Michael Faraday

from *Faraday as a Discoverer*

It has been thought desirable to give you and the world some image of Michael Faraday as a scientific investigator and discoverer. The attempt to respond to this desire has been to me a labour of difficulty, if also a labour of love. For however well acquainted I may be with the researches and discoveries of that great master, however numerous the illustrations which occur to me of the loftiness of Faraday's character and the beauty of his life—still to grasp him and his researches as a whole; to seize upon the ideas which guided him, and connected them; to gain entrance into that strong and active brain, and read from it the riddle of the world—this is a work not easy of performance, and all but impossible amid the distraction of duties of another kind. That I should at one period or another speak to you regarding Faraday and his work is natural, if not inevitable; but I did not expect to be called upon to speak so soon. Still the bare suggestion that this is the fit and proper time for speech sent me immediately to my task; from it I have returned with such results as I could gather, and also with the wish that those results were more worthy than they are of the greatness of my theme.

It is not my intention to lay before you a *life* of Faraday in the ordinary acceptation of the term. The duty I have to perform is to give you some notion of what he has done in the world, dwelling incidentally on the spirit in which his work was executed, and introducing such personal traits as may be necessary to the completion of your picture of the *philosopher,* though by no means adequate to give you a complete idea of the *man.*

The newspapers have already informed you that Michael Faraday was born at Newington Butts, on September 22, 1791, and that he died at Hampton Court, on August 25, 1867. Believing, as I do, in the general truth of the doctrine of hereditary transmission—sharing the opinion of

Mr. Carlyle, that "a really able man never proceeded from entirely stupid parents"—I once used the privilege of my intimacy with Mr. Faraday to ask him whether his parents showed any signs of unusual ability. He could remember none. His father, I believe, was a great sufferer during the latter years of his life, and this might have masked whatever intellectual power he possessed. When thirteen years old, that is to say in 1804, Faraday was apprenticed to a bookseller and bookbinder in Blandford Street, Manchester Square; here he spent eight years of his life, after which he worked as a journeyman elsewhere.

You have also heard the account of Faraday's first contact with the Royal Institution; that he was introduced by one of the members to Sir Humphry Davy's last lectures, that he took notes of those lectures, wrote them fairly out, and sent them to Davy, entreating him at the same time to enable him to quit trade, which he detested, and to pursue science, which he loved. Davy was helpful to the young man, and this should never be forgotten; he at once wrote to Faraday, and afterwards, when an opportunity occurred, made him his assistant.[1] Mr. Gassiot has lately favoured me with the following reminiscence of this time:

Clapham Common, Surrey
November 28, 1867

My Dear Tyndall—Sir H. Davy was accustomed to call on the late Mr. Pepys, in the Poultry, on his way to the London Institution, of which Pepys was one of the original managers. The latter told me that on one occasion Sir H. Davy, showing him a letter, said: "Pepys, what am I to do, here is a letter from a young man named Faraday; he has been attending my lectures, and wants me to give him employment at the Royal Institution. What can I do?" "Do?" replied Pepys, "put him to wash bottles; if he is good for anything he will do it directly, if he refuses he is good for nothing." "No, no," replied Davy, "we must try him with something better than that." The result was that Davy engaged him to assist in the Laboratory at *weekly* wages.

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1. Here is Davy's recommendation of Faraday, presented to the managers of the Royal Institution, at a meeting on the 18th of March, 1813, Charles Hatchett, Esq., in the chair:

"Sir Humphry Davy has the honour to inform the managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good; his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as given to Mr. Payne at the time of quitting the Institution.

"*Resolved*—That Michael Faraday be engaged to fill the situation lately occupied by Mr. Payne, on the same terms."

Davy held the joint office of Professor of Chemistry and Director of the Laboratory; he ultimately gave up the former to the late Professor Brande, but he insisted that Faraday should be appointed Director of the Laboratory, and, as Faraday told me, this enabled him on subsequent occasions to hold a definite position in the Institution, in which he was always supported by Davy. I believe he held that office to the last.

Believe me, my dear Tyndall, yours truly,

J. P. Gassiot

From a letter written by Faraday himself soon after his appointment as Davy's assistant, I extract the following account of his introduction to the Royal Institution:

London, Sept. 13, 1813

As for myself, I am absent (from home) nearly day and night, except occasional calls, and it is likely shall shortly be absent entirely, but this (having nothing more to say, and at the request of my mother) I will explain to you. I was formerly a bookseller and binder, but am now turned philosopher,[2] which happened thus: Whilst an apprentice, I, for amusement, learnt a little chemistry and other parts of philosophy, and felt an eager desire to proceed in that way further. After being a journeyman for six months, under a disagreeable master, I gave up my business, and through the interest of a Sir H. Davy, filled the situation of chemical assistant to the Royal Institution of Great Britain, in which office I now remain; and where I am constantly employed in observing the works of nature, and tracing the manner in which she directs the order and arrangement of the world. I have lately had proposals made to me by Sir Humphry Davy to accompany him in his travels through Europe and Asia, as philosophical assistant. If I go at all I expect it will be in October next—about the end; and my absence from home will perhaps be as long as three years. But as yet all is uncertain.

This account is supplemented by the following letter, written by Faraday to his friend De la Rive,[3] on the occasion of the death of Mrs. Marcet. The letter is dated September 2, 1858:

My Dear Friend—Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of thirteen, in the year 1804, remained there eight years, and during the chief part of my time bound books. Now it was in those books, in the hours after work, that I found the beginning of my philosophy. There

2. Faraday loved this word and employed it to the last; he had an intense dislike to the modern term *physicist*.
3. To whom I am indebted for a copy of the original letter.

