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Division of the History of Chemistry of the American Chemical Society

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A Special Bicentennial Issue:
Michael Faraday - Chemist and Popular Lecturer

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The Cover...

This issue shows a drawing of Michael Faraday lecturing at the Royal Institution taken from *The Museum of Foreign Literature, Science and Art* for 1836. The original carries the caption "Michael Faraday - Author of Chemical Manipulation". *Chemical Manipulation* was the title of Faraday's first book and is discussed in this issue in the articles by Ross and by Jensen.

All illustrations unless otherwise indicated are from the Oesper Collection in the History of Chemistry of the University of Cincinnati.

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SYMPOSIUM INTRODUCTION

Michael Faraday: Chemist and Popular Lecturer

1991 was a bicentennial year for both Michael Faraday and for Wolfgang Amadeus Mozart and at times there seemed to be almost as many celebrations of Faraday's Jovian talents as there were performances of the Jupiter symphony. The two men are alike only in their genius and the sense that on occasion both of them appeared to be taking dictation directly from God. Faraday's first biographer, Henry Bence Jones, put it more reverently:

> ... his second great characteristic was his imagination. It rose at times to divination, or scientific second sight, and led him to anticipate results that he or others afterwards proved to be true.

During the course of 1991 the physicists celebrated their Faraday, the electrochemists theirs, while the platinum, colloid and catalysis chemists also paid their respects. The British Royal Mint honored Faraday with a handsome guest appearance on the new £20 note in which he is depicted lecturing at the Royal Institution. By contrast, the British Post Office dishonored him with an execrable commemorative stamp in which he appears to be undergoing some kind of electrical lobotomy.

Faraday's appearance on the new £20 note.

At the Spring meeting of the American Chemical Society in Atlanta it was the turn of the chemists, chemical educators and chemical historians. Faraday himself would, of course, have been appalled by such parochial distinctions but we seem to have lost his gift of seeing Nature whole. As the title of the present symposium suggests, it was our intent to concentrate on Faraday-the-chemist and Faraday-the-popular-lecturer. Faraday served his apprenticeship under a chemist, grew to maturity as one of the first professional chemists in England, and held the Fullerian Professorship of Chemistry at the Royal Institution from 1834 until his retirement in 1861. In spite of this his current reputation lies chiefly in what we now choose to call physics (his electrochemistry is happily ambivalent) and it is not surprising that most historical scholarship has been concentrated in that area. It was our hope in Atlanta to cast a little more light in the ill-lit if not exactly dark chemical places.

The first paper is by Sir John Thomas, inheritor of the Faraday mantle at the Royal Institution. As in his similarly titled recent book, *Michael Faraday and the Royal Institution*, we are treated to an eloquent reminder of the inseparability of the man and the place and to the first full-length account since John Tyndall of living with the almost palpable ghost of a great predecessor. Sir John's is the latest in a long line of Faraday biographies going back to Tyndall and Bence Jones. L. Pearce Williams is the author of the standard "Life" and in "Faraday and his Biographers" he gives a critical assessment of the competition. Faraday also had to live with his predecessor, the far from ghostly Humphry Davy, and the relationship was not always an easy one as Usselman and Fullmer show in "Faraday's Election to the Royal Society: A Reputation in Jeopardy". Faraday's legendary skills as a lecturer are addressed in Geoffrey Cantor's "Educating the Judgment: Faraday as a Lecturer" and in part in Frank James' "The Military Context of Chemistry: The Case of Michael Faraday". James' paper also illustrates that the balancing act involving research, teaching, service and financial security is by no means of recent origin.

Herbert Pratt's "Michael Faraday's Bibles as Mirrors of his Faith" addresses Faraday's Sandemanian beliefs, a subject treated at much greater length in Geoffrey Cantor's recently published book *Michael Faraday: Sandemanian and Scientist*. Faraday was dogged by ill health for much of his life, some of it no doubt brought about by his almost superhuman work habits. James O'Brien describes these problems in "Faraday's Health Problems".

The paper "Faraday's 1822 'Chemical Hints' Notebook and the Semantics of Chemical Discourse" by Ryan Tweney discusses an early, unpublished manuscript in which Faraday meditates on his current and future chemical interests. One of these interests is described in Harold Goldwhite's "Faraday's Search for Fluorine". Like Davy before him and many another after him, Faraday failed in his attempt to isolate fluorine and it was not until 50 years later that Moissan was to succeed. Though not given in the present form in Atlanta, Derek Davenport's "Observations on Faraday as Organic Chemist Manqué" takes a retrospective look at the remarkable body of work in the emerging field of organic chemistry that Faraday had completed by 1826.

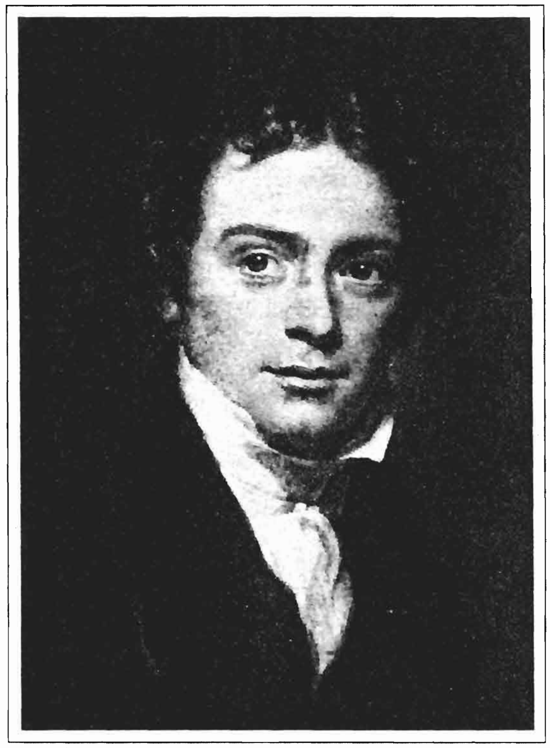
With the exception of *Chemical Manipulation*, all of Faraday's books had previously appeared in the periodical literature or else were transcriptions of public lectures. Like many another text, it arose out of a course, this one given at the Royal Institution as William Jensen describes in his "Michael Faraday and the Art and Science of Chemical Manipulation". Faraday had plans for a new edition of *Chemical Manipulation* and as a former bookbinder he prepared an interleaved copy that he annotated with new material. This copy is now the proud possession of Sydney Ross who describes his treasure in "The Chemical Manipulator". Ross is also the author of "Unpublished Letters of Faraday and Others to Edward Daniel Clarke", interesting examples of the minutiae, then as now, of scientific life. For chemists Faraday's crowning achievement was the enunciation of the law(s) of electrolysis, as described in John Stock's "The Pathway to the Laws of Electrolysis". The final paper, "From Electrochemical Equivalency to a Mole of Electrons: The Evolution of the Faraday" by Marcy Hamby Towns and Derek Davenport, was first presented at a Workshop for Teachers held during the Great Lakes Regional Meeting of the American Chemical Society in Indianapolis in late May. It is directed more at teachers of chemistry than at historians of chemistry but it may serve to illustrate a firmly held belief that history is too important to be left entirely to the historians.

Acknowledgements: Several years ago The Camille and Henry Dreyfus Foundation, Inc., was one of the principal donors when Faraday's basement laboratory at the Royal Institution was converted into the wonderfully elaborative Faraday Museum. Their fealty to Faraday was again manifested with the award in 1990 of a special grant in the chemical sciences that helped to underwrite this symposium and to make possible the production of this bumper issue of the *Bulletin*. Thanks are also due to the Petroleum Research Foundation of the American Chemical Society for providing travel support for two of our visitors from England.

Derek A. Davenport, Purdue University

THE ROYAL INSTITUTION & MICHAEL FARADAY: A PERSONAL VIEW

John Meurig Thomas, Royal Institution of Great Britain



Young Michael Faraday

Having lived and worked for five years in Michael Faraday's home and laboratory, my initial interest in, and curiosity about, the great scientist has developed into a passionate admiration for all that he stood for and achieved (1). His scientific and spiritual presence at the Royal Institution confers a unique aura that pervades the whole place. One cannot escape it. Whenever I stand at the lecturer's desk, where Faraday stood on more than a 1000 occasions, and where Davy, Dewar, Young, Rayleigh, Rutherford, Arrhenius, Cannizarro, Mendeleev, Hoffmann, Bridgman, Lawrence and William Bragg, and Pauling have also stood, Wordsworth's reference to "the spiritual presence of absent friends" comes to mind.

No chemist (organic, physical, analytical, surface or electro-), no physicist, no engineer or materials scientist is unaware of Faraday's towering contributions to their subject. No experimentalist has ever bequeathed to posterity a greater body of pure scientific achievement than Faraday and the practical consequences of his discoveries have profoundly influenced the very nature of civilized life. Yet he was self-taught: he left school at the age of 12, and started his career as an errand boy, then as a bookbinder. He rose to be one of the greatest scientists of the age. At the same time, he remained morally incorruptible and throughout his life retained his boyish sense of awe and humility. In reading his work, just as in contemplating his astonishing range of accomplishments, we are conscious of the presence of a unique human being (2):

> Nothing is too wonderful to be true, if it be consistent with the laws of nature and, in such things as these, experiment is the best test of such consistency.

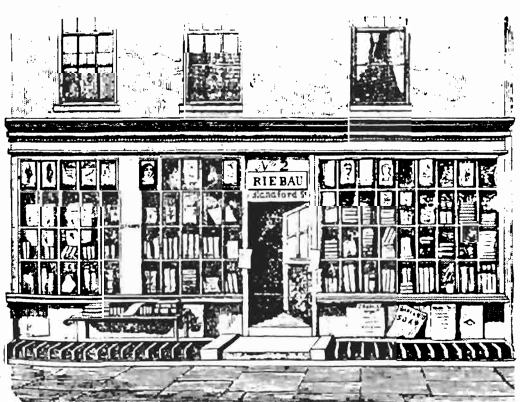
In none of his 450 publications is there a single differential equation, for he knew no mathematics. But, according to Albert Einstein, Faraday was responsible, along with Clerk

Maxwell, for the greatest change in the theoretical basis of physics since Newton.

The story of Faraday's life speaks to us across the years, and its romance bears repetition to every generation. Who was this man? What were his precise contributions? And why is it that no name stands higher in the general esteem of scientists the world over than that of Faraday? In the definitive biography of Faraday by L. Pearce Williams these questionsand others are answered in great detail (3). In a more recent, less comprehensive analysis, I have endeavoured to weave the genius of Faraday the man with that of the place where he lived for nearly 50 years and where he worked for a somewhat longer period - the Royal Institution (4). Fresh light on the private, public and religious life, as well as the scientific work, of Faraday continues to be shed (5, 6). These complement a number of classic, earlier studies, dating from the fascinating and elegant survey of *Faraday as a Discoverer* (7) by his contemporary and successor, John Tyndall, and the *Life and Letters of Michael Faraday* (8) by his friend, the eminent contemporary physician, H. Bence Jones, who was Secretary of the Royal Institution from 1860 to 1873.

It is convenient to enumerate Faraday's contributions under two somewhat arbitrary headings: chemistry on the one hand, and electricity and magnetism on the other (see Tables 1 and 2). These enumerations reflect the vast extent of Faraday's canvas and cogently exemplify the interdisciplinary nature of his pursuits. Little wonder that the Faraday Society (now the Faraday Division of the Royal Society of Chemistry) was set up to explore the territory between well-established divisions of natural philosophy. No one has more brilliantly investigated the interfacial regions separating existing disciplines than did Michael Faraday.

Vast as the subject matter encompassed by Tables 1 and 2 is, it is somewhat paradoxical that the enumerations therein tend to conceal the really major breakthroughs that Faraday made. It has often been said that, had there been Nobel prizes



Riebau's book store where Faraday worked as an apprentice.

Table 1. Faraday's principal contributions to chemical science.

1816	(With Davy) Evolution of miners' safety lamp.
1818-24	Preparation and properties of alloy steels (study of Indian Wootz). Metallography.
1812-30	Analytical chemistry. Determination of purity and composition of clays, native lime, water, gunpowder, rust, dried fish, various gases, liquids and solids.
1820-26	Organic chemistry. Discovery of benzene, isobutene, tetrachloroethene, hexachlorobenzene, isomers of alkenes and of α and β napthalenesulphonic acids, vulcanization of rubber. Photochemical preparations.
1825-31	Improvements in the production of optical grade glass.
1823&	Liquefaction of gases (H_2S, SO_2 and six other gases).
1845	Recognized existence of critical temperature and established reality of continuity of state.
1833-36	Electrochemistry and the electrical properties of matter. Laws of electrolysis. Equivalence of voltaic, static, thermal and animal electricity. First example of thermistor action. Fused salt electrolytes, superionic conductors.
1834	Heterogeneous catalysis. Poisoning and inhibition of surface reactions. Selective adsorption. Wettability of solids.
1835	"Plasma" chemistry and the magnetic properties of matter. Magneto-optics. Faraday effect. Diamagnetism. Para-magnetism. Anisotropy.
1857	Colloidal metals. Scattering of light. Sols and hydrogels.

in Faraday's day, he would have won at least six! The citations could well have been for the:

* Discovery of electromagnetic induction which, along with his earlier related work on the relationship between electricity and magnetism, brought forth the first transformer, dynamo and electric motor.

* Laws of electrolysis, which rank among the most accurate generalizations in science. (These led, through the subsequent work of Johnstone Stoney, Helmholtz and J. J. Thomson, to the realization that matter is electrical in nature. They also led to the idea of ions, electrodes, electrolytes - all terms that Faraday, along with his polymathic Cambridge friend, Whewell, coined - and to electrodeposition, electroplating, coulometry and electrochemical analysis).

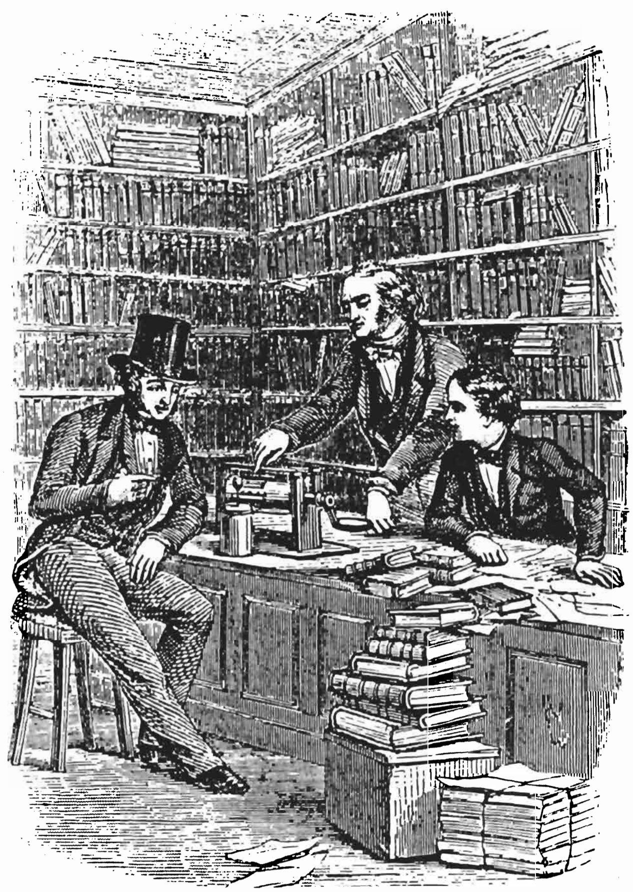
* Discovery of the magnetic properties of matter and the foundation of magnetochemistry (the terms paramagnetism and diamagnetism; paramagnetism of oxygen gas).

* Discovery of benzene and his analysis of its composition. (Faraday was as much the founder of the chemical - certainly the dyestuffs and explosives - industry as of the electrical power generation and electroplating industries. Table 3 summarizes the set of compounds intimately associated with

Faraday's work as an organic chemist).

* Discovery of the Faraday effect (the rotation of the plane of polarization of light by a magnetic field) and for laying the foundations of magneto-optics.

* Introduction of the notion of a field. (Unlike his contemporary scientists, Faraday refused to be guided solely by the mathematical precision of Coulomb's law in interpreting the forces between charges. He reflected deeply upon what occurred in the intervening space. This led him, in turn, to discover induction, inductive capacity and permittivity. He also convinced himself that the energy of a magnet could extend beyond the perimeter of the magnet itself).



An imaginative 19th-century reconstruction of Faraday's first experiments with electricity while still a bookbinder's apprentice.

Einstein is said to have kept a portrait of Faraday on the wall of his study. How appropriate, for it was Faraday who served as the pioneer and prophet of the grand revision that made Einstein's work possible. Many chemists - fewer physicists! - are unaware of the major theoretical impetus provided by Faraday. They tend to think of him largely as the experimental genius that he undoubtedly was, a man for whom the primacy of experiment always took precedence over speculation. Chemists are also unaware of the role that Faraday played in laying the foundation of the technology of the modern world.

Table 2. Faraday's principal contributions to physical science.

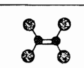
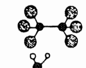
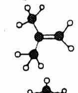
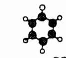
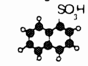
1821	Electromagnetic rotations.
1831	Electromagnetic induction. Acoustic vibrations.
1832	Identity of electricities from various sources.
1833	Electrolytic decompositions.
1835	Discharge of electricity through evacuated gases. (Plasma physics and chemistry.)
1836	Electrostatics. Faraday cage.
1845	Relationship between light, electricity and magnetism; diamagnetism; paramagnetism.
1846	"Thoughts on ray vibrations".
1849	Gravity and electricity.
1857	Time and magnetism.
1862	Influence of a magnetic field on the spectral lines of sodium. Lines of force and the concept of a field. The energy of a magnet lies outside its perimeter. The notion that light, magnetism and electricity are interconnected.

To his numerous gifts as an experimentalist and theorist, Faraday possessed two other talents that set him apart from most other scientists (9). First, his published papers are masterpieces of lucidity, self-criticism and insight, and conjure up a sense of perspective that is rather rare in the writings of chemists and physicists. Second, he believed passionately in the importance of conveying the essence of science to lay audiences and to young children. A few examples will suffice.

Faraday begins his classic paper "On the Magnetization of Light and the Illumination of Magnetic Lines of Force", which he wrote on 5 November 1845, thus (10):

I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that

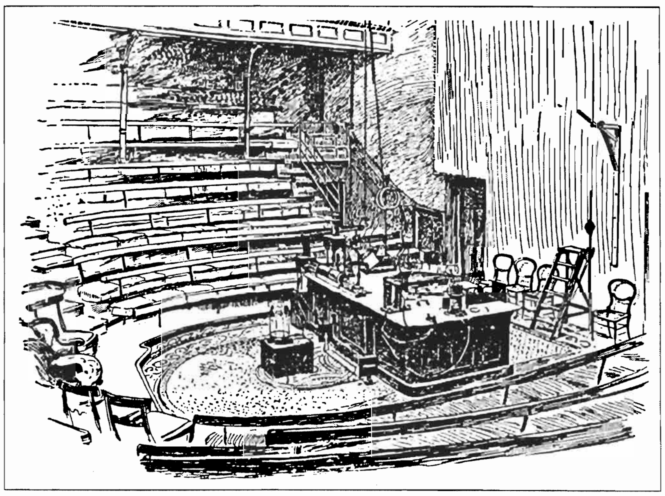
Table 3. The compositional and structural formulas of substances discovered by Faraday.

Substance	Chemical Formula	Structural Formula
C_2Cl_4	Tetrachloroethylene (Tetrachloroethene)	
C_2Cl_6	Hexachloroethane	
$(CH_2)_4$	Isobutylene (Isobutene)	
C_6H_6	Bicarburet of Hydrogen (Benzene)	
$C_{10}H_7SO_3H$	Naphthalene Sulphonic Acid	

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 modern times the proofs of their convertibility have been accumulated to a very considerable extent, and a commencement made of the determination of their equivalent forces.

Faraday expressed the view that evening lectures, of the kind introduced by him at the Royal Institution in 1826 - the famous Friday Evening Discourses - should amuse and entertain as well as educate, edify and, above all, inspire. This is the principle that continues to inspire the Royal Institution's numerous educational activities, especially the Christmas Lectures, which were also introduced by Faraday in 1826, and given by him on 19 occasions.

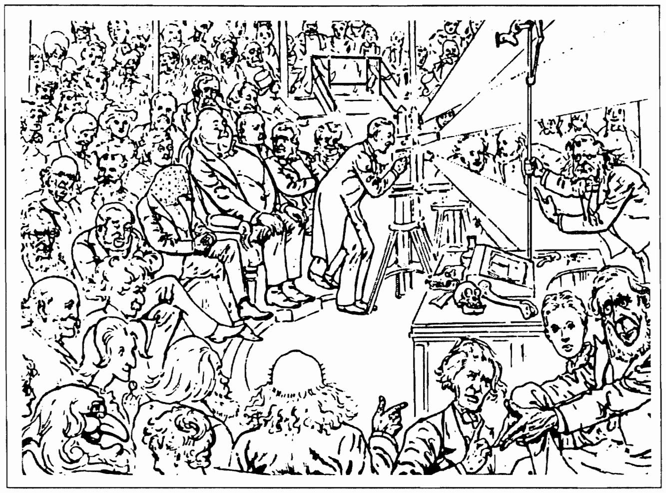
The most famous series of Christmas Lectures remains



A late 19th-century etching of the main lecture hall at the Royal Institution.

those given by Michael Faraday on *The Chemical History of a Candle*. (Japanese translations of this book have run to over 70 editions.) In his opening remarks in the first of his six lectures, Faraday spoke to the multitude of children assembled in the theatre of the Royal Institution in December, 1860 thus (11):

I have taken this subject on a former occasion; and were it left to my own will, I should prefer to repeat it almost every year - so abundant is the interest that attaches itself to the subject, so wonderful are the varieties of outlet which it offers into the various departments of philosophy. There is not a law under which any part of this universe is governed which does not come into play, and is touched upon in the phenomena. There is no better, there is no more open door by which you can enter into the study of natural philosophy, than by considering the physical phenomena of a candle. I trust, therefore, I shall not

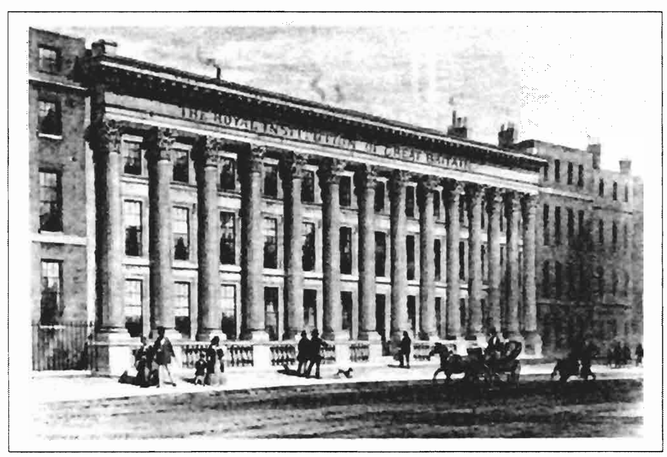


A cartoon by Harry Furniss depicting some of the famous scientists who have lectured at the Royal Institution. Faraday and John Tyndall are in the immediate foreground while Thomas Huxley holds forth at the lecture desk.

disappoint you in choosing this for my subject rather than any newer topic, which could not be better, were it even so good.

And before proceeding, let me say this also - that though our subject be so great, and our intention that of treating it honestly, seriously, and philosophically, yet I mean to pass away from all those who are seniors amongst us. I claim the privilege of speaking to juveniles as a juvenile myself. I have done so on former occasions - and, if you please, I shall do so again. And though I stand here with the knowledge of having the words I utter given to the world, yet that shall not deter me from speaking in the same familiar way to those whom I esteem nearest to me on this occasion.

And now, my boys and girls, I must first tell you of what candles are made. Some are great curiosities. I have here some bits of timber,



Exterior of the Royal Institution in 1838. (From an etching by Thomas S. Sheppard) branches of trees particularly famous for their burning. And here you see a piece of that very curious substance taken out of some of the bogs in Ireland, called candle-wood, - a hard, strong, excellent wood, evidently fitted for good work as a resister of force, and yet withal burning so well that where it is found they make splinters of it, and torches, since it burns like a candle, and gives a very good light indeed. And in this wood we have one of the most beautiful illustrations of the general nature of a candle that I can possibly give. The fuel provided, the means of bringing that fuel to the place of chemical action, the regular and gradual supply of air to that place of action - heat and light - all produced by a little piece of wood of this kind, forming, in fact, a natural candle.

At the end of his sixth and last lecture he said (12):

All I can say to you at the end of these lectures (for we must come to an end at one time or other) is to express a wish that you may, in your generation, be fit to compare to a candle; that you may, like it, shine as lights to those about you; that in all your actions, you may justify the beauty of the taper by making your deeds honourable and effectual in the discharge of your duty to your fellow men.

These words, and those that follow them, along with the elegant sequence of simple experiments described by Faraday in the written version of his Christmas Lectures have made *The Chemical History of a Candle* a classic in the annals of science. The Preface, composed with much felicity for a later edition by William Crookes (1832-1919), a prominent member and active participant in the affairs of the Institution (and President of the Chemical Society), adds to the charm (13):

From the primitive pine-torch to the paraffin candle, how wide an interval! Between them how vast a contrast! The means adopted by man to illuminate his home at night, stamp at once his position in the scale of civilisation. The fluid bitumen of the far East, blazing in rude vessels of baked earth; the Etruscan lamp, exquisite in form, yet ill adapted to its office; the whale, seal, or bear fat, filling the hut of the Esquimaux or Lap with odour rather than light; the huge wax candle on the glittering altar, the range of gas lamps in our streets, - all have their stories to tell. All, if they could speak (and, after their own manner, they can), might warm our hearts in telling, how they have ministered to man's comfort, love of home, toil, and devotion.

Apart from his astonishing range of discoveries, at least four other factors have helped to make Faraday immortal. First, he wrote and spoke about his work in memorable ways. Second, he recorded everything that he observed experimentally at the time of the observation. (His diaries (14) reveal that he invariably recorded the key points of each experiment; he also had the habit of writing up his work promptly for publication. 'Work, finish, publish' was one of his mottos!) Third, almost all the successful experiments that he carried out he proceeded to refine, with a view to demonstrating them publicly at Discourses in the Royal Institution. They were intended to leave an indelible impression and in this he succeeded triumphantly. Last, he had the good fortune to have as one of his interpreters one of the greatest physicists since Newton - J. Clerk Maxwell. Maxwell selected "Faraday's Lines of Force" as the title of his brilliant paper delivered to the Cambridge Philosophical Society in December 1855 and February 1856, when he was a 24-year old Fellow of Trinity College, Cambridge (15). With that monumental work, mathematical precision and quantitative prediction were added to Faraday's qualitative views on field theory in general and to electromagnetism in particular. With this event a new era dawned.

References and Notes

1. This paper is an abbreviated version of a lecture given at the American Chemical Society "Faraday Symposium", April 1991. An extended version of the author's views on this theme has recently been published as reference 4. Copies may be obtained in North America from The American Institute of Physics, c/o AIDC, 64 Depot Road, Colchester, VT 05445, USA.

2. T. Martin, ed., *Faraday's Diary, (1820-1862)*, 7 Vols., Bell, London 1931-1936, Entry for 19 March 1849, Vol. 5, p. 152.

3. L. P. Williams, *Michael Faraday, A Biography*, Chapman and Hall, London, 1965.

4. J. M. Thomas, *Michael Faraday and The Royal Institution: The Genius of Man and Place*, Adam Hilger, Bristol, 1991.

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

6. B. Bowers, *Michael Faraday and the Modern World*, EPA Press, Saffron Walden, England, 1991.

7. J. Tyndall, *Faraday as a Discoverer*, Longman, Green and Co., London, 1868.

8. H. Bence Jones, *The Life and Letters of Faraday*, Longmans, Green & Co., London, 1870.

9. Though Faraday knew no mathematics, he was, according to J. Clerk Maxwell, "in reality a mathematician of a very high order - one from whom the mathematicians of the future may derive valuable and fertile methods".

10. M. Faraday, "On Magnetization of Light and the Illumination of Magnetic Lines of Force", *Phil. Trans. Roy. Soc.*, **1846**, *136*, 1-62.

11. M. Faraday, *The Chemical History of a Candle*, Harper, New York, NY, 1861, pp. 9-11.

12. *Ibid.*, p. 183.

13. *Ibid.*, p. v. At various times, William Crookes was President of the Chemical Society, the Institution of Electrical Engineers, the Society of Chemical Industry, the British Association and the Royal Society. He discovered the element thallium, and invented the radiometer and the evacuated tubes named after him.

14. Reference 2. Faraday did not commence numbering his paragraphs until 29 August 1831, the date he discovered electromagnetic induction. From that date to his last entry, on 12 March 1862, there

are some 17,000 paragraphs.

