are some 17,000 paragraphs.

15. J. Clerk Maxwell, "Faraday's Lines of Force", *Proc. Camb. Phil. Soc.*, **1856**, *10(1)*, 1-76.

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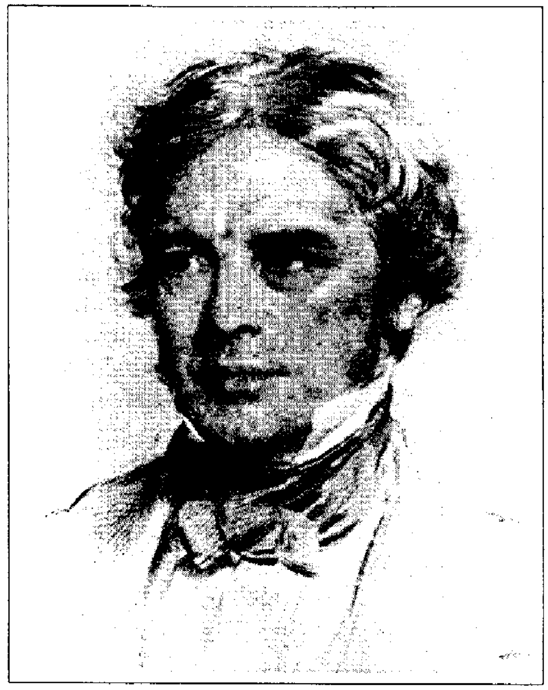
FARADAY AND HIS BIOGRAPHERS

L. Pearce Williams, Cornell University

Before examining the biographers of Faraday, it is worth raising the question of the value of biography where scientists are concerned. For artists and writers, movers and shakers in the political and military spheres, the answer is obvious. Biography permits us to understand the motives and the influences that shaped these people and this gives us real insight into their works. The case with scientists would seem to be quite different. The same nature is there for everyone and differences of education, religion, private thoughts or what have you cannot change it. Biographies of scientists, it would appear, therefore, are useful only in the sense that they permit a person's life work to be easily summarized and presented.

This was the view that prevailed until quite recently. Biographies of scientists tended to be eulogies and, with the truly great ones such as Isaac Newton or Charles Darwin, hagiographies detailing and celebrating their achievements. No one claimed that biographies of scientists could tell us much about science and how it works, except insofar as they focused on persistence, experimental expertise and theoretical insights. All that has now changed. Ever since the publication of Thomas S. Kuhn's *The Structure of Scientific Revolutions*, the whole picture of the nature of science has been dramatically altered. As is well known, Kuhn's major point was that scientists do not "discover" nature; they "construct" it. The raw materials are, of course, the phenomena of the natural world but the selection of which materials to use and the arrangement of these materials into coherent theories are the product of the scientists, not of nature. Furthermore, which theories survive and which die aborning is not determined, according to Kuhn, by their "fit" with observed phenomena but



Michael Faraday
(Drawing by George Richmond)

is the result of extremely complex social negotiations that lead to a consensus. Science, then, is as much a social product of human beings as it is a description of some posited objective nature. Indeed, for some of the more extreme social constructionists, nature itself places no constraints upon the construction of scientific theories. This position is occupied by very few, yet it does serve to illustrate just how far from the old views we (meaning historians, philosophers and sociologists of science) have come. I would not expect that these views will be greeted with wild enthusiasm by practicing chemists, but you should be aware of them and, perhaps, even invest some time in studying them.

In this new world of social construction, biography moves to a central position. The source of original ideas and hypotheses is to be sought in the rich internal lives of creative scientists. Or, to put it another way, new ideas can come from anywhere - they are not, necessarily, the product of the study of nature. So, for example, it has been demonstrated rather clearly that Isaac Newton drew some of his most important scientific hypotheses from his concept of the nature of God, not from the study of the world. And, as will become evident here, the same is true of Faraday. The only way to discover these sources is to examine closely the lives of these innovators upon whom the life of science depends. Furthermore, the fate of what starts out as hypotheses, if it depends upon social negotiation, can only be understood if these negotiations are examined in detail. Once again, the essential fulcrum for prying into historical reality is the life of individuals. This is a very complicated problem for it involves the total scientific work of the person being investigated. Faraday, for example, bewildered his contemporaries by his ability to discover new phenomena and new laws that eluded them. In his monumental *Experimental Researches in Electricity*, he faithfully reported the experimental bases for his discoveries but kept back the theoretical ideas that had guided him. He did this, apparently, for two reasons: he, himself, never really trusted theory or, to put it another way, never felt that theories were as permanent as experimental facts. Just as important was his realization that his own theoretical ideas were not those accepted by his contemporaries and to put them forward would be to weaken the force of his experimental arguments. His contemporaries had no choice but to accept the new discoveries, since they could replicate them, but they were puzzled by the fact that it was Faraday and not they who had come up with them. As we shall see, this puzzlement deeply affected the biographies that they wrote of him.