> were two that especially helped me, the *Encyclopædia Britannica,* from which I gained my first notions of electricity, and Mrs. Marcet's *Conversation on Chemistry,* which gave me my foundation in that science.
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> Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a very lively imaginative person, and could believe in the *Arabian Nights* as easily as in the *Encyclopædia.* But facts were important to me, and saved me. I could trust a fact, and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first as one who had conferred great personal good and pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught, and inquiring mind.
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> You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backward, delighting to connect the past and the present; how often, when sending a paper to her as a thank offering, I thought of my first instructress, and such like thoughts will remain with me.
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> I have some such thoughts even as regards *your own father;* who was, I may say, the first who personally at Geneva, and afterwards by correspondence, encouraged, and by that sustained me.

Twelve or thirteen years ago Mr. Faraday and myself quitted the Institution one evening together, to pay a visit to our friend Grove in Baker Street. He took my arm at the door, and, pressing it to his side in his warm genial way, said, "Come, Tyndall, I will now show you something that will interest you." We walked northwards, passed the house of Mr. Babbage, which drew forth a reference to the famous evening parties once assembled there. We reached Blandford Street, and after a little looking about he paused before a stationer's shop, and then went in. On entering the shop, his usual animation seemed doubled; he looked rapidly at everything it contained. To the left on entering was a door, through which he looked down into a little room, with a window in front facing Blandford Street. Drawing me towards him, he said eagerly, "Look there, Tyndall, that was my working place. I bound books in that little nook." A respectable-looking woman stood behind the counter: his conversation with me was too low to be heard by her, and he now turned to the counter to buy some cards as an excuse for our being there. He asked the woman her name—her predecessor's name—his predecessor's name. "That won't do," he said, with good-humoured impatience; "who was *his* predecessor?" "Mr. Riebau," she replied, and immediately added, as if suddenly recollecting herself, "He, sir, was the master of Sir Charles Faraday." "Nonsense!" he responded, "there is no such person." Great was her delight when I told her the name of her visitor; but she assured me that as soon as she saw him running about the shop, she felt—though she did not know why—that it must be "Sir Charles Faraday."

Faraday did, as you know, accompany Davy to Rome; he was re-engaged by the managers of the Royal Institution on May 15, 1815. Here he made rapid progress in chemistry, and after a time was entrusted with easy analyses by Davy. In those days the Royal Institution published *The Quarterly Journal of Science,* the precursor of our own *Proceedings.* Faraday's first contribution to science appeared in that journal in 1816. It was an analysis of some caustic lime from Tuscany, which had been sent to Davy by the Duchess of Montrose. Between this period and 1818 various notes and short papers were published by Faraday. In 1818 he experimented upon "sounding flames." Professor Auguste De la Rive had investigated those sounding flames, and had applied to them an explanation which completely accounted for a class of sounds discovered by himself, but did not account for those known to his predecessors. By a few simple and conclusive experiments, Faraday proved the explanation insufficient. It is an epoch in the life of a young man when he finds himself correcting a person of eminence, and in Faraday's case, where its effect was to develop a modest self-trust, such an event could not fail to act profitably.

From time to time between 1818 and 1820 Faraday published scientific notes and notices of minor weight. At this time he was acquiring, not producing; working hard for his master and storing and strengthening his own mind. He assisted Mr. Brande in his lectures, and so quietly, skilfully, and modestly was his work done, that Mr. Brande's vocation at the time was pronounced "lecturing on velvet." In 1820 Faraday published a chemical paper "on two new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen." This paper was read before the Royal Society on December 21, 1820, and it was the first of his that was honoured with a place in the *Philosophical Transactions.*

On June 12, 1821, he married, and obtained leave to bring his young wife into his rooms at the Royal Institution. There for forty-six years they lived together, occupying the suite of apartments which had been previously in the successive occupancy of Young, Davy, and Brande. At the time of her marriage Mrs. Faraday was twenty-one years of age, he being nearly thirty. Regarding this marriage I will at present limit myself to quoting an entry written in Faraday's own hand in his book of diplomas, which caught my eye while in his company some years ago. It ran thus:

> *25th January, 1847*
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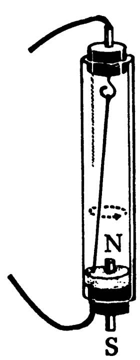
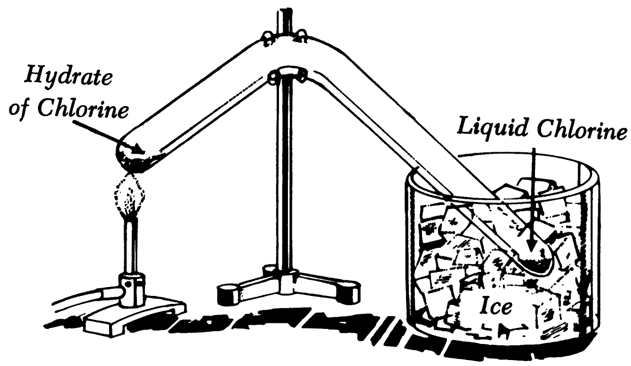
> Amongst these records and events, I here insert the date of one which, as a source of honour and happiness, far exceeds all the rest. We were *married* on June 12, 1821.
>
> *M. Faraday*

Then follows the copy of the minutes, dated May 21, 1821, which gave him additional rooms, and thus enabled him to bring his wife to the Royal Institution. A feature of Faraday's character which I have often noticed makes itself apparent in this entry. In his relations to his wife he added *chivalry* to affection.