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

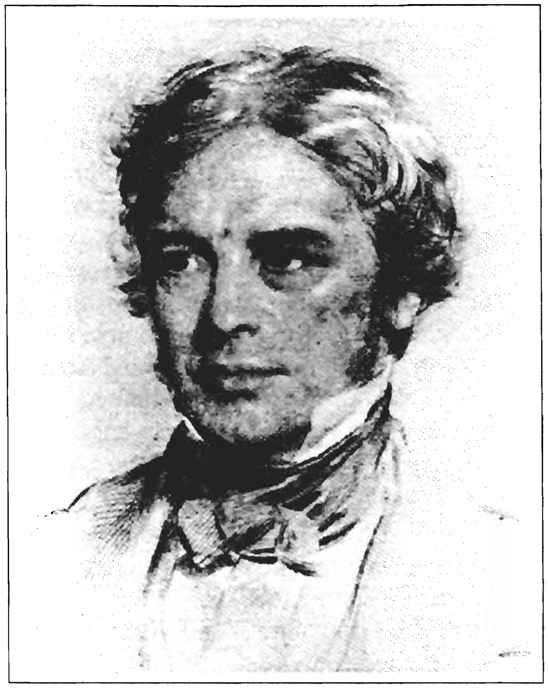
John Meurig Thomas is Fullerian Professor of Chemistry and formerly Director of the Royal Institution of Great Britain, 21 Albermarle St., London, WIX 4BS, England. He also serves as Deputy Pro-Chancellor of the University of Wales and was knighted in 1991 in recognition of his researches on catalysis and for his work in popularizing science. Professor Thomas is the author of the book "Michael Faraday and the Royal Institution: The Genius of Man and Place" and coeditor of "Selections and Reflections: The Legacy of Sir Lawrence Bragg".

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.

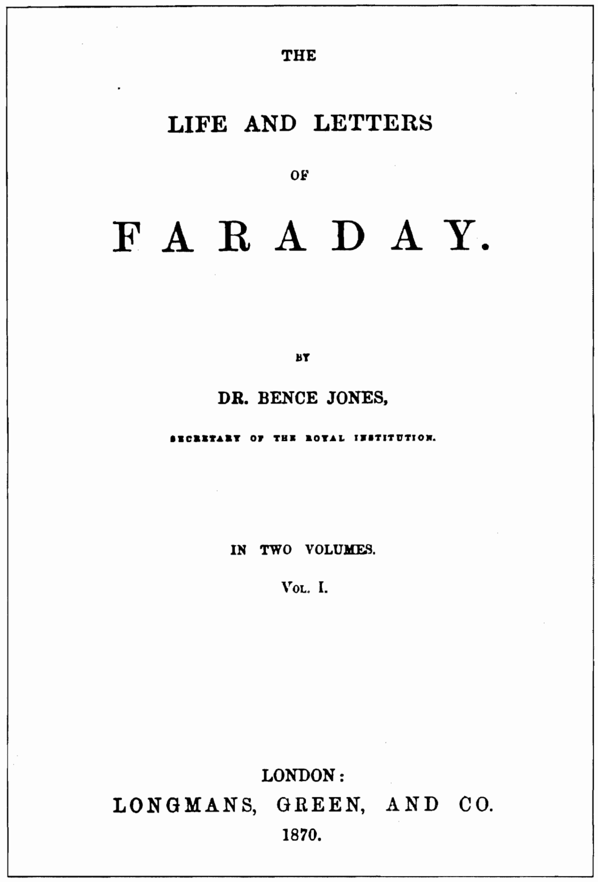
In this new world of social construction, biography moves to a central position. The source of original ideas and hypotheses is to be sought in the rich internal lives of creative scientists. Or, to put it another way, new ideas can come from anywhere - they are not, necessarily, the product of the study 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):

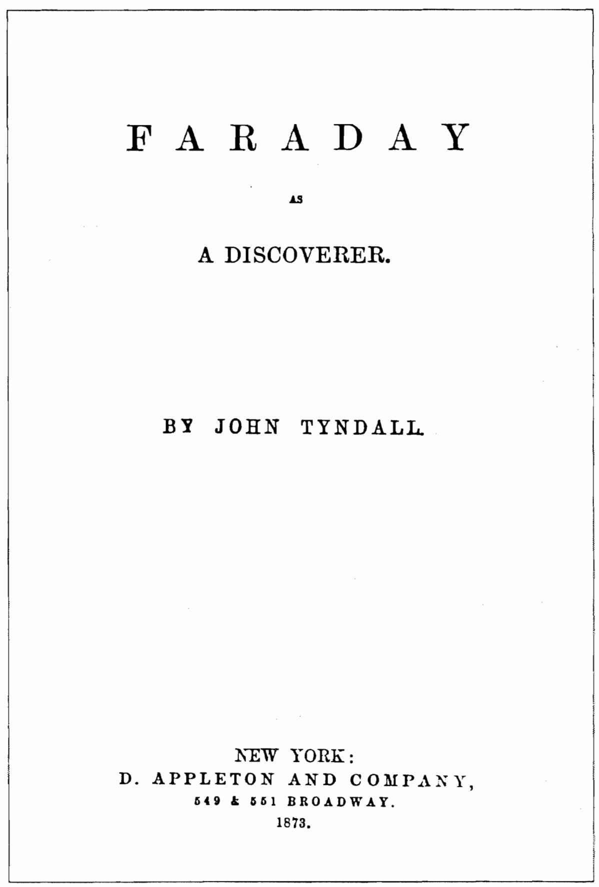
> ... I asked him what directed his attention to the magnetization of light. It was his theoretic notions. He had certain views regarding the unity and convertibility of natural forces; certain ideas regarding the vibrations of light and their relations to the lines of magnetic force; these views and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

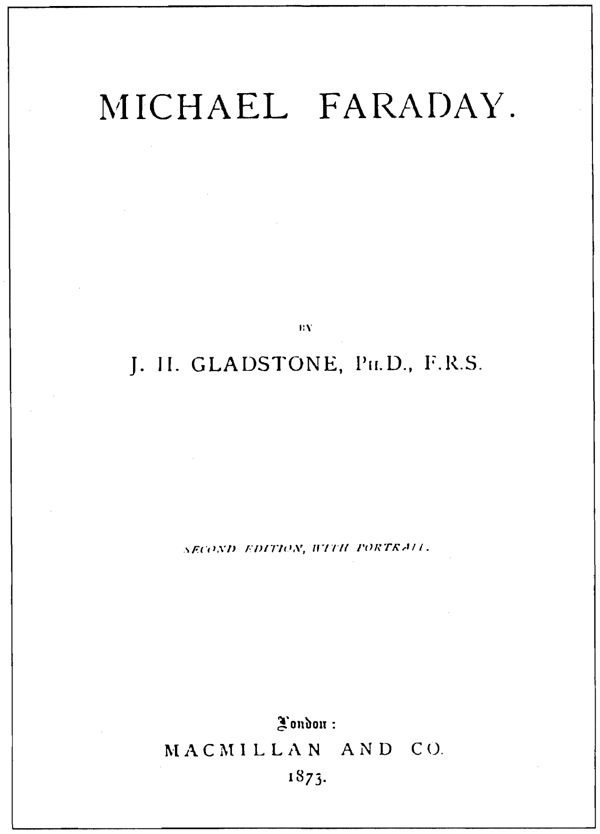
> None more than the bad! This is a bit of Faraday's innermost nature ... But is he not all the more admirable ... so as to render himself able to write thus as a little child.

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,

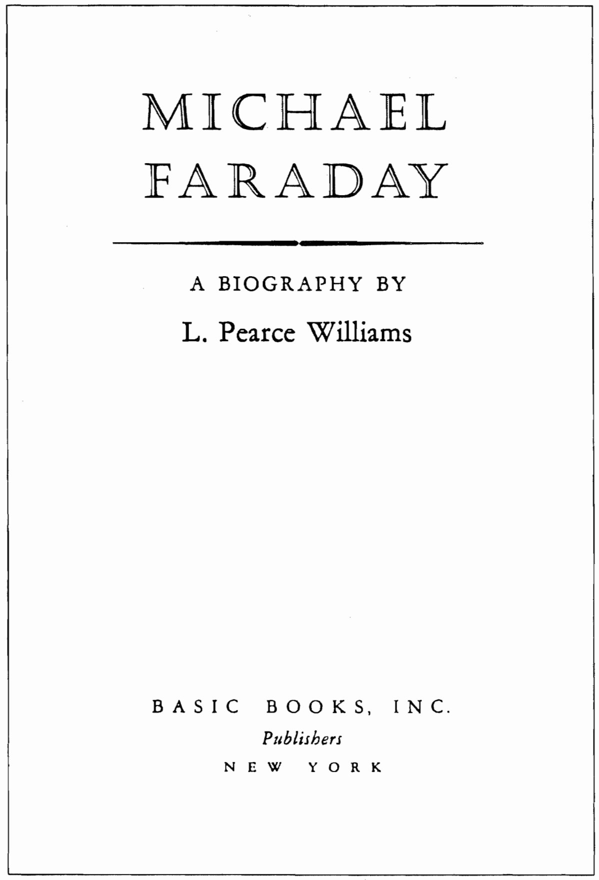
Recording Angel, it is white as snow.
Ah God, a fitting messenger was he
To show Thy mysteries to us below.
Child as he came has he returned to Thee.
Would he could come but once again to show
The wonder-deep of his simplicity.

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.

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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.

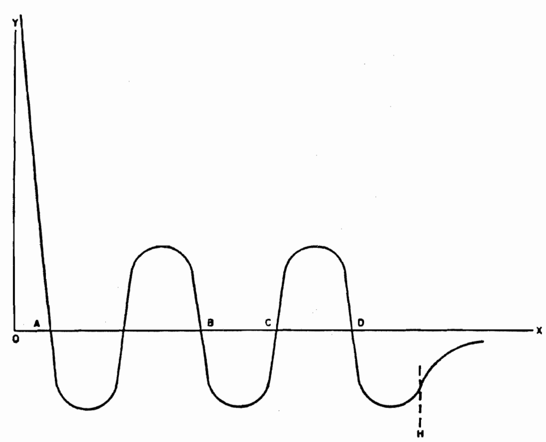
6. T. Martin, ed., *Faraday's Diary (1820-1862)*, 7 Vols., Bell, London, 1931-1936.

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

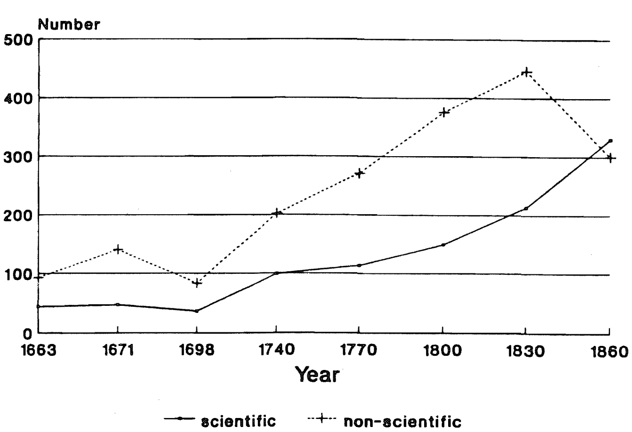
June Z. Fullmer, Ohio State University, and Melvyn C. Usselman, University of Western Ontario

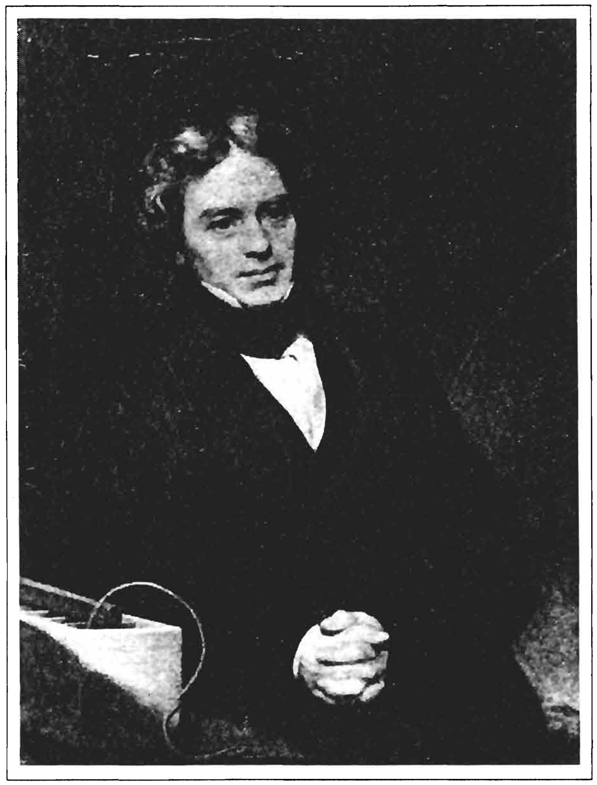
On Thursday, 8 January 1824, the meeting of the Royal Society had, as one order of business, a ballot to elect (or not) Michael Faraday to the Fellowship of the Society. According to established custom, in the absence of the President, Sir Humphry Davy, the Vice President of the Society, Sir Everard 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 a white or black marble from one of the attached bags and dropping it into the box. This noisy, disruptive process continued while the Secretary read to the group the learned paper selected for presentation that day. When the ballot box had made its rounds, Sir Everard counted the votes. Faraday's election had been nearly unanimous, there being but a single black ball in the "nay" drawer. Still, the business of making Faraday a Fellow was not yet concluded, for at the next sitting of the Society (15 January 1824), with Sir Humphry Davy, P.R.S., in the chair, Faraday paid his admission Fee "and the usual sum in lieu of Annual Contributions," and "signed the Obligation in the Charter Book". Sir Humphry then shook his hand, and Faraday officially became "F.R.S".

The election result must have been very gratifying for Faraday, for the period preceding the election had been a stressful one. Membership in the Royal Society meant a lot to him, since it certified that he was an accomplished natural philosopher whose researches merited the attention of the world's scientific community. In 1838 when Spring-Rice, Chancellor of the Exchequer, asked Faraday why he had received a pension (granted in 1835) from the Crown, what titles had he? Faraday replied: "One title namely, that of F.R.S., was sought and paid for; all the rest [and there were many] were spontaneous offerings of kindness and goodwill from the bodies named" (2). Faraday's unguarded response shows how distressing the process had been. What price had he paid? What had his fight for recognition of his scientific abilities cost him?

In the early stages Faraday's election had not been a foregone conclusion. During the eight months comprising the ten "regular" meetings of his candidacy, there had been rumors that Faraday's scientific achievements owed much to unacknowledged contemporaries. On 30 May 1823, an angry Sir Humphry Davy had ordered Faraday to remove his certificate. Throughout the period the prevailing sentiment within the Society was to reduce the number of Fellows by restricting the intake of new members (3). Faraday, caught on the cusp of two worlds, one dying, the other struggling to be born, faced genuine threats which forced him into intensive lobbying for votes. By the day of his election, however, his success was assured.

FIG.1 ROYAL SOCIETY FELLOWS,NUMBER





Michael Faraday
(After a painting by T. Phillips)

Consider first the atmosphere within the Royal Society. The death of Sir Joseph Banks in June of 1820 provided an opportunity to institute changes within the Society (4). The reformers aimed primarily to make the Society more scientific, chiefly by increasing the proportion of members actively engaged in the sciences and by achieving a stronger voice for those members in the governance of the Society. Quantitative analysis of the composition of the fellowship reveals the magnitude of the problem. As shown in figure 1, total membership in the Society had increased continuously from a low of 119 in 1698 to 659 in 1830 (5). Though the majority of the Fellows were "cultivators" of science, only about 30% of them could be loosely termed "scientific" Fellows (figure 2) (6). In his caustic attack on the Royal Society in 1830, Augustus B. Granville wrote that he could find in the membership only "thirty really illustrious men of science," all the rest being "either mere lookers on - indifferent spectators - or, at most, cultivators of what beds of flowers they found in the rich garden of natural knowledge when they first entered it" (7). Furthermore, during Banks' tenure as president (1778-1820), scientific fellows had always been in a minority on the Council, whose 21 members governed the Society (figure 3) (8).

William Hyde Wollaston served as temporary President for the five months of the term which remained after Banks' death; Sir Humphry Davy succeeded him. Davy tried to institute the reforms most wanted by the scientific membership, the group to which he owed the near unanimity of his election (9). The reformers sought to decrease the influx of new members by scrutinizing the scientific credentials of candidates more closely than had been done in the past. If the members were doubtful of a candidate's worthiness, Davy encouraged them to cast negative votes at election time. His recommendation was initially heeded, for the number of new Fellows decreased in 1823 (10). In 1822 John Herschel wrote Charles Babbage that (11):

I think Hamilton had better not be proposed at present. I talked to Davy about him, who of course could have no personal feeling about it and spoke very sensibly on the subject. What he has lately said in the Society has had its full effect ... No ballot I dare say now will pass for a long time without a sharp contest and discussion of the merits of candidates.

Further, Davy had not hesitated to act autocratically when he thought it necessary. After the certificate of Sir Francis Schuckburgh was introduced in December of 1823, Davy wrote at the top, "No qualifications mentioned" and across the bottom, "This certificate ought not to have been presented, there being no qualifications mentioned. H.D." (12). Davy's demand for qualifications could explain why Faraday waited until 1823 before he sought election, for by that time he had published more than 37 scientific papers, three of them in the *Philosophical Transactions* (13). His publication record was explicitly noted in the statement of qualifications accompanying his certificate (14):

Mr. Michael Faraday, a gentleman eminently conversant in chemical science, and author of several papers, which have been published in

FIG.2 ROYAL SOCIETY FELLOWS,PERCENT

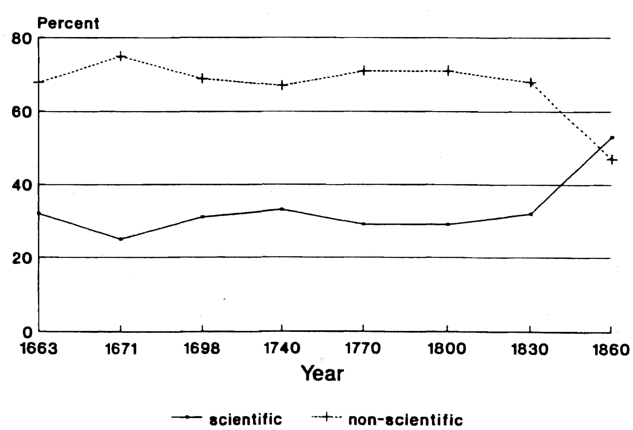
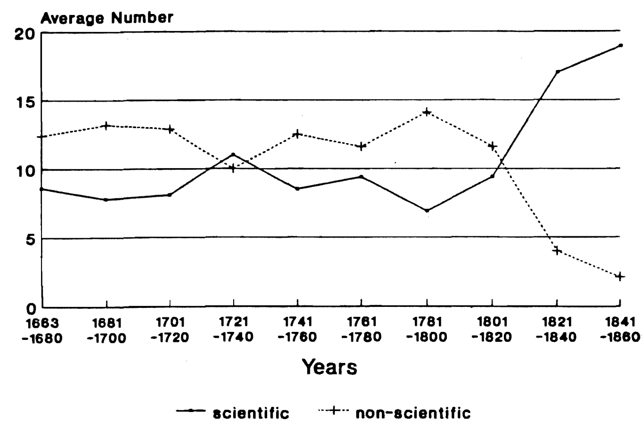


FIG.3 ROYAL SOCIETY COUNCIL,MEMBERS



the "Transactions" of the Royal Society, being desirous of becoming a Fellow thereof, we, whose names are undersigned, do of our personal knowledge recommend him as highly deserving that honour, and likely to become a useful and valuable member.

Faraday's worthiness can be further emphasized by comparison with others elected at about the same time. The four persons elected immediately before him were (15):

19 June 1823	Sir John Murray, military general
20 Nov. 1823	John Bayley, antiquary
	Rev. Daniel Creswell, divine and mathematician
27 Nov. 1823	A. Mervin Storey, M.A. (Oxon)

and the four immediately following were:

15 Jan. 1824	Charles Scudamore, physician
22 Jan. 1824	Thomas Amyott, antiquary
5 Feb. 1824	William Wavell, physician
19 Feb. 1824	Rev. Edward Maltby, bishop

Election of such a scientifically undistinguished group illustrated that, whatever the qualfications deemed requisite for successful election, they had not functioned to render many candidates ineligible. To the extent that the reformers within the Society had an impact on the election process, in Faraday's case their efforts would have been positive. Scientific support alone could guarantee election since a large majority of those who regularly attended meetings were science-minded. At least this was true if the science supporters were not themselves divided. Babbage, for one, pointed out how scientific bickering could harm a candidate's chances (16):

... if [a candidate] A. B. had the good fortune to be perfectly unknown by any literary or scientific achievement, however small, he is quite sure of being elected as a matter of course. If, on the other hand, he has unfortunately written on any subject connected with science, or is supposed to be acquainted with any branch of it, the members begin to inquire what he has done to deserve the honour; and, unless he has powerful friends, he has a fair chance of being black-balled.

There was no evidence of such a split in Faraday's scientific backing, for the 29 signatures on his certificate showed impressive support from natural philosophers, physicians and surgeons (17).

Special importance attached to the first few names on the certificate because they represented Faraday's "proposers" and served as an advertisement of his suitability. Richard Phillips, who sponsored Faraday's application and arranged for the opening signatures, wrote with delight to Faraday shortly after the certificate was first hung: "Did it well I thinks - *Wollaston, Children, Babington, Herschel*" (18). In his reply, Faraday indicated his approval (19):

A thousand thanks for your kindness - I am delighted with the Names - Mr. Brande had told me of it before I got your note and thought it impossible to be better.

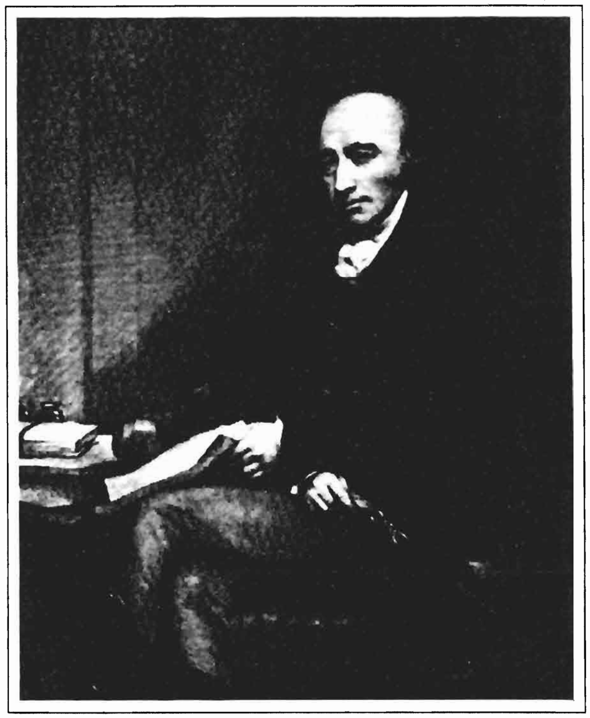
The four leading sponsors represented different constituencies in the Royal Society: John G. Children, whose job at the British Museum owed much to Davy's support, was Davy's long-time friend; Dr. William Babington was a member of the "old guard" of the Society who viewed reform with suspicion, and John W. Herschel was the most highly-regarded of the younger, reform-minded Fellows.

Wollaston's name at the head of the list served two important purposes. He had been President for a short period in 1820, and he was widely admired among the scientific Fellows for his support of reform, his scientific achievements, his intellect and his independence. Nearly Davy's equal in international stature he had, in fact, been the reformers' first choice as successor to Banks. Wollaston championed individualism and readily admired ability in others. His name had also been first on John Dalton's certificate (Dalton, like Faraday a non-conformist, had been made a Fellow in March 1822). Above all, Wollaston's prominent support laid to rest any suspicions that ill feelings remained from the Wollaston/Faraday misunderstanding over the discovery in 1821 of electromagnetic rotation.

On the final day of his interim presidency in 1820, Wollaston had delivered the discourse which accompanied the awarding of the Society's Copley Medal to Hans C. Oersted for his discovery of electromagnetism. In the oration, Wollaston praised the discovery with presidential grandiloquence (20):

... by the very important researches of Professor Oersted, a very intimate relation is established between electricity and magnetism.

Let us hope that the gleam of light which thus beams upon us may be the dawn of a new day in which the clouds that had hitherto veiled from our sight the hidden mysteries of light and heat, of electricity and magnetism, may be dispelled, that the real nature and relation of these imponderable agents may be revealed to us, that truths most important to the advancement of natural knowledge may burst forth in public splendour and complete the series of wonders that we have lived to witness.



William Hyde Wollaston

Extending Oersted's ideas, Wollaston concluded that the magnetic power of an electric current acted "circumferentially round its axis," and thus a current-carrying wire might be made to spin about its own axis under the influence of an external magnet (21). In April 1821, he and Davy tried unsuccessfully to achieve the predicted result at the Royal Institution. A few months later, and quite independently, Faraday discovered a way to effect electromagnetic rotation; he sent the results for publication in October 1821 (22). Shortly thereafter, Faraday began to hear rumours that he had failed to acknowledge Wollaston's contributions.

The details of the drama that ensued are presented elsewhere; its denouement was important (23). Faraday had been accused of stealing Wollaston's ideas, but Wollaston himself believed Faraday to be innocent of any wrong-doing, for he wrote to Faraday (24):

Sir - You seem to me to labour under some misapprehension of the strength of my feelings upon the subject to which you allude.

As to the opinions which others may have of your conduct, that is your concern, not mine; and if you fully acquit yourself of making any incorrect use of the suggestions of others, it seems to me that you have no occasion to concern yourself much about the matter.

Ultimately, Henry Warburton, the man chiefly responsible for the whispering campaign against Faraday, was swayed to make an explicit promise to repair any damage that may have been done. A few weeks after Faraday's certificate had been posted, Warburton wrote him (25):

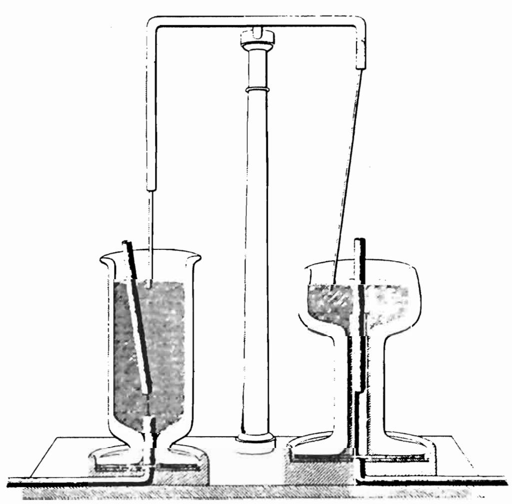
Sir, I have read the article in the "Royal Institute Journal" (vol. XV, p. 288) on electro-magnetic rotation; and without meaning to convey to you that I approve of it unreservedly, I beg to say that, upon the whole, it satisfied me, as I think it will Dr. Wollaston's other friends.

Having everywhere admitted and maintained that on the score of scientific merit you were entitled to a place in the Royal Society, I never cared to prevent your election, nor should I have taken any pains to form a party in private to oppose you. What I should have done would have been to take the opportunity which the proposing to ballot for you would have afforded me to make remarks in public on that part of your conduct to which I objected. Of this I made no secret, having intimated my intention to some of those from whom I knew you would hear of it, and to the President himself.

When I meet with any of those in whose presence such conversation may have passed, I shall state that my objections to you as a Fellow are and ought to be withdrawn, and that I now wish to forward your election.

Warburton could easily enough change his mind, but it took much effort to undo the damage his accusations had wrought. We know that Davy had been moved, likely by Warburton, to oppose Faraday's election. In notes appended to a copy of a letter to Warburton, Faraday wrote (26):

1823. In relation to Davy's opposition to my election at the Royal Society



Faraday's apparatus for demonstrating the existence of "electromagnetic rotation".

Sir H. Davy angry, May 30.

Elsewhere, Faraday had been more explicit (27):

Sir H. Davy told me I must take down my certificate. I replied that I had not put it up; that I could not take it down, as it was put up by my proposers. He then said I must get my proposers to take it down. I answered that I know they would not do so. Then he said, I as President will take it down. I replied that I was sure Sir H. Davy would do what he thought was for the good of the Royal Society.

Bence Jones, in his biography of Faraday, reported that (28):

Faraday also said that one of his proposers told him that Sir H. Davy had walked for an hour round the courtyard of Somerset House, arguing that Faraday ought not to be elected.

While it is not possible to say precisely what transpired, it is possible to determine one catalyst for Davy's anger by examining Faraday's activities after the quarrel, to see who had to be pacified. Since Faraday noted that Davy reproached him on 30 May, it appears likely that Warburton had spoken to Davy about Faraday's candidacy at the meeting of the Royal Society on 29 May. In his letter to Faraday on 8 July, Warburton allowed that he had read Faraday's paper, which recounted why Faraday had not acknowledged Wollaston's work on electromagnetic induction in his two publications of October and December, 1821. Thus Warburton learned that in 1821 Faraday had taken his first paper on electromagnetic induction to Wollaston, prepared to ask him for permission to refer to his work - at the time unpublished - but had not found Wollaston at home. For the second paper he was able to get in touch with Wollaston, who had by then witnessed some of Faraday's newest experiments. Faraday asked Wollaston if he could refer to his work "in correction of the error of judgment in not having done so before." Wollaston's view, as Faraday recalled it, was as follows (29):

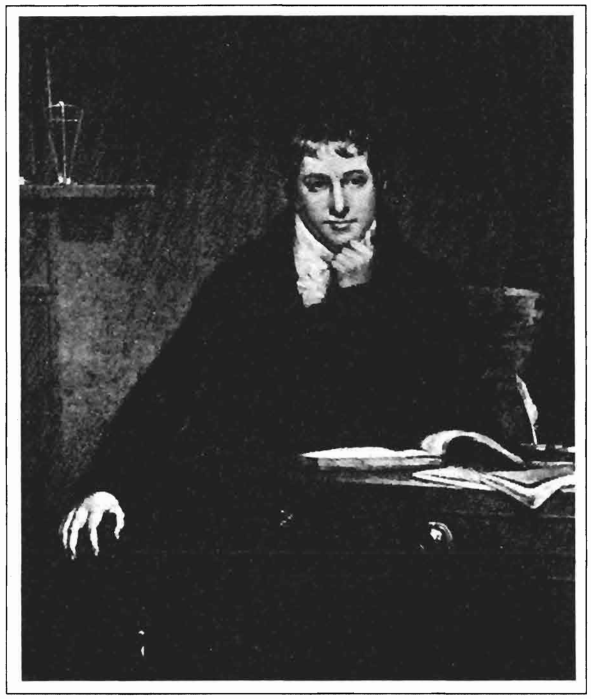
The impression that has remained on my mind ever since (one-and-twenty months), and which I have constantly expressed to everyone when talking on the subject, is that he wished me not to do so. Dr. Wollaston has lately told me that he cannot recollect the words he used at the time; that as regarded himself his feelings were it should not be done, as regarded me, that it should, but that he did not tell me so. I can only say that my memory at this time holds most tenaciously the following words, 'I would rather you should not;' but I must of course have been mistaken.