There were three serious biographies of Faraday produced in the 19th century. The first was a two-volume *Life and Letters* published by Faraday's doctor and friend, Henry Bence Jones, in 1869 (1). It is a typical example of Victorian piety. It must be admitted that Michael Faraday was a perfect subject for Victorian culture for his life illustrated all those virtues that the Victorians held dear. Bence Jones faithfully recorded them. Here is Michael Faraday, son of a poor and often ill blacksmith and an uneducated but loving mother, whose early years were spent largely on the streets of London. His education was minimal, consisting of only the elements of reading, writing and arithmetic. At times, he went hungry. Life improved when he was apprenticed to a French emigré bookbinder and bookseller. Here the wide world of books was opened to him and his mind began its ascent to the empyrean. I shall not continue in this Dickensian mode for I think my point is made. Faraday was a real-life Dickensian hero. Overcoming the obstacles of poverty and lack of formal education, he rose by his own efforts and genius to the scientific heights.

Bence Jones did not merely narrate this life. He collected great quantities of letters and other intimate documents which he published, sometimes *in toto*, sometimes in generous extracts. Bence Jones' major contribution to the understanding of Faraday was these documents, for every biographer since has used them and the published papers to draw their pictures of Faraday. Bence Jones made no attempt to analyze or explain Faraday's scientific work. Instead, he wrote a chronology of Faraday's discoveries and concentrated, instead, on Faraday the man.

What emerged was a rather sanitized version of what Faraday must have been like. Other commentators remarked on the fact that Faraday had occasional bursts of temper when he suffered or observed injustice and impropriety. Bence Jones never mentions this. And, although Bence Jones remarks favorably on how much Faraday enjoyed life, playing with nieces, riding a bicycle around the outside of the lecture theater in the Royal Institution, singing and taking long walks, he never mentions the fact that Faraday also made his own gin at the Royal Institution. This would have shocked the staid Victorians!

The first serious attempt to come to grips with Faraday the scientist came at about the same time as Bence Jones produced his life and letters. John Tyndall was both a first-rate scientist and a good friend of Faraday. Unlike Faraday, he had received a formal education in science, bringing home a Ph.D. from Germany and a firm knowledge of applied mathematics. His *Faraday as a Discoverer* (1868) is a first-rate account of Faraday's scientific career (2). Tyndall saw Faraday, quite correctly, as a superb experimentalist. This was, no doubt, because Faraday began his scientific life as a chemist, and a damned good one. It was as a chemist that he sharpened his experimental abilities but, for Tyndall, it was as a physicist that he gained scientific immortality. As Tyndall wrote (p. 18), "[H]e swerved incessantly from chemistry into physics." It was this idea that Faraday was doing physics that threw Tyndall off in his account of Faraday's work. As we shall see in a moment, Faraday started as a chemist and ended as a chemist. His interest in electricity came from electrochemistry and his probing of the nature of electricity, magnetism, crystal-

THE

LIFE AND LETTERS

OF

FARADAY.

BY

DR. BENCE JONES,

SECRETARY OF THE ROYAL INSTITUTION.

IN TWO VOLUMES.

VOL. I.

LONDON:
LONGMANS, GREEN, AND CO.
1870.

lization, and light were all part of his obsession with what he called the "powers of matter". As an apprentice chemist he had defined chemistry precisely as the study of the powers of matter.

Tyndall was never able to appreciate this. This is what made him puzzle over Faraday's incredible ability to discover new phenomena that escaped the best "physical" minds of the time. By Tyndall's day, physics was firmly wedded to mathematics and it was almost a matter of deep faith for physicists that mathematical illiterates, like Faraday, could not possibly do physics. Yet here was Faraday doing it quite well. So Tyndall's account contains a constant strain of incredulity. How was Faraday able to do what Tyndall and his fellow physicists could not? It was Tyndall who gave rise to a myth about Faraday that carried the day until quite recently. For Tyndall, Faraday's originality arose from his meticulous use of experiment and the constant questioning of his results until no doubt of the effects produced was possible combined with a superb "intuition" about Nature (p. 80). In this context, that word "intuition" is merely a confession of ignorance. What Tyndall meant by it was that he had no clue as to what ideas were guiding Faraday. Thus, he could write (p. 86):

> Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation.

This religious metaphor will occur more than once in Faraday's biographers.

But to return to Tyndall, he was completely puzzled by Faraday's theoretical ideas. This was not entirely Tyndall's fault for Faraday wrote, over and over again, that theories were always to be held tentatively whereas experiments, properly conducted, led to undeniable truths. So, again, Tyndall could write "His theoretic notions were *fluent*; and when minds less plastic than his own attempted to render those fluxional images rigid, he rebelled." (p. 146). Yet, as I shall try to show later, there were certain "hypotheses" which Faraday explicitly and publicly declared he could not do without. The problem here was that Tyndall, the hard-headed, mathematical physicist, could not take them seriously. Yet Tyndall realized that hypotheses drove Faraday's researches. "Faraday," he wrote, "has been called a purely inductive philosopher. A great deal of nonsense is, I fear, uttered in this land of England about induction and deduction" (p. 27). Later he writes (p. 94):