Oersted, in 1820, discovered the action of a voltaic current on a magnetic needle; and immediately afterwards the splendid intellect of Ampère succeeded in showing that every magnetic phenomenon then known might be reduced to the mutual action of electric currents. The subject occupied all men's thought; and in this country Dr. Wollaston sought to convert the deflection of the needle by the current into a permanent *rotation* of the needle round the current. He also hoped to produce the reciprocal effect of causing a current to rotate round a magnet. In the early part of 1821, Wollaston attempted to realize this idea in the presence of Sir Humphry Davy in the laboratory of the Royal Institution. This was well calculated to attract Faraday's attention to the subject. He read much about it; and in the months of July, August, and September he wrote a *History of the Progress of Electromagnetism,* which he published in Thomson's *Annals of Philosophy.* Soon afterwards he took up the subject of "magnetic rotations," and on the morning of Christmas Day, 1821, he called his wife to witness, for the first time, the revolution of a mag-



Faraday's electromagnetic rotation apparatus, designed in January, 1822. It was made of a piece of glass tubing, with stoppers at each end. The lower stopper carried a magnet and a pool of mercury. A loose piece of platinum wire, hung by a loop from the top connection, revolved rapidly around the magnet pole when the outside current was connected to the wire terminals of the apparatus.



Faraday's apparatus for liquefying the gas chlorine produced the gas by heating the hydrate. Both chlorine and water vapour were formed, thus giving pressure within the tube. At the right the tube was cooled with ice. The combination of pressure with cooling changed the chlorine to a liquid.

netic needle round an electric current. Incidental to the "historic sketch," he repeated almost all the experiments there referred to; and these, added to his own subsequent work, made him practical master of all that was then known regarding the voltaic current. In 1821, he also touched upon a subject which subsequently received his closer attention—the vaporization of mercury at common temperatures; and immediately afterwards conducted, in company with Mr. Stodart, experiments on the alloys of steel. He was accustomed in after years to present to his friends razors formed from one of the alloys then discovered.

During Faraday's hours of liberty from other duties, he took up subjects of inquiry for himself; and in the spring of 1823, thus self-prompted, he began the examination of a substance which had long been regarded as the chemical element chlorine, in a solid form, but which Sir Humphry Davy, in 1810, had proved to be a hydrate of chlorine, that is, a compound of chlorine and water. Faraday first analyzed this hydrate, and wrote out an account of its composition. This account was looked over by Davy, who suggested the heating of the hydrate under pressure in a sealed glass tube. This was done. The hydrate fused at a blood heat, the tube became filled with a yellow atmosphere, and was afterwards found to contain two liquid substances. Dr. Paris happened to enter the laboratory while Faraday was at work. Seeing the oily liquid in his tube, he rallied the young chemist for his carelessness in employing soiled vessels. On filing off the end of the tube, its contents exploded and the oily matter vanished. Early next morning, Dr. Paris received the following note:

Dear Sir—The *oil* you noticed yesterday turns out to be liquid chlorine.

Yours faithfully,
M. Faraday

The gas had been liquefied by its own pressure. Faraday then tried compression with a syringe, and succeeded thus in liquefying the gas.

To the published account of this experiment Davy added the following note: "In desiring Mr. Faraday to expose the hydrate of chlorine in a closed glass tube, it occurred to me that one of three things would happen: that decomposition of water would occur; . . . or that the chlorine would separate in a fluid state." Davy, moreover, immediately applied the method of self-compressing atmosphere to the liquefaction of muriatic gas. Faraday continued the experiments, and succeeded in reducing a number of gases till then deemed permanent to the liquid condition. In 1844 he returned to the subject, and considerably expanded its limits. These important investigations established the fact that gases are but the vapours of liquids possessing a very low boiling point, and gave a sure basis to our views of molecular aggregation. The account of the first investigation was read before the Royal Society on April 10, 1823, and was published, in Faraday's name, in the *Philosophical Transactions*. The second memoir was sent to the Royal Society on December 19, 1844. I may add that while he was conducting his first experiments on the liquefaction of gases, thirteen pieces of glass were on one occasion driven by an explosion into Faraday's eye.

Some small notices and papers, including the observation that glass readily changes colour in sunlight, follow here. In 1825 and 1826 Faraday published papers in the *Philosophical Transactions* on "new compounds of carbon and hydrogen," and on "sulphonaphthalic acid." In the former of these papers he announced the discovery of benzol,[4] which, in the hands of modern chemists, has become the foundation of our splendid aniline dyes. But he swerved incessantly from chemistry into physics; and in 1826 we find him engaged in investigating the limits of vaporization, and showing, by exceedingly strong and apparently conclusive arguments, that even in the case of mercury such a limit exists; much more he conceived it to be certain that our atmosphere does not contain the vapour of the fixed constituents of the earth's crust. This question, I may say, is likely to remain an open one. Dr. Rankine, for example, has lately

4. *Benzol.* A colourless liquid obtained by the distillation of coal tar; now known chemically as benzene, C_6H_6.

drawn attention to the odour of certain metals; whence comes this odour, if it be not from the vapour of the metal?

In 1825 Faraday became a member of a committee, to which Sir John Herschel and Mr. Dollond also belonged, appointed by the Royal Society to examine, and if possible improve, the manufacture of glass for optical purposes. Their experiments continued till 1829, when the account of them constituted the subject of a "Bakerian Lecture." This lectureship, founded in 1774 by Henry Baker, Esq., of the Strand, London, provides that every year a lecture shall be given before the Royal Society, the sum of four pounds being paid to the lecturer. The Bakerian Lecture, however, has long since passed from the region of pay to that of honour, papers of mark only being chosen for it by the council of the Society. Faraday's first Bakerian Lecture, "On the Manufacture of Glass for Optical Purposes," was delivered at the close of 1829. It is a most elaborate and conscientious description of processes, precautions, and results; the details were so exact and so minute, and the paper consequently so long, that three successive sittings of the Royal Society were taken up by the delivery of the lecture. This glass did not turn out to be of important practical use,[5] but it happened afterwards to be the foundation of two of Faraday's greatest discoveries.