This published acknowledgment of Wollaston's cooperation evidently mollified Warburton. It may even have caused him to regret questioning Faraday's integrity, or at least to rue carrying those doubts to Davy, for Davy could not help but be offended by criticisms of his colleague. Every project on which Faraday had worked had Davy's blessing: at times the two men had worked side by side; at other times Faraday undertook experiments at Davy's behest. In addition, Davy patently groomed Faraday for Fellowship (as he had groomed his younger brother, Dr. John Davy, in much the same way) by suggesting avenues for his research, by editing his papers before they were to be read and published, and by frequently sponsoring Faraday's attendance at Royal Society meetings. His protégé was in danger of being publicly accused of appropriating another's scientific ideas. Moreover, that accusation would come from a man with power in the Royal Society, a Council member who had much preferred Wollaston over Davy as President.

Davy had wanted the Presidency as badly as Faraday had wanted to be a Fellow. To gain that office, Davy had personally solicited votes and had ardently promised reform of the Society to Wollaston and his supporters. He was working very hard at the tasks reform required, with the result that his time for experimenting was limited. Now he was faced with the possibility that Warburton would rise before the entire group and in a bitter speech heap calumny on Faraday, and through him, on Davy. The only certain way to prevent that from happening was to insure that Faraday not come up for election. This could be done simply by removing Faraday's certificate. Perhaps, after Warburton had suggested that Faraday lacked the moral character required of Royal Society candidates, Davy's well-known temper transformed a minor irritant into major confrontation. John Herschel, for example, had opposed Davy's bid for the presidency of the Society in 1820 on the basis of perceived weaknesses in Davy's character (30):

> The reasons for wishing that Davy should be opposed are grounded solely on his personal character, which is said to be arrogant in the extreme, and impatient of opposition in his scientific views, and likely if power were placed in his hands to oppose rising merit in his own line ... [for example] Davy, in consequence of Berzelius's repugnance to admit his views on ye simple nature of chlorine was so personally incensed at him, as to exert all his influence (& with success) to procure his rejection, when proposed, during his stay in England [Jun-Nov, 1812] as an honourary Member of ye R.I.

Davy's antipathy toward Berzelius soon passed, however, and his signature was the first on Berzelius's certificate for election as a Foreign Member of the Royal Society (first read 26 November 1812). It is not improbable that his opposition to Faraday's candidacy evaporated just as quickly. After Faraday told Davy on 17 June that Wollaston had been consulted and had not contested Faraday's priority, Davy's anger appears to have dissipated. On 29 June, Davy, in a note to Faraday, hoped he would have "health and success during the summer"; he signed it, as he always had before, "very sincerely your friend and well-wisher." On 23 July he signed another note to Faraday, written in great haste, "your sincere friend."



Humphry Davy

On 28 July, when he had reason to write again, he signed himself "I am Dear Mr. Faraday/very truly your friend &/well wisher" (31).

Perhaps Davy's anger was fueled by uncertainties he himself held about Faraday's conduct in their complementary researches on the liquefaction of gases. In the spring of 1823, when Davy was out of town on his annual fishing trip, Faraday took advantage of the cold weather and some free time to work "upon frozen chlorine," which he said represented a "favourite object" for his research. He used as his starting material chlorine hydrate, a substance Davy had earlier identified as a compound. When Davy returned he asked Faraday what laboratory work he had in hand. Upon hearing Faraday's account, Davy suggested to him that he try heating the solid in a closed tube. He did not tell Faraday what he expected to be the outcome. On carrying out Davy's suggestion, Faraday, somewhat to his amazement, found an oily yellow liquid produced. He repeated the experiment, now using a sealed, "bent" tube. He was able to distill the liquid to one end and subsequently identified it as liquid chlorine. Dr. John Ayrton Paris, present when Faraday first performed the experiment, reported in his biography of Davy that he told Davy at dinner that evening about the puzzling appearance of the liquid (32). Paris' account, in addition to suggesting that Faraday left to his own devices would have been led to make the experiment on his own, also insinuated that Sir Humphry was a liar (33):

> Upon mentioning the circumstance [the disappearance of the yellow

liquid from the closed, bent tube when it was cut open] to Sir Humphry Davy after dinner, he appeared much surprised; and after a few moments of apparent abstraction, he said "I shall enquire about this experiment tomorrow".

Paris' claim that Davy "appeared much surprised" slyly misrepresented the situation since, in Davy's brief addendum to Faraday's first paper on the liquefaction of the gases, Davy had pointed out (as had Faraday) that Faraday initially made the experiment at his suggestion. Davy also said he had anticipated liquefaction of the chlorine as one probable result. Faraday for his part said that he had "no doubt" that Davy had foreseen the result.

John Davy, a more reliable reporter than Paris about Sir Humphry, but also a man of exquisite sensitivity with respect to his brother's reputation, was triggered to rebuttal by what Paris had written. In his biography of Sir Humphry, John Davy declared (34):

... the account which Dr. Paris has given [of the condensation of the gases] in his work is partial, and, as it appears to me, incorrect and unjust, and not borne out by the published statements either of Mr. Faraday or my brother ... Dr. Paris's narrative imparts to the reader the impression, that Mr. Faraday was very unjustly treated; that Sir Humphry Davy took advantage of his situation, and endeavoured to appropriate to himself part of the merit of a discovery to which he was in nowise entitled ... I am surprised that Mr. Faraday has not come forward to do him justice.

This complaint galvanized Faraday to reflect again, now with the perspective gained by the passage of time and the death of Sir Humphry, on all of the events surrounding his election to Fellowship. As a result he added some details to the written record about his relationship with Davy. The response which he prepared for John Davy paralleled in a way his earlier response to Warburton's charge. He first established a two-part "diary", the initial section of which he titled "Electro-Magnetism", the second, "Condensation of Gases". Eventually he inserted both parts into his copy of Paris' biography. The second part read as follows (35):

Condensation of gases

Before my account of the Hydrate could be printed, the other expts were made & Davys note to the R.S. read

Davy was Honorary Profr. until May 1824

Mar 1823 My paper on cmpd. hydrate chln. Quar Jour xv 71 April 1823

13 Mar 1823 Mine on fluid chlorine read 13 Mar 1823 Phil Trans 1823 p 160 Mr. Brande secy R.S.

19 Mar 1823 Davys note to my paper read 19 Mar 1823 Phil Trans 1823 p 164

Mar 1823 Mr. Brandes note to my paper Quar Journ xv 74. April 1823

10 Apr 1823 Mine on condensation of several gases read 10 April 1823 - Phil Trans 1823 p 189

17 Apr 1823 Davy on appl of conden gas as Mech Agnt. read 17 Apr 1823 Phil Trans 1823 p 199

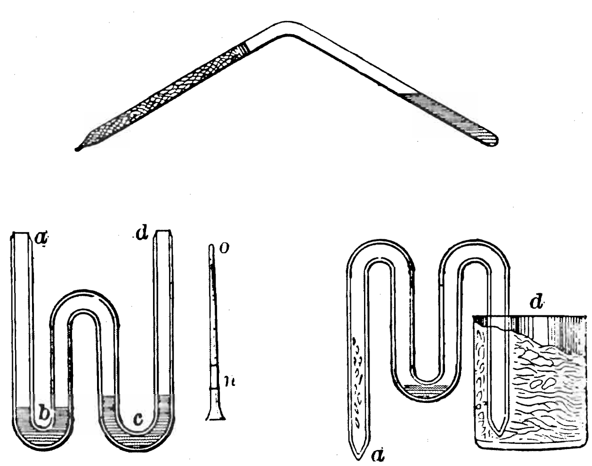
1 May 1823 Davy on change of vol by heat - read 1 May 1823 Phil Tr. 1823 p 204

Decr. 1823 My Historical Statement Quar Jour xvi 229. Jany 1824

8 Jany 1824 My Election as F.R.S. 8 Jany 1824 names to my certificate

This list of events, a product of Faraday's passion for accuracy and of his habitually meticulous approach to a problem, showed how the discovery of the liquefaction of chlorine plunged both Davy and Faraday into feverish activity as they sought to liquefy other gases. Faraday's second paper, "On the Condensation of Several Gases into Liquids," read to the Royal Society on 10 April, was supplemented by Davy with "On the Application of Liquids Formed by the Condensation of Gases as Mechanical Agents," read on 17 April. Davy further presented to the Society on 1 May his "Appendix to the Preceding Paper. On the Changes of Volume Produced in Gases in Different States of Density by Heat" (36).

Faraday's memorandum seems to have been an outline for a formal account he meant to write. Unquestionably it provided part of the data for the long letter he wrote to Richard Phillips, subsequently published in *Philosophical Magazine* (37). Faraday here revealed that he understood the full force of Paris' remarks, for he was at pains to show that while Sir Humphry may have anticipated liquefaction, he had not so informed Faraday. Faraday suggested that "[p]erhaps he left me unac-



Apparatus used by Faraday in his experiments on the liquefaction of gases.

quainted with ... [the results anticipated] to try my ability," conceivably because Davy had adopted such a strategy with him from time to time. Faraday wrote (38):

I have no doubt that he had them [expectations that chlorine would be liquefied]; and though perhaps I regretted losing my subject, I was too much indebted to him for much previous kindness to think of saying that that was mine which he said was his. But *observe* (for my sake), that Sir H. Davy nowhere stated that he told me what he expected, or contradicts the passages in the first paper of mine which describe the course of my thought, and in which I claim the development of the actual results. All this activity in the condensing of gases was simultaneous with the electro-magnetic affair; and I had learned to be cautious upon points of right and priority (38).

Frank and revealing as his letter was, Faraday had not acknowledged the existence and grace of Davy's compliment. Davy wrote that his conjecture had "been proved by experiments made [by Faraday] with ... much industry and ingenuity, and which I have had the pleasure of communicating to the Society" (39). Davy's tribute was both deserved and deliberate - Faraday's experiments displayed to splendid advantage his inventiveness and his extraordinary ability in chemical manipulation. In addition, by assisting his colleague toward Fellowship in the Royal Society, Davy also subtly enhanced the reputation of the Royal Institution, an action which would hardly go unnoticed. As President of the Royal Society, he could neither initiate nor sign Faraday's certificate, but he could aid his protégé's cause by providing, shortly before the election, a deft testimonial to Faraday's scientific ability.

Davy's addenda did more than establish priority and praise Faraday's ability. His brief notices enlarged the conceptual base for the phenomena Faraday had observed. In 1823 Faraday had called Davy's note "important"; in 1836 he acknowledged Davy's contribution by admitting that he "had not reasoned so deeply as [Davy] appears to have done", a justifiable admission. Both of Faraday's liquefaction papers were conceptually meagre. By initiating a discussion to account for all of the observed liquefaction phenomena, Davy enriched Faraday's experiments, cementing them firmly within accepted scientific doctrine (40).

Nonetheless, taken together, the papers still were incomplete, lacking what since has been called "der Anstand der Frage". Faraday's "Historical Statement Respecting the Liquefaction of Gases", published just a few days before his election, took care of the omission (41). (The appearance of the "Historical Statement" showed that it, together with Faraday's two papers and Davy's three supplements, comprised the totality of their research findings. Their great haste to publish had led to the fragmentation.) His report stood as a tacit warning to himself, to Davy, and to all scientists - searching the literature for prior pertinent accounts is an integral part of the research process. Faraday found that the literature yielded accounts of several attempts to liquefy gases. At least one of them, that by the poet and inventor, Thomas Northmore (1766-1851), in 1805 reported the successful liquefaction under compression of both chlorine (not, of course, called by that name) and of sulfur dioxide. Nor had Northmore placed the report of his discovery where few would see it - *Nicholson's Journal* published his results in two parts (42). Northmore's third paragraph proffered a small surprise (42):

I communicated [my idea that "the various affinities which take place among the gases under the common pressure of the atmosphere, would undergo considerable alteration by the influence of condensation"] ... to the late chemical operator in the Royal Institution, a gentleman eminently conversant in the science, and with whom I was then engaged in a series of experiments: he not only approved of my design, but seemed to think it not improbable that an extensive field might thus be opened to future discoveries.

Until some time in 1804 the title "chemical operator" at the Royal Institution belonged to John Sadler (43). Because Davy appeared to have been unaware of Northmore's experiments (he had several opportunities for recall, since he read Faraday's paper before it was presented to the Royal Society, he presented it, and he also corrected the proofs for *Phil. Trans.*) the conclusion is forced on us that Sadler had not mentioned the incident to him, and that either Davy had not read the papers in *Nicholson's Journal*, or, if he had, he had forgotten them. There is also the possibility that some sort of primitive recollection of Northmore's results lingered in Davy's mind without a direct association; perhaps that slight memory trace inspired Davy to think that chlorine could be liquefied. Still, Davy's own conceptual base easily could have led him to the same conclusion. In all fairness it must be recalled that Northmore, unlike Faraday, employed fairly elaborate apparatus and that he offered little in the way of explanation of what he had observed (44). The simplicity of Faraday's experimental approach, heating substances in closed, bent tubes, beautifully exploited Davy's and his conceptualization about the nature and behavior of gases and liquids. Northmore's more elaborate attempts similarly exploited a conceptualization, but it was one derived from an intellectual base of about 1800, somewhat different from that of Faraday and Davy in 1823.

The matter was, to all intent, closed in 1824, and Faraday won his F.R.S. However, in 1844 Faraday returned to the subject, presenting to the Society his observations "On the Liquefaction and Solidification of Bodies Generally Existing as Gases" (45). He admitted to a "constant desire on my mind to renew the investigation", occasioned by the publication of papers by M. Thilorier, coupled "with considerations arising out of the apparent simplicity and unity of the molecular constitution of all bodies when in the gaseous or vaporous state ..." Passage of 20 years had altered neither the tenor of Faraday's papers nor the brilliance of his experimentation, but his conceptualizations had altered.

The events preceding Faraday's successful election must have been an anguished time for both Davy and Faraday. The months during which Faraday's certificate was posted saw difficulties arise between two decent men: both were ambitious, both were proud, both were honorable, and both were bound by codes of proper behavior. Those difficulties cast a shadow over the balloting process of 8 January 1824. Davy could not risk being present, should Warburton, or one of his friends, change his mind and speak against Faraday - and Warburton by his own admission had, on 19 May 1823, told several people of his objection to Faraday's election. Davy - the embodiment of caution - did not go to the Royal Society meeting that day.

Who cast the black ball? We may probably never know, but we know some of the people it cannot have been. We know it could not have been Davy, nor could it have been Wollaston, because both were absent for the voting. It could not have been Warburton, since he promised Faraday his support. We know also the names of 29 others who would not have black-balled Faraday, those who had signed his certificate. We can say that whoever it was may have been moved by Warburton's first denunciation (46).

Despite the happy outcome of the election, the events of the summer produced repercussions. Faraday, in a letter to Warburton on 29 August, in which he thanked him for his support, described his own feelings. "Two months ago", Faraday wrote (47):

> I had made up my mind to be rejected by the Royal Society as a Fellow, notwithstanding the knowledge I had that many would do me justice: and, in the then state of my mind, rejection or reception would have been equally indifferent to me. Now that I have experienced so fully the kindness and liberality of Dr. Wollaston, which has been constant throughout the whole of this affair, and that I find an expression of good-will strong and general towards me, I am delighted by the hope I have of being honoured by Fellowship with the Society ...

Faraday got his wish; no one could deny his scientific credentials. But what of Davy? In 1836 Faraday recalled (48):

> I was by no means in the same relation as to scientific communication with Sir Humphry Davy after I became a fellow of the Royal Society as before that period ...

Faraday thought Davy now behaved guardedly in his presence, if not downright cautiously. When Faraday said he had "paid for" his Fellowship, he meant he had written a public apology to Wollaston, he had mollified a crusty Warburton, and he had lost Davy's closest scientific confidences. Still, outwardly things remained the same. Not only did Davy see and revise Faraday's manuscripts, but they went to the Royal Society "through his hands", and Davy saw and revised the printer's proofs for *Phil. Trans.*. Faraday saw these acts as a "great kindness", saving him from committing "various grammatical mistakes", as well as removing "awkward expressions ... which might also have remained." Yet, although Faraday and Davy continued as colleagues, to Faraday it seemed as if they had become colleagues on a different level.

Davy's actions were predictable and complexly motivated. He did not want to create the impression that Royal Institution men had taken over the scientific community. Faraday's memorandum of events showed that Davy's resignation as Honorary Professor at the Royal Institution in May of 1824 was part of the sequence played out over Warburton's charges. While Davy continued as President of the Royal Society (Brande was Secretary) he was also working for the government on naval ship corrosion, a project he could not share with Faraday. It meant adopting a new level of behavior - Davy could no longer afford to be Faraday's scientific intimate.

Although one would be hard-pressed to think of a candidate more deserving of Fellowship in the Royal Society, or of one less likely to have advanced his reputation at another's expense, Faraday's candidacy became entwined with the desire for reform inflaming some of the Fellows. The events offer an abrupt and unanticipated glimpse into the complex politics operating within the Society. Whatever else might be said of it, the Royal Society was not a placid, untroubled body solely preoccupied with the generation and contemplation of scientific knowledge. After the death of Sir Joseph Banks, reform became a continual irritant. Reform meant more than redressing grievances of mathematicians and astronomers; it also meant recognition, within the Society and without, voiced or unvoiced, that the Society existed primarily to honor those whose main occupation was science; and that being a "scientist" (the word was not coined until 1841) meant pursuing a "profession". Faraday belonged on the rolls of the Society because he was an accomplished and brilliant professional.

When Davy wrote across Shuckburgh's certificate that it was unacceptable "there being no qualifications mentioned", he wished to establish a fundamental tenet of professionalization. One measure of "qualification" - publication - especially publication in the pages of *Philosophical Transactions*, paralleled a measure used by the older, established professions. They committed their members to the public performance of certain rituals: administering the sacraments, for example, or meting justice. That public performance, however, could not occur until an aspiring professional had undergone certain rites of passage and had met established standards for performance. For the practicing scientist, rites of passage were not clear cut. In 1821 British law had spoken directly to the issue, deeming that chemists were not to be regarded as "professionals", but were to be regarded as mechanics (49). The distinction arose because it did not seem to the judges that chemists were privy to a body of peculiar knowledge: some of them (Faraday had been a prominent witness in the widely-reported case) obviously had not attended a university; some spoke with "barbarous" accents; some maintained suspect political affiliations; some espoused unconventional religious beliefs. Above all, scientific knowledge appeared to accrue to anyone who took the trouble to acquire it, frequently without guidance from established "professionals". Yet, the Royal Society, by asking that proposed Fellows exhibit certain "qualifications", had, in a way, asked for the public performance of a ritual - publication of scientific work previously reviewed by "professional" peers. Such a requirement introduced a unique condition into the requirements for a "professional" scientist. Unlike the clergyman, for example, whose license to baptize, once granted, endured, the professional natural philosopher or scientist had, in effect, to continue to renew his license by asserting his proficiency anew with each publication. Faraday could not be faulted on such a score, for he had several times offered up his "qualifications" for public scrutiny.

The professions, moreover, commonly claimed adherence to a set of ethical or moral guidelines. When Warburton set out to challenge Faraday's candidacy, he raised questions about Faraday's honesty. Clearly Warburton, viewing the community of illustrious men of science from the periphery, saw it as one whose members cleaved to a code of ethical behavior. That he eventually publicly absolved Faraday indicated that he thought Faraday had operated within acceptable boundaries - although Warburton, a hard man to shake from an opinion, declared that he was only marginally pleased with Faraday's explanation and apologies. Faraday's difficulties around his election dissolved when he showed that he had, indeed, behaved professionally and adhered to the established code of behavior. Before the election 29 members were convinced (if any of them ever had doubts); many more expressed their conviction by voting for him on 8 January 1824.

The circumstances surrounding Faraday's election to the Royal Society reveal an emerging consensus within its membership. Scientific achievement was becoming a sufficient criterion for election. In a few more decades it would become the only criterion. The aristocratic Fellows lost interest in science as it became increasingly specialized and less comprehensible to the dilettante. The power of the President passed to the Council, which achieved a majority of scientific members within a few years of Faraday's election. The Royal Society was on the verge, after a century and a half of existence, of becoming a "scientific" society, and Fellowship in it was to be reserved for scientific professionals.

References and Notes

1. This account of the election procedure is based on the *Journal Book of the Royal Society*, Vol. XLIII, 1819-1823 and Vol. XLIV, 1823-1827; on A. B. Granville, *The Royal Society in the XIXth Century*, Longmans et al, London, 1836, *passim*; and on J. Davy's *Memoirs of the Life of Sir Humphry Davy*, 2 Vols, London, 1836, Vol. 2, pp. 126-138.

2. H. Bence Jones, *The Life and Letters of Faraday*, 2 Vols, 2nd ed., Longmans, London, 1870, Vol. 2, p. 97.

3. Others refer to the social context surrounding Faraday's election as a "web of conflicting loyalties and commitments". See D. Gooding and F. A. J. L. James, *Faraday Rediscovered*, Macmillan, London, 1985, p. 7.

4. D. P. Miller, *The Royal Society of London 1800-1835: A Study in the Cultural Politics of Scientific Organization*, Ph.D. thesis, University of Pennsylvania, 1981; also "Between Hostile Camps: Sir Humphry Davy's Presidency of the Royal Society of London 1820-1827", *Brit. J. Hist. Sci.*, **1983**, *16*, 1-47.

5. H. Lyons, *The Royal Society, 1600-1940*, Cambridge University Press, Cambridge, 1944, p. 341.

6. *Ibid.*, pp. 341-342. We have accepted for the purposes of our argument the numbers reported by Lyons, even though his compilation has anachronistic weaknesses. For the period 1740-1860, he counted as "scientists" all Fellows categorized as mathematicians, astronomers, chemists, physicists, engineers, surveyors and hydrographers, instrument-makers and opticians, physicians and surgeons, botanists, geologists, zoologists and naturalists. Although one may quarrel with some of his assignments, there can be little doubt that his numbers reveal that the "makers" of science were a significant minority in the Royal Society until at least the mid-19th century.

7. A. B. Granville, *Science Without a Head*, Printed for the Author, London, 1830, p. 51.

8. Reference 5, p. 342.

9. For an analysis of the controversy surrounding Davy's election to the Presidency in 1820, with specific reference to the actions of the scientific "reformers" see L. F. Gilbert, "The Election to the Presidency of the Royal Society in 1820," *Notes Rec. Roy. Soc.*, **1955**, *11*, 256-279.

10. Reference 7, p. 97.

11. Herschel to Babbage, 26 April 1822, John Herschel papers, Royal Society of London library. Both Herschel and Babbage were active in the movement among the scientific Fellows to reform the Society. The "Hamilton" referred to was probably Rev. H. P. Hamilton, elected FRS in 1828.

12. Fellowship certificate of Sir Francis Shuckburgh, Royal Society archives. Quoted in J. Z. Fullmer, "Humphry Davy, Reformer," in S. Fagen, ed., *Science and the Sons of Genius: Studies on Humphry Davy*, Science Reviews, London, 1980, p. 84. Despite Davy's disapproval, Shuckburgh was successfully elected on 11 March 1824, two months after Faraday's election.

13. *Royal Society Catalogue of Scientific Papers* (1800-1863), The Royal Society, London, 1867. Jeffreys, a more reliable source, listed a total of about 70 publications; see A. E. Jeffreys, *Michael Faraday: A List of his Lectures and Published Writings*, Chapman and Hall, London, 1960.

14. Quoted in reference 2, Vol. 1, p. 334.

15. Reference 7, p. 244.

16. C. Babbage, *Reflections on the Decline of Science in Eng-*

land, Printed for the Author, London, 1830, p. 51.

17. The 29 signatures on the certificate were as follows: William Hyde Wollaston, John George Children, William Babington, John Frederick William Herschel, John Lewis Guillemard, Richard Phillips, Charles Babbage, Grant David Yeats, Thomas Colby, William Prout, William Daniel Conybeare, Henry Thomas De La Beche, Daniel Moore, John Bostock, Peter Mark Roget, Augustus Bozzi Granville, Jonathan Frederick Pollock, Richard Horsman Solly, George Pearson, Henry Earle, Gilbert Blane, James South, Davies Gilbert, Robert Bingley, Alexander Crichton, John Ayrton Paris, John Frederick Daniell, Charles Hatchett, James Stodart. The identity of each proposer is given in the biographical register (or in note 1 accompanying letter 195) in F. A. J. L. James, ed., *The Correspondence of Michael Faraday*, Vol. 1, Institute of Eleetrical Engineers, 1991.

18. Phillips to Faraday, 3 May 1823, James, reference 17, letter 195, p. 315.

19. Faraday to Phillips, 5 May 1823, *ibid.*, letter 196, p. 316.

20. W. H. Wollaston, P. R. S., Copley Award Speech, 30 November 1820, *Royal Society Journal Book.*

21. Reference 2, Vol. 1, p. 310.

22. M. Faraday, "On Some New Electro-Magnetical Motions, and on the Theory of Magnetism", *Quart. J. Sci.*, **1822**, *12*, 416-421.

23. See L. Pearce Williams, *Michael Faraday*, Simon and Schuster, New York, 1971, pp. 137-190 for an extensive discussion of these events. Also useful for its liberal use of primary sources is reference 2, Vol. 1, p. 299-314.

24. W. H. Wollaston to M. Faraday, 1 November 1821. Quoted in reference 2, Vol. 1, p. 305. Williams (reference 23) saw this as a "chillingly cold" reply; more likely it was only representative of Wollaston's normally terse, often laconic, writing style.

25. H. Warburton to M. Faraday, 8 July 1823. *ibid.*, p. 311-312. Warburton referred to Faraday's paper "Historical Statement Respecting Electro-magnetic Rotation," published in July 1823 in the *Quarterly J. Sci.*, **1823**, *15*, 288-292. The manuscript of the paper in the Faraday Collection at the Royal Institution bears the following note in Faraday's hand: "this shown to Wollaston before being printed who made some pencil alterations and declared it to be 'perfectly satisfactory'". So corrected, the transcript was bound with Faraday's copy of his papers from *Phil. Trans.* Henry Warburton was a timber merchant, philosophical radical and Whig politician whose close friendship with Wollaston grew out of their mutual interest in geology. Warburton had been Wollaston's chosen biographer; his work never appeared.

26. M. Faraday to H. Warburton, 3 May 1823. *ibid.*, p. 308-309.

27. Reference 2, pp. 339-340.

28. *Ibid.*

29. *Ibid*, p. 311.

30. J. F. W. Herschel Ms. Quoted in reference 9, p. 259.

31. Davy to Faraday, 29 June 1823, 23 July 1823 and 28 July 1823, letters all in the collection of the Institution of Electrical Engineers, London.

32. J. A. Paris, *Life of Sir Humphry Davy*, 2 Vols, Colburn and Bentley, London, 1830. Paris embroidered and enlarged anecdotes in much of what he wrote; see J. Z. Fullmer, "Davy's Biographers: Notes on Scientific Biography," *Science*, **1966**, *155*, 285-291.

33. *Ibid.*, Vol. 2, p. 210.

34. Davy, reference 1, Vol. 2, p. 164. John Davy's grievance was indeed proper, but in directing it at Faraday he had missed his true target. It is also unlikely that the younger Davy was "jealous" of Faraday. The quoted passage sounded a note of wounded astonishment that Faraday had not felt impelled to defend Sir Humphry, as a dutiful younger "brother" might be expected to do. Dr. Davy and Faraday were bound by several ties: not only had they been roughly coeval at the Royal Institution, they appear to have been treated intellectually in about the same fashion by Sir Humphry.

35. Mrs. I. M. McCabe, Librarian of the Royal Institution, graciously provided the copy.

36. M. Faraday, "On the Condensation of Several Gases into Liquids", *Phil. Trans.*, **1823**, *113*, 189-198; H. Davy, "On the Application of Liquids Formed by the Condensation of Gases as Mechanical Agents", *Phil. Trans.*, **1823**, *113*, 199-203 and "Appendix to the Preceding Paper. On the Changes in Volume Producted in Gases in Diferent States of Density by Heat", *Ibid.*, 204-205.

37. M. Faraday, "On the History of the Condensation of the Gases, in Reply to Dr. Davy, Introduced by Some Remarks on that of Electro-magnetic Rotation," *Phil. Mag.*, **1836**, *8*, 521-529.

38. Reference 2, Vol. 1, p. 338.

39. H. Davy, "On the Application of Liquids Formed by the Condensation of Gases as Mechanical Agents," *Phil. Trans*, **1823**, *113*, 199-203, p. 199.

40. These remarks are not meant to imply that Faraday's "ideas" were meager. His introduction of the "bent" sealed tube for heating the chlorine hydrate simultaneously exhibited conceptual understanding coupled with beautiful operational simplicity. What was "meager" was Faraday's discussion of the phenomena he exploited and described.