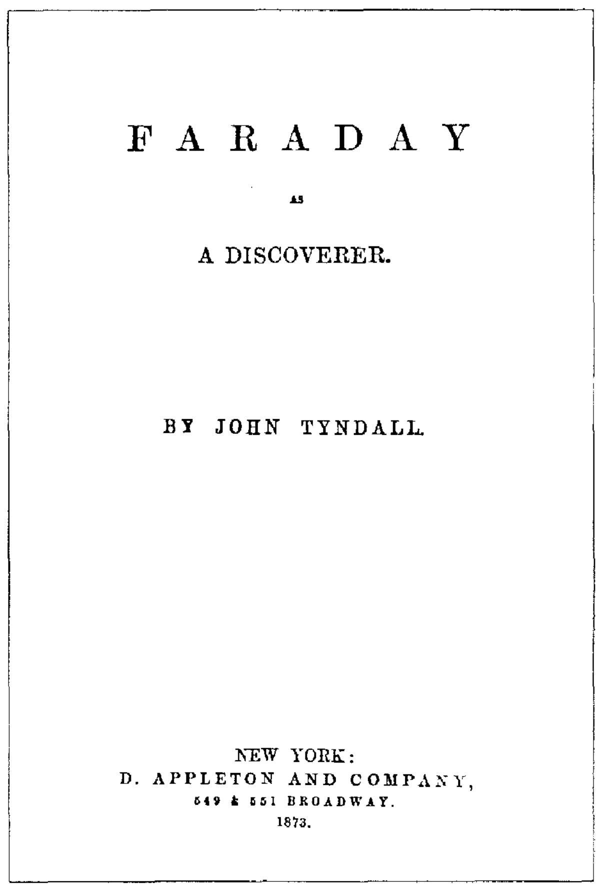
> ... I asked him what directed his attention to the magnetization of light. It was his theoretic notions. He had certain views regarding the unity and convertibility of natural forces; certain ideas regarding the vibrations of light and their relations to the lines of magnetic force; these views and ideas drove him to investigation.

But, the reader must object, what were these views? Surely Tyndall, a friend and confidante must know them. On pages 140ff., he discusses two lectures given by Faraday in 1844 and 1846 in which Faraday discussed them rather specifically. What does Tyndall make of them? He simply dismisses them. First, he argues, that they were not that important to Faraday, a charge to which I shall return (p. 146):

> It must be remembered here, that though Faraday lived amid such speculations he did not rate them highly, and that he was prepared at any moment to change them or let them go.

That this was the sensible thing for Faraday to do, Tyndall had no doubt, for "Let it then be remembered that Faraday entertained notions regarding matter and force altogether distinct from the view generally held by scientific men."

These passages are fundamental for an understanding of how Faraday was regarded by his scientific contemporaries. As we shall see, one of the fundamental criticisms of my views of Faraday is that my reconstruction of his theoretical ideas rests upon a very slim body of evidence from Faraday himself. Yet, he did reveal them in the 1840s and we may probably take Tyndall's response as being typical of his scientific colleagues. Faraday was a brilliant experimenter with a vivid imagination, but his ideas on the nature of matter and force were not to be taken seriously by "real" scientists of Tyndall's ilk. And, by


FARADAY

AS

A DISCOVERER.

BY JOHN TYNDALL.

NEW YORK:
D. APPLETON AND COMPANY,
549 & 551 BROADWAY.
1873.

the 1850s Tyndall's ilk were becoming the dominant figures in Victorian science.

Tyndall's biography of Faraday is, therefore, a rather paradoxical one. On the one hand, Tyndall clearly loved Faraday the man, respected Faraday's experimental ability enormously, knew that Faraday was inspired by speculative flights, but was convinced that these were flights of fancy. That is, they were not the elements of good science. Yet Faraday was a popular hero when he died, and Tyndall could not end on such a note. So, in the end, he turned to a description of Faraday that was both patronizing and insulting. He quotes Faraday's preface to a collection of articles that he had written prior to 1832 in which he had stated that (p. 44):

> Some, [of the papers] I think (at this date) are good; others moderate; and some bad. But I have put them *all* into the volume, because of the utility they have been of to me - and none more than the bad - in pointing out to me in future, rather, after times the faults it became me to watch and to avoid.

Tyndall then remarks (p. 45):

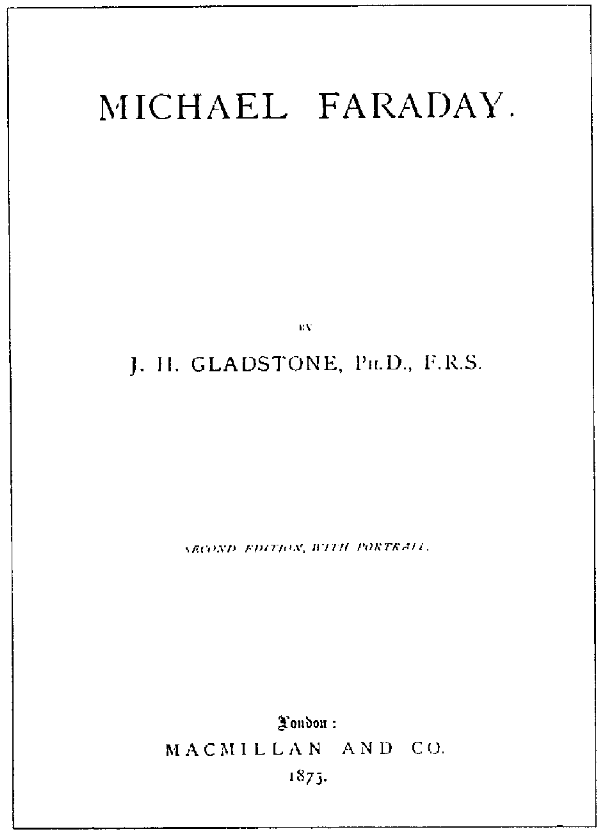
> None more than the bad! This is a bit of Faraday's innermost nature ... But is he not all the more admirable ... so as to render himself able to write thus as a little child.