The experiments here referred to were commenced at the Falcon Glass Works, on the premises of Messrs. Green and Pellatt, but Faraday could not conveniently attend to them there. In 1827, therefore, a furnace was erected in the yard of the Royal Institution; and it was at this time, and with a view of assisting him at the furnace, that Faraday engaged Sergeant Anderson, of the Royal Artillery, the respectable, truthful, and altogether trustworthy man whose appearance here is so fresh in our memories. Anderson continued to be the reverential helper of Faraday and the faithful servant of this Institution for nearly forty years.[6]

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5. I make the following extract from a letter from Sir John Herschel, written to me from Collingwood, on the 3rd of November, 1867:

> I will take this opportunity to mention that I believe myself to have originated the suggestion of the employment of borate of lead for *optical* purposes. It was somewhere in the year 1822, as well as I can recollect, that I mentioned it to Sir James (then Mr.) South; and, in consequence, the trial was made in his laboratory in Blackman Street, by precipitating and working a large quantity of borate of lead, and fusing it under a muffle in a porcelain evaporating dish. A very limpid (though slightly yellow) glass resulted, the refractive index 1.866! (which you will find set down in my table of refractive indices in my article "Light," *Encyclopædia Metropolitana*). It was, however, too soft for optical use as an object glass. This Faraday overcame, at least to a considerable degree, by the introduction of silica.

6. Regarding Anderson, Faraday writes thus in 1845: "I cannot resist the occasion

In 1831 Faraday published a paper, "On a Peculiar Class of Optical Deceptions," to which I believe the beautiful optical toy called the chromatrope owes its origin. In the same year he published a paper on vibrating surfaces, in which he solved an acoustical problem which, though of extreme simplicity *when solved*, appears to have baffled many eminent men. The problem was to account for the fact that light bodies, such as the seed of lycopodium, collected at the vibrating parts of sounding plates, while sand ran to the nodal lines. Faraday showed that the light bodies were entangled in the little whirlwinds formed in the air over the places of vibration, and through which the heavier sand was readily projected. Faraday's resources as an experimentalist were so wonderful, and his delight in experiment was so great, that he sometimes almost ran into excess in this direction. I have heard him say that this paper on vibrating surfaces was too heavily laden with experiments.

The work thus referred to, though sufficient of itself to secure no mean scientific reputation, forms but the vestibule of Faraday's achievements. He had been engaged within these walls for eighteen years. During part of the time he had drunk in knowledge from Davy, and during the remainder he continually exercised his capacity for independent inquiry. In 1831 we have him at the climax of his intellectual strength, forty years of age, stored with knowledge and full of original power. Through reading, lecturing, and experimenting, he had become thoroughly familiar with electrical science; he saw where light was needed and expansion possible. The phenomena of ordinary electric induction belonged, as it were, to the alphabet of his knowledge; he knew that under ordinary circumstances the presence of an electrified body was sufficient to excite, by induction, an unelectrified body. He knew that the wire which carried an electric current was an electrified body, and still that all attempts had failed to make it excite in other wires a state similar to its own.

What was the reason of this failure? Faraday never could work from the experiments of others, however clearly described. He knew well that from every experiment issues a kind of radiation, luminous in different degrees in different minds, and he hardly trusted himself to reason upon

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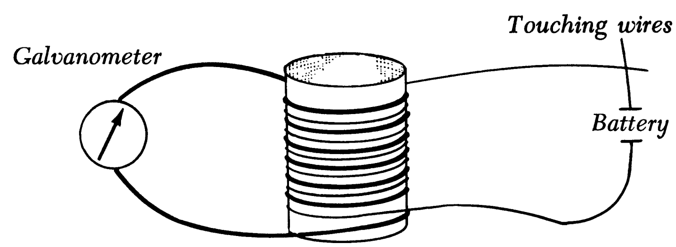
that is thus offered to me of mentioning the name of Mr. Anderson, who came to me as an assistant in the glass experiments, and has remained ever since in the laboratory of the Royal Institution. He assisted me in all the researches into which I have entered since that time; and to his care, steadiness, exactitude, and faithfulness in the performance of all that has been committed to his charge, I am much indebted.—M. F." [See *Experimental Researches, Great Books of the Western World* (Ed.).]

an experiment that he had not seen. In the autumn of 1831 he began to repeat the experiments with electric currents, which, up to that time, had produced no positive result. And here, for the sake of younger inquirers, if not for the sake of us all, it is worth while to dwell for a moment on a power which Faraday possessed in an extraordinary degree. He united vast strength with perfect flexibility. His momentum was that of a river, which combines weight and directness with the ability to yield to the flexures of its bed. The intentness of his vision in any direction did not apparently diminish his power of perception in other directions; and when he attacked a subject, expecting results, he had the faculty of keeping his mind alert, so that results different from those which he expected should not escape him through preoccupation.

He began his experiments on the induction of electric currents by composing a helix of two insulated wires which were wound side by side round the same wooden cylinder. One of these wires he connected with a voltaic battery of ten cells, and the other with a sensitive galvanometer. When connection with the battery was made, and while the current flowed, no effect whatever was observed at the galvanometer. But he never accepted an experimental result until he had applied to it the utmost power at his command. He raised his battery from 10 cells to 120 cells, but without avail. The current flowed calmly through the battery wire without producing, during its flow, any sensible result upon the galvanometer.