41. M. Faraday, "Historical Statement Respecting the Liquefaction of Gases," *Quart. J. Sci.*, **1823**, *16*, 229-240; published January 1824.

42. T. Northmore, "Experiments on the Remarkable Effects Which Take Place in the Gases, by Change in their Habitudes, or Elective Attractions, When Mechanically Compressed," *Jour. Nat. Phil. Sci. Arts.*, **1805**, *12*, 369-373; "Experiments on Condensed Gases," *ibid.*, **1806**, *13*, 233-236.

43. See J. Sadler, *An Explanation of Terms Used in Chemistry*, London: Royal Institution (1804) 22 pp. Sadler was designated "Chemical Operator to the Royal Institution" on the title page; W. G. Farrant held the same job from 1804-1806. Since in 1805 Farrant could not have been referred to as "late," in all likelihood Northmore experimented and discussed the liquefaction problem with Sadler.

44. A brass condensing pump, 3.5 to 5 in^3 pear-shaped glass receivers a quarter inch thick, and a siphon gauge. Northmore also reported he wore wooden guards on his legs to prevent possible injury from shattered glass; his full report makes the reader rejoice that he was thus protected.

45. M. Faraday, "On the Liquefaction and Solidification of

Bodies Generally Existing as Gases," *Phil. Trans.*, **1845**, *135*, 155-177.

46. It is not possible to list all those who did attend the meeting and participated in the vote, as an attendance register was not kept. There is a record of Fellows who signed in guests, however, and from the appropriate entry in the *Journal Book of the Royal Society* for 1824, we know the following Fellows were in attendance: William Wilkins, James South (*), Alexander Crichton (*), John Knowles, Thomas Young, Thomas Tooke, Graves Haughton, Richard Phillips (*), Edward Troughton, Grant David Yeats (*), Edward Sabine, John Frederick Daniell (*), Temple Chevallier. There is also a record of those who were present for the meal of the Royal Society dining club that followed the meeting. It is likely (but not certain) that they also voted in Faraday's election: Dr. William G. Maton, Wilbraham, T. Murdock, J. F. W. Herschel (*), W. Wilkins (*), W. Marsden, A. Johnston, Lambert, Raper, Branene (probably Brande). The names marked with (*) had signed Faraday's certificate.

The authors thank Dr. Frank James of the Royal Institution for providing the lists of names.

47. Faraday to Warburton, 29 August 1823. Quoted in reference 2, Vol. 1, p. 313.

48. *Ibid.*, p. 340.

49. J. Z. Fullmer, "Technology, Chemistry and the Law in Early 19th Century England," *Technology and Culture*, **1980**, *21*, 1-29.

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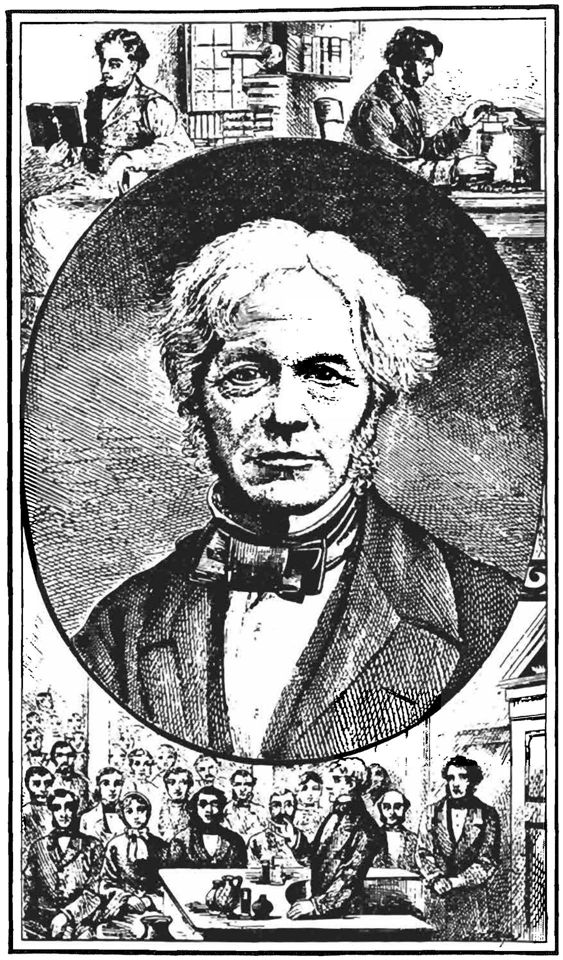

A late 19th-century woodcut of Faraday with scenes from his life in the margins. Note that the bottom scene shows him lecturing. No other scientist has so often been depicted giving popular lectures. In addition to the three illustrations in this article and the view on the front cover, at least two additional period woodcuts of Faraday lecturing are known to exist.

EDUCATING THE JUDGMENT: FARADAY AS A LECTURER

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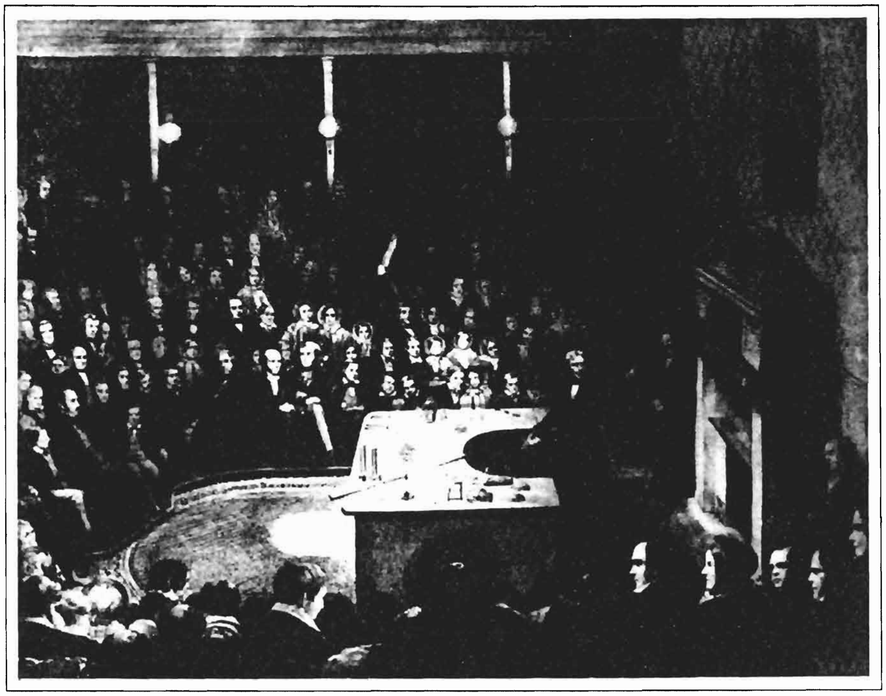
Those who heard Faraday lecture unanimously declared that he was a superb teacher. Moreover, they claimed that attendance at his lectures - whether a Friday Evening Discourse, a series on a specific topic, or a set of Juvenile Lectures - was a memorable experience. While there was consensus on these matters, his auditors differed in their reactions to Faraday and his style of lecturing. This diversity is worth exploring and in the ensuing discussion I shall divide assessments into three categories, starting with references to the specific skills he deployed in the lecture theatre. The second group of comments refer to the personal qualities he projected and particularly to his ability to relate to his audience. Thirdly, and most importantly for the purpose of this paper, will be his appeal to ideas and values that transcended the particular scientific topics he discussed.

Turning first to Faraday's lecturing skills we find that many of his auditors praised his eloquence and the clarity of his exposition. For example, one lay member of his audience noted that he was "Always clear in his statements and explanations" (1). Others, especially men of science, were particularly attracted to his judicious use of illustrative experiments. Thus the American electrician Joseph Henry was impressed by Faraday's "inimitable tact of experimenting" while William Crookes described Faraday's virtuosity as "a sparkling stream of eloquence and experimental illustration" (2). Likewise the Genevan scientist Auguste De la Rive commented on Faraday's ability to "combine animated and often eloquent language with a judgment and art in his experiments which added to the clearness and elegance of his exposition" (3).

However impressive the quality of Faraday's lectures it must be remembered that he had to acquire his lecturing skills through hard work and, moreover, his practiced verbal delivery and his "inimitable tact of experimenting" were developed over a long period of time. We can identify some of his steps in this direction. While still an assistant in the Royal Institution's laboratory in the 1810s he reported to his friend Benjamin Abbott on the strengths and weaknesses of the lecturers he heard and on the responses of their auditors. He noted the appropriate shape and illumination of the lecture theatre, the best method of delivery of a lecture, its speed and duration (4). He later trained himself by taking elocution lessons and by asking his friend Edward Magrath to audit his lectures and note any faults in his delivery (5). Further evidence of his attempts to improve his style are the many notes in his own hand that contain rules on how to deliver lectures - for example, he cautioned himself "Never to repeat a phrase" (6). He likewise spent much time preparing his experiments which were an integral part of his performance and he rehearsed them carefully beforehand. Faraday was a performer of consummate skill.



Faraday giving one of his Christmas lectures to an audience that includes Prince Albert and his two sons.

The second type of response to Faraday's lectures evoked his personal qualities. He paid great attention to his appearance and deportment, and his manners were correct and congenial. Thus Richard Owen's wife, who attended a number of his lectures, reported that Faraday was charming and humorous. She also commented on his tact when he rebuked some of the male members of the audience who had invaded the ladies' gallery (7). Another female auditor was impressed by "his great talent, great goodness, and the wonderful simplicity of his nature" (8). Despite (if not because of) his humble background and his membership of a Christian sect that set him apart from polite Victorian society, he appeared as a polished gentleman before his audience.

Several contemporaries also noted that Faraday created a bond with his listeners. For example, one woman auditor felt that "he was full of sympathy with his audience" and that his "lectures were '*mind addressing mind*'" (9). Despite his acknowledged expertise in science, he strove to set aside social differences and to appeal directly to the individual. We also see this emphasis on *ad hominem* communication in his juvenile lectures. At the commencement of one series he stated that "I will return to second childhood and become, as it were, young again amongst the young" (10). His series on the chemical history of a candle likewise opened with the assertion that he claimed "the privilege of speaking to juveniles as a juvenile myself" (11).

Faraday's public persona is a complex subject but suffice it to say that it was partially shaped by his religion which emphasized how a true Christian should deport him/herself in public. For example, the Sandemanians' concern with love and fellowship is a counterpart to Faraday's interpersonal skills which helped him relate directly to his audience.

The third reaction by his contemporaries drew attention to Faraday's evocation of feelings that transcended the strict subject matter of science. Thus one auditor reported that she found his lectures spiritually uplifting and noted that he managed to convey "the *deepest* sense of religion" (12). Cornelia Crosse, the wife of the electrician Andrew Crosse, likewise considered that "No attentive listener ever came away from one of Faraday's lectures without having the limits of his spiritual vision enlarged, or without feeling that his imagination had been stimulated to something beyond the mere exposition of physical facts" (13). Auguste De la Rive, who was less prone to hyperbole, also claimed that Faraday generally ended his lectures "by rising into regions far above matter, space, and time, [and] the emotion which he experienced did not fail to communicate itself to those who listened to him" (14).

From extant versions of Faraday's lectures it appears that De la Rive was correct in claiming that Faraday often ended his lectures on an hortatory note. For example, the six lectures on the chemical history of a candle concluded with an appeal to young people in the audience to "shine as lights to those about you" and to make "your deeds honourable and [to be] effectual in the discharge of your duty to your fellowmen" (15). In these finales he often ruminated on the nature of science and on its theological significance. Thus his eight-lecture series on physico-chemical philosophy, delivered in 1847, ended with a train of speculation about how all particles of matter work in harmony and for a purpose. These considerations, asserted Faraday (16):

> ... should lead us to think of Him who hath wrought them; for it is said by an authority far above even that which these works present, that "the invisible things of Him from the creation of the world are clearly seen, being understood by the things that are made, even His eternal power and Godhead" (Romans 1:20).

A cartoon from the 14 March 1857 issue of *Punch* showing Faraday charming an audience of young ladies with one of his popular lectures.

To understand Faraday the lecturer we will need to look beyond the comments of his contemporaries since they are limited to Faraday's performance and therefore do not adequately disclose what might be called his philosophy of education. However, we are able to pursue this topic since Faraday recorded his views on science education in several places. The foremost source is his famous "Observations on Mental Education" which he delivered at the Royal Institution on the afternoon of Saturday 6 May 1854 before Prince Albert and other dignitaries. This was the second of a series of seven lectures which Faraday helped to organise at the Royal Institution. The series seems to have been the brainchild of Henry Bence Jones, the Secretary of the Royal Institution and later Faraday's biographer, who was "full of a project for getting seven great guns to lecture on education" (17). Faraday did not intend contributing to the series but asked the polymathic William Whewell (Master of Trinity College, Cambridge) to deliver a general lecture "shewing the idea of education as needed for all classes of men & minds" (18). However, although he did not consider himself competent to lecture on education, he claimed that he overcame his reservations when the Managers pressed him to speak on the subject (19).

Six of the seven lectures in the series were concerned with the educational significance of specific subjects - the history of science, languages, chemistry, physics, physiology and economics. Faraday chose the most general subject and, according to E. Ray Lankester, who brought out an edition in 1917, Faraday's was the "most interesting and in many respects the most valuable" of the series (20). The impact of the lecture is difficult to gauge but seems to have been rather slight. While it has been printed on six occasions (1854, 1855, 1859, 1867, 1917 and 1991), it was not reported in the contemporary press. *The Times*, *The Athenaeum* and *The Illustrated London News* remained silent on the subject. Moreover, as I shall argue, it was generally ignored by Victorian educationalists.

The other main source for Faraday's educational views is the evidence he gave in 1862 to the Royal Commission on colleges and public schools chaired by the fourth Earl of Clarendon (21).

Faraday's "Observations on Mental Education" was a direct response to spiritualism, especially table-turning, which had been imported from America in the early 1850s and had rapidly become both popular and fashionable (22). As the spiritualist craze spread through all classes of society Faraday's views were frequently sought by an insatiable public. As a result of this clamour he conducted some simple experiments in the summer of 1853 and concluded that the table's movement was due to an involuntary muscular motion by the participants pressing down on the table. He publicised this conclusion in both *The Times* and *The Athenaeum* but failed both to arrest the craze and to prevent further solicitations from proponents of table-turning. In response to the continuing popularity of spiritualism, a recurrent theme in his "Mental Education" lecture of 1854 was the need for the public to become better educated in science since an adequate education would not leave the public susceptible to the influence of the table-turners. He considered that a properly trained mind would have no truck with table-turning and would readily be able to distinguish legitimate science from such imposters. However, since most people lacked an education in science they were easy prey to mesmerists, spiritualists and other charlatans. As he wrote to a scientific friend in uncharacteristically vituperative style (23):

> What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a ridiculous world ours is, as far as concerns the

mind of man. How full of inconsistencies, contradictions and absurdities it is. I declare that taking the average of many minds that have recently come before me ... I should far prefer the obedience, affections and instinct of a dog.

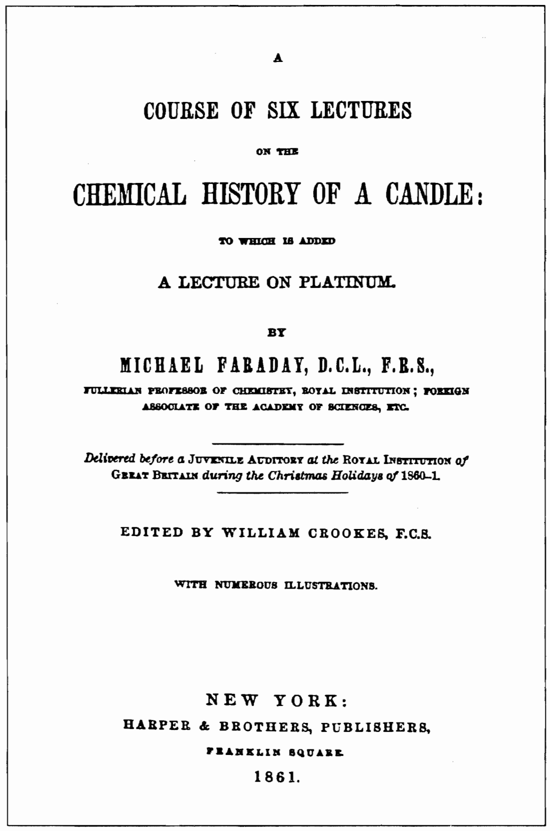
Although Faraday's objections to spiritualism were both religious and scientific, he limited his public opposition to the latter and remained silent on the former (24).

It would be incorrect to read Faraday's "Observations on Mental Education" solely as an attack on spiritualism since the lecture was of far broader significance. In addressing his audience, Faraday's primary concern was with the nature of mind, particularly the judgmental faculty, and how it should be educated. The centrality of this theme can be gauged from the frequent recurrence of the noun "judgment" and its associated verb which appeared 59 times in the lecture - an average of more than twice a page in the printed version. Moreover, an edition published at about the time of Faraday's death bore the title "Observations on the Education of the Judgement", although it is not known whether Faraday approved this change (25).

Faraday understood the judgment to be that faculty which enables a person to discriminate between truth and error, right from wrong, good from evil and, of course, between the valid claims of science and the fantasies perpetrated by table-turners. As he emphatically stated near the opening of his lecture, the major defect in the human mind can be expressed in just "three words ... deficiency of judgment" (26). This declaration established the theme for the remainder of the lecture.

Before proceeding it will be necessary to comment briefly on the word "judgment" and how it was used by earlier and contemporary writers. A classic discussion of the judgment occurred in John Locke's *An Essay Concerning Human Understanding* (1690). For Locke the judgment operates when we lack certain knowledge but need to make a decision based on an inductive inference from the available evidence. In making such a judgment, Locke considered that the mind forges links of agreement or disagreement between the idea under consideration and ideas already existing in the mind. Such comparisons are based on our experience of previously observed conjunctions (27). According to this theory the judgment operates by inductive reasoning and many commentators have classified the judgment as one of the mind's *reasoning* faculties. In an early essay dating from 1818, Faraday adopted a Lockean view by aligning the judgment with rational thought (28). However, it is important to notice that he implicitly rejected Locke's theory in 1854 since he did not conceive judgment as a rational act. Moreover, the comparison of ideas, which was central to Locke's account, found no place in Faraday's discussion.

By contrast, Faraday's analysis bears a closer resemblance


A

COURSE OF SIX LECTURES

ON THE

CHEMICAL HISTORY OF A CANDLE:

TO WHICH IS ADDED

A LECTURE ON PLATINUM.

BY

MICHAEL FARADAY, D.C.L., F.R.S.,

FULLERIAN PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION; FOREIGN ASSOCIATE OF THE ACADEMY OF SCIENCES, ETC.

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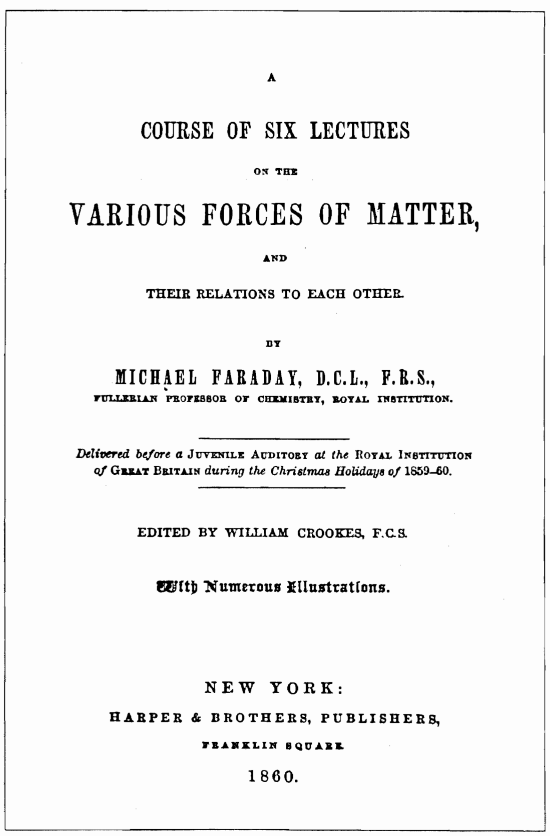
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to the moral sense theories propounded in the early 18th century by Francis Hutcheson and subsequently extended by such writers as David Hume, Samuel Taylor Coleridge and William Whewell. For these authors the moral sense is an internal sense which judges between right and wrong. Indeed, it has sometimes been related to the conscience which acts as a touchstone when we are faced with making moral decisions. Moreover, for many of the philosophers who supported this theory, the judgment was not a rational faculty but operated intuitively although its ability can be refined by our experience (29). Faraday was closer to these authors who considered the judgment to be an internal moral sense than to Locke and his followers who propounded more rationalist theories.

Although Faraday may have been familiar with some of the authors in this moral sense tradition, there is another and more plausible source for Faraday's account of the judgment. The role of judgment figures prominently in several biblical passages that were familiar to Faraday the Sandemanian. For example, the Psalmist speaks of the king judging "thy people with righteousness, and thy poor with judgment" (Psalm 72:2). Again, on gaining the throne, Solomon did not ask for riches or honour but for "an understanding heart to judge thy people, that I may discern between good and bad". God then expressed His pleasure with Solomon for wisely requesting an "understanding to discern judgment" (1 Kings 3:9-11). Solomon appears to have been Faraday's exemplar as he urged his audience to become wise through the exercise of their judgment. Indeed, Faraday's lecture on "Mental Education" possesses an exhortatory quality and reads like a sermon. Its style and Faraday's rather idiosyncratic use of the term judgment indicates that he was drawing on the Bible at least as much as, if not more than, on contemporary moral theories.

We shall now examine his theory of judgment. One of his basic premises was that we all possess the judgmental faculty. However, in most people it is a crude and unrefined instrument and therefore many of the judgments we make are incorrect ones. An untrained judgment would not readily be able to distinguish truth from error, or a piece of legitimate science from a manifestly false claim about spirits moving tables. However, just as we can train our voices by frequent practice or learn to discriminate between different types of wine, so the judgment can be trained. Educating the judgment is not rapidly achieved but "will require *patience* and *labour of thought*" (30). Moreover, as part of this training we must frequently and consciously reflect on the workings of our own judgment.

In his "Mental Education" lecture Faraday offered many general hints on how to educate the judgment. For example, he suggested that we should take full cognizance of the information supplied by our senses but treat this data with caution since the senses can deceive. Likewise, we should not make judgments too hastily. Instead, we should frame our ideas with precision and clarity. Moreover, we must learn from our errors. The judgment thus emerges as a ringmaster trying to

A cartoon entitled "Faraday giving his card to Father Thames" which appeared in the 21 July issue of *Punch*. As with his criticism of spiritualism, this was the result of a letter written by Faraday to the editor of the *Times* deploring the extent of the river's pollution.

keep in check the senses, the intellect, the imagination and language. Each has its rightful place in the ring but any one of them is likely to press forward, gain control and consume the others, including the ringmaster. The judgment therefore requires proper education in order to perform its task effectively.

In training the judgment, an education in science is particularly useful. "I am persuaded", wrote Faraday, "that all persons may find in natural things an admirable school for self-instruction and a field for the necessary mental exercises" (31). While the judgment was to be used in all other fields (except possibly religion), the sciences offered the best ground for training the mind and increasing our self-awareness (32). The several examples offered throughout Faraday's lecture were to confirm this point. Thus a scientific training provides the mental discipline to weigh evidence with care - this exercises the discriminatory power of the judgment. Through the practice of science we also become aware of our own ignorance and the deficiency of our judgmental power. Science teaches us not to be seduced by our pet hypotheses or by our imagination but to subject these honestly and critically to the outcome of experiments. We must also pay due attention to the laws of nature which cannot be suspended at our whim but provide touchstones against which to judge facts. Furthermore, science trains us to withhold our judgment unless the evidence is compelling. It also teaches us to frame our ideas with precision and to use language with clarity. Most importantly "This education has for its first and last step *humility*" - a term which was often applied to Faraday and possesses strong religious connotations (33).

Before the Public Schools' Commission in 1862, Faraday again stressed the importance of science education in training the mind. He claimed that there cannot be a better school for educating the mind than the study of natural science which encompasses "the laws impressed on all created things by the Creator and the wonderful unity and stability of matter and the forces of matter" (34).

These claims about the value of a scientific education were illustrated by several examples taken from the experience of Faraday and his contemporaries. Thus he claimed that some of his early hypotheses were proved wrong and had to be abandoned. In other cases, such as his theory of electrolytic conduction, he accepted the criticisms of his fellow scientists but, while holding his hypothesis in abeyance, became increasingly convinced of its validity (35). He also cited the example of D. F. J. Arago who, while describing the phenomenon which has come to be known as Arago's disc, judiciously avoided attributing a physical cause to the disc's rotation (36).

The correct exercise of the judgment was very important to Faraday not only in scientific matters but in all other aspects of life (again with the possible exception of religion). I have argued elsewhere that in many different areas of his life Faraday created strong demarcations between opposing concepts. Thus he sharply distinguished order from confusion, safety from danger and good from evil. His emphasis on the operation of the role of the judgment takes on broader significance in this psychological context. It is clear that he possessed a powerful drive to discriminate between right and wrong, good and evil. For example, as a Sandemanian he was committed to live strictly according to the demanding moral code laid down in the Bible and therefore had to decide the correct action in any circumstance. Imbued with the sect's stern religious values, he was conscience stricken when he thought he had adopted the wrong course of action through the inadequate exercise of his judgment (37).

The notion of judgment is itself one of a pair of opposites, its contrary being prejudice which is the failure to make a balanced judgment, owing to some prior conviction. Throughout his writings Faraday launched attacks on the various forms of prejudice. For example, a scientist who became too attached to an hypothesis would not be able to perceive the facts clearly or be able to appreciate alternative hypotheses. Thus in his 1844 attack on atomism Faraday urged scientists to distinguish fact from theory and he stressed that theories are only assumptions and should be treated as such. However, if scientists "forget that it is an assumption" then the theory "becomes a prejudice, and inevitably interferes, more or less, with a clear-sighted judgment" (38). Likewise in his lecture on "Mental Education" Faraday noted our tendency to deceive ourselves but he then argued that if we are aware of our prejudices we should strive to eliminate them by the proper exercise of the judgment (39). Prejudices also pervaded society. Thus he identified pervasive prejudices propagated by the British school system, and on being asked by the Commissioners why science was so neglected in schools, he answered that "it is only a matter of habit and of prejudice, derived from pre-existing conditions" (40).

Faraday's discussion of the judgment was highly reflexive since in his "Observations on Mental Education" he offered a very personal view based on his own mental development. As he stated in a prefatory note to the 1859 edition, his observations were "immediately connected in their nature and origin with my own experimental life" (41). Moreover, at the end of the lecture Faraday admitted that he had delivered "an open declaration, almost a [personal] confession" based on his own experience (42). What is most striking about these reflections is that Faraday nowhere discussed the role of educational institutions such as schools and universities - even the Royal Institution was not mentioned. Instead he emphasized *self-education*. He was an autodidact and he referred all educational questions to the development of mind and not to institutions. He even annoyed the Royal Commissioners by failing to respond to their questions about public schools (of which he had no experience) and instead insisting on talking generally about educating the mind (43). His comments about teachers were equally robust and individualistic. When the Commissioners pressed Faraday on the question whether boys should receive instruction in science prior to the age of 12 or 13, they received little assistance in being told that schools should not employ a man "who is a pedant in his science, and delights in abstract terms ... You want men who can teach". Moreover, he asserted that lectures "depend entirely for their value upon the manner in which they are given. It is not the matter, it is not the subject, so much as the man" (44). Such advice was of little use to Her Majesty's Commissioners in formulating educational policy on the amount of science to be taught and at what ages.

While Faraday acknowledged that "any useful education must be of the *self*", he considered that "society, as a body, must act powerfully in its cause" (45). Moreover, he informed the Commissioners that the "first thing to do is to give scientific teaching an assured and honoured place in education" (46). There was, he asserted, plenty of scope for Britain to encourage scientific education, which had been sorely undervalued. One telling comparison was with France where science was better appreciated and understood by all ranks in society.

Since education was of the self, ignorance and lack of judgment were manifest in all classes. Faraday found not only British workmen deficient in science but also the army officers he taught at Woolwich and his auditors at the Royal Institution, who were drawn principally from the higher ranks of society (47). He was particularly critical of the prevailing emphasis on teaching Latin and Greek to the upper classes who were manifestly ignorant of science. Indeed, men and women highly educated in the classics were, he claimed, the most ignorant in natural knowledge. They pestered him about mesmerism and table-turning and were so convinced of the truth of these soi-disant sciences that they could not be dissuaded by informed argument. "They are ignorant of their ignorance at the end of all that education", Faraday noted sadly (48).

In his scientific research Faraday employed no mathematics beyond simple ratios and was on several occasions hostile to the increasing deployment of mathematics, especially algebra, in the inductive sciences. Not surprisingly, this opposition to mathematics is also found in his educational views. Although he recognised that mathematics was the only branch of science generally included in the public school curriculum, he told the Public Schools' Commissioners that mathematics offered only a very limited training for the mind since it dealt with logical relationships and not with the behaviour of physical objects in the world. Hence those who were trained in mathematics could often "make no useful judgment at the sight of a machine". Moreover, perhaps with Augustus de Morgan in mind, he chastised those "excellent mathematicians" who were prejudiced in favour of table-turning and mesmerism (49). In his opinion the study of mathematics did not significantly improve the faculty of judgment (50).