And later, to drive home this point (p. 91):

> He was unfit to mingle in society, for conversation was a pain to him; but let us observe the great Man-child when alone.

Let me suggest why Tyndall, far less of a scientist than Faraday, adopted this tone towards someone whom he claimed to love as a friend. I think Tyndall simply could not follow Faraday's thoughts and ideas which, I will maintain, remained fairly stable throughout his entire scientific career. However, they were thoughts and ideas that the new generation of materialist, mathematical physicists considered to be metaphysical vaporings, and so Tyndall, as a member of this generation, could not take them seriously, in spite of Faraday's rather explicit statements that he himself did. So Tyndall fell back on the concept of the innocence of the child to whom Nature reveals her secrets through intuition. It is this picture of Faraday that was to characterize him throughout the 19th century.

Shortly after the appearance of Bence Jones' and Tyndall's accounts, another friend of Faraday's, Dr. John H. Gladstone published another biography that reflected his own relations with Faraday (3). Much of this volume derives from both Bence Jones and Tyndall, but Gladstone is able to add a few more touches. It has escaped all of Faraday's biographers, including me, I am sorry to say, that Faraday apparently served as a kind of Ann Landers to technically-minded artisan readers


MICHAEL FARADAY.

BY

J. H. GLADSTONE, PH.D., F.R.S.

SECOND EDITION, WITH PORTRAIT.

London:
MACMILLAN AND CO.
1873.

of the *Mechanics' Magazine* (4):

> Old volumes of the *Mechanic's Magazine* bear testimony to the way in which he was asked questions by people in all parts of the kingdom, and that he was accustomed to give painstaking answers to such letters.

Gladstone does not materially alter, however, the picture painted by Tyndall. The Gladstonian Faraday is an experimental genius, a speculative and imaginative spirit who always suspected his own flights of fancy and, withal, the most prolific scientific discoverer of the first half of the 19th century. Again, the mystery of his extraordinary creativity and again the falling back on his childlike simplicity. Gladstone celebrates it (p. 82):

> As to simplicity of character: when, in the course of writing this book, I have spoken to his acquaintances about Faraday, the most frequent comment has been in such words as, "Oh! he was a beautiful character, and so simple-minded".

I shall try to deal with this "simple-mindedness" later.

In 1898, the final Victorian biography of Faraday appeared in *The Century of Science Series* edited by Sir Henry Roscoe, the famous chemical spectroscopist (5). It was written by an electrical engineer, Silvanus P. Thompson, at precisely the time when Faraday's field theories conquered the world of electrical engineering. The last decades of the 19th century were when Oliver Heaviside put the finishing touches to his mathematical theory of signal and electrical transmission, summed up in his famous equations which most physicists persist in calling Maxwell's equations.

Thompson's biography is far and away the best biography to be produced in the 19th century in terms of his discussion of Faraday's scientific works. He had access to papers deposited in the Institution of Electrical Engineers of which he was a leading member. He had, as well, Faraday's manuscript laboratory journal. He also had a knowledge of electromagnetic laws and phenomena that was far beyond that of Faraday's earlier biographers. In particular, he celebrated one of Faraday's "speculative" lectures, "Thoughts on Ray Vibrations" (1846) in which Faraday suggested that light was the vibrations of lines of force (or strains in space), rather than the undulations of an elastic ether. This had been rejected as heresy by Tyndall and, one suspects, by Faraday's contemporaries. It was not until James Clerk Maxwell presented his hypothesis that light was, as Faraday had hinted, an electromagnetic disturbance that Faraday's ideas gained respectability in optics. Once again, Faraday had proved that he saw farther than his contemporaries who could not share his vision.

As for Faraday's personal life, Thompson relied very heavily on Bence Jones, Tyndall and Gladstone. What was gained was a much more detailed knowledge of how Faraday went about his work. Thompson, however, was no more able than Tyndall to penetrate to the why of Faraday's experiments. We get here, the same dichotomy between experiment and speculative imagination. Thompson wrote (pp. 241-2):