"During its flow," and this was the time when an effect was expected—but here Faraday's power of lateral vision, separating, as it were, from the line of expectation, came into play. He noticed that a feeble move-



Two completely independent coils of insulated wire were wound side by side on a wooden cylinder. One coil was connected to a voltaic battery, the current flowing when the battery wires were touched together. The other coil was connected to a sensitive galvanometer. At the moment that the battery wires were touched, the galvanometer moved, though the circuits were separate.

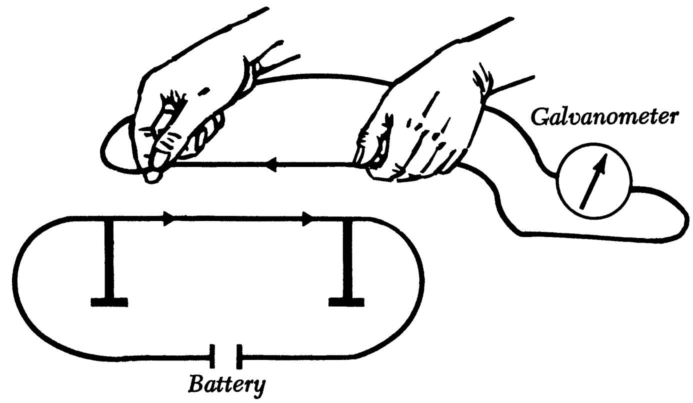
ment of the needle always occurred at the moment when he made contact with the battery; that the needle would afterwards return to its former position and remain quietly there unaffected by the *flowing* current. At the moment, however, when the circuit was interrupted the needle again moved, and in a direction opposed to that observed on the completion of the circuit.

This result, and others of a similar kind, led him to the conclusion that the battery current through the one wire did in reality induce a similar current through the other; but that it continued for an instant only, and partook more of the nature of the electric wave from a common Leyden jar than of the current from a voltaic battery. The momentary currents thus generated were called *induced* currents, while the current which generated them was called the *inducing* current. It was immediately proved that the current generated at making the circuit was always opposed in direction to its generator, while that developed on the rupture of the circuit coincided in direction with the inducing current. It appeared as if the current on its first rush through the primary wire sought a purchase in the secondary one, and, by a kind of kick, impelled backward through the latter an electric wave, which subsided as soon as the primary current was fully established.

Faraday, for a time, believed that the secondary wire, though quiescent when the primary current had been once established, was not in its natural condition, its return to that condition being declared by the current observed at breaking the circuit. He called this hypothetical state of the wire the *electrotonic state;* he afterwards abandoned this hypothesis, but seemed to return to it in later life. The term electrotonic is also preserved by Professor Du Bois Reymond to express a certain electric condition of the nerves, and Professor Clerk Maxwell has ably defined and illustrated the hypothesis in the tenth volume of the *Transactions of the Cambridge Philosophical Society.*

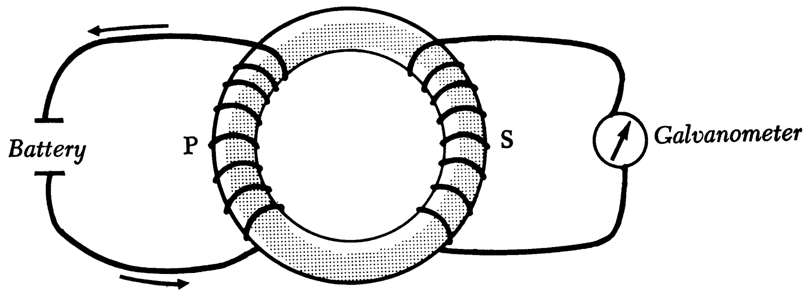
The mere approach of a wire forming a closed curve to a second wire through which a voltaic current flowed was then shown by Faraday to be sufficient to arouse in the neutral wire an induced current, opposed in direction to the inducing current; the withdrawal of the wire also generated a current having the same direction as the inducing current; those currents existed only during the time of approach or withdrawal, and when neither the primary nor the secondary wire was in motion, no matter how close their proximity might be, no induced current was generated.

Faraday has been called a purely inductive philosopher. A great deal



An induced current was produced when a wire forming a closed circuit through a galvanometer was brought near a wire carrying a voltaic current. The direction of the induced current was opposed to that of the inducing current.

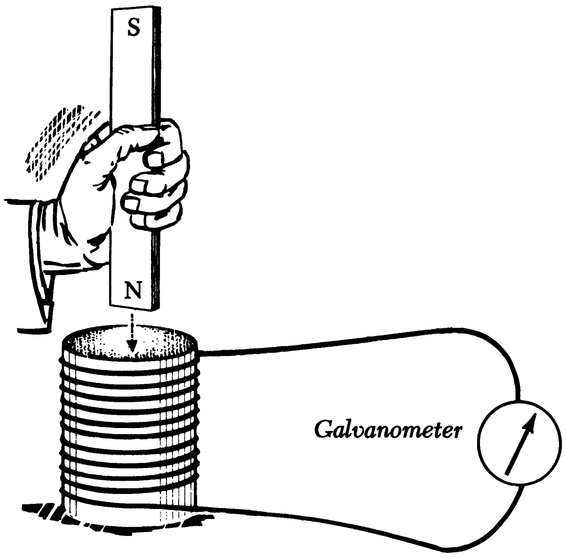
of nonsense is, I fear, uttered in this land of England about induction and deduction. Some profess to befriend the one, some the other, while the real vocation of an investigator, like Faraday, consists in the incessant marriage of both. He was at this time full of the theory of Ampère, and it cannot be doubted that numbers of his experiments were executed merely to test his deductions from that theory. Starting from the discovery of Oersted, the illustrious French philosopher had shown that all the phenomena of magnetism then known might be reduced to the mutual attractions and repulsions of electric currents. Magnetism had been produced from electricity, and Faraday, who all his life long entertained a strong belief in such reciprocal actions, now attempted to effect the evolution of electricity from magnetism. Round a welded iron ring he placed two distinct coils of covered wire, causing the coils to occupy opposite halves of the ring. Connecting the ends of one of the coils with a galvanometer, he found that the moment the ring was magnetized, by sending a current through *the other coil,* the galvanometer needle whirled round four or five times in succession. The action, as before, was that of a pulse, which vanished immediately. On interrupting the circuit, a whirl of the needle in the opposite direction occurred. It was only during the time of magnetization or demagnetization that these effects were produced. The induced currents declared a *change* of condition only, and they vanished the moment the act of magnetization or demagnetization was complete.