Mathematics was one of the two subjects well represented in the curriculum of public schools. The other was classics and the Commissioners were particularly interested whether the scientists called to give evidence considered that science should be taught at the expense of classics. Faraday's comments were rather equivocal since his questioners pressed him on the educational value of classical learning - a subject outside his experience. Yet he was clearly dubious about the role claimed for classics in educating the mind and instead argued for the importance of the physical sciences.

The question whether science should be taught in schools was one of several educational issues hotly debated at the mid-century. At that time a number of science-related innovations were implemented, such as the Cambridge Natural Sciences Tripos in 1848 and the School of Natural Science at Oxford two years later. Much controversy centered on the ancient universities and both were subjected to examination by Royal Commission. Other major foci for science education were the Great Exhibition and the foundation of the Government School of Mines (1851), the Royal College of Chemistry (1853) and the Department of Science and Art (1853). Science teaching in schools was a politically fraught issue with arguments raging over whether, and to what extent, it should replace classics in the public schools, how it should be examined and whether it should be introduced to the lower classes. Moreover there was much debate over whether science should be taught as a pure, morally-elevating form of knowledge or whether its utilitarian value should be emphasized (51).

Some of these issues were aired in the 1854 series at the Royal Institution but were more central to the centenary celebrations organized in the summer of the same year by the Society of Arts. These celebrations included both a large educational exhibition containing displays of school books and apparatus, pupils' work, maps, scientific apparatus, etc., and a series of lectures which opened with William Whewell speaking "On the Material Helps of Education" (52).

Faraday's intervention on the topic of science education was thus part of a much broader educational debate and many of his general comments on the importance of science should have been welcomed by a wide range of educational reformers including radicals and utilitarians. However, I want to end this paper by drawing attention to two ironies implied by Faraday's lecture on "Mental Education".

First, as I have shown, his lecture was fundamentally concerned with moral values and with the role of the judgment; as such, it was principally an exercise in moral philosophy. As far as it engaged questions of education, it was about self-education. These subjects existed outside the main arena of educational debate in the 1850s and 1860s. Indeed, no other commentator engaged questions about the judgment and the issue of self-education was very low on the educational agenda.

Similarly, Faraday paid no attention to the leading issues of the day. For example, while he ignored the issue of social class, the British educational debate was fundamentally concerned with the question of determining which aspects of science should be taught to each class. Thus all the other six lectures delivered in the same series at the Royal Institution were addressed specifically to the upper classes, while the series organized by the Society of Arts was concerned with science for lower echelons of society. Furthermore, as I have already noted, Faraday's evidence to the Royal Commissioners did not assist them in framing a science policy for public schools - how much should be taught, to which age-groups, and how it should be examined.

Although Faraday's "Mental Education" lecture was an impressive tour de force, it was an idiosyncratic performance and it proved largely irrelevant to the main educational concerns of the 1850s and 1860s.

The second irony connects the above with my opening comments. Although Faraday's views on education were out of key with those of his contemporaries, he was nevertheless the foremost science lecturer of the day. He could excite his audience and convey science so eloquently, yet his views on education were idiosyncratic and found few resonances among contemporaries. This second irony underscores Faraday's paradoxical position in Victorian science and emphasizes the contrast between the public Faraday and the private Faraday. Against our image of the successful researcher and the renowned lecturer must be set the very private world of Faraday the Sandemanian (53).

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THE MILITARY CONTEXT OF CHEMISTRY: THE CASE OF MICHAEL FARADAY

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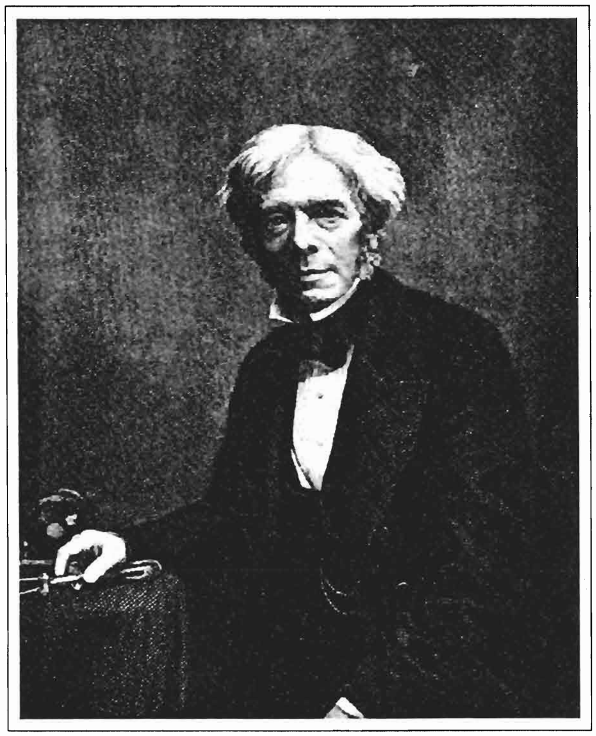
There are many essential requirements for a person to become a successful scientist. One of them is the availability of sufficient time to perform research. Michael Faraday (1791-1867) was perfectly well aware of this and frequently commented that, lacking property, time was his "only estate" (1). However, as I shall show, for various institutional and personal reasons time for research was in short supply during the latter part of the 1820s.

Faraday's opportunity to do original research, while he was still Laboratory Assistant in the Royal Institution, occurred following the discovery in 1820 of electromagnetism by the Danish natural philosopher Hans Christian Oersted (2). Men of science all over Europe conducted many further experiments in the subsequent months and advanced theories to understand this phenomenon. In the summer of 1821 Richard Phillips (3), a close friend of Faraday's, asked him to survey this activity for the *Annals of Philosophy* which Phillips edited. This Faraday did, writing up his conclusions in his only anonymous paper, "Historical Sketch of Electro-magnetism" (4). During this process he discovered electro-magnetic rotation - the principle behind the electric motor (5). He quickly published this discovery and promptly got into a priority dispute involving William Hyde Wollaston (6), the interregnum President of the Royal Society for a few months in 1820 between the death of Joseph Banks (7) and the election of Humphry Davy (8), Faraday's patron at the Royal Institution. It was claimed that Wollaston had predicted the existence of such a phenomenon, that Faraday had known this, but had not acknowledged it. However, Wollaston did not press the claim and the dispute was short lived, not at that time reaching the press (9).



Faraday in his later years.

However, it resurfaced over a more serious priority dispute in 1823 after Faraday had liquefied chlorine. He had been conducting an experiment suggested by Davy, the unexpected result of which led to the liquefaction of chlorine under pressure (10). When Davy demanded a share of the credit, Faraday demurred. A published report claimed that Davy, speaking from the Presidential Chair of the Royal Society, had stated that Faraday had been following Wollaston's suggestion when he discovered electro-magnetic rotation (11). Although Davy quickly said he had been misreported (12), the damage was done and Faraday was forced to declare his authorship of the "Historical Sketch" so as to defend his priority in public (13).

Worse was to follow. Faraday was nominated, without Davy's prior knowledge, to be a Fellow of the Royal Society (14). Davy opposed Faraday's election, since otherwise, because of their close association, it might be assumed, by members of various factions within the Royal Society, that he had prompted it. He did not want to be seen as continuing the Banksian tradition of supporting his friends and opposing his enemies irrespective of their scientific merit (15). The reason why Davy wanted to distance himself from the Banksian tradition was his hope that a firmer relationship would develop between the Society and Government, particularly the Admiralty. He wanted to encourage the state to ask for scientific advice from the Society and also to provide support for science. Davy was firmly committed to this policy and thus it was essential that it was seen that the misuse of patronage had ceased to flourish in the Society. Davy, now past his prime as a researcher, also seems to have been unable to accept the success of his protégé. As Faraday commented in 1835: "I was by no means in the same relation as to scientific communication with Sir Humphry Davy after I became a Fellow of the Royal Society [in January 1824] as before that period" (16). This did not mean that their relationship had completely ended. It is noticeable that Davy was now prepared to use Faraday's undoubted abilities for his own purposes without worrying about the effect these demands might have on Faraday's career.

An example of this occurred when very shortly after Faraday's election to the Royal Society, Davy secured his services as unpaid secretary to help form a new club which Davy and John Wilson Croker had decided to found (17). This club, which shortly became the Athenaeum, involved Faraday in a large amount of correspondence and administration between March and June 1824 to the almost complete neglect of his research (18). When the club was able to offer a salary to its secretary, Faraday passed the position on to his friend Edward Magrath (19).

By the mid-1820s he was responsible for more Royal Institution activities, particularly after he was appointed Director of the Laboratory in February 1825 (on Davy's recommendation) (20). For example he initiated the Friday Evening Discourses, the Christmas Lectures for young people, and generally strove to help the Royal Institution out of the difficult financial position it then found itself in. Nevertheless, his duties at the Royal Institution should in theory have allowed him sufficient time to do research.

That there was not time for research in the latter 1820s was almost entirely due to the time-consuming project to improve optical glass. This began in April 1824 while Davy was enjoying considerable success with the Admiralty after apparently solving the problem of preventing the corrosion of the copper sheeting of ships' bottoms by in effect inventing what we now call sacrificial cathodic protection (21). At the meeting of the Board of Longitude on 1 April 1824 it was proposed, at Davy's suggestion, that a Joint Committee of the Royal Society and Board of Longitude be established to try to improve optical glass (22). It was argued that this would be valuable for improving the accuracy of navigation. Although this was the explicit rationale, the foundation of this committee should be understood as a defensive move to preserve the very existence of the Board of Longitude. The Board had been founded in 1714 with the aim of improving methods of finding longitude at sea. This problem had been largely solved by the 1770s by the use of Tobias Mayer's method of lunar distances (23). By the 1820s the Board, which drew its membership from the scientific community, Parliament and the Admiralty, was increasingly coming under threat during the government's retrenchment program. Its major task, in the early 1820s, that of preparing the *Nautical Almanac*, could be quite easily transferred elsewhere (24). As one of the few established institutions to receive government funding for science through the Admiralty, it would be embarrassing for its abolition to occur during the term of a President committed to increase state support of science.

Members of The Joint Committee included Davy, Wollaston, the optician George Dollond (25), Davies Gilbert (26) (one of Davy's early patrons) and later John Herschel (27) (son of the discoverer of Uranus and a distinguished man of science in his own right). The Joint Committee first met on 20 May 1824 (28). They appointed the glass-making firm of Pellatt and Green to build a furnace for the project and asked Faraday to analyze chemically the glass produced - the kind of work that Faraday would normally do in the course of his Royal Institution chemical consultancy work.

At its fourth meeting on 5 May 1825 the Joint Committee appointed Faraday as a member and also appointed an experimental sub-committee comprising Herschel, Dollond and Faraday (29). Faraday was to supervise making the glass at Pellatts, Dollond was to grind it and Herschel was to determine its optical properties. Faraday's activities on this sub-committee entailed far more than his normal Royal Institution consultancy. Faraday's task was to prove difficult since Pellatts was some three miles distant from the Royal Institution. Thus there was a lack of proper supervision and the results were disappointing during the ensuing year (30).

Davy's health began to give way during this period; the last time he chaired the Joint Committee was on 25 May 1826 (31). The next two meetings were chaired by Wollaston, before Gilbert took over (32). On 8 May 1827 the Joint Committee met to discuss the continuingly disappointing results (33). Because of the financial difficulties of the Royal Institution, on which he was economically completely dependent, Faraday was not in a position at that meeting of the Joint Committee to refuse to take part in extending the project if it entailed support for the Royal Institution (34). Thus he actively supported the decision made at that meeting to approach the Royal Institution for permission to build a glass furnace there and for Faraday to take over personally the making of the glass. The negotiations were duly completed by the end of May. When the furnace was installed, in the back yard of the Royal Institution, Faraday began what turned out to be two years of arduous work.

The story is told through the highly detailed notebooks that Faraday kept of the project and which are now in the archives of the Royal Society (35). Of the 731 days between 3 December 1827, when the work began in earnest, and 2 December 1829, by which time it had effectively ceased, Faraday worked on glass on 337 days (46.1%). If one excludes the 104 Sundays (for Faraday was a deeply religious man (36)) and at least 104 days spent outside London (for he suffered badly from headaches very possibly brought about by close work with the furnace), then the number of available working days was 507. In other words, on 66.4% of available working days Faraday spent some time working on glass. Of course he did not spend every minute of these days working on glass, but what time he did spend was taken from time he could have devoted to research. Nor did he work unaided. He had the help of Charles Anderson (37) (formerly a Sergeant in the Royal Artillery), but he was little more than a pair of hands. It was Faraday who decided where the crucibles should be placed in the furnace, what temperature the furnace should be heated to, for how long, what the chemical composition of the glass should be and so on (38).

The institutional and personal contexts which had brought this about were beginning to dissolve but had been replaced by others. In 1827 Davy's health and thus position had been further weakened by a stroke. He went abroad after resigning the Presidency of the Royal Society on 6 November 1827 (to be succeeded by Gilbert). In July 1828 the Board of Longitude was finally abolished (39), the *Nautical Almanac* ultimately being transferred to the Royal Astronomical Society in the early 1830s (40). Instead the Admiralty appointed a resident committee of three scientists at a salary of £100 a year each (41). The first committee comprised Faraday, Thomas Young (42) (former Secretary of the Board of Longitude) and Edward Sabine (43) (a Royal Artillery Captain and a Secretary of the Royal Society). The committee took over the supervision of the glass project with funding directly from the Admiralty. Evidently they still believed that improved navigational instruments would emerge from the project.

On 29 May 1829 Davy died in Geneva but his death was not reported in the *Times* until 9 June (44). By the end of 1829 Faraday had effectively stopped doing any glass work. He made his views of the project plain in a letter to Gilbert written in May 1830 (45):

> I further wish you most distinctly to understand that I regret I ever allowed myself to be named as one of the committee. I have had in consequence several years of hard work; all the time that I could spare from necessary duties (and which I wished to devote to original research) [has] been consumed in the experiments.

Since by this time the finances of the Royal Institution were on a much better footing than in the middle of the 1820s (mainly due to the success of the Friday Evening Discourses initiated by Faraday), there were no financial worries for the Royal Institution occasioned by Faraday's withdrawal from the glass work (46).

This did not mean that Faraday refused to continue providing advice to the Admiralty. In his capacity as resident scientific adviser, Faraday helped the Admiralty with many analytical chemical problems. For example, following the failure of Davy's method of copper protection to be uniformly applicable, Faraday analysed copper sheets for ships' bottoms. In 1830 nine companies sent samples of copper sheets to the Navy to be analysed - the company that produced the best sheet would be awarded a large contract to supply 45 tons of copper sheeting. Faraday's report has not survived, but from correspondence it is clear that he did not believe that analyzing the small impurities contained in the copper sheets was sufficient to determine whether sacrificial cathodes would protect them. In the end the order for the copper was divided equally between the nine companies. Faraday, unlike Davy, had a good grasp of the limitations of science (47).

Faraday was thus happy to work for the Admiralty provided it was on his own terms and did not take up much of the time he could otherwise devote to research. What he wanted was to ensure that in future he would be able to avoid burdensome tasks such as the glass work. Before he knew of Davy's death, Faraday was contemplating leaving the Royal Institution and thus the glass work, while continuing to give lectures there (48). Now that Davy was no longer alive, Faraday decided to remain at the Royal Institution. The only way he could be sure of having the necessary time for research in the future was by obtaining some economic freedom from the Royal Institution in case it again fell on hard financial times. Within a month of Davy's death, Faraday was actively negotiating with the Royal Military Academy in Woolwich for a position there (49). The Academy had been founded in 1741 to train cadets for the Army, particularly the Royal Artillery and Royal Engineers. Its courses had a strong scientific and technical component to allow cadets to learn how to take advantage of the new industrial processes for warfare. Faraday negotiated the professorship of chemistry there whilst retaining his position at the Royal Institution.

To secure sufficient economic freedom, Faraday drove a hard bargain with the Academy. His work for the sub-committee had been done gratis and he seems never to have drawn his salary from the Admiralty (no doubt to avoid being under an obligation to undertake all their requests). At the end of June 1829 the Commandant of the Academy, Colonel Percy Drummond, visited Faraday by prior appointment, following which Faraday wrote to him giving his terms (50). He said that he received the equivalent of £8-15s per lecture at the Royal Institution and for 20 lectures - the minimum he believed necessary for a course of chemistry - that came to £175, but as he would be willing to give a lecture or two extra he thought a fee of £200 a year would be sufficient (51). It seems that it was taken for granted, or else the documentation has not survived, that he would have an assistant, James Marsh (52), and expenses for chemicals and apparatus. On the slightly modified terms of giving 25 lectures a year, Faraday was appointed Professor of Chemistry at the Royal Military Academy in mid-December 1829 (53).

From the point of view of the Academy, what is particularly interesting is that they accepted Faraday with very little alteration to his terms. That these were very favourable can be seen when compared with the appointment of the Professor of

French at exactly the same time. Mr. Tasche was appointed in September at a salary of £150 per year and with the requirement that he reside in Woolwich (54). This reflects the belief by the military establishment (both in the army and the navy) that science had a vital role to play in the future of the armed services and that it was pointless to employ, or continue to employ, second-rate men. As a contemporary commented in a letter to Drummond, "Faraday ... is not only one of the best chemists of the day, but certainly the best lecturer, qualities not always combined" (55). One has to pay for the best and Tasche was not noted for anything distinguished.

In practice what happened was that from 1830 until 1851, when he retired, Faraday spent two days a week at Woolwich during their terms. To many creative scientists this might have been an onerous burden. But for Faraday, who had suffered for two years doing glass work two out of every three days, it must have seemed a happy release; his time devoted to utilitarian ends had been drastically reduced, potentially to one day in six.

Faraday had now achieved the economic security and the time, albeit still under several constraints, to pursue his own researches. As it turned out, the Royal Institution remained in a reasonable financial position for the remainder of his career and no project, like the glass making, ever got beyond the proposal stage. He never seems to have contemplated leaving the Royal Institution again.

To conclude, it is not a coincidence that Faraday made his discovery of electromagnetic induction shortly after he dropped the glass work. As David Gooding and his students have shown, it took considerable time to build the induction ring (56). Such investment of time was impossible for Faraday while he was working on glass. This is a social contingent argument. We cannot explain from this analysis what prompted Faraday to undertake this 1831 work. But it does tell us how Faraday negotiated the time to undertake this work.

References and Notes

Acknowledgements: I wish to thank the Royal Society (RS), the Royal Greenwich Observatory (RGO) and the General Register Office (GRO) for permission to work on Manuscripts in their possession. I would also like to thank Dr. Geoffrey Cantor for many useful comments on this paper. Additional abbreviations used in the notes include: DSB = C. Gillespie, ed., *Dictionary of Scientific Biography*, 14 Vols., Scribners, NY, 1970 - 1975 and DNB = *Dictionary of National Biography*, 63 Vols.

1. For example, see Faraday to Drummond, 29 June 1829, in Frank A. J. L. James, ed., *The Correspondence of Michael Faraday*, Vol. 1, Peregrinus, London, 1991, letter 404 and Faraday to Thompson, 3 July 1832, in L. P. Williams, R. Fitzgerald and O. Stallybrass, eds., *The Selected Correspondence of Michael Faraday*, 2 Vols., Cambridge, 1971, letter 142. It is surely not a coincidence that both of these were written in a military context.

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5. M. Faraday, "On Some New Electro-Magnetical Motions, and on the Theory of Magnetism", *Quart. J. Sci.*, **1821**, *12*, 74-96. For discussions of this see D. Gooding "'In Nature's School': Faraday as an Experimentalist", in D. Gooding and F. A. J. L. James, eds., *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday 1791-1867*, Macmillan, London, 1985, pp. 105-35, esp. 110-23; D. Gooding, "History in the Laboratory: Can We Tell What Really Went On?" in F. A. J. L. James, ed., *The Development of the Laboratory: Essays on the Place of Experiment in Industrial Civilization*, Macmillan, London, 1989, pp. 63-82, esp. 71-9; and D. Gooding, *Experiment and the Making of Meaning*, Kluwer, Dordrecht, 1990.

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22. RGO 14 / 8, f. 17-20.

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27. John Frederick William Herschel (1792-1871, DSB). Secretary of the Royal Society, 1824-7.

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43. Edward Sabine (1788-1883, DSB). Secretary of the Royal Society, 1827-1830.

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49. *Ibid.*, Faraday to Pasley, 26 June 1829, letter 402.

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MICHAEL FARADAY'S BIBLES AS MIRRORS OF HIS FAITH

Herbert T. Pratt, New Castle, Delaware

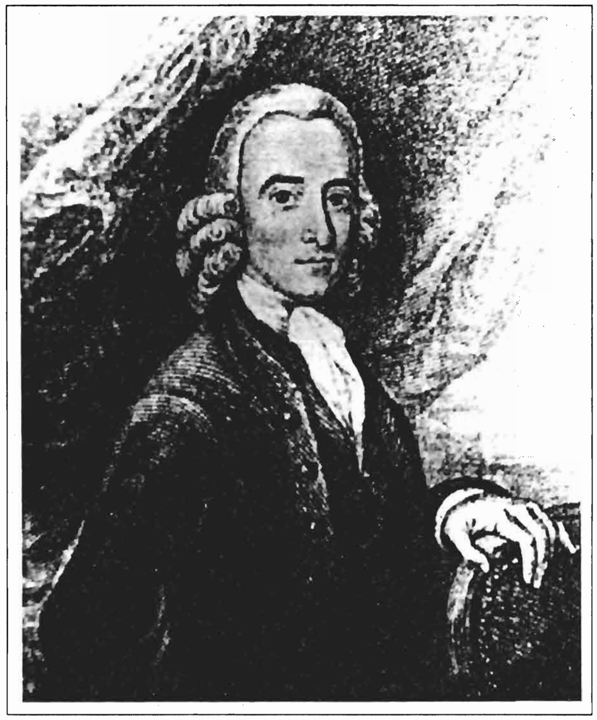
A devout Christian's Bible is a cherished and very personal possession. Although after long usage its spine hinges crack, its covers loosen and its pages become dog-eared, the owner will not lightly put it aside for a newer one because it has become a familiar old friend. Part of its attraction is likely to be the markings, underlinings, and notes that have accumulated around passages which strike a familiar chord, support a cherished belief, note a fact to be recalled, or that are simply inspirational. I believe that to a great extent such marginalia mirror what the Bible owner holds relevant to his faith.

Two well-worn Bibles that belonged to Michael Faraday (1791-1867) are now in the archives of the Royal Institution in London. Both are heavily marked in pencil. Both are the King James version of 1611. One was published in 1776 and the other in 1817, but there are no handwritten dates or other direct clues to indicate when the bibles were used, or whether they were used consecutively or simultaneously. Although they were subsequently presented by Mrs. Faraday to relatives, there is nothing to indicate they were ever used by anyone other than Faraday (1).

In July 1990, I copied all of the markings in these Bibles into two new Bibles so as to duplicate, as nearly as possible, every mark, word change, underscore, marginal note, etc., given in the originals. Study of these copies provided the foundation for this paper, the purpose of which is to determine if the markings shed any light on what religious beliefs Faraday held near and dear.

In July 1821, Faraday, at age 30, made a profession of faith in the Church of Christ, popularly known as the Sandemanians and fully committed his life to the cause of Jesus Christ, a commitment from which he never wavered (2). The Sandemanians were a small, virtually unheard of religious sect, having no more than a few hundred members in all of Great Britain. Faraday came by his choice naturally. His father James (1761-1810) had been a Sandemanian, as had his mother, an aunt and an uncle. Like himself, his wife, Sarah Barnard, whom he married on 12 June 1821, had been reared as a Sandemanian, and his father-in-law was an elder in the London congregation (3).

Sandemanianism, a name drawn from that of Robert Sandeman (1718-1771) (4), the sect's leading thinker, was an uncompromising, totally demanding, but unemotional religion (5). Of Sandemanian beliefs, the most important to this study are their beliefs about the Bible. They held the Bible to be the Word of God, turned to it for every item of faith and practice, and took what they found there at face value. Faith, to them, was simply an intellectual assent to the facts in the Bible. They needed no proof of the Bible's validity, never considered for a moment the possibility of human flaws in its translation and never desired to know how it compared with the oldest manuscripts. Fully believing the Bible to be its own best interpreter, Sandemanians supported the meaning of one scripture verse by citing another (6). As God's Word, the Bible was a living document through which God spoke directly to the hearts and minds of individual readers. Sermons, or exhortations as they were called, given by the elders as part of public worship, carried the full authority of God himself.



Robert Sandeman

Michael and Sarah (née Barnard) Faraday

From what is known about Faraday, it is reasonable to infer that he read these Bibles for knowledge, inspiration, and encouragement, in family devotions, in front of his congregation as an elder and as a participant in public worship, in preparing sermons delivered to his and other congregations, in following sermons and devotionals given by others, and in ministering to the needs of members of his congregation (7).

Faraday used at least 11 types of markings in his Bibles (Table 1), the most prevalent, by far, being one or more vertical lines beside a verse or group of verses. The basic assumption of this paper is that the greater the number of vertical lines, the greater the importance Faraday attached to the passage. To quantify his ideas of importance, the total number of verses marked by one or more vertical lines was tablulated for each Bible and the percentage of marked verses calculated for each chapter and book. Also, the number of verses marked with seven or more vertical lines was tabulated for each chapter and book. Commentary in this paper will be confined to the vertical lines, primarily those from the New Testament of the 1776 Bible. A detailed study and analysis of all markings is in preparation.

Of the 31,483 verses in the bible, Faraday placed vertical marks by more than 4300 in the 1776 printing, or about 14%

(Table 2). He marked 10% of the verses in the Old Testament and 25% in the New Testament. The percentages of verses marked in the various books varies from zero to 53%. Only about half as many verses are marked in the 1817 Bible, which suggests that either it was not used for as many years or was used only on special occasions. The character or nature of the verses marked is relatively consistent between the two Bibles.

Faraday lived by the precepts and examples of the New Testament. Therefore, it is here that one would expect to find the key to his beliefs. Markings in the various books (Table 3) show that he was most interested in the stories of Jesus' life and Paul's letters to churches and least interested in the apocalyptic book of Revelation. Subject matter of marked verses indicates that Faraday was more concerned with the practical aspects of religion than with the theoretical. For example, he marked two thirds of all the verses in Chapters 5, 6, and 7 of the Gospel of Matthew, the so-called Sermon on the Mount, which contain the very essence of Jesus' teachings.

Faraday's New Testament markings can be broadly classed as scriptures relating to God, scriptures relating to Jesus, and scriptures relating to the Christian life. Sandemanians did not believe in creeds, and Faraday would have been aghast at the idea of writing one. However, statements of his beliefs, when strung together from passages he marked in his Bibles, certainly have a creed-like quality. In the discussion that follows, subscript numbers following passages cited indicate the number of vertical marks.

God existed from the beginning (John $1{:}1_1$), is Spirit (John $4{:}24_1$), and only He can be called good (Mark $10{:}18_6$). With God nothing is impossible (Luke $1{:}37_4$). He sent Jesus as a Light into the world (John $1{:}5\text{-}7_1$; 2 Cor. $4{:}6_1$) to deliver man from the power of sin and darkness (Col. $1{:}13_7$). This is God's

Table 1. Types of markings and marginalia in Faraday's Bibles.

1. One or more straight vertical marks beside a verse or verses
2. Keyed vertical marks to identify passages of scripture used by others (1776 Bible only)
3. Short horizontal lines that set off a group of verses (1817 Bible only)
4. Wavy vertical lines beside a verse or verses
5. Notes, comments and word changes
6. Corrections of printer's errors in the text
7. Cross-references
8. Underscored words
9. Question marks, asterisks, and check marks by verses or words
10. Consecutive numbers denoting lists or series
11. Accent-like marks in the margin at the first and last lines of verses

Table 2. Bible verses having vertical marks.

Data	1776 Bible		1817 Bible	
	OT	*NT*	*OT*	*NT*
Total no. books	39	27	39	27
Total no. verses	23,561	7922	23,561	7922
Verses marked	2354	1978	1721	460
Verses marked, %	10	25	7	6
Range among books, %	0-23	0-53	0-19	1-23
Key verses *	39	49	6	2

* Defined as those have seven or more vertical marks

free gift to mankind (Eph. $2{:}8_4$). God's Word was also in the beginning (John $1{:}1_1$). It is quick and powerful, sharper than a two-edged sword, and is a discerner of the thoughts and intents of man's heart (Heb. $5{:}12_2$). The Word is the helmet of salvation and the sword of the Spirit (Eph. $6{:}17_1$).

God is to be worshipped in spirit and in truth (John $4{:}24_1$), is to be praised (Acts $2{:}47_6$), and is to be accorded blessing, glory, wisdom, honor, power, and might forever (Rev. $7{:}12_7$). By either command or example, the Bible tells exactly how God is to be worshipped. Man's ideas about worship are not to be followed, for the Lord had said, "But in vain they do worship me, teaching for doctrine the commandments of men" (Matt. $15{:}9_4$).

Jesus of Nazareth was Son of God (John $1{:}34_2$), was pleasing to God (Matt. $3{:}17_6$, Mark $14{:}61\text{-}62_7$), and came into the world to take away man's sin (Mark $10{:}45_2$, John $1{:}24_7$, John $3{:}16\text{-}17_2$, 1 Cor. $15{:}3_1$).

Although Jesus lived the perfect life, he was arrested by the Roman authorities, tried on trumped up charges (Mark $14{:}55_7$), and executed as a common criminal after being accused of being King of the Jews (Matt. $27{:}37_7$). Even though he was tortured and demeaned, Jesus did not lash out at his captors (Mark $14{:}61_8$).