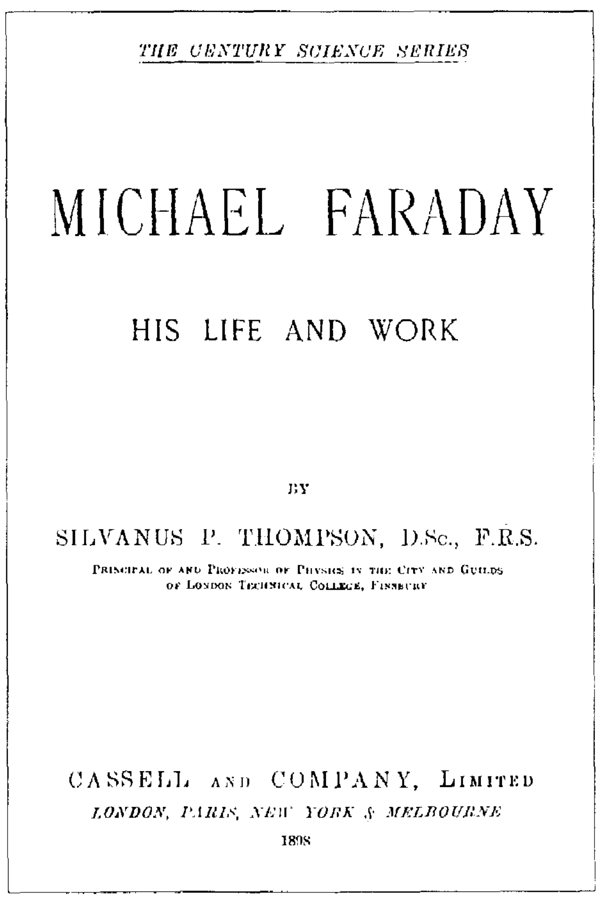
> His dogged tenacity for exact fact was accompanied by a perfect fearlessness of speculation. He would throw overboard without hesitation the most deeply-rooted notions if experimental evidence pointed to newer ideas. He had learned to doubt the idea of *poles*; so he outgrew the idea of *atoms*, which he considered an arbitrary conception. Many who heard his bold speculations and his free coinage of new terms deemed him vague and loose in thought. Nothing could be more untrue. He let his mind play freely about the facts; he framed thousands of hypotheses, only to let them go by if they were not supported by facts.

This is very much like Tyndall's portrait. Speculation, imagination, the wild inconsistencies of a child's mind that can dare to think anything. It is not entirely false for Faraday did remark once, with emphasis, to let the imagination soar, but hold it in with judgment and experiment. But speculation and imagination were, for Faraday, not just the entry to experiment, but also the ends to which experiment should lead. There are many examples of Faraday rejecting the results of his experiments because he was convinced that his theoretical speculations must be true. Thompson seems to miss this. Immediately before the passage cited above, he quotes a slip of paper found in Faraday's "research drawer" that seems to encapsulate Faraday's views of the scientific adventure (p. 241). It is entitled, "The Four Degrees" and is hierarchical in importance. These degrees of scientific progression are, respectively:

> The discovery of a fact
> The reconciling of it to known principles
> Discovery of a fact not reconcilable
> He who refers all to still more general principles

Thompson, like Bence Jones, Tyndall and Gladstone, could not find the key to Faraday's incredible scientific creativity. And, like his predecessors, he retreated to the childlike simplicity of Faraday which made him receptive, apparently, to the voice of Nature. This process of "intuition" is nowhere better illustrated than in the (very bad) poem with which Thompson prefaced his work. It is by a poet today forgotten, Cosmo Monkhouse, and is entitled, "On a Portrait of Faraday." I give it in its entirety and it should make the reader mildly ill:

> Was ever man so simple and so sage,
> So crowned and yet so careless of a prize!
> Great Faraday, who made the world so wise,
> And loved the labour better than the wage.


THE CENTURY SCIENCE SERIES

MICHAEL FARADAY

HIS LIFE AND WORK

BY

SILVANUS P. THOMPSON, D.Sc., F.R.S.

PRINCIPAL OF AND PROFESSOR OF PHYSICS IN THE CITY AND GUILDS OF LONDON TECHNICAL COLLEGE, FINSBURY

CASSELL AND COMPANY, LIMITED
LONDON, PARIS, NEW YORK & MELBOURNE
1898

And this you say is how he looked in age,
With that strong brow and those great humble eyes
That seem to look with reverent surprise
On all outside himself. Turn o'er the page,

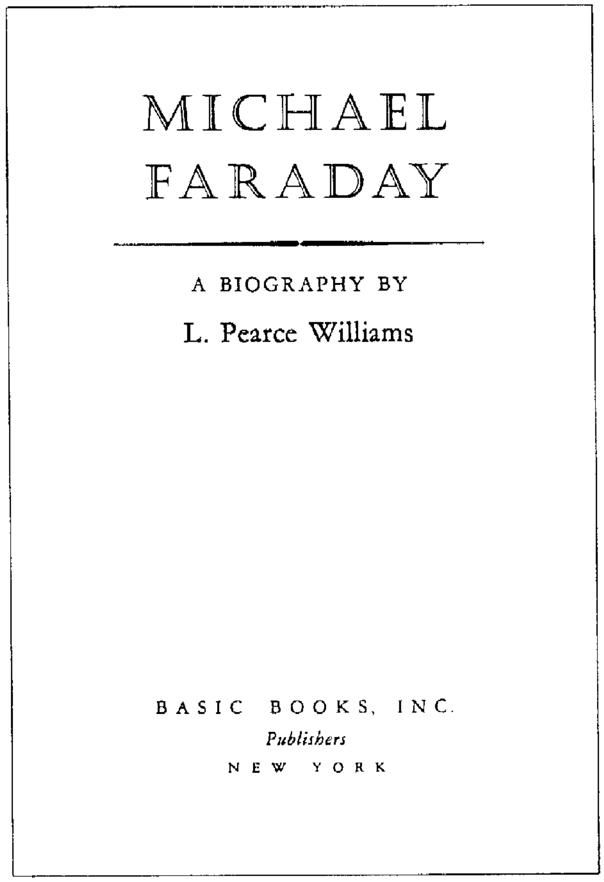
Recording Angel, it is white as snow.
Ah God, a fitting messenger was he
To show Thy mysteries to us below.
Child as he came has he returned to Thee.
Would he could come but once again to show
The wonder-deep of his simplicity.