Two coils, P *and* S, *were wound on an iron ring. The primary coil,* P, *was connected to a battery; the secondary coil,* S, *to a galvanometer. When a current was started in* P, *a current was induced in* S *in a direction opposite to that in* P. *With the current flowing in* P, *there was no current in* S. *When the current in* P *was interrupted, there was a reverse current in* S.

The effects obtained with the welded ring were also obtained with straight bars of iron. Whether the bars were magnetized by the electric current, or were excited by the contact of permanent steel magnets, induced currents were always generated during the rise, and during the subsidence of the magnetism. The use of iron was then abandoned, and the same effects were obtained by merely thrusting a permanent steel magnet into a coil of wire. A rush of electricity through the coil accompanied the insertion of the magnet; an equal rush in the opposite direction accompanied its withdrawal. The precision with which Faraday describes these results, and the completeness with which he defines the boundaries of his facts, are wonderful. The magnet, for example, must not be passed quite through the coil, but only half through; for if passed wholly through, the needle is stopped as by a blow, and then he shows how this blow results from a reversal of the electric wave in the helix. He next operated with the powerful permanent magnet of the Royal Society, and obtained with it, in an exalted degree, all the foregoing phenomena.

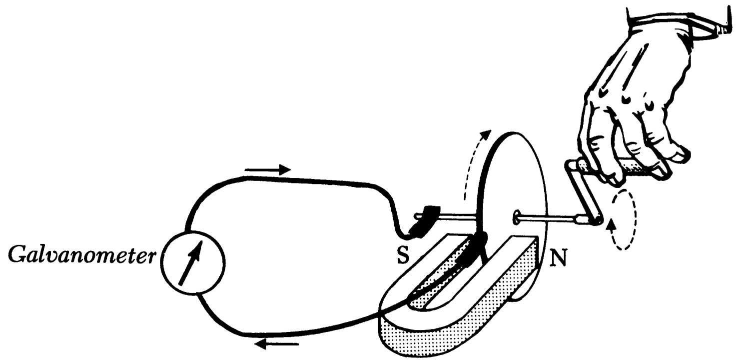
And now he turned the light of these discoveries upon the darkest physical phenomenon of that day. Arago had discovered, in 1824, that a disk of nonmagnetic metal had the power of bringing a vibrating magnetic needle suspended over it rapidly to rest; and that on causing the disk to rotate the magnetic needle rotated along with it. When both were quiescent, there was not the slightest measurable attraction or repulsion exerted between the needle and the disk; still, when in motion the disk was competent to drag after it not only a light needle, but a heavy magnet. The question had been probed and investigated with admirable skill



An induced current was produced by the mere motion of a permanent steel magnet in a coil of wire. A rush of electricity through the coil occurred when the magnet was inserted. An equal rush in the opposite direction occurred when the magnet was withdrawn.

by both Arago and Ampère, and Poisson had published a theoretic memoir on the subject; but no cause could be assigned for so extraordinary an action. It had also been examined in this country by two celebrated men, Mr. Babbage and Sir John Herschel; but it still remained a mystery. Faraday always recommended the suspension of judgment in cases of doubt. "I have always admired," he said, "the prudence and philosophical reserve shown by M. Arago in resisting the temptation to give a theory of the effect he had discovered, so long as he could not devise one which was perfect in its application, and in refusing to assent to the imperfect theories of others." Now, however, the time for theory had come. Faraday saw mentally the rotating disk, under the operation of the magnet, flooded with his induced currents, and from the known laws of interaction between currents and magnets he hoped to deduce the motion observed by Arago. That hope he realized, showing by actual experiment that, when his disk rotated, currents passed through it, their position and direction being such as must, in accordance with the established laws of electromagnetic action, produce the observed rotation.

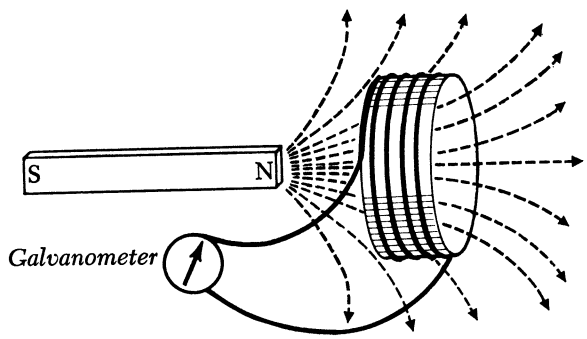
Introducing the edge of his disk between the poles of the large horseshoe magnet of the Royal Society, and connecting the axis and the edge



Faraday's electrical generator to produce a steady current by rotating a copper disk between the poles of a magnet.

of the disk, each by a wire with a galvanometer, he obtained, when the disk was turned round, a constant flow of electricity. The direction of the current was determined by the direction of the motion, the current being reversed when the rotation was reversed. He now states the law which rules the production of currents in both disks and wires, and in so doing used, for the first time, a phrase which has since become famous. When iron filings are scattered over a magnet, the particles of iron arrange themselves in certain determinate lines called magnetic curves. In 1831, Faraday for the first time called these curves "lines of magnetic force"; and he showed that to produce induced currents neither approach to nor withdrawal from a magnetic source, or center, or pole, was essential, but that it was only necessary to cut appropriately the lines of magnetic force. Faraday's first paper on magnetoelectric induction, which I have here endeavoured to condense, was read before the Royal Society on the 24th of November, 1831.