God raised Jesus from the dead, a fact that became the cornerstone of the preaching in the early church (Acts $2{:}30\text{-}31_5$, $13{:}33_4$, $17{:}31\text{-}32_4$, Romans $11{:}9_9$, 1 Cor. $15{:}4_1$, Col. $2{:}12_4$). By his death, Jesus purged our sins (Heb. $1{:}3_4$) and was raised for our justification (Rom. $4{:}25_4$).

After his resurrection, Jesus returned to the Father (John $16{:}28_8$), where he sits at the right hand of God (Heb. $1{:}3_4$). Although he was dead, he is now living with God (John $16{:}28_8$) and holds the keys of hell and death (Rev. $1{:}18_5$). He will come to earth again (1 Thes. $4{:}13\text{-}18_1$), but no one knows when (Mark $13{:}26_1$). Meanwhile, his followers are to be prepared and to watch and wait (Mark $13{:}37_5$).]

The end result of sin is death, but man can escape both sin and death by becoming a follower of Jesus (Rom. $6{:}33_{10}$). To become a Christian, one must turn away from sin (Matt. 4:17$_3$), believe that Jesus is the Son of God through faith in the testimony of the word (2 John 5:5$_3$, Acts 16:30-31$_4$), believe that God raised Jesus from the dead, and say so in public (Rom. 10:9$_7$), and, finally, be baptized (Mark 16:16$_7$). Through faith one is buried with Jesus in baptism and raised with him as a new person, and thus saved (1 Peter 3:21$_4$), not because of anything he has done, but because God is merciful (Col. 2:12$_4$, Titus 3:5$_4$). Upon baptism, the Spirit of God comes into the believer's life and the Lord adds him to his church (Acts 2:47$_6$). Bodies once dead because of sin are now alive because of righteousness (Rom. 8:9-10$_4$). And those led by the Spirit of God are the sons of God (Romans 8:14$_4$).

The Christian is to live so that his life glorifies God (Matt. 5:16$_4$), and to find God's will for his life through study of the Word (2 Tim. 2:15$_7$). Christians are to be the best persons they can be (Matt. 5:48$_5$), realizing that God provides strength to do all things (Phil. 4:13$_5$).

Some attributes of the Christian are humility (Acts 20:19$_6$), love of right living, mercy, ability to withstand ridicule for one's beliefs (Matt. 5:3, 6, 8, 10, 11$_1$), steadfastness (Heb. 3:14$_7$), and never tiring of doing good (Gal. 6:9$_4$).

Christians are to forgive completely those who wrong them (Matt. 18:35$_7$), are not to take note of other people's faults, but to be introspective of their own (Matt. 7:1-5$_1$). They are to work for a living, not to be busy bodies or disorderly (1 Thess. 3:11-12$_7$), and avoid senseless arguments, particularly those over religion (1 Tim. 6:20$_7$).

Not only should they not kill people, which is forbidden by the law, but they should not even get angry or call people names. Rather, they should do their utmost to reconcile differences (Matt. 5:24$_1$).

Table 3. Verses marked in New Testament grouped according to subject matter of books.

Nature of Book	Percent Verses Marked		Key Verses*	
	1776	*1817*	*1776*	*1817*
Gospels	24	3	24	1
Historical	27	6	2	1
Letters of Paul:				
To churches	28	11	16	0
To persons	15	7	2	0
Letter to Hebrews	27	2	1	0
Catholic Letters	17	12	2	0
Apocalyptic	12	4	2	0
Total			49	2

* Defined as those have seven or more vertical marks. All counts are averages.

Christians should not be caught up in the race to acquire things, but should be content with what they have (Heb. 13:5$_4$). Living for wealth as an end unto itself is of no value in God's sight (Matt. 6:19-20$_1$). One is to ask God for daily needs (Matt. 6:25-34$_1$) fully trusting and believing that He will supply them (Matt. 7:7-8$_6$).

Others are to be served with humility (John 13:15$_8$) without affectation (Matt. 6:1$_2$). Likewise, prayers are to be simple and not showy (Matt. 6:5-8$_2$).

In no case is a woman to be regarded as only an object for sexual gratification (Matt. 5:28$_{10}$). In the family, husbands are to love their wives (Col. 3:19$_1$). Parents are to be held in honor (Eph. 6:2$_6$); children are to obey parents (Col. 3:20$_1$), and servants, their masters (Col. 3:22$_1$). Parents are not to provoke their children to anger (Col. 3:24$_1$). Christians are to comfort one another, edify one another (1 Thess. 5:11$_4$), be at peace with one another, and hold the elders of their congregation in high regard (1 Thess. 5:13$_6$). They are to beware of heretical teaching (2 John 2:7$_8$) and be wary lest they accept man-made philosophies as truth (Col. 2:8$_6$).

In summary, Christians are to focus their hearts and minds on those things that are true, honest, just, pure, lovely, virtuous, praiseworthy, and of good report (Phil. 4:8$_1$).

The essence of Faraday's deeply entrenched commitment to Christianity can be distilled into three words: faith, hope, and love. In faith, he believed that the Bible, as God's word, was true in all respects. Through its pages, he was convinced that his life would not end with the grave, but that God had promised something beyond. Because of that hope, he believed that he could best show his love to God by loving and honoring other people.

As already mentioned, Sandemanians held that faith was simply intellectual assent to the facts in the Bible about God and Christ. The just, namely, those who have been rightly treated by God, shall live by faith (Rom. 3:28$_5$, Gal. 3:11$_5$). Great works will not save a person, but only God's grace working through faith (Eph. 2:8$_4$):

> ... without faith it is impossible to please him; for he that cometh to God must believe that he is, and that he is the rewarder of them that diligently seek him (Heb. 11:6$_8$).

Faraday's treatment of passages relating to miracles performed by Jesus are indirectly a commentary on faith. Of the 33 miracles, Faraday marked one or more verses in 13 (39%) in the 1776 Bible and 8 (24%) in the 1817 Bible. Three miracles are marked in both Bibles: dumb son healed (Mathew 9:14-27$_1$), finding a school of fish (Luke 5:1-11$_{2\text{-}3}$), and healing of lepers (Luke 5:12-13$_1$).

Among the 15 miracles not having at least one verse marked in either Bible are some of the best known: Jesus' turning water into wine at the wedding feast (John 2:1-11), his walking on water (Matt. 14:25-26, Mark 6:48-51, John 6:19-21), healing of a demoniac (Mark 1:23-26, Luke 4:33-35), and feeding of 5000 people with five loaves and two fish (Matt. 14:19-20, Mark 6:35-44, Luke 9:12-17, John 6:5-13).

One common thread running through 14 of the 15 miracles that Faraday did not mark is that Jesus acted voluntarily on his own because he saw a need. By contrast, of all the miracles that Faraday marked, the recipient of Jesus' action, or someone acting for the recipient, first believed that Jesus could do something or intervene in the course of events. The one miracle that Faraday marked that did not require such personal belief was his withering of a fruitless fig tree by cursing it (Matt. 21:18-22). But belief was involved indirectly since he said he did it to teach his disciples that by simple trust in God's power, they could do as much or more, even move mountains into the sea.

In opening a lecture on education at the Royal Institution in May 1854, Faraday sharply distinguished between ordinary and religious beliefs, between scientific truth, which man can learn by applying one's mind, and truth about a future life which he said, "cannot be brought to his knowledge by any exertion of mental powers, ... [but] is received through simple belief in testimony given". The means of educating oneself about science, he said, were not applicable to educating oneself about "the hope set before us, as if men by reasoning could find out God". Then, quoting part of verse 20 from chapter one of the Apostle Paul's letter to the church in Rome, he continued, that in earthly matters he believed that "the invisible things of him from the creation of the world are clearly seen, being understood by the things that are made, even His eternal power and Godhead" [Romans 1:20] (8).

On the surface, the quotation suggests that Faraday drew spiritual strength from natural theology, namely, that God can be found through the study of nature; that an intricate design demands a designer (9). No doubt, his use of Romans 1:20 was more than an offhand comment, since he also had used it in the same way in a lecture in 1847 (10).

Faraday's usage of Romans 1:20 takes on new meaning when it is looked at in the context of other verses he marked in the first chapter. The content of the chapter runs like this - After a lengthy salutation (verses 1-14), the Apostle says (verses 10-15) that he hopes to find a way to visit Rome so that he can strengthen the church's faith. In verse 16 he testifies to the power of the gospel, and in verse 17, he argues that the righteousness of God is revealed in the Gospel and that the just shall live by faith. Then, starting at verse 18 through the end of the chapter, he berates unbelievers for their unbelief, concluding (verse 32) that the ungodly and sinners are worthy of death.

In his 1776 Bible, Faraday marked verse 16 on the power of God, apparently on two different occasions (figure 1), and verse 17 on faith, he emphasized with a seven marking. But verse 20, which implies that anyone could know that God

11. For I long to see you, that I may impart unto you some spiritual gift, to the end ye may be established;
12. That is, that I may be comforted together with you by the mutual faith both of you and me.
13. Now I would not have you ignorant, brethren, that oftentimes I purposed to come unto you, (but was let hither to,) that I might have some fruit among you also, even as among other Gentiles.
14. I am debtor both to the Greeks, and to the Barbarians; both to the wise, and to the unwise.
15. So, as much as in me is, I am ready to preach the gospel to you that are at Rome also.
16. For I am not ashamed of the gospel of Christ: for it is the power of God unto salvation to everyone that believeth; to the Jew first, and also to the Greek.
17. For therein is the righteousness of god revealed from faith to faith: as it is written, The just shall live by faith.
18. For the wrath of God is revealed fromheaven against all ungodliness and unrighteousness of men, who hold the truth in unrighteousness;
19. Because that which may be known of God is manifest in them; for God hath shewed it unto them.
20. For the invisible things of him from the creation of the world are clearly seen, being understood by the things that are made, even his eternal power and Godhead; so that they are without excuse:
21. Because that, when they knew God, they glorified him not as God, neither were thankful; but became vain in their imaginations, and their foolish heart was darkened.
22. Professing themselves to be wise, they became fools,
23. And changed the glory of the uncorruptible God into an image made like to corruptible man, and to birds, and four-footed beasts and creeping things.

Figure 1. The marking of Romans 1:11-23 in Faraday's Bibles (1776 Bible left, 1817 Bible right).

exists by looking at his creation, is not marked at all! Note that in quoting verse 20 in the lecture, Faraday omitted the last phrase, "so they [e.g. the ungodly and unrighteous from verse 18] are without excuse," thus removing the verse from its context. This pattern of marking in the 1776 Bible is essentially repeated in the 1817 Bible, except that verse 20 is marked (11).

From this evidence, it seems that Faraday's interest in Romans I was not so much in what it said about creation, but what it said about faith. His reference in the lecture to "simple belief in testimony given" is virtually the Sandemanian understanding of the meaning of the word faith, and, as he used the phrase, refers not to testimony about God as seen in the creation, but to that testimony about God found in the Bible. Does that mean that those who don't believe the Bible have an excuse for their ignorance of God? No, he says, because they could know about God simply by studying the order of the world around them. Thus, Faraday was using verse 20 not as evidence of his reasons for believing in God, but, as had the Apostle Paul, was directing unbelievers to examine the evidence for God in the creation all about them. Sandemanians generally believed that only they were true Christians. If Faraday also held to this narrow view, and there is some evidence that he did not, then verse 20 was not a statement of his own beliefs, but rather was directed at his audience of unbelievers (12).

Further evidence of Faraday's lack of interest in natural theology is his failure to mark in either Bible any verse about the creation in Genesis 1 and 2, or other well-known passages, such as Psalm 19:1:

The heavens declare the glory of God; and the firmament sheweth his handiwork.

To Faraday, God was not an unknown but a given. He knew that God existed and had created the earth. His Bible, the Word of God, told him so, as he had marked in the Gospel of John (1:1-3$_1$):

In the beginning was the Word, and the Word was with God, and the Word was God. The same was in the beginning with God. All things were made by him, and without him was not anything made that was made ...

With such assurance from scripture, Faraday did not need to look for evidence of God in nature; he had no need for natural theology.

Of all the passages which Faraday marked in his Bibles, one of the most intriguing is in the Old Testament and concerns King David's plans to build the temple, ca. 1000 B.C. It is found in 1 Chronicles 28:9-19, is marked in both the 1776 and 1817 Bibles, and raises a profound question about how Faraday perceived his mission in life.

9. And thou, Solomon my son, know thou the God of thy father, and serve him with a perfect heart and with a willing mind: for the Lord searchest all hearts, and understandeth all the imaginations of the thoughts: if thou seek him, he will be found of thee; but if thou forsake him, he will cast thee off forever.

10. Take heed now: for the Lord hath chosen thee to build a house for the sanctuary: be strong, and do it.

11. Then David gave to Solomon his son the pattern of the porch, and of the houses thereof, and of the treasuries thereof, and of the upper chambers thereof, and of the inner parlours thereof, and of the place of the mercy seat,

12. And the pattern of all that he had by the spirit, of the courts of the house of the Lord, and of all the chambers round about, of the treasuries of the house of God, and of the treasuries of the dedicated things:

13. Also for the courses of the priests and the Levites, and for all the work of the service of the house of the Lord, and for all the vessels of service in the house of the Lord.

14. He gave of gold by weight for things of gold, for all instrments of all manner of service; silver also for all instruments of silver by weight, for all instruments of every kind of service:

15. Even the weight for the candlesticks of gold, and for their lamps of gold, by weight for every candlestick, and for the lamps thereof: and for the candlesticks of silver by weight, both for the candlestick, and also for the lamps thereof, according to the use of every candlestick.

16. And by weight he gave gold for the tables of shewbread, for every table; and likewise silver for the tables of silver:

17. Also pure gold for the fleshhooks, and the bowls, and the cups: and for the golden basons he gave gold by weight for every bason; and likewise silver by weight for every bason of silver:

18. And for the altar of incense refined gold by weight; and gold for the pattern of the chariot of the cherubims that spread out their wings, and covered the ark of the covenant of the Lord.

19. All this, said David, the Lord made me understand in writing by his hand upon me, even all the works of this pattern.

Figure 2. The marking of I Chronicles 28:9-19 in Faraday's Bibles (1776 Bible left, 1817 Bible right).

In a rousing speech before the nation's most important citizens, David tells about how he had planned to build a temple for the Lord God, but had been stopped because the Lord had condemned him as a man of war. Nevertheless, the temple will be built, he says, because the Lord has found favor with Solomon, David's son, and has chosen him to carry out the work. Then, charging Solomon to serve God "with a perfect heart and a willing mind" (28:9), David turns over to him a complete set of blueprints for the new building, or, as verse 12 says:

... the pattern of all that he [David] had by the spirit, of the courts of the house of the Lord, and of the chambers round about and of the treasures of the house of God and of the treasury of dedicated things.

Verses 13 through 18 continue with specifications for the temple's candlesticks, figurines, and other furnishings, and the story climaxes at verse 19 with:

All this, said David, the Lord made me understand in writing by his hand upon me, even the works of his pattern.

As Faraday was reading, he was also marking the verses (figure 2): five lines by verse 9; three by the first line of verse 12, a long sweep past 13 through 19, then a second line beside 15 through 19, a third by 17 through 19, and finally, as the full meaning of what he was reading hit him, a burst of five more lines beside verse 19! The blueprints and specifications were not the product of David's "own" genius; rather, they were drawn by the "Spirit of the Lord" working through David!

The question is this - Did Faraday, as he was reading, suddenly sense that the same hand of God which had worked through David to draw the blueprints of the temple was also working through Michael Faraday to reveal the unknown laws of God's creation? If so, it provided him with a driving force and sense of purpose, which, in the privacy of his faith and in his humility, he would have never shared with another living soul.

Faraday fully believed in, and looked forward to, a future life - a life after death. As Romans 8:24 put it, "We are saved by hope". He knew that, as Jesus had been raised from the dead, so will those be who have faith in him (1 Cor. 15:16-17$_2$). Death will be destroyed (1 Cor. 15:26-28$_2$) and Jesus' followers will take on a new body (1 Cor. 15:49-53$_2$). Death will lose its sting because the grave cannot hold them (1 Cor. 15:55$_4$). All men must appear before the judgment seat of Christ and answer for the way they have spent their lives, whether it be good or evil (2 Cor. 5:10$_2$). To his niece, Faraday wrote in 1859 (13):

Though death be repugnant to the flesh, yet where the spirit is given, to die is gain. What a wonderful transition it is! ... Though the fear of death be a great thought, the hope of eternal life is far greater ...

Three days before his 70th birthday, he wrote to his old friend August de la Rive (1801-1873) that, although he had no science to write about, he could write of a stronger bond (14):

The future life that lies before us. I am, I hope, very thankful that in the withdrawal of powers and things of this life, - the good hope is left with me, which makes the contemplation of death a comfort - not a fear.

Several months before his death, on being asked how he was, he replied, "Just waiting" (15).

In 1 Corinthians 13, the so-called "love chapter", Faraday marked ten (77%) of the chapter's 13 verses. It is here that the Apostle Paul defines a loving person as one who is long suffering, kind, does not put self first, is not easily provoked, does not think evil, finds no joy in sin, rejoices over finding truth, bears all burdens, and never fails others. That definition certainly is in accord with everything Faraday's contemporaries said about the kind of person he was.

Throughout the Bible, Faraday marked passages containing the word love more intensely than those on any other subject. Of 21 chosen at random from the New Testament, he marked ten in his 1776 Bible, three of which follow:

Ye have heard that it hath been said, Thou shalt love thy neighbor, and hate thine enemy. But I say unto you, Love your enemies, bless them that curse you, do good to them that hate you, and pray for them which despitefully use you, and persecute you; That ye may be the children of your Father which is in heaven: for he maketh his sun to rise on the evil and on the good, and sendeth rain on the just and the unjust (Matt. $5{:}43\text{-}45_7$).

A new commandment I give unto you, That ye love one another; as I have loved you, that ye also love one another. By this all men know that ye are my disciples, if ye love one another (John $13{:}34\text{-}35_7$).

[There is one God] and to love him with all the heart, and with all the understanding, and with all the soul, and with all the strength, and to love his neighbor as himself is more than all burnt offerings and sacrifices (Mark $12{:}33_5$).

First Corinthians 13 ends with the thought that of the great concepts of the Christian life - faith, hope, and love - the greatest is love. And so it seems to have been in Faraday's life.

Following Faraday's death, Dr. Bence Jones, Faraday's first biographer, wrote to Mrs. Faraday wishing to know more about her husband's religious beliefs. She replied that she felt inadequate to speak for Faraday, but remembering how he often said that we should be "always ready to give a reason for the hope that is within us with meekness and fear" [1 Peter 3:15], she wished that Jones had asked him. She continued (16):

... I only point to the New Testament as being his *guide and rule*; for he considered it as the Word of God (as you know) and equally binding on Christians today as when written, so that such scriptures as the following were continually on his mind:

If ye love me, keep my commandments - [John 14:15]

Whosoever shall confess me before men, him I confess also before my father who is in heaven - [Matt. 10:32]

Do unto others as you would they should do unto you - [Matt. 7:12, Luke 6:31]

Perhaps Faraday's personal values are best summed up in a letter written in 1860 to his friend of many years, German chemist Christian Schönbein (1799-1868). Faraday's memory was failing and he confessed that he could not remember the contents of Schönbein's previous letter, but added (17):

Though your science is much to me, we are not friends for science sake only but for something better in a man, something more important in his nature, affection, kindness, good feeling, moral worth; and so, in remembrance of these, I now write to place myself in your presence ...

So as we honor Faraday in this 200th anniversary year of his birth, let us believe that those qualities of affection, kindness, good feeling, and moral worth that placed him in the presence of his friend Schönbein so long ago, also place him in our presence today.

References and Notes

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1. A Bible belonging to James Faraday, Michael Faraday's father, is in the Cuming Museum in London, as well as one given by Michael Faraday to a niece in 1865. So far as can be determined by the museum staff, neither of these Bibles was used by Michael Faraday.

2. H. T. Pratt, "Brother Faraday", *Restoration Quarterly*, **1989**, *31 (4)*, 219-229.

3. S. P. Thompson, *Michael Faraday: His Life and Work*, Macmillan, New York, 1898, pp. 245, 254, 286, 290-299. L. P. Williams, *Michael Faraday: A Biography*, Chapman and Hall, London, 1965, pp. 102-106.

4. Sandeman moved to America in 1764 and is buried in

Danbury, Connecticut. See W. Walker, "The Sandemanians of New England," *Ann. Report Am. Hist. Assoc.*, **1901**, *1*, 131-162.

5. L. A. McMillon, *Restoration Roots*, Gospel Teachers Publications, Dallas, 1983.

6. Although this style of preaching has largely disappeared, it can still be heard in many of the more conservative Churches of Christ, particularly in the southern states of the United States. Churches of Christ, like the Sandemanians, are rooted in restoration principles that sprang from Scottish Presbyterianism.

7. Seventeen sermons by four Sandemanian elders, including four by Faraday, first published by James Rorie in 1910, are discussed by P. Eichman in "Selected Exortations: Sermons From a Lost Branch of the Restoration Movement", *Restoration Quarterly*, **1990**, *32 (1)*, 23-35. An undated sermon by Faraday taken down by shorthand is in the archives of The Royal Institution. Examples of two of Faraday's sermon note cards are given in Jones, reference 9, Vol. 2, p. 101. Several other note cards are in the archives of The Royal Institution.

8. Michael Faraday, "Observations on Mental Education", *Experimental Researches in Chemistry and Physics*, Taylor and Francis, London, 1859, p. 465.

9. Early biographers treated Faraday's usage of Romans 1:20 as no more than a simple affirmation of faith. More recently, Clark, Levere, and Cantor have seen Faraday as supporting natural theology, while Russell and Eichman have taken an opposing view. See H. Bence Jones, *The Life and Letters of Faraday*, Vol. 1, Longmans, Green, London, 1870, pp. 337-338; J. H. Gladstone, *Michael Faraday*, Harper, New York, NY, n.d., pp. 130-131; Thompson, reference 3, pp. 291-293; R. E. D. Clark, "Michael Faraday on Science & Religion", *The Hibbert J.*, **1967**, *65 (259)*, 144-147; T. H. Levere, "Faraday, Matter, and Natural Theology", *Brit. J. Hist. Sci.*, **1968**, *4*, 95-107; C. A. Russell, *Cross-Currents: Interactions Between Science and Faith*, Eerdmans, Grand Rapids, MI, 1985, pp. 258-259; G. N. Cantor, "Reading the Book of Nature: The Relation Between Faraday's Religion and His Science", in D. Gooding and F. A. J. L. James, eds., *Faraday Rediscovered*, Stockton Press, New York, 1985, pp. 69-81; and P. Eichman, "Michael Faraday: Man of God - Man of Science", *Perspectives on Science and Faith*, **1988**, *40(2)*, 91-97.

10. Cantor, reference 9, p. 71.

11. It can be argued that Faraday used the 1776 Bible in his youth and the 1817 Bible in later years. If so, his marking of Romans 1:20 in the 1817 Bible suggests that he found some value in natural theology later in life.

12. When asked once by Roman Catholic Cardinal Wiseman (1802-1865) if he believed that all of the Church of Christ was comprised in his little sect, Faraday replied, "Oh no! ... but I do believe from the bottom of my soul that Christ is with us." [Thompson, reference 3, pp. 297-298.]

13. Faraday to Mrs. Deacon, 12 August 1859, in Jones, reference 9, pp. 428-430.

14. Faraday to A. de la Rive, 19 September 1861, in L. P. Williams, ed., *The Selected Correspondence of Michael Faraday*, Vol. 2, Cambridge University Press, Cambridge, 1971, p. 1001.

15. G. Caroe, *The Royal Institution, An Informal History*, Murray, London, 1985, p. 67.

16. Letter, Sarah Faraday to Dr. H. Bence Jones, 22 November 1867, Archives of The Royal Institution.

17. Faraday to Schönbein, 27 March 1860, in Jones, reference 9, pp. 438-439.

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FARADAY'S HEALTH PROBLEMS

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In 1926 the German chemist Alfred Stock published a very interesting article in the *Zeitschrift für angewandte Chemie* (1). The title of the article was "The Danger of Quicksilver Vapor". Quicksilver is, of course, the common name for metallic mercury. In it Stock described his own experiences with mercury poisoning and suggested that Michael Faraday suffered the same affliction. Stock had very good credentials for making such a diagnosis about Faraday, who died in 1867.

Alfred Stock (1876-1946) is remembered for the Stock system of naming inorganic compounds. He invented the glass vacuum line to handle the very reactive silicon and boron compounds in which he was interested (2). He then proceeded to work for an extended period of time, over 25 years, with devices containing mercury: manometers, pumps, valves, etc.

Eventually Stock began to experience a series of health problems, including headaches, dizzy spells, and memory loss (3). For several years his problems were diagnosed as nasal in origin. He underwent treatment for ten years, including cauterization, electrolysis, and surgery. By 1924 his memory had gotten so bad that he could not deliver a lecture without an extensive set of notes. He found himself forgetting telephone numbers between looking them up in the directory and reaching the phone.

Stock began his 1926 article by describing his own ordeal with mercury (1):

> For almost 25 years I suffered from maladies which were at first weak and appeared only occasionally, but became gradually worse and worse, until they became finally almost unbearable, so that I doubted whether I would be able to continue my scientific work.

When an assistant, Wolfhart Siecke, developed similar symptoms, which were diagnosed as due to mercury poisoning, Stock considered the possibility that his health problems may also have been due to exposure to mercury. He had his laboratory outfitted with seamless floors so that droplets of mercury from spills could not collect in cracks. He greatly increased the ventilation in his lab. Gradually, he began to recover and was able to report (1):

All symptoms have, if not completely disappeared, at least more or less subsided since I began two years ago, *without other treatment*, to protect myself from inhalation of mercury vapor (emphasis added).

In this way Alfred Stock demonstrated that his many health problems were due to exposure to mercury vapor. What were these health problems? In the 1926 article Stock lists over 20 symptoms which he experienced. We shall see that Michael Faraday had many of the same symptoms as Alfred Stock.

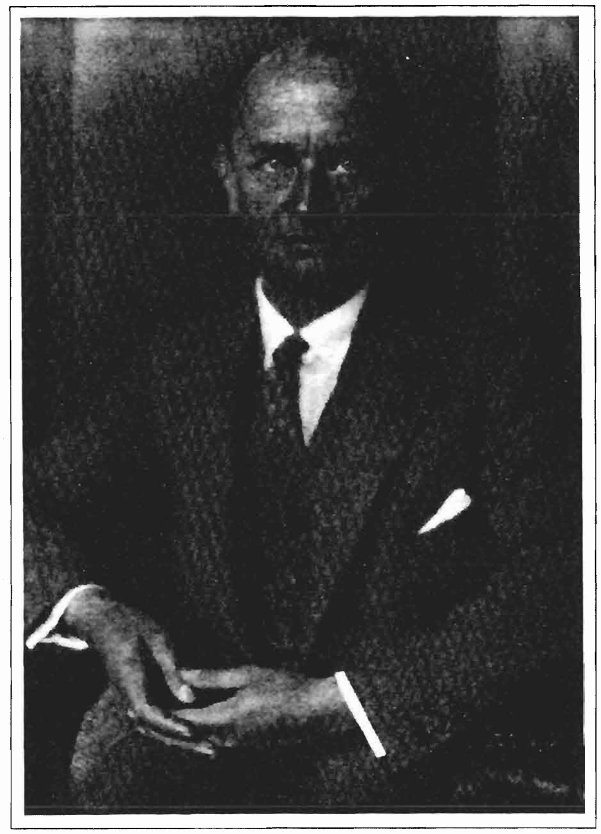
At the conclusion of the article in the *Zeitschrift für angewandte Chemie*, Stock proposed that Michael Faraday (and Blaise Pascal as well) also suffered from mercury poisoning. A number of the same symptoms that afflicted Alfred Stock at age 48 and Michael Faraday at age 49 were also experienced by Isaac Newton at age 49. Stock says nothing about Newton. Yet the possibility that Newton suffered from mercury poisoning has received more attention recently than has the Faraday hypothesis. One reason for this, perhaps, is the relatively recent publication of Newton's correspondence (4) and the subsequent mercury analyses done on his hair (5).

Is there any basis to the suggestion that Michael Faraday had mercury poisoning? At this point the claim rests on circumstantial evidence of two types. First, Michael Faraday did have health problems corresponding to those well documented by Stock as being due to mercury poisoning. Second, Faraday was exposed to mercury vapor on a long term basis. Both his exposure and his health symptoms are heavily documented in Faraday's own writings.

In 1840, at age 49, Faraday's health was bad enough that he had to stop his scientific efforts and take an extended vacation. Professor L. Pearce Williams has referred to this period as a "breakdown" (6). Faraday and wife Sarah went to the continent where she reported that he took very long walks, up to 30 or even 45 miles in a day (7). Gradually he regained his health; though as Professor Williams points out, he never fully recovered (8).

Another Faraday scholar, Geoffrey Cantor, has suggested that there were three distinct periods of crisis in Faraday's life when his mental faculties were not capable of coping with the pressures upon him. These incidents occurred in 1840, 1850, and 1864 (9). The 1840 episode is reminiscent of the breakdown mentioned by all Newton biographers and recently attributed to mercury poisoning (5).

Michael Faraday was exposed to mercury constantly for many years. This is very well documented in his own writings.



Alfred Stock

In his book, *Chemical Manipulation*, there are several passages that reveal Faraday's exposure to mercury vapor. First, a description of the mercurial trough then used to collect gases that are soluble in water (10):

Newman also has a much smaller trough for the use, though in a confined manner, of jars 1.5 inches in diameter and six inches in length; it has only 30 square inches of shelf room. It requires 20 lbs. of mercury to fill it ... A mercurial trough should always stand in a tray, and likewise have a cover to keep out dust and dirt when not in use. Its place ... should be upon the table grooved round the edge, that waste mercury may be avoided as much as possible. When the metal is spilled it is best collected by being swept together and then gathered up by a card.

