

SCIENCE:

A WEEKLY NEWSPAPER OF ALL THE ARTS AND SCIENCES

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THE FARADAY CENTENARY.

On Wednesday, June 17, at the Royal Institution, London, Lord Rayleigh delivered a lecture in connection with the one hundredth anniversary of Faraday's birth. The following abstract of the lecture is from *Nature* of June 25.

Lord Rayleigh said that the man whose name and work they were celebrating was identified in a remarkable degree with the history of that institution. If they could not take credit for his birth, in other respects they could hardly claim too much. During a connection of fifty-four years, Faraday found there his opportunity, and for a large part of the time his home. The simple story of his life must be known to most who heard him. Fired by contact with the genius of Davy, he volunteered his services in the laboratory of the institution. Davy, struck with the enthusiasm of the youth, gave him the desired opportunity, and, as had been said, secured in Faraday not the least of his discoveries. The early promise was indeed amply fulfilled, and for a long period of years, by his discoveries in chemistry and electricity, Faraday maintained the renown of the Royal Institution and the honor of England in the eye of the civilized world. He should not attempt in the time at his disposal to trace in any detail the steps of that wonderful career. The task had already been performed by able hands. In their own "Proceedings" they had a vivid sketch from the pen of one whose absence that day was a matter of lively regret. Dr. Tyndall was a personal friend, had seen Faraday at work, had enjoyed opportunities of watching the action of his mind in face of a new idea. All that he could aim at was to recall, in a fragmentary manner, some of Faraday's great achievements, and if possible to estimate the position they held in contemporary science.

Whether they had regard to fundamental scientific import, or to practical results, the first place must undoubtedly be assigned to the great discovery of the induction of electrical currents. He proposed first to show the experiment in something like its original form, and then to pass on to some variations, with illustrations from the behavior of a model, whose mechanical properties were analogous. He was afraid that these elementary experiments would tax the patience of many who heard him, but it was one of the difficulties of his task that Faraday's discoveries were so fundamental as to have become familiar to all serious students of physics.

The first experiment required them to establish in one coil of copper wire an electric current by completing the communication with a suitable battery; that was called the primary circuit, and Faraday's discovery was, that, at the moment of the starting or stopping of the primary current, then, in a neighboring circuit, in the ordinary sense of the words, completely detached, there was a tendency to induce a current. He had said that those two circuits were perfectly distinct, and they were distinct in the sense that there was no conducting communication between them, but, of course, the importance of the experiment resided in this, — that it proved that in some sense the circuits were not distinct; that an electric current circulating in one does produce an effect in the other, which is propagated across a perfectly blank space occupied by air, and which might equally well have been occupied by vacuum. It might appear that that was a very simple and easy experiment, and of course it was so in a modern laboratory, but it was otherwise at the time when Faraday first made it. With all his skill, Faraday did not light upon truth without delay and difficulty. One of Faraday's biographers thus wrote: "In December, 1824, he had attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate and unsuccessful attempts to produce a current in one wire by means of a current in another wire, or by a magnet. He still persevered, and on August 29, 1831, — that is to say, nearly seven years after his first attempts, — he obtained the first evidence that an electric current induced another in a different circuit. On Sept. 23 he writes to a friend, 'I am busy just now again with electro-magnetism, and think I have got hold of a good thing, but cannot say; it may be a weed instead of a fish that, after all my labor, I at last haul up.'" We now know that it was a very big fish indeed.

About the time that the experiments of which he had been speaking were made, Faraday evidently felt uneasiness as to the soundness of the views about electricity held by his contemporaries, and to some extent shared by himself, and he made elaborate experiments to remove all doubt from his mind. He re-proved the complete identity of the electricity of lightning and of the electricity of the voltaic cell. He was evidently in terror of being misled by words which might convey a meaning beyond that which facts justified. Much use was made of the term "poles" of the galvanic battery. Faraday was afraid of the meaning which might be attached to the word "pole," and he introduced a word since generally substituted, "electrode," which meant nothing more than the way or path by which the electricity was led in. "Electric fluid" was a term which Faraday considered dangerous, as meaning more than they really knew about the nature of electricity, and, as was remarked by Maxwell, Faraday succeeded in banishing the term "electric fluid" to the region of newspaper paragraphs.