We shall, in a moment, look at this "simplicity" that Faraday's contemporaries all commented upon. It was not, as I shall hope to show, something arcane and "childlike" but the clear result of the development of his life.

After the biography by Silvanus Thompson, no new work appeared for more than 50 years. There was a brief splash of interest in Faraday in 1931 when the centenary of his discovery of electromagnetic induction was celebrated. The major product of that year was the publication of Faraday's laboratory journal in seven stout quarto volumes (6). This enables the biographer literally to look over Faraday's shoulder and follow almost his every move in the laboratory. After 1831, Faraday numbered every paragraph in his laboratory notebooks, as he did every paragraph in the magisterial series of "Experimental Researches in Electricity". In his bound copy of the notebooks and his papers, he cross-referenced each to the other, thus indicating clearly the experimental foundations for his published works. Needless to say, this work and the published papers are the fundamental documents for the understanding of Faraday's work.

In 1957, I decided to write a biography of Faraday (7). He had intrigued me ever since a professor in a course on physical chemistry that I was taking remarked that Faraday did not believe in atoms. I could not understand how he could come up with his famous laws of electrolysis, which seemed even to imply the atomicity of electricity, without believing in atoms and it was in search for the solution to this puzzle that I began my researches.

Chemists might be interested in how a historian works. What did I look for, how did I hope to find it, and what did I do with it? My undergraduate training was as a chemical engineer and the engineering tradition had taken hold of me. Since a biography is a finite subject - it begins with the birth of the person in whom you are interested, and it ends with his death - it is theoretically possible to do a total documentary induction. So, I set out to discover all of Faraday's manuscript remains. There are some obvious places to start. The Royal Institution of Great Britain and the Institution of Electrical Engineers in London both had masses of manuscripts - letters, commonplace books, lecture notes, diaries of trips, and so on that were central to my work. I was, however, also interested


MICHAEL
FARADAY

A BIOGRAPHY BY

L. Pearce Williams

BASIC BOOKS, INC.
Publishers
NEW YORK

in Faraday's correspondence and his letters tended to be in the hands of the descendants of the recipients and my task was to find them. The first thing I did when I got to London in 1959 to begin a year of research was to look in the London telephone directory for Faradays. There were three, all of whom I contacted and one of whom had some interesting Faraday materials. I discovered a Faraday great grand niece in Oxford by observing her name in the guest book of the Sheldonian Library. In the course of the year, I found 135 relatives of Faraday, since each always knew one or two that were unknown to the rest, and part of the fruit of my work was to reunite the Faraday family.

I also put a request for help in all the newspapers of Great Britain with, sometimes, bizarre results. The letter that appeared in *Sporting Life*, a racing sheet, drew a postcard from a reader asking if Faraday had been a jockey!

Finally, I wrote to all of the Archives, Libraries and Museums listed in the publication *The World of Learning* that mentioned a manuscript collection. Here the advantage of doing biography over, say, the history of electromagnetism in the 19th century was clearly revealed. Biographical materials are catalogued under the name of the person in whom you are interested, whereas manuscripts referring to subject are scattered in the archives and not always identified by archivists who are not scientists.

The result of all these efforts was a huge amount of material in microfilm or photocopy, much of which I have passed on to Dr. Frank James for incorporation in the *Complete Correspondence* of Faraday, the first volume of which has appeared in this bicentennial year. These were the documents from which I constructed my biography.

My approach should also be noted. Like most of my generation of historians of science, I was heavily influenced by the works of Alexandre Koyré, particularly his *Etudes Galiléennes* that appeared in the late 1930s. Koyré's portrait of Galileo turned Galilean studies on its head. Earlier biographers and eulogists had praised Galileo as the pioneer of experimental science; Koyré insisted that Galileo never performed most of the experiments that he described, and that it was Galileo's philosophical reorientation that led to the creation of his science. Koyré and his disciples generalized this picture and laid out a program of research that would concentrate on the philosophical, rather than the experimental, dimension.

As mentioned above, I was trained as a chemist and so was Faraday, and experiment seemed to me to be the absolutely essential element of the science. I set out, therefore, to refute Koyré by showing that Faraday was not concerned with general philosophical issues, but was led to his views strictly through the chain of brilliant experiments that created field theory. In short, I began by agreeing with Tyndall and Thompson. Of course, Faraday had to be guided by imagination and speculation since, as Tyndall rightly pointed out, few experiments are ever done without guiding theoretical ideas. But, like Tyndall and Thompson, I felt that Faraday used imagination and speculation as *ad hoc* hypotheses from which a chain of experiments could develop and that he was never committed to any philosophical or scientific overview of Nature.

As I penetrated deeper and deeper into Faraday's mental development, his experimental results and his guiding ideas, I had to abandon my original goal. It would be tedious here to repeat the rather long and intricate chain of argument I developed in my biography, but it can be summarized rather easily.

As Thompson and Tyndall pointed out, most of Faraday's scientific contemporaries did not understand his speculations and, like Tyndall, simply disregarded them. This, I claim, is why Faraday remained almost completely silent about them throughout his life. The key texts appear in his published works when he felt it necessary to reveal his deepest theoretical concepts in order to make his work comprehensible. I shall cite them out of chronological order so that their logical coherence is evident.