A second example from *Chemical Manipulation* is Faraday's suggestion on how to fill a capped jar with liquid mercury (11):

When a capped jar is to be filled with mercury, by the assistance of the mouth, the jar should be inclined as much as possible to diminish the height of the column of air within it, as well as the labor attending the operation; then by applying the mouth to the stopcock and using it to exhaust in a manner almost the reverse of that described for blow pipe practice, the air may be withdrawn and the mercury gradually raised until it fills the jar.

Faraday was also familiar with the use of mercury as a fluid for the attainment of high temperatures in baths (12):

To achieve bath temperatures above 212°F one can use liquid mercury as the fluid. If the experiments be made altogether in tubes, a temperature of 600°F may easily be communicated by means of it; but if the bath be an open vessel, a dish or a crucible for instance, then temperatures higher than 450°F should not be given to it; for the metal soon rises in vapor ...

A final example from *Chemical Manipulation* deals with electrical discharge machines, which were used to inflame mixtures of gases (13):

It is often advantageous, especially when the machine is required in haste, to hold a piece of silk with some amalgam upon it against the plate or cylinder, whilst it is turned, and also to rub up the surface of the amalgam upon the rubber with the same amalgamated silk.

These quotes show that Faraday used mercury in a great variety of ways; and that he was by no means afraid of or even careful about exposure to the fumes. Additional evidence of exposure to mercury comes from his diary.

Faraday near the end of his life.

Table 1. Symptoms of mercury poisoning.

* Intermittent slight headaches
* Continuous tormenting headaches
* Difficulty in thinking
* Receding of the gums
* Loosening of the teeth
* Trembling
* Blurred vision
* Dizziness; Giddiness
* Frequent sore throats
* Inflammation of the eyes

That Michael Faraday dealt with mercury right from the start of his scientific career is made clear in his laboratory diary. There we find the first reference to mercury on page 28 of volume 1 of the seven-volume set (14). One of his main uses for mercury was as an electrical contact. For this purpose he employed cups of mercury. The first reference to such a cup is found in his diary for the date 1 November 1832 (15). The last reference to a cup of mercury is in volume 7 for the date 15 May 1858 (16). Thus we see that he employed these cups of mercury for at least a period of 25 years. Were they constantly about, or did he take the time each day to empty the cups and cap the mercury? Given his willingness, as shown by the quotes from *Chemical Manipulation*, to be exposed to mercury, it is conceivable that the electrical contacts he needed so frequently were merely covered and not capped. Another factor relative to the cups of mercury, related by several physicists, is that insertion of electrical leads very likely would cause sparks and increased evaporation (17). Michael Faraday very probably was exposed to mercury vapor every day for over 25 years.

Faraday's writings document not only exposure to mercury, but also the presence of symptoms consistent with mercury poisoning. Table 1 lists symptoms from which Stock recovered and which Faraday mentions in his writings.

In his correspondence Faraday refers to headaches a number of times over a period of years. In 1840 he wrote to Charles Babbage telling him of vacation plans where he hoped "... to get rid of a headache there which as some people say I have enjoyed for the last four months" (18). Years later, in 1856, we find him complaining to Liebig that, "when I sit too, to think, I become headachy and giddy and think to no purpose" (19).

Of course his problems with receding of the gums, loosening of the teeth, and frequent sore throats could very easily have been mercury related (20). In 1849-50 Faraday had a persistent sore throat. Removal of five teeth in the summer of 1850 stopped the sore throats.

Faraday's struggle with memory problems are heavily documented in both his diary and correspondence. In volume

3 of the diary he discussed retaining some notes (21):

I do not know if they are of further use, but because of my bad memory would rather keep them together here, lest I may want them.

Still later in 1852, he wrote (22):

... but I want more and more distinct results, and only reason thus to preserve under the disadvantage of a sadly failing memory, the ideas that I may want to reconsider hereafter.

In 1849 Faraday wrote to Carlo Matteucci telling about redoing experiments because he had forgotten that he had already done them. In a letter to Christian Schönbein Faraday wrote, "When I try to write of Science, it comes back in confusion" (23), and in his last letter to Schönbein we read the very sad (24):

Again and again I tear up my letters, for I write nonsense. I cannot spell or write a line continuously ... I will not write anymore.

There is ample documentation in Faraday's own writings of exposure to mercury vapor and of symptoms consistent with mercury poisoning. The sources cited here are only a few of many. But is there proof? From a modern medical perspective four points can be made (25, 26):

* Headaches and memory loss are caused by too many things to be considered diagnostic for mercury poisoning. They are consistent with mercury poisoning.
* Detection of kidney problems in the 1800s was extremely difficult. The only way to detect such problems was by the appearance of blood in the urine. So the absence of a kidney diagnosis by no means rules out mercury poisoning. Today's blood serum test was not available.
* Cardiovascular insufficiency, which has been suggested as the cause of Michael Faraday's health problems, would have led to isolated strokes and Faraday would have been incapacitated for a period of time (and not gone off on 30 mile walks).
* Heavy metal poisoning would have resulted in tremors.

At this point the case for a mercury poisoning diagnosis is inconclusive. If Michael Faraday was coping with mercury problems, his already admirable achievements become even more remarkable. Perhaps some workers, motivated by Frank James efforts in compiling the complete correspondence, will seek to examine Faraday's hair so that, as with Newton, some direct chemical evidence can be had (27).

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Acknowledgements: A travel grant from the Beckman Center for the History of Chemistry is gratefully acknowledged. The translation of reference (1) by Robert Pietsch is appreciated.

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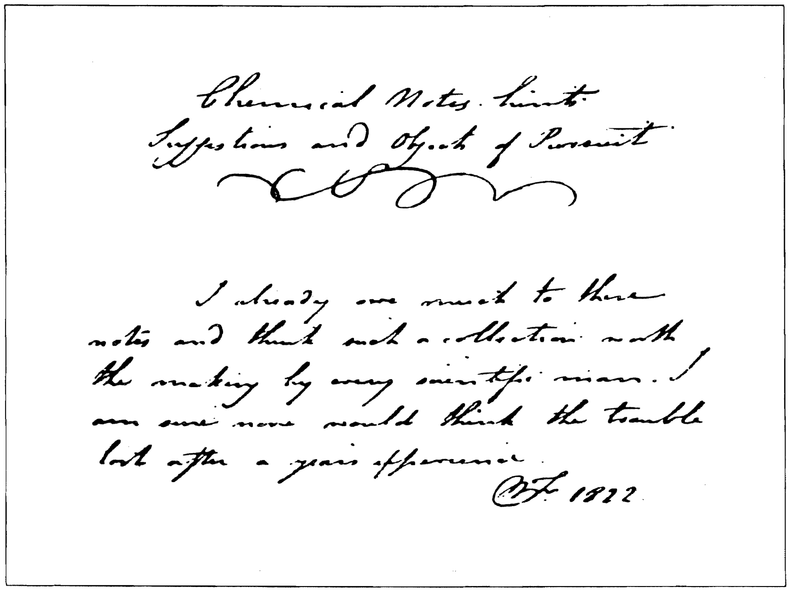
FARADAY'S 1822 "CHEMICAL HINTS" NOTEBOOK AND THE SEMANTICS OF CHEMICAL DISCOURSE

Ryan D. Tweney, Bowling Green State University

To examine the notebook of a famous scientist is a special experience. One feels privileged, blessed with a chance to see into the inner workings of genius. But coming to grips with that genius is a subtler and more difficult process than one might at first imagine, not least because, amid all of the awe and reverence appropriate to the occasion, one can't sometimes avoid a contrary feeling, that the notebook in hand is really a sparse thing, ephemeral stuff hardly worthy of serious attention except, perhaps, for reasons of sentiment.


Chemical Notes, Hints, Suggestions and Objects of Pursuit

I already owe much to these notes and think such a collection worth the making by every scientific man. I am sure none would think the trouble lost after a year's experience.

MF 1822

Handwritten title page of the "Chemical Hints" notebook.

Michael Faraday's 1822 notebook, which he titled *Chemical Notes, Hints, Suggestions, and Objects of Pursuit*, must have struck many of its examiners over the years in something of this fashion (1). A brief glance reveals only a modest volume, far shorter than the celebrated multi-volumed *Diary* (2). By contrast, the *Chemical Notes* seems a group of jottings, characterized by large runs of blank pages (especially at the end) and by seemingly disjointed lists of topics, substances, and unsolved problems. It lacks the chronological flow of the great *Diary* and does not reward the reader with detailed accounts of great discoveries. Many must, over the years, have glanced once or twice at it and returned to the safer, richer haven of the laboratory records, seemingly fitter tributes to one of the greatest scientists of the 19th century.

Yet Faraday himself felt differently. On the title page of the 1822 notebook, he placed the following initialed and dated (1822) note (1):

> I already owe much to these notes and think such a collection worth the making by every scientific man. I am sure none would think the trouble lost after a year's experience.

What led him to make such a strong claim, the like of which appears in none of his other notebooks? Why would he single out for praise a book which he clearly abandoned (as we know from the many blank spaces left unused)? The puzzle is even greater when one realizes that the notebook was of a type that played only a transient role in the long development of his active organization of records and notes, falling roughly halfway between his earliest efforts, the 1809-10 *Common-Place Book* (3), and the emergence of his full-blown, numbered *Diary*, after 1832 (4). What's so special about this transient, short effort?

To answer the question requires a closer look at the content of the notebook. I'd like to do this in two stages, describing first a relatively conventional view, one that singles out the 1822 notebook because it provides tantalizing anticipations of some of his famous later discoveries. Secondly, I'd like to take a deeper view, discussing some of the semantic principles that emerge when we look closely at the contextual meanings of the terms used by Faraday in the notebook. It's this second view that "opens" the notebook for a modern reader in a way which is more valid historically, more accurate cognitively, and more interesting. Along the way, there might be a lesson or two about the nature of scientific thought; insights into the mysterious workings of genius.

The notebook is a small volume, 6 1/2" by 8" and about 3/4" thick. It is bound in paper-covered boards with a sewn leather spine which is quite worn. Faraday was a skilled bookbinder by training and apparently bound the notebook himself (5). The notebook is written on paper watermarked "H. Smith & Son 1821"; thus 1821 is the *earliest* date that Faraday could have written the notebook, especially since the clustering of watermarks suggests that the notebook was never disbound. Some of the entries could, of course, have been recopied from earlier notes, though this seems unlikely. The volume bears the marks of frequent use, showing that it was not a static repository, to be ignored after entries were made. Many leaves in the book are blank, indicating that Faraday bound and numbered the notebook pages expecting to make later entries under the topics listed in the contents.

The dated comment on the title page, and the watermarks, make it clear that he bound the book no later than 1822. It is also clear that he used it after 1822, because of the dated comments on some entries and the dated deletion of others. Exactly when Faraday composed and wrote the entries is important. If we can determine something of the chronology of the entries, then the notebook can serve as a clue to his working methods. For example, if we were to conclude that it was written primarily between 1821 and 1822, then we would have to conclude further that the notebook is a remarkable prevision of a lifetime's worth of research - that Faraday had anticipated himself in 1822. This is too extreme, however, since there is clear evidence that Faraday made entries after 1822. For example, Bradley has noted that the rotating copper plates sketched by Faraday on pages 72 and 73 are remarkably like those used by Arago in the 1825 discovery now known as "Arago's Effect", (the tendency of a copper plate mounted on an axis to turn along with a magnet which is rotating nearby) (6). If Faraday's sketch was inspired by Arago, then clearly he was still making entries in this notebook as late as 1825, by which date he was clearly keeping other notebooks as well. There is further evidence that Faraday was using the notebook after 1822. For example, there are a number of crossed out passages, some of which are dated, the latest of which is 3 November 1824. Such crossed out entries represent experiments or suggestions which he later conducted, updating his earlier entries in the 1822 book.

A quick browse through the notebook turns up many examples of the prescient character of the 1822 notebook. On page 73 is an entry and a sketch that suggests his later much-heralded discovery of electromagnetic induction in 1831. The description of "magnets in copper coils connected with other coils and galvanometer," and the accompanying sketch (figure 1), are uncannily like the apparatus used to first identify the occurrence of induction. In fact, this apparatus could have served for the discovery, provided that the magnet was moved. Why did Faraday not make the discovery in 1822, or, allowing for the possibility of a later entry, in 1825? The answer is complex and must rely on the fact that it was not until 1831 that he realized the importance of looking for a "transient" induction effect rather than a continuous one (7). What is clear is that he had most of the essential components of a successful experiment in the 1820s.

It is interesting to note that the entry occurs in the context of the section on "Heat & Light". This is not so puzzling as it may seem at first sight, since for Faraday, as for most other scientists at that time, heat and light were regarded as "imponderable matter", which, together with Davy's then-recent ar-

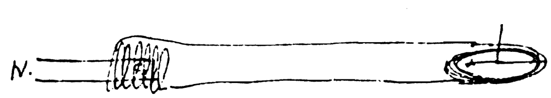


Figure 1. Sketch from the notebook suggesting an anticipation of Faraday's later work on electromagnetic induction.

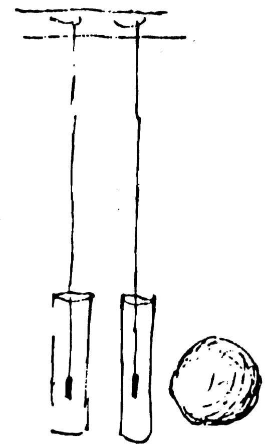


Figure 2. A sketch from the notebook outlining an experiment to detect a possible relationship between gravity and electromagnetism. This search for a unification of the various forces of nature was one of Faraday's life-long preoccupations.

guments for the centrality of electricity in the constitution of matter, made the topics of electricity, magnetism, heat, and light closely associated problems. In the 1820s there was no way to rule out (or rule in, of course) the possibility that electricity and magnetism were entities of a basically similar sort as heat and light.

Many of the other research programs that Faraday carried out in later years are foreshadowed in the 1822 notebook. His 1831 researches on vibrating plates, for example, are prefigured on page 93, where he devotes an entire page to the "Motions of fine particles on elastic plates" (8). Here we have what is apparently a late entry, done not too long in advance of the research itself (which is in the *Diary* for 1831), since he refers to issues that were only raised in Savart's 1827 research on such plates. Savart thought he had found a place of secondary vibration, in addition to the already-known places of nodal vibration familiar from Chladni's research. In 1831, Faraday showed that Savart was wrong, that some of the peculiarities of particle motion on the surface of vibrating plates could be attributed to air currents. In the notebook, we can see that this idea had already occurred, since he refers to "Currents of smoke on plates in still air" and to "Currents under water - shown by dropping coloured particles on to different parts of the plates".

Faraday's life-long preoccupation with the possible relation of gravity and electricity appears on page 10 (figure 2), where he also suggests that magnetism might be relevant. Faraday's predilection for a "unified force" view of the world is reflected here as well. He failed, of course, but not for want of trying. Over the years he repeatedly returned to the possible relation of gravity and electricity or magnetism, but, like Einstein, he never found his unified field theory!

Some discoveries show up after-the-fact, for example, his discovery of benzene ("Bi Carburet of Hydrogen") in 1825 appears in the section on "Sulphur", where he suggests "Bi car hydrogen & sulphur in bottom of a flask - heat" as a possible experiment, and, a few lines further on, to the possibility of a reaction with sulphurets of lead or antimony. Clearly this is a late entry (and it does occur at the end of a section, followed by a page and a half left blank), but it illustrates the fertility of his questioning approach - a newly discovered substance is no excuse for sitting back! Reacting benzene with sulphur or a sulphur compound makes sense here, since, in the original report, he paid special attention to the reaction of sulphuric acid and benzene (9).

Perhaps the most interesting aspect of the 1822 Notebook requires another level of reading. When, for example, Faraday heads a section "Heat & Light", we think we know what he means. The terms are familiar ones, and we find ourselves a bit surprised when he includes material on electricity and magnetism. As we have seen, the inclusion of these seemingly disparate topics is quite understandable once we have looked a bit deeper into the concepts of heat and of light as they were understood by Faraday. In fact, we must be careful to do this for all of his terminology - the language of chemistry has changed a great deal since 1822!

This problem was brought home to me in the course of editing the 1822 notebook for publication (10). My co-editor, David Gooding, and I wanted to include a glossary in the book, thinking particularly of those users who might find themselves puzzled by the many terms that are no longer in current use. "Iodide of zinc" should cause no problems, but what about "liquopodium" or "tutenage"? At first this seemed a straightforward editing task - just find some old dictionaries and look up the terms. This worked for "liquopodium" (a dry, powdered moss) and for "tutenage" (a zinc alloy). But then the problems began to mount. Should we state the modern equivalents and leave it at that? Such a strategy would work for the well-known "bi carburet of hydrogen" (now known as benzene), but it was going to be quite a task to correctly identify all of Faraday's terminology in this fashion! Furthermore, the strategy wouldn't work at all in those cases where the modern term and Faraday's term were the same, but had different meanings (as for "heat").

We ended up with a different sort of glossary than we had envisioned at the start. The best way out of the dilemma, it seemed to us, was to base all of the entries on sources as close to 1822 as we could find and to define all the terms, common and uncommon, familiar and unfamiliar, using quoted definitions appropriate to the times. This meant a very much longer glossary than first planned; in fact, the glossary is longer than the notebook itself. Since we couldn't always find definitions as such, we frequently had to quote passages from non-dictionary sources in the form of passages that revealed the meaning of the term. In effect, the glossary became almost entirely a list of quotations. As work progressed, the temptation to include interesting other bits of information (in the form, again, of contemporaneous quotes) was overwhelming. For example, the following entry for "hydriodic acid" includes information about its composition, its preparation, and its discovery (10):

Hydriodic Acid. "A gaseous compound of hydrogen and iodine, obtained by the mutual decomposition of iodide of phosphorous and water. It is composed of 126 iodine + 1 water" (Brande, 1845, p. 576). "First examined by Davy and Gay-Lussac ... 1814" (Brande, 1836, p. 367).

Much insight into the notebook is possible in this way. First, note that hydriodic acid was a "new" substance, having been discovered only eight years before Faraday began the notebook. Second, note that it was a "local" discovery - Faraday's mentor Humphry Davy shared in the discovery. Since Faraday and Davy were in Europe together in 1814, Faraday himself probably was a participant in the discovery. Note also that the composition is given in terms of the parts of water included.

Obviously, the choice of sources was important for this strategy to work. William Brande's two books (11, 12) were especially nice sources because Brande was Faraday's associate at the Royal Institution, having become Professor of Chemistry in 1813, the same year Faraday arrived as Davy's assistant. Another good source was the manuscript of Faraday's lectures on chemistry delivered before the City Philosophical Society from 1816 to 1818 (13). These were especially revealing for the basic terms (10):

Light. "Imponderable matter produces its most important effects and is best known to us when it is in a state of motion, or radient [sic]; hence it is called Radient [sic] Matter" (Faraday, 1816-18, p. 113).

Sometimes an entire program of research becomes meaningful when we see Faraday's starting point (10):

Gold. "... When beaten out and laid upon glass forms a screen of much transparency ... It has been said that this is occasioned by the existence of small holes in the leaves, which permit the light to pass ... supposing it to be true, the light which passes should be white, whereas it is coloured, and the colour is found to depend on the metal ... Pure gold appears by transmitted light of a purplish colour, gold with a little silver bluish with a little copper green ... and these changes of colour prove that light does not pass through such small accidental holes, but actually through the pores of the metal" (Faraday, 1816-18, pp. 118-119).

Here we get a sense of how certain topics and problems cluster together for Faraday. Consider, for example, Faraday's juxtaposition of queries about gold foil and electrical experiments on page 72 of the 1822 notebook. The topics move from the transparency and color of foil to the remarkably prescient "magnet in a good helix" comment. These seem unrelated, until one realizes that for Faraday a "unity of force" view of the world means that light and its interactions with material substances is a central topic. From the standpoint of the corpuscular theory of light, such interaction, in the absence of chemical change, is puzzling. But if the elementary forces of electricity and magnetism are indeed involved in the construction of matter, it is not so surprising. Gold foil is clearly a good place to look because gold foil changes color (from gold to green) when one changes from reflected light to transmitted light. Something is going on that could be relevant and so one naturally is lead to the possibility that a "Magnet behind gold leaves" will show something new. In later years, Faraday would spend a good deal of time on the investigation of gold by its optical effects on light (14). Thus, placed in its proper context, the juxtaposition is not so surprising and is certainly far from arbitrary!

Even such commonly used terms as "chlorine" reflect the very different context of Faraday's use of this term - for him it was a new word, reflecting its newly discovered elemental status - not a familiar element surrounded only by a technical definition. Similarly, the definitions given in the glossary are much closer to the everyday context of life in the early 19th century than the comparable terms would be today. Definitions are not given in terms of, say, atomic number (an unknown concept in 1822) but in terms of a substance's sensory attributes, its production, its use in commerce, its standing within someone's theory, etc. In general the definitions are closer to what Roberts calls the "sensuous" character of 18th century chemistry than to the 20th century abstractions of elements and compounds (15).

The notebook is a product of a specific context, a time and place which can be detected on every page. For example, it opens a window into one of the main centers of resistance to the new Daltonian atomic theory. Humphry Davy and W. T. Brande, and their protégé, Michael Faraday, did not believe Dalton's hypothesis that chemical phenomena could be explained by positing different, indivisible constituent atoms for each chemical element. To them, Dalton's views smacked of a static, mechanistic system that simply could not explain the active, dynamic universe. They saw this dynamism - not the mechanical interaction of inert corpuscles - as Newton's true legacy (16). Davy, in particular, was heavily influenced by Boscovich, for whom matter was constituted, not of hard material "stuff", but of active, immaterial, centers of force extending out to infinity. For Davy such views were closer to the nature of reality and the only ones capable of explaining his discoveries in electrochemistry. The elementary parts of a chemical substance had to be active, changeable things, capable of at least interacting with forces in a way that Dalton's "little circles" could not do (17, 18).

Faraday's 1822 notebook reflects similar concerns. Chemical questions appear to predominate, yet it includes queries and suggestions about electricity, heat, light, and many other topics that betray the force-centered chemistry of the anti-Dalton school. It should be remembered, too, that for Faraday (as for Davy), the electric current was a new and powerful research tool, to be used alongside more traditional analytical techniques (19). Although Faraday does use chemical equivalents, he avoids atomistic explanations and uses the more neutral term "particles" rather than the term "atom". Even as early as 1822, the notebook shows that Faraday was trying to link together the forces of nature as they were manifested to chemical philosophers through the chemical transformations familiar in the laboratory, in commerce, and in everyday life. In this sense, the notebook prefigures his 1832 discovery of "Faraday's Law of Electrolysis", perhaps the greatest triumph of this view (20).

Sometimes the most consequential evidence in historical study is derived from the smallest of details. This case is no exception. For the historian, it is perhaps an old lesson to say that one must understand the documents of the past in the terms that were relevant in that past - something akin to "translation" is central to all historical scholarship. But the point extends far beyond the literal meanings of terms for a very basic reason having to do with the organization of human memory. We do not construe meaning as simply a one-to-one identification of one term with its corresponding defining proposition. Instead, meaning arises out of networks of associated items. Until we penetrate the web of associations that constitute such networks, we cannot hope to penetrate the thoughts of those figures, like Faraday, that we hope to understand.

Glossaries of the type described help in this endeavor, but only to the extent that the user participates in them to a degree that allows an approximation to the original network. Simply looking up the term "Bi Carburet of Hydrogen" and translating it into "Benzene" does not help with such insight and can, to the extent that a modern network is invoked by the modern term, actually hinder a reading that approximates Faraday's understanding. Just as we learn more about his achievements by a close understanding of his laboratory apparatus, so too do we benefit by a close understanding of his linguistic tools. In this respect, each reader needs to be something of a cognitive scientist, constructing a model of Faraday's cognition that approximates as closely as possible the context of his own thought. To do that is to approach the mind of the master himself, to begin to appreciate the richness of Faraday's achievements and to feel a hint of the excitement that must have been his.

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FARADAY'S SEARCH FOR FLUORINE

Harold Goldwhite, California State University - Los Angeles

In a relatively concentrated and intense period of experimentation, from January 1834 to December 1835, Michael Faraday attempted to prepare elemental fluorine. He was not successful in those attempts. This article presents the background to Faraday's work, the status of fluorine in 1834, the details of Faraday's experimentation, and an assessment of the chemistry involved. It also speculates on Faraday's motivation in undertaking this endeavor.

In 1771 Carl Scheele, repeating and reinterpreting an experiment first reported by Marggraff in 1764, demonstrated that reaction between fluorspar (calcium fluoride) and sulfuric acid liberated a peculiar acid which was combined with lime in the fluorspar (1). This "flussaüre" was always accompanied by deposits of silica in the receiver, for Scheele used glass retorts for his experiments, and he opined that flussaüre might contain silica. In 1781 it was shown that the source of the silica in Scheele's experiments was the glass vessels (2). When Lavoisier advanced his new system in the *Traité élémentaire* in 1789, he described Scheele's acid as "l'acide fluorique" and, following his oxygen system of acids, asserted that it contained oxygen combined with an as yet unknown radical, "fluoricum" (3).

While Humphry Davy was engaged in clarifying the ele-

Carl Wilhelm Scheele

mentary nature of chlorine (4), the French physicist A. Ampère drew his attention, in two letters sent in late 1810 and in 1812, to the many similarities between hydrochloric and hydrofluoric acids. A passage from Ampère's first letter to the master electrochemist makes some striking points (5):

It remains to be seen whether electricity would not decompose liquid hydrofluoric acid if water were removed as far as possible, hydrogen going to one side and oxyfluoric acid to the other, just as when water and hydromuriatic acid are decomposed by the same agent. The only difficulty to be feared is the combination of the oxyfluoric acid set free with the conductor with which it would be brought into contact in the nascent state. Perhaps there is no metal with which it would not combine, but supposing that oxyfluoric acid should, like oxymuriatic acid, be incapable of combining with carbon, this latter body might be a sufficiently good conductor for it to be used with success as such in this experiment.

In the second letter to Davy, Ampère proposed the name "le fluor", or fluorine, for the new radical by analogy with the recently adopted name of chlorine for oxymuriatic acid.

During 1813 and 1814 Davy pursued fluorine but with no success (6):

I undertook the experiment of electrizing pure liquid fluoric acid with considerable interest, as it seemed to offer the most probable method of ascertaining its real nature, but considerable difficulties occurred in executing the process. The liquid fluoric acid immediately destroys glass and all animal and vegetable substances, it acts on all bodies containing metallic oxides, and I know of no substances which are not rapidly dissolved or decomposed by it, except metals, charcoal, phosphorus, sulfur, and certain combinations of chlorine. I attempted to make tubes of sulfur, of muriates of lead, and of copper containing metallic wires, by which it might be electrized, but without success.

André-Marie Ampère

Humphry Davy

I succeeded, however, in boring a piece of horn silver [i.e., native silver chloride] in such a manner that I was able to cement a platina wire into it, by means of a spirit lamp, and by inverting this in a tray of platina filled with liquid fluoric acid I contrived to submit the fluid to the agency of electricity in such a manner that in successive experiments it was possible to collect any elastic fluid that might be produced.

But to no avail. Davy's hydrofluoric acid was not anhydrous, and the only decomposition products collected were hydrogen and oxygen from the water it contained. Davy's attempts to prepare fluorine chemically, by treating heated metal fluorides with a stream of chlorine gas, were equally unproductive (7). This was the situation in the 1820s and early 1830s.

One of the leading chemical characteristics of chlorine, noted by all early workers, was its ability to function as a supporter of combustion, in some respects analogous to oxygen. The story of Davy's tour of Europe, accompanied by the young Faraday, from 1813 to 1815 is a familiar one, and so is that of Davy's brilliant series of investigations made with his portable laboratory during that tour, which indicated the elementary nature of Courtois' dark crystals, more or less simultaneously with Gay Lussac, and the similarity between this novel iodine and chlorine. The excitement of these discoveries by his mentor made a strong impression on the young Faraday. We find him writing to Benjamin Abbott, his closest friend, from Geneva in July 1814 (8):

Before I leave iodine I must ask you & also desire you to inform me of the state of your sentiments respecting chlorine whether you class that substance & fluorine with oxygene ...

After Davy and Faraday returned to England in 1815 Faraday began to broaden his horizons, and undertook in 1816 a course of lectures on chemistry to the City Philosophical Society, a group of young men bent on self-improvement. In his fifth lecture, on the supporters of combustion, he suggested that "It is probable that none of these bodies, oxygen, chlorine, iodine & fluorine, are really simple in their nature" (9). Here we see Faraday following Davy in his rejection of Dalton's chemical atomism. Incidentally, Faraday was steadfast in this opposition and we find him writing to Charles Babbage in April 1837 that (10):

> The simple substances known are fifty three to which if you add fluorine or the X which must stand in its place the number will be 54. I hope the progress of discovery will be rather to diminish than increase the number.

Faraday's interest in the halogens, stimulated by his early contact with Davy's work on chlorine and iodine, showed up in a number of his early chemical investigations. In his first contribution to *Philosophical Transactions*, in 1821, he described the first reported binary compounds of chlorine and carbon, which he obtained in the course of his investigations of the reactions between olefiant gas (ethylene) and chlorine. He isolated and characterized two new compounds, the crystalline perchloride of carbon (hexachloroethane) and the liquid protochloride of carbon (tetrachloroethylene) (11). In 1822 he worked with the crystalline hydrates of chlorine and observed liquid chlorine in the same year (12). The progression from chlorine to fluorine may have been an attractive one to him.