Diamagnetism was a subject upon which Faraday worked, but it would take too long to go into that subject, though a word or two must be said. Faraday found that whereas a ball of iron or nickel or cobalt, when placed near a magnet or combination of magnets, would be attracted to the place where the magnetic force was the greatest, the contrary occurred if for the iron was substituted a corresponding mass of bismuth or of many other substances. The experiments in diamagnetism were of a microscopic character, but he would like to illustrate one position of Faraday's, developed years afterwards by Sir William Thomson, and demonstrated by him in many beautiful experiments, only one of which he now proposed to bring before them. Supposing they had two magnetic poles, a north pole and a south pole, with an iron ball between them, free to move along a horizontal line perpendicular to that joining the poles, then, according to the rule he had stated, the iron ball would seek an intermediate position, the place at which the magnetic force was the greatest. Consequently, if the iron ball be given such a position, they would find it tended with considerable force to a central position of equilibrium; but if, instead of using opposite poles, they used, e.g., two north poles, they would find that the iron ball did not tend to the central position, because that was not the position in which the magnetic force was the greatest. At that position there was no magnetic force, for the one pole completely neutralized the action

of the other. The greatest force would be a little way out, and that, according to Faraday's observations, systematized and expressed in the form of mathematical law by Sir William Thomson, was where the ball would go.

The next discovery of Faraday to which the lecturer called attention was one of immense significance from a scientific point of view, the consequences of which were not even yet fully understood or developed. He referred to the magnetization of a ray of light, or what was called in more usual parlance the rotation of the plane of polarization under the action of magnetic force. It would be hopeless to attempt to explain all the preliminaries of the experiment to those who had not given some attention to those subjects before, and he could only attempt it in general terms. It would be known to most of them that the vibrations which constituted light were executed in a direction perpendicular to that of the ray of light. By experiment he showed that the polarization which was suitable to pass the first obstacle was not suitable to pass the second, but if by means of any mechanism they were able, after the light had passed the first obstacle, to turn round the vibration, they would then give it an opportunity of passing the second obstacle. That was what was involved in Faraday's discovery. As he had said, the full significance of the experiment was not yet realized. A large step towards realizing it, however, was contained in the observation of Sir William Thomson, that the rotation of the plane of polarization proved that something in the nature of rotation must be going on within the medium when subjected to the magnetizing force, but the precise nature of the rotation was a matter for further speculation, and perhaps might not be known for some time to come.

When first considering what to bring before them, the speaker thought, perhaps, he might include some of Faraday's acoustical experiments, which were of great interest, though they did not attract so much attention as his fundamental electrical discoveries. He would only allude to one point which, as far as he knew, had never been noticed, but which Faraday recorded in his acoustical papers. "If during a strong steady wind, a smooth flat sandy shore, with enough water on it, either from the receding tide or from the shingle above, to cover it thoroughly, but not to form waves, be observed in a place where the wind is not broken by pits or stones, stationary undulations will be seen over the whole of the wet surface. . . . These are not waves of the ordinary kind; they are (and this is the remarkable point) accurately parallel to the course of the wind." When he first read that statement, many years ago, he was a little doubtful as to whether to accept the apparent meaning of Faraday's words. He knew of no suggestion of an explanation of the possibility of waves of that kind being generated under the action of the wind, and it was, therefore, with some curiosity that two or three years ago, at a French watering-place, he went out at low tide, on a suitable day when there was a good breeze blowing, to see if he could observe anything of the waves described by Faraday. For some time he failed absolutely to observe the phenomenon, but after a while he was perfectly well able to recognize it. He mentioned that as an example of Faraday's extraordinary powers of observation, and even now he doubted whether anybody but himself and Faraday had ever seen that phenomenon.