On January 12, 1832, he communicated to the Royal Society a second paper on terrestrial magnetoelectric induction, which was chosen as the Bakerian Lecture for the year. He placed a bar of iron in a coil of wire, and lifting the bar into the direction of the dipping needle, he excited by this action a current in the coil. On reversing the bar, a current in the opposite direction rushed through the wire. The same effect was produced when, on holding the helix in the line of dip, a bar of iron was thrust into it. Here, however, the earth acted on the coil through the intermediation of the bar of iron. He abandoned the bar and simply set a copper plate spinning in a horizontal plane; he knew that the earth's lines of magnetic force then crossed the plate at an angle of about 70°.

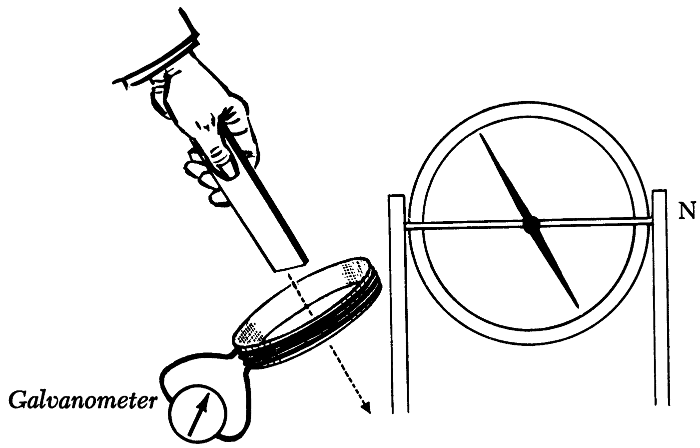


Lines of magnetic force pass out of the north pole of a magnet, and in at the south pole. To produce induced currents in a coil it was only necessary to cut appropriately the lines of magnetic force. For the arrangement shown, the coil may be spun around on its vertical axis or drawn back and forth across the end of the magnet.

When the plate spun round, the lines of force were intersected and induced currents generated, which produced their proper effect when carried from the plate to the galvanometer. "When the plate was in the magnetic meridian, or in any other plane coinciding with the magnetic dip, then its rotation produced no effect upon the galvanometer."

At the suggestion of a mind fruitful in suggestions of a profound and philosophic character—I mean that of Sir John Herschel—Mr. Barlow, of Woolwich, had experimented with a rotating iron shell. Mr. Christie had also performed an elaborate series of experiments on a rotating iron disk. Both of them had found that when in rotation the body exercised a peculiar action upon the magnetic needle, deflecting it in a manner which was not observed during quiescence; but neither of them was aware at the time of the agent which produced this extraordinary deflection. They ascribed it to some change in the magnetism of the iron shell and disk.

But Faraday at once saw that his induced currents must come into play here, and he immediately obtained them from an iron disk. With a hollow brass ball, moreover, he produced the effects obtained by Mr. Barlow. Iron was in no way necessary; the only condition of success was that the rotating body should be of a character to admit of the formation of currents in its substance; it must, in other words, be a conductor of electricity. The higher the conducting power the more copious were the currents. He now passes from his little brass globe to the globe of the earth. He plays like a magician with the earth's magnetism. He sees the invisible lines along which its magnetic action is exerted, and sweeping his wand across these lines evokes this new power. Placing a simple loop



A dipping needle, shown in the background, is so balanced that it points slantingly downward toward the earth's north magnetic pole. [At Washington, D.C., the inclination, or dip, is 70 degrees.] Faraday found that, when an iron bar was thrust into a coil in the direction of the dip, a momentary current was induced.

of wire round a magnetic needle he bends its upper portion to the west; the north pole of the needle immediately swerves to the east; he bends his loop to the east, and the north pole moves to the west. Suspending a common bar magnet in a vertical position, he causes it to spin round its own axis. Its pole being connected with one end of a galvanometer wire, and its equator with the other end, electricity rushes round the galvanometer from the rotating magnet. He remarks upon the "singular independence" of the magnetism and the body of the magnet which carries it. The steel behaves as if it were isolated from its own magnetism.

And then his thoughts suddenly widen, and he asks himself whether the rotating earth does not generate induced currents as it turns round its axis from west to east. In his experiment with the twirling magnet the galvanometer wire remained at rest; one portion of the circuit was in motion *relatively* to *another portion.* But in the case of the twirling planet the galvanometer wire would necessarily be carried along with the earth; there would be no relative motion. What must be the consequence? Take the case of a telegraph wire with its two terminal plates dipped into the earth, and suppose the wire to lie in the magnetic meridian. The ground underneath the wire is influenced like the wire itself by the earth's rotation; if a current from south to north be generated in the wire, a similar current from south to north would be generated in the earth under the wire; these currents would run against the same terminal plate, and thus neutralize each other.