In 1845, Faraday announced what he called "the magnetization of light", which was the rotation of the plane of plane polarized light in a strong magnetic field. He began his paper with the words (8):

> I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action.

In 1844, he sent a letter to the editor of the *London and Edinburgh Philosophical Magazine*, known today simply as the *Phil. Mag.*, to explain in some detail remarks that he had made at a Friday evening Discourse at the Royal Institution. His subject was the conduction of electricity and the nature of matter. His lecture examined what he considered to be a serious paradox. Nature, according to most of his contemporaries, is composed of solid, spatially defined atoms and space empty of such matter. What he tried to prove in the letter was that this concept led to a contradiction for it could be shown that, under certain circumstances, space must be capable of conducting electricity and matter must be an insulator, and other conditions required that matter be the conductor and space the insulator. His proposed solution was, to be sure, hypothetical and he earlier warned in the letter that the natural philosopher (a term Faraday much preferred to scientist) should (9):

> ... be most careful for his own safe progress and that of others, to distinguish that knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws; never raising the former to the dignity or authority of the latter, nor confusing the latter more than is inevitable with the former.

It was to escape the contradiction that Faraday gave a very rare account of his own theoretical ideas (10):

> I am not ignorant that the mind is most powerfully drawn by the phenomena of crystallization, chemistry and physics generally to the acknowledgement of centres of force. I feel myself constrained, for the present hypothetically, to admit them and *cannot do without them*. ... [my emphasis]
>
> If we must assume at all, *as indeed in a branch of knowledge like the present we can hardly help it* [my emphasis], then the safest course appears to be to assume as little as possible, and in that respect the atoms of Boscovich appear to me to have a great advantage over the more usual notion.

I have shown in my life of Faraday that his commitment to the unity of force and his use of Boscovichean atoms did not begin in the 1840s. Indeed, his whole career was spent investigating things like crystallization, chemistry and physics and his language seems to me here to be absolutely unequivocal. Some hypothesis is necessary to guide research and his famous experimental caution dictated that these hypotheses be kept as simple as possible. There is no doubt that Faraday was willing to abandon these ideas if they turned out to be inconsistent with his experiments and, indeed, in his later researches on magnetism, he did abandon them to become much more of a phenomenologist content to describe rather than to attempt to explain magnetic results.

Where did Faraday come into contact with these ideas and when? Both concepts show up very early in Faraday's career. The unity of forces had probably two sources: his deep religious conviction that God was active in the world and worked in the simplest possible way, and his contact, through his mentor, Sir Humphry Davy, with the philosophy of Immanuel Kant. Scientists have a tendency to wince when Kant's name is mentioned since they picture him as a fuzzy-minded metaphysician. It is true that Kant's works are extraordinarily difficult, but his message was not. His whole purpose, in a sense, was to destroy metaphysics, particularly in the sciences. The sciences of nature had to be strictly experimental and Kant's views are in perfect harmony with Faraday's. If one accepts my argument that Faraday used these hypotheses to guide him, at least in his work on electricity, then I believe I can show that they make sense in terms of these ideas. As Tyndall and Thompson show, I am the first biographer who can claim this. I must leave it to readers to judge the validity of the claim.

Where does all this leave us with the relation to Faraday's putative "simple-mindedness"? Faraday was simple in the social sense. He obviously felt uncomfortable in the company of those whose manners were of a different class from his own and that is why, one suspects, he shunned social occasions for most of his professional life. But he was certainly not simple in his mental operations. He had, first of all, received a first-rate education in chemistry by his close association with Sir Humphry Davy. This, after all, is what we, today, consider to be the best kind of education for our Ph.D. students. His mind ranged deeply and widely, to the point of occasional mental exhaustion. He was, in fact, more philosophically and scientifically sophisticated than many of his contemporaries who patronized him. His work cannot simply be described as brilliantly experimental but hopelessly speculative and imaginative. The two went together to produce one of the giants of modern science. If he was simple, then he was simple in the same way that Einstein was.

My biography of Faraday appeared in 1965. Since then, no other full biography has emerged. Joseph Agassi, a philosopher, published a study of Faraday as a natural philosopher in 1971 that contained biographical references but made no serious attempt to link life and work intimately (11). Agassi is very careless with his sources and much of the work is vitiated by profound inaccuracies. His consideration of Faraday's science draws heavily on my work and he is one of the very rare scholars who accepts my emphasis on point atoms. His analysis here is well worth reading, although he tends to place Faraday in a world that he sees as far more hostile than do I.

Joseph Agassi

Faraday
as a natural Philosopher

The University of Chicago Press, Chicago & London

Beginning in the late 1970s, David Gooding of the University of Bath has published a series of careful and probing articles that deal with Faraday's experiments in far more detail than I was able to do within the limits of a single volume. His general analysis of what Faraday was up to follows mine, usually without acknowledgment. Where he differs from me is over Boscovich and his influence. He insists that Faraday's results emerge solely from his experiments and, thereby, denies Tyndall's, Thompson's and my insistence on the hypothetical dimension of Faraday's thought. He is currently working on a biography, and I look forward eagerly to reading it.

In 1985, Gooding and Frank James edited a volume entitled, rather hubristically, *Faraday Rediscovered* (12). I am sure I was not the only scholar surprised by this title since few of us thought Faraday had been lost. In any case, the essays do flesh out some aspects of Faraday's life and work but there is little in the volume that is startlingly new.