Many matters of minor theoretic interest were dealt with by Faraday, and reprinted by him in his collected works. The speaker was reminded of one the other day by a lamentable accident which occurred owing to the breaking of a paraffin lamp. Faraday called attention to the fact, though he did not suppose he was the first to notice it, that, by a preliminary preparation of the lungs by a number of deep inspirations and expirations, it was possible so to aerate the blood as to allow of holding the breath for a much longer period than without such a preparation would be possible. He remembered some years ago trying the experiment, and running up from the drawing-room to the nursery of a large house without drawing any breath. That was obviously of great practical importance, as Faraday pointed out, in the case of danger from suffocation by fire, and he thought that possibly the accident to which he alluded might have been spared had the knowledge of the fact to which Faraday drew attention been more generally diffused.

The question had often been discussed as to what would have been the effect upon Faraday's career of discovery had he been subjected in early life to mathematical training. The first thing that occurred to him about that, after reading Faraday's works, was that one would not wish him to be anything different from what he was. If the question must be discussed, he supposed they would have to admit that he would have been saved much wasted labor, and would have been better *en rapport* with his scientific contemporaries if he had had elementary mathematical instruction. But mathematical training and mathematical capacity were two different things, and it did not at all follow that Faraday had not a mathematical mind. Indeed, some of the highest authorities (and there could be no higher authority on the subject than Maxwell) had held that his mind was essentially mathematical in its qualities, although they must admit it was not developed in a mathematical direction. With these words of Maxwell he would conclude: "The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of electric induction shows him to have been a mathematician of high order, and one from whom the mathematicians of the future may derive valuable and fertile methods."

THE "SUBMARINE SENTRY."

At a recent meeting of the Royal United Service Institution, London, a lecture upon sounding machines was given by Professor Lambert of the Royal Naval College, Greenwich. In the course of the lecture (some details of which appear in *Engineering* of June 26) a description was given of an instrument called a "submarine sentry," which has been successfully experimented with on some ships of the British navy. It is the invention of Mr. Samuel James, a civil engineer.

As described by the lecturer, the sentry is intended to give a continuous under-water look-out, and to automatically give warning of the approach of shallow water. It consists of an inverted wooden kite, which can be trailed at the stern of a vessel at any required depth to forty-five fathoms. On striking bottom, the blow, acting on a projecting trigger, releases the slings of the kite and causes it to rise to the surface and trail in the wake of the vessel. At the instant of striking, the sudden loss of tension in the wire sounds a gong attached to a winch on board the ship. The wire used is of steel, and of the highest tenacity attainable. Its diameter is 0.067 of an inch, and it is capable of bearing a stress of fully a thousand pounds. During towing the vibration of the wire causes a continuous rattle in a sounding box, and the cessation of this noise gives an additional indication when the sentry has struck the bottom. The vertical depth of the kite at any time is indicated on the dial plate of the winch. The curve formed by the wire while towing is concave downwards, and at first sight it would appear as if this curve would change its form, and the sinker trail further astern and at less depth when the ship's speed was increased. Professor Lambert had carefully plotted out this curve, and showed the results on a diagram. By a mathematical analysis he showed that the instrument would remain constant in its record at any speed of the ship between five and fifteen knots. The weight of the kite is equal to, and is therefore neutralized by, its own buoyancy, and the weight of the wire is negligible compared to the forces due to the motion through the water.

The forces which remain to be considered are, (1) the fluid pressure on the kite, (2) the fluid pressure on the under side of the wire, and (3) the tension of the wire. The latter is the result of the two former. Pressures due to fluid motion vary nearly as the square of the velocity. If, therefore, the velocity of the ship be doubled, forces (1) and (2) will each be multiplied by four, the three forces will all be changed proportionally, and there will be no change in the direction in which they act. This is only put forward as a rough explanation of the phenomena, but that it is practically true has been, it is claimed, corroborated by practical tests, — the depth of the sinker not varying more than half a fathom in thirty at speeds of from five to thirteen knots, above which speed the instrument is not designed to be used.

There are two descriptions of kite, one set at an angle to give