This inference appears inevitable, but his profound vision perceived its possible invalidity. He saw that it was at least possible that the difference of conducting power between the earth and the wire might give one an advantage over the other, and that thus a residual or differential current might be obtained. He combined wires of different materials, and caused them to act in opposition to each other, but found the combination ineffectual. The more copious flow in the better conductor was exactly counterbalanced by the resistance of the worse. Still, though experiment was thus emphatic, he would clear his mind of all discomfort by operating on the earth itself. He went to the round lake near Kensington Palace, and stretched 480 feet of copper wire, north and south, over the lake, causing plates soldered to the wire at its ends to dip into the water. The copper wire was severed at the middle, and the severed ends connected with a galvanometer. No effect whatever was observed. But though quiescent water gave no effect, moving water might. He therefore worked at London Bridge for three days during the ebb and flow of the tide, but without any satisfactory result. Still he urges, "Theoretically it seems a necessary consequence, that where water is flowing there electric currents should be formed. If a line be imagined passing from Dover to Calais through the sea, and returning through the land, beneath the water to Dover, it traces out a circuit of conducting matter one part of which, when the water moves up or down the channel, is cutting the magnetic curves of the earth, whilst the other is relatively at rest. . . .

"There is every reason to believe that currents do run in the general direction of the circuit described, either one way or the other, according as the passage of the water is up or down the channel." This was written before the submarine cable was thought of, and he once informed me that actual observation upon that cable had been found to be in accordance with his theoretic deduction.[7]

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7. I am indebted to a friend for the following exquisite morsel: "A short time after the publication of Faraday's first researches in magnetoelectricity, he attended the meeting of the British Association at Oxford, in 1832. On this occasion he was requested by some of the authorities to repeat the celebrated experiment of eliciting a spark from a magnet, employing for this purpose the large magnet in the Ashmolean Museum. To this he consented, and a large party assembled to witness the experiments, which, I need not say, were perfectly successful. While he was repeating them a dignitary of the university entered the room, and addressing himself to Professor Daniell, who was standing near Faraday, inquired what was going on. The Professor explained to him as popularly as possible this striking result of Faraday's great discovery. The Dean listened with attention and looked earnestly at the brilliant spark, but a moment after he assumed a serious counte-

Three years subsequent to the publication of these researches—that is to say, on January 29, 1835—Faraday read before the Royal Society a paper "on the influence by induction of an electric current upon itself." A shock and spark of a peculiar character had been observed by a young man named William Jenkin, who must have been a youth of some scientific promise, but who, as Faraday once informed me, was dissuaded by his own father from having anything to do with science. The investigation of the fact noticed by Mr. Jenkin led Faraday to the discovery of the *extra current*, or the current *induced in the primary wire itself* at the moments of making and breaking contact, the phenomena which he described and illustrated in the beautiful and exhaustive paper referred to.

Seven and thirty years have passed since the discovery of magneto-electricity; but if we except the *extra current*, until quite recently nothing of moment was added to the subject. Faraday entertained the opinion that the discoverer of a great law of principle had a right to the "spoils"—this was his term—arising from its illustration; and guided by the principle he had discovered, his wonderful mind, aided by his wonderful ten fingers, overran in a single autumn this vast domain, and hardly left behind him the shred of a fact to be gathered by his successors.

And here the question may arise in some minds: What is the use of it all? The answer is, that if man's intellectual nature thirsts for knowledge, then knowledge is useful because it satisfies this thirst. If you demand practical ends, you must, I think, expand your definition of the term practical, and make it include all that elevates and enlightens the intellect, as well as all that ministers to the bodily health and comfort of men. Still, if needed, an answer of another kind might be given to the question "What is its use?" As far as electricity has been applied for medical purposes, it has been almost exclusively Faraday's electricity. You have noticed those lines of wire which cross the streets of London. It is Faraday's currents that speed from place to place through these wires. Approaching the point of Dungeness, the mariner sees an unusually brilliant light, and from the noble *phares* of La Hève the same light flashes across the sea. These are Faraday's sparks exalted by suitable machinery to sunlike splendor. At the present moment the Board of Trade and the

nance and shook his head; 'I am sorry for it,' said he, as he walked away. In the middle of the room he stopped for a moment and repeated, 'I am sorry for it,' then, walking towards the door, when the handle was in his hand he turned round and said, '*Indeed* I am sorry for it; it is putting new arms into the hands of the incendiary.' This occurred a short time after the papers had been filled with the doings of the hayrick burners. An erroneous statement of what fell from the Dean's mouth was printed at the time in one of the Oxford papers. He is there wrongly stated to have said, 'It is putting new arms into the hands of the infidel.' "

Brethren of the Trinity House, as well as the Commissioners of Northern Lights, are contemplating the introduction of the magnetoelectric light at numerous points upon our coasts; and future generations will be able to refer to those guiding stars in answer to the question, what has been the practical use of the labours of Faraday? But I would again emphatically say, that his work needs no such justification, and that if he had allowed his vision to be disturbed by considerations regarding the practical use of his discoveries, those discoveries would never have been made by him. "I have rather," he writes in 1831, "been desirous of discovering new facts and new relations dependent on magnetoelectric induction, than of exalting the force of those already obtained, being assured that the latter would find their full development hereafter."

In 1817, when lecturing before a private society in London on the element chlorine, Faraday thus expressed himself with reference to this question of utility: "Before leaving this subject, I will point out the history of this substance, as an answer to those who are in the habit of saying to every new fact, 'What is its use?' Dr. Franklin says to such, 'What is the use of an infant?' The answer of the experimentalist is, 'Endeavour to make it useful.' When Scheele discovered this substance, it appeared to have no use; it was in its infancy and useless state, but having grown up to maturity, witness its powers, and see what endeavours to make it useful have done."

The foregoing consists of Chapters I–III of Tyndall's FARADAY AS A DISCOVERER.