothing akin to *signalling* exhibited in this lecture, as there was in the subsequent lecture in 1894.

Nevertheless, Sir William Crookes, on the strength of these experiments—which he mentions—wrote a brilliant article in the *Fortnightly Review* for February, 1892 (vol. 51, p. 173) in which he foreshadows actual telegraphic accomplishment by that means, and indicates also the possibility of tuning or selective telegraphy, which was not actually born till 1897. He is evidently impressed with the experiments both of Hertz and of myself, and he quotes from my *Philosophical Magazine* paper of August 1888 in confirmation and illustration of his prevision. For he says—after speaking of choosing wave-length with which to signal to specific people—" This is no dream of a visionary philosopher. All the requisites needed to bring it within the grasp of daily life are well within the possibility of discovery, and are so reasonably and clearly in the path of researches now being actually prosecuted in every capital of Europe, that we may any day expect to hear they have emerged from the realm of speculation into that of sober fact." And then he goes on—evidently referring to the experiments of D. E. Hughes, at which he must have been present—" Even now indeed telegraphy without wires is possible within a restricted radius of a few hundred yards, and some years ago I assisted at experiments where messages

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were transmitted from one part of a house to another, without any intervening wire, by almost the identical means here described."

That article appeared in 1892, and was an inspiration of genius. Too little appreciation is felt to-day for the brilliant surmises and careful and conscientious observations of a great experimental worker like William Crookes; and on some of it orthodox science still turns its weighty and respectable back.

In 1889 I had come across the effect of cohesion under electric impetus, and employed it to ring a bell under the stimulus of the overflow of a Leyden jar, as described in my paper to the Institution of Electrical Engineers in 1890. What I had specifically called a "coherer" was a needle-point arrangement, or the end of a spiral spring touching an aluminium plate; which was and is extremely sensitive, but rather unmanageable. A crystal instead of an aluminium plate makes it all right, and it is now in constant use.

A less troublesome though similar method of detecting electric surgings was later discovered by Branly, in France, who was measuring the resistance of powdered metals, metallic smears on paper, and other finely divided substances. He found that the resistance of such smears varied capriciously, becoming much less

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when electric sparks were taken in the neighbourhood, but that a mechanical shock restored their high resistance. This was the same in essence as the coherer effect, and it was the origin of Branly's filings-tube, which, in one form or another, remained for some little time the standard method of detection, being employed by Popoff in Russia, Righi in Italy, Sir Henry Jackson, myself, and, in an improved form, Marconi. Branly's results were demonstrated in 1893 to the Physical Society of London by Dr. Dawson Turner, of Edinburgh, and thereafter optical experiments on Hertzian waves were rendered much easier than they had been.