At this meeting of the American Chemical Society, Geoffrey Cantor pulled his mint copy of his new biography of Faraday out of his briefcase (13). I look forward eagerly to reading it, for it promises to fill in, in rich detail, Faraday's religious life and its influence on his science which I only mentioned in my work. This should be a major contribution to Faraday studies, and, as I hope this last section shows, the last word on Faraday certainly has not been said.

References and Notes

1. H. Bence Jones, *The Life and Letters of Faraday*, 2 Vols., Longmans, Green & Co., London, 1869.

2. J. Tyndall, *Faraday as a Discoverer*, Longman, Green & Co., London, 1868.

3. J. H. Gladstone, *Michael Faraday*, Macmillan, London, 1872.

4. Since I read this paper to the ACS, I have examined all the volumes of the *Mechanics Magazine* and do not find the letters Gladstone mentioned. He must have confused the magazine with another journal that I have not discovered.

5. S. P. Thompson, *Michael Faraday: His Life and Work*, Macmillan, New York, NY, 1898.

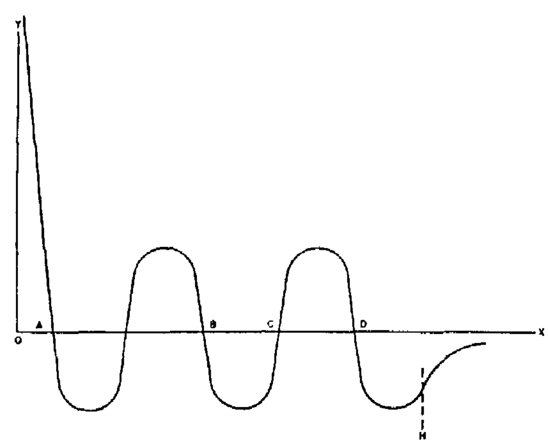
6. T. Martin, ed., *Faraday's Diary (1820-1862)*, 7 Vols., Bell, London, 1931-1936.

7. L. P. Williams, *Michael Faraday, A Biography*, Chapman & Hall, London, 1965.

8. M. Faraday, *Experimental Researches in Electricity*, 3 Vols., Quaritch, London, 1839-1855, Vol 3, p. 1.

9. *Ibid.*, Vol. 2, p. 286.

10. *Ibid.* R. J. Boscovich was an 18th-century Jesuit who was a deep student of Newtonian physics. He puzzled over the problem of the collision of the infinitely hard atoms that Newton had suggested in the 31st query of his *Opticks*. Infinite hardness is not compatible with elasticity, so Boscovich insisted that the reversal of motion in a collision would have to be instantaneous, resulting in the absurd notion that the two colliding atoms would each be going in opposite directions at the moment of collision. To solve this problem, Boscovich posited atoms as centers of force, with no material component. The curve below graphs the attractive and repulsive forces associated with such an atom in terms of distance from the atomic center. Forces above the line are repulsive; those below it are attractive. At some distance OA, the force becomes asymptotically repulsive, preventing two points from being in the same place, thus preserving the material property of impenetrability. At H, the attractive force turns into the hyperbola of universal gravitation. Points D and B are stable points for two atoms since the forces will resist displacement; C is an unstable point since the slightest displacement will cause the particle either to recede from or approach O. It should be emphasized that, for Boscovich, these atoms were fundamental particles and that chemical atoms were compounds of these whose complex patterns of force could be used to account for "elective affinities" and for the regularity of crystals.



11. J. Agassi, *Faraday as a Natural Philosopher*, University of Chicago, Chicago, IL, 1971.

12. D. Gooding and F. James, eds., *Faraday Rediscovered*, Macmillan, London, 1985.

13. G. Cantor, *Michael Faraday: Sandemanian and Scientist*, Macmillan, London, 1991.

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FARADAY'S ELECTION TO THE ROYAL SOCIETY: A REPUTATION IN JEOPARDY

June Z. Fullmer, Ohio State University, and Melvyn C. Usselman, University of Western Ontario

On Thursday, 8 January 1824, the meeting of the Royal Society had, as one order of business, a ballot to elect (or not) Michael Faraday to the Fellowship of the Society. According to established custom, in the absence of the President, Sir Humphry Davy, the Vice President of the Society, Sir Everard Home, presided (1). He was flanked by the two secretaries, William T. Brande and Taylor Combe. After opening formalities, one of the secretaries read the names of those candidates whose certificates for Fellowship had been newly presented. Sir Everard then asked the Fellows if the Society wished to elect these candidates immediately, (certain members of the nobility and other distinguished folk were always accorded "instant" Fellowship - for example, Prince Christian of Denmark on 6 June 1822; Robert Peel, Secretary of State, on 5 December 1822) or ballot for them after their certificates had been displayed over a ten-meeting period. At this juncture Sir Everard announced that the Society would be balloting on the question of Fellowship for Michael Faraday. His certificate had been displayed for the appropriate length of time and had received 29 supporting signatures. After inviting comments from the Fellows about the candidate, Sir Everard demonstrated the ballot-box to be empty before handing it to the Assistant Secretary, John Hudson, who carried it from Fellow to Fellow. Each Fellow registered his vote by choosing either