Faraday's diary allows us to follow the course of his experimental attempts to prepare fluorine (13). Amongst the wide range of his exploratory electrochemical investigations in mid-January 1834, he tried the electrolysis of aqueous solutions of potassium fluoride, both without and with the addition of sulfuric acid. In the latter case he noted the action of hydrogen fluoride on the glass of his apparatus and also (14):

> "N electrode 0.67 c:i: this hydrogen ... P Electrode only 0.20 c:i:, which was oxygen. Still the Platina Electrode was not apparently acted upon and the glass in the neighborhood was. From which, and from the small quantity of oxygen, I conclude that Fluorine had been evolved at the Electrode and dissolved in the water *without decomposing it*, but something in the manner of chlorine and iodine. If not so, then the platina ought to have been corroded or else the full equivalent of oxygen set free.

A week later a similar experiment in platinum apparatus gave him a solution which bleached a solution of indigo, and he took this for "additional proof of the production of an aqueous solution of fluorine ... in its bleaching power of analogy with chlorine" (15).

Chemical attempts to prepare fluorine followed (16). By analogy with Scheele's original preparation of chlorine, Faraday mixed "Fluor Spar and Ox. Manganese well powdered" in

Michael Faraday

a platinum crucible with sulfuric acid, but only hydrofluoric acid was evolved. Potassium fluoride substituted for the fluor spar gave similar results, but when "Fluate Potassa, Red lead and Oil Vitriol" were heated in the same crucible the fumes smelt "as if a little Euchlorine with it". However, red lead and oil of vitriol with no added fluoride seemed to give a similar smell. Nevertheless, the slight possibility that the lead compound might be producing a little fluorine induced Faraday to continue working with lead salts. He prepared lead fluoride by reaction between potassium fluoride and lead nitrate, and noted that it fuses unchanged at red heat in platinum. Preliminary attempts to make silver fluoride led Faraday to conclude that it must be soluble in water (as indeed it is) and he did not initially isolate it. Lead fluoride became the focus of Faraday's subsequent electrolytic experiments.

In platinum apparatus that would be dubbed microscale today, Faraday electrolyzed fused lead fluoride (m.p. 822°C) and observed (17):

> Much good action at N. foil; lead reduced and Platina alloyed and fuzed. At P. Electrode effervescence much; vapours transparent, pungent etc.

When he used a plumbago (graphite) positive electrode there was evolution of a pungent gas or vapor, but no action on the plumbago (17). An elegant new apparatus allowed Faraday to observe the vapor more carefully (18):

> Although the gas or vapor at the P. electrode came off irregularly and soon mixed with air in and about the tube, I was able to make the

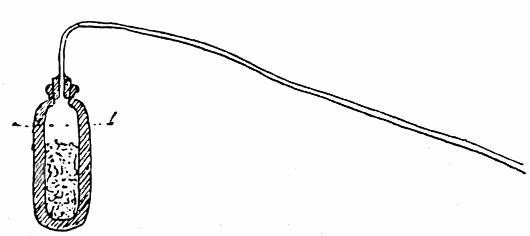
following observations. It must I think have been *fluorine*. It was colorless and transparent. It did *not* produce fumes in the air like hydro fluoric acid, but when an open glass was held over the end, thus, fumes at the aperture above were produced ... The transparent fluorine vapors pungent to the mouth and nostrils. They reddened litmus paper but did not bleach it ... A little copper leaf tarnished in the fumes. Silver leaf also tarnished ... but I could dissolve no fluate off from it.

The experiments continued for a month. Faraday isolated silver(I) fluoride, but it did not break down thermally, as he had hoped, to give fluorine. He observed decomposition but concluded that it was due to the presence of water in the silver fluoride, a salt that is hard to dehydrate fully. Electrolysis of molten lead fluoride in another new apparatus confirmed his earlier observations, and his opinion that he had prepared fluorine, but added nothing new to his observations (19). The topic was put aside for a time in mid-February 1834.

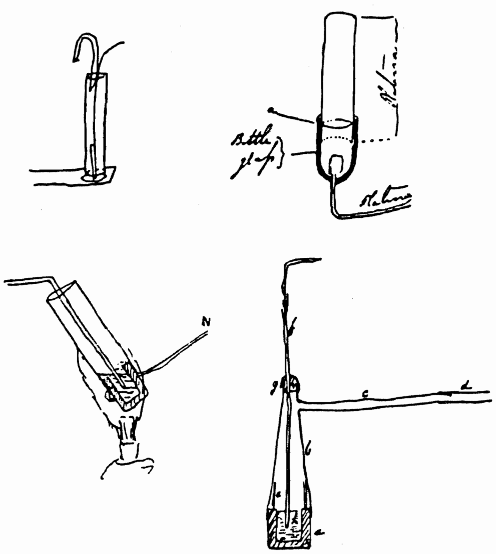
Faraday came back to it in January 1835. He essayed new ways of making lead fluoride, and noted that lead chloride and lead fluoride, when fused together, apparently form a mixed compound - "probably a useful mixture in voltaic decompositions for fluorine". After a month's work on the chemistry of lead and silver fluorides, and after preparing a large batch of lead fluoride that was probably purer than his earlier samples, he was ready to resume electrolyses. His Diary entry of 19 February 1835 is confidently headed "Fluorine" (20). Another new electrolysis vessel was devised, but this time (21):

... there were very little signs of any gaseous or vaporous matter being produced in the retort ... By far the greatest portion of the electricity passes as through an undecomposable conductor, a metal for instance; but there appears to be a little action, for lead is rendered at the cathode and the platina wire is corroded at the anode. In this state this body [fused lead(II) fluoride] presents an extraordinary case between ordinary electrolytes and ordinary good conductors ... I must look out for a fluoride not having the peculiar properties this possesses. Try several.

His passing comment on the intermediate conductivity of lead fluoride represents one of the first recorded observations of the



Faraday's apparatus for reacting lead fluoride and graphite in an attempt to prepare "fluo-carbon" (32).



The evolution of Faraday's vessels for electrolysis of fused lead fluoride: 20 Jan. 1834 (top l), 29 Jan. 1834 (top r), 10 Feb. 1834 (bottom l), and 19 Feb. 1835 (bottom r) (31).

phenomenon of semiconductors.

Fluorine from lead fluoride was proving elusive, and Faraday was quick to suggest what had been the problems in his earlier experiments (22):

As the first fluoride of lead [January-February 1834] gave gas at P. Electrode when electrolyzed, suspected now this was from nitrate or oxide present left in from nitrate when washed. Added a little pure oxide of lead to this pure fluoride of lead, and now gas *was evolved* at the Anode. But this probably oxygen from the oxide. This confirms my view.

He continued to explore the chemistry of fused lead fluoride, noting that it apparently reacted with charcoal, and possibly even with diamond. On minimal evidence he suggested that a *"fluo-carbon* had been formed, analogous to *fluo-silicon* and acting in the same manner on water". He repeated the experiment with lead fluoride and plumbago on a larger scale, but the results were inconclusive (23).

In October 1835 Faraday was still looking for suitable fluorine precursors. He examined routes to platinum fluorides and gold fluorides, but found nothing encouraging (24). Then in November 1835, he started his last concentrated attack on the problem (25):

I required a solution equivalent to hydro fluoric acid in which I might render platina, Gold, etc. positive by the Voltaic battery: for this purpose I added strong sulphuric acid to the strong solution of fluoride Pm. [potassium] ... No fumes of H. f. acid were produced (unless large quantities were used), nor did the solution taste as sour as I expected.

Many different metals were tried as electrodes in this solution: platinum, gold, palladium, silver, iron, tin, antimony, copper, and zinc. No fluorine was obtained, but Faraday was led to speculate on the properties of his unobtained element (25):

Supposing fluorine obtained and held in platina vessels. Would it not abstract hydrogen from water at common temperatures? Would it not inflame in hot steam? Would not a mixture of fluorine and steam burn spontaneously or be ignited by a flame or an electric spark? Would not fluorine serve as an abstractor of hydrogen, and so be opposed to bodies generally as abstractors of oxygen.

By the end of 1835 Faraday had apparently decided to move to more promising areas: "I cannot go on at present with the fluorine experiments". When he reprinted his earlier publications on electrochemistry in the first volume of his *Experimental Researches in Electricity* in 1839 he quoted (26):

Hydrofluoric acid and fluorides. Solution of hydrofluoric acid did not appear to be decomposed under the influence of the electric current: it was the water which gave way apparently. The fused fluorides were electrolysed; but having during these actions obtained *fluorine* in the separate state, I think it better to refer to a future series of these researches, in which I purpose giving a fuller account of the results than would be consistent with propriety here.

but he added, in December 1838, for the reprint, the following significant footnote:

I have not obtained fluorine: my expectations, amounting to conviction, passed away one by one when subjected to rigorous examination; some very singular results were obtained.

Faraday himself pointed to some of his earlier spurious sightings of "fluorine" as probably due to oxygen from traces of oxide or nitrate in his samples. That may have been part of the problem, but the earliest lead fluoride electrolyses he conducted were in equipment open to the air. Lead(II) fluoride has a substantial vapor pressure at its melting point of 822°C; its vapor is readily hydrolyzed to produce lead oxide and hydrogen fluoride (27). Consequently the vapors above Faraday's molten lead fluoride in equipment open to the air reddened litmus and inevitably produced oxide, a source of oxygen in the electrolysis. It is significant that in his final experiments, in closed apparatus, there was very little gas evolution. The electrochemistry of pure molten lead fluoride *in vacuo* does not appear to have been explored. Since lead(IV) fluoride exists, as do well-characterized hexafluoroplumbates(IV) (28), it seems possible that the redox process in molten lead(II) fluoride might give elemental lead and lead(IV) fluoride which, in lead(II) fluoride, might be present as $Pb^{2+}PbF_6^{2-}$ (equivalent stoichiometrically to a novel lead trifluoride).

The episode with fluorine demonstrates many of Faraday's skills as an experimenter: the visualization of an important problem; the breadth of the attack on it; the range of ingenious equipment and experiments devised for that attack; the repetition to check on reliability and reproducibility; the rapid grasp of anomaly, and the suggestions of reasons for it; and the decision to abandon an unprofitable area when the returns were not up to his expectations. It sounds like (and is) a check-list for any aspiring scientist. Faraday's early views on "philosophical deduction", enunciated in his final lecture to the City Philosophical Society, stood him in good stead (29):

The man who is certain he is right is almost sure to be wrong; and he has the additional misfortune of inevitably remaining so. All our theories are fixed upon uncertain data, and all of them want alteration and support. All I wish to point out is ... the continual guard against philosophical prejudices which should be preserved in the mind. The man who wishes to advance in knowledge should never of himself fix obstacles in the way.

Let me close with a speculation. Among all Faraday's electrochemical experiments, the attempt to prepare fluorine is in some ways an oddity. Faraday did, of course, carry out many other qualitative experiments to determine products of electrolyses, and was certainly a pioneer in fused salt electrolysis, but the attack on the fluorine problem was unusually sustained, and Faraday seems to have been uncharacteristically optimistic in his view of his early results. It seems to me that he wanted to believe he had prepared fluorine, and I am drawn to speculate on why this was so. One reason may have been the significance of the result. During most of the 19th century, the isolation of elemental fluorine was a, or perhaps the, major challenge to inorganic chemists. Moissan's eventual success in 1886 gained him international recognition. Faraday was aware of the recognition that discovery of new elements (through the application of electrochemistry) could bring to an investigator, for he had Davy's example before him. I believe Faraday would not have been indifferent to the acclaim that would have greeted him as the discoverer of fluorine.

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OBSERVATIONS ON FARADAY AS ORGANIC CHEMIST MANQUÉ

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Faraday's work in pure chemistry has been treated somewhat condescendingly by commentators. Partington dismisses it in less than a page and Williams' rather fuller treatment is consigned to an omnibus chapter titled "The Fallow Years" (1, 2). There is perhaps some justice in this. If we exclude the work on liquefaction of gases as being *sui generis* non-chemical and if we yield that on "Relations of Gold and Other Metals to Light" to the uncoagulated and as yet unnamed colloid chemists (3), we are forced to concede that even those chemical papers Faraday chose to reprint in *Experimental Researches in Chemistry and Physics* (4) are, for the most part, minor work. Faraday's touching footnote to the first of these, "Analysis of Native Caustic Lime", could with equal humility have been appended to several others (5):

> I reprint this paper at full length. It was the beginning of my communications to the public, and in its results very important to me. Sir Humphry Davy gave me the analysis to make as a first attempt in chemistry at a time when my fear was greater than my confidence, and both far greater than my knowledge; at a time also when I had no thought of ever writing an original paper on science. The addition of his own comments and the publication of the paper encouraged me to go on making, from time to time, other slight communications, some of which appear in this volume. Their transference from the "Quarterly" into other Journals increased my boldness; and now that forty years have elapsed and I can look back on what the successive communications have led to, I still hope, much as their character has changed, that I have not, either now or forty years ago, been too bold.

There are six papers, however, that taken together provide a striking exception to this generalization: "Two New Compounds of Chlorine and Carbon, etc."; "New Compound of Chlorine and Carbon"; "Hydriodide of Carbon"; "New Compounds of Carbon and Hydrogen"; "Pure Caoutchouc"; and "Mutual Action of Sulphuric Acid and Naphthaline". These reveal that in the years between 1820 and 1826, Faraday had mastered those arts - synthesis, separation, purification, characterization, analysis - necessary to the emerging subdiscipline of organic chemistry. Indeed, it is doubtful that any of his contemporaries could claim greater achievement in that area. Even so he abandoned the subject in 1826, never to return, and if we are to judge by his letters to Liebig and Dumas, among others, he subsequently evinced little interest in the extraordinary efflorescence of organic chemistry that took place during the rest of his life.

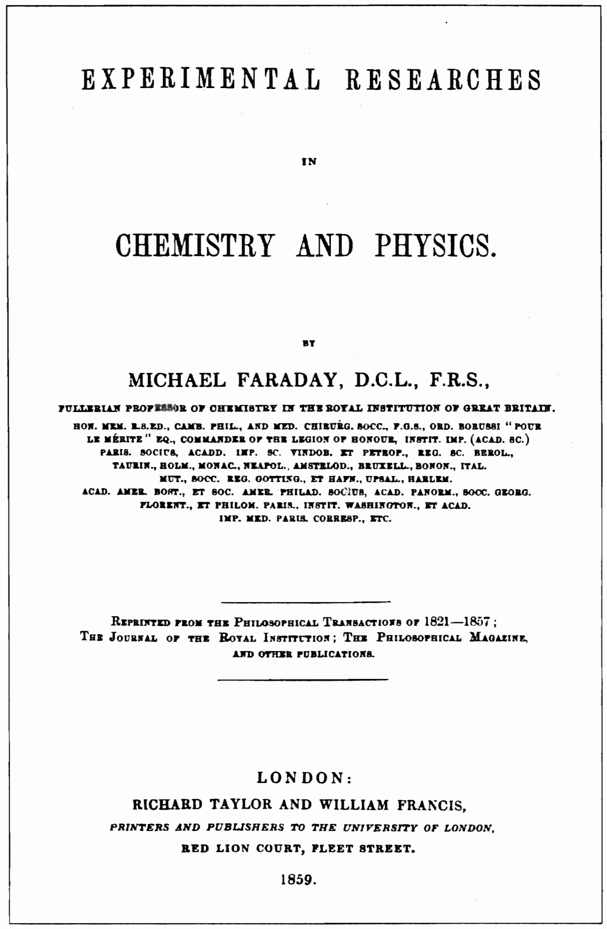
"On Two New Compounds of Chlorine and Carbon, and on a New Compound of Iodine, Carbon, and Hydrogen" (6) describes the preparation and characterization of perchloride of carbon (C_2Cl_6) and protochloride of carbon (C_2Cl_4) while "On a New Compound of Chlorine and Carbon" (7) deals with what the *Merck Index* rather quaintly identifies as hexachlorobenzene or "Anticarie; Bunt-cure; Bunt-no-more; Julin's carbon chloride" (C_6Cl_6) (8). Both early (9) and late (10) Faraday had a particular fondness for the element chlorine, a fondness no doubt devolving from his admiration of Davy. In the published version of the paper on the perchloride and protochloride of carbon he justifies at considerable length the reasons for his investigations. Not so in the *Diary* entry for September 1820, which peremptorily begins (11):

Chlorine and olefiant oil exposed in a retort to sun light soon act; the vessel becomes misty, the colour of the chlorine disappears, a little heat is extricated and the bulk of the gas perhaps from that cause appears increased. The gas contains much M.A. [muriatic acid gas, HCl] and there is a smell as of Phosgene gas. (Query oxygen present?)

Dendritical crystals gradually form; these may be washed in water, dissolved in Alcohol and crystallized.

Three months of intense experimentation (involving at least 20 separate runs and multicomponent analyses) were necessary before Faraday felt confident enough to claim (12):

Other experiments gave very nearly the same results; and I have deduced from them, that one volume of olefiant gas requires five volumes of chlorine for its conversion into muriatic acid and chloride of carbon; that four volumes of muriatic acid gas are formed; that three volumes of chlorine combine with the two volumes of carbon in the olefiant gas to form the solid crystalline chloride; and that, when chlorine acts on the fluid compound of chlorine and olefiant gas, for every volume of chlorine that combines, an equal volume of hydrogen is separated.


EXPERIMENTAL RESEARCHES

IN

CHEMISTRY AND PHYSICS.

BY

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1859.

Michael Faraday (Etching by McGuire)

Faraday not only measured in terms of volumes he also reasoned in terms of them. Nonetheless his results translate unambiguously into the modern representation:

$$h\nu + C_2H_4(g) + 5Cl_2(g) \rightarrow C_2Cl_6(s) + 4HCl(g)$$

He noted that "no muriatic acid gas formed unless chlorine in excess of olefiant gas":

$$C_2H_4(g) + Cl_2(g) \rightarrow C_2H_4Cl_2(g)$$

and that this was followed by:

$$h\nu + C_2H_4Cl_2(l) + 4Cl_2(g) \rightarrow C_2Cl_6(s) + 4HCl(g)$$

He verified these volumetric arguments by gravimetric determination of the composition of the binary compound: first by converting the carbon to carbon dioxide by heating with "peroxide of copper" or lime and second by converting the chlorine to silver chloride. In the published paper he cites only "two results from a number of experiments agreeing well with each other". Faraday then gives an indirectly calculated value for the carbon content of the perchloride corresponding to 10.19% (modern value 10.15) and a direct value for the ratio of silver chloride produced to that of perchloride reacted of 3.54 (modern value 3.63). The former agreement may be partly coincidental. Not only did Faraday have to make do with the best available value for the carbon content of his measured volume of carbon dioxide, the *Diary* also reveals much variability with such comments as "Irregular Results do not promise much", "Pretty good but repeat", "Very good expt.", "Too little carbon, too much chloride". The last comment suggests that Faraday, as Kohlrauch was later to remark, "smelt the truth" and thus felt confident in stating that "Three proportions of chlorine 100.5 and Two proportions of carbon 11.4" represented the composition of the perchloride of carbon (13). Allowance must be made for the fact that Faraday's interpretation of his data involved what was tantamount to an atomic weight of approximately six for carbon. It would appear that he rested more confidence in the volumetric than in the gravimetric determination of composition.

The liquid protochloride of carbon was made by passing the perchloride through a hot tube. The reaction was shown to be reversible in sunlight:

$$\text{heat} + C_2Cl_6(s) \rightarrow C_2Cl_4(l) + Cl_2(g)$$

$$h\upsilon + C_2Cl_4(g) + Cl_2(g) \rightarrow C_2Cl_6(s)$$

Separation was achieved by bulb-to-bulb distillation. Similar experiments to those used to establish the composition of the perchloride enabled Faraday to conclude "the composition of the fluid chloride of carbon to be one proportion of chlorine and one of carbon, or 33.5 of the former, and 5.7 of the latter" (14). Again the *Diary* reveals considerable variability but once again Faraday's "nose" stood him in good stead.

Given the armamentarium of the time, the investigation of chemical and physical properties of the two binary chlorides was wide-ranging: their high temperature interactions with metals, non-metals, and metal oxides; their unreactivity towards acids, bases and, at less than red-heat, oxygen. *Apropos* of the last-mentioned, Faraday notes that when heated with oxygen over mercury "there was no decomposition, or action, until so much mercury had risen in vapour as to aid the oxygen by a kind of double affinity in decomposing the chloride of carbon" (15).

Faraday also reports the addition of iodine to olefiant gas to give a solid, white crystalline body, "having a sweet taste and aromatic smell ... The alcoholic solution is of a very sweet taste, but leaves a peculiarly sharp biting taste on the tongue" (16). The analysis, described in a later note "On Hydriodide of Carbon" is exemplary (17):

> Four grains were passed in vapour over heated copper, in a green glass tube; iodide of copper was formed, and pure olefiant gas evolved, which amounted to 1.37 cubic inch. As 100 cubic inches of olefiant gas weigh about 30.15 grs., so 1.37 cubic inch will weigh 0.413 gr. Now 4 grains minus 0.413 leaves 3.587 iodine, and 3.587 : 0.413 :: 117.75 : 13.55 nearly. Now 13.55 is so nearly the number of two proportions of olefiant gas, that the substance may be considered as composed of
>
> 1 proportion of Iodine 117.75
> 2 proportions of Olefiant gas . . . 13.4
>
> and is therefore analogous in its constitution to the compound of chlorine and olefiant gas, sometimes called chloric ether.

Faraday found 89.7% of iodine in the compound as compared with the modern value of 90.0% for ethylene di-iodide. In this case exposure of the di-iodide to sunlight caused no further reaction in the presence of excess iodine.

Faraday had established the existence of two chlorocarbons of carbon: the perchloride with three proportions of chlorine and two of carbon and the protochloride with one proportion of chlorine and one of carbon. With impressive pre-homologous logic, he speculated that there should be a third compound containing "two proportions of chlorine and one of carbon" (18). In modern terms, this is carbon tetrachloride, a substance not isolated until 1839 (19).

Chance, however, quickly placed a third perchlorocarbon in Faraday's hands. One "M. Julin, of Abo in Finland", had unaccountably obtained a white solid, seemingly containing only carbon and chlorine, from a process in which "nitric acid is prepared by distilling calcined sulphate of iron with crude nitre in iron retorts". In "On a New Compound of Chlorine and Carbon" (20), Faraday and Phillips establish that this solid contains "one portion of chlorine and two portions of carbon" (21). In 1869 Muller showed it to be hexachlorobenzene.

The most celebrated of Faraday's achievements in organic chemistry is his isolation of bicarburet of hydrogen or benzene. This is described, along with the isolation of isobutylene, in "On New Compounds of Carbon and Hydrogen, and On Certain Other Products Obtained During the Decomposition of Oil by Heat" (22). The work started on 26 April 1825 and was pursued with mounting intensity through May and into early June. The "liquor from condensed Oil gas sent to me by Mr. Gordon" was a fiendishly complicated mixture and Faraday spent most of May trying to resolve it into demonstrably pure components. By dint of repeated fractional distillation followed by selective fractional freezing, each stage monitored by analysis, a fairly pure sample of what proved to be bicarburet of hydrogen was obtained. On 25 May, Faraday's *Diary* reports a carbon/hydrogen weight ratio of 11.305. On 4 June, he finds 11.44, 12.4, and 11.16. Of the second value he notes in the *Diary*: "Must be some mistake here: weight of products surpass original weight." Even so in the published paper he includes this value in calculating a mean value of 11.576. With characteristic intuition Faraday explains away the somewhat low mean value (23):

Now considering that the substance must, according to the manner in which it was prepared, still retain a portion of the body boiling at 186°, but remaining fluid at 0°, and which substance I find, as will be seen hereafter, to contain less carbon than the crystalline compound (only about 8.25 to 1 of hydrogen), it may be admitted, I think, that the constant though small deficit of carbon found in the experiments is due to the portion so retained; and that the crystalline compound would, if pure, yield 12 of carbon for each 1 of hydrogen, or two proportions of the former element and one of the latter.

2 proportionals Carbon	12	} 13 bicarburet of hydrogen
1 proportionals Hydrogen	1	

He finds confirmation for his rationale in a vapor density of 40 (H = 1) and in his experimental demonstration of the gaseous volume-ratios corresponding to the modern representation:

$$C_6H_6(g) + 15/2\ O_2(g) \rightarrow 6CO_2(g) + 3H_2O(g)$$

It is easy to describe such results but hard to do justice to the experimental skill and perseverance necessary at a time when the classical method of organic analysis was still evolving (24 - 26). In one of the few treatments of "Faraday as a Chemist", William Jackson Pope comes close (27):

He determined the composition of the hydrocarbon by a method so ingenious that it might well tax the skill of the modern worker. He evaporated the hydrocarbon into a known volume of oxygen, noted the increase in gaseous volume, exploded the mixture in the eudiometer and noted the diminution in volume, then treated with caustic potash solution and observed the further diminution in volume due to the removal of the carbon dioxide. The data thus obtained gave the proportion of carbon to hydrogen, and also the density of benzene vapour as compared with hydrogen as the standard; Faraday hence calculated the vapour density as 39, which is the correct value.

Faraday also investigated the chemical properties of "bicarburet of hydrogen" noting its reaction with nitric acid (here Faraday's real nose misled him into suspecting the formation of hydrogen cyanide rather than nitrobenzene). Reaction with sulfuric acid was studied in detail and Faraday was particularly impressed that "no sulphurous acid was formed" as a result of reaction. He was unable to isolate pure benzene sulfonic acid though later he was successful in obtaining barium salts of both the α- and β-isomers of naphthalene sulfonic acid (28). Bicarburet of hydrogen and chlorine gas did not react in the dark but did so in the presence of sunlight. A solid with "an odour something resembling perchloride of carbon but more resembling artificial camphor" (no doubt *p*-dichlorobenzene) and an unresolved liquid residue (probably largely *o*-dichlorobenzene) resulted (28). A rich and, as it proved, vastly important chemistry was opening up.

From the same oil-gas source Faraday also managed to isolate and largely to purify the most volatile component. Analysis showed that "four volumes or proportionals of hydrogen = 4, are combined with four proportionals of carbon = 24, to form one volume of the vapour, the specific gravity of which would therefore be 28" (29). As Faraday was quick to note "the proportions of the elements in this vapour appear to be the same as in olefiant gas" with its specific gravity of 14 (H = 1). Faraday concluded: "This is a remarkable circumstance, and assists in showing that though the elements are the same, and in the same proportions as in olefiant gas, they are in a very different state of combination" (30). It would be many years before Faraday's discovery could be fully explained. In 1819 Berzelius had called Davy to task for delegating critical analyses to an assistant, in this case the young Michael Faraday (31):

If M. Davy would be so kind as to take the pains of repeating these experiments himself he should be convinced of the fact that when it comes to exact analyses, one should never entrust them into the care of another person; and this is above all a necessary rule to observe when it comes to refuting the works of other chemists who have not shown themselves ignorant of the art of making exact experiments.

Faraday had clearly learned both his lesson and his trade and he must have taken particular pleasure in Berzelius' encomium (32):

One of the most important chemical investigations which has enriched chemistry during 1825 is without doubt that of Faraday on the oily compounds of carbon and hydrogen obtained by compressing the gases obtained by the decomposition of fatty oils.

The paper "On the Mutual Action of Sulphuric Acid and Naphthaline" calls for little further comment (28). From the reaction mixture two new organic acids were isolated in the form of their barium salts. One Faraday dubbed the "flaming salt", the other the "glowing salt". Using "Dr. Prout's newly perfected mercurial trough", Faraday obtained the following remarkable analyses (28):

	Flaming Salt	*Glowing Salt*
Baryta	27.57 or 78	28.03 or 78
Sulphuric Acid	30.17 or 85.35	29.13 or 81.41
Carbon	41.90 or 118.54	42.40 or 118
Hydrogen	2.877 or 8.13	2.66 or 7.4
	102.517	102.22

The second and fourth columns "do not differ far from the following theoretical statement" (33):

Baryta	1	proportional	78
Sulphuric Acid	2	proportionals	80
Carbon	20	proportionals	120
Hydrogen	8	proportionals	8

Nowadays we do not write $BaO{\cdot}(SO_3)_2(C_{10}H_8)_2$ for the barium salt of naphthalene sulfonic acid, but once again Faraday's almost unerring nose for the truth is in evidence. Faraday ends his paper with characteristically pragmatic circumspection (34):

As the appropriation of a name to this acid will much facilitate future reference and description, I may perhaps be allowed to suggest that of *sulphonaphthalic acid*, which sufficiently indicates its source and nature without the inconvenience of involving theoretical views.

The last of Faraday's organic researches "On Pure Caoutchouc, and the Substances by which it is Accompanied in the State of Sap or Juice" also had its beginnings in a commercial analysis (35). The fluid provided by a Mr. Hancock was a "pale yellow, thick, creamy looking substance" possessed of a "disagreeable acescent odour, something resembling that of putrescent milk". Bulk analysis yielded (35):

Caoutchouc	317.0
Albuminous precipitate	19.0
Peculiar bitter colouring matter, a highly azotated substance Wax	71.3
Substance soluble in water, not in alcohol	29.0
Water, acid, etc.	563.7
	1000.0

Of more interest (though not perhaps to Mr. Hancock) was the carbon to hydrogen ratio found for pure caoutchouc. The *Diary* reveals four values: 6.875, 6.582, 7.8(04), and 6.98. The published paper uses the "mean of three best" or 6.812 to 1.000 and concludes that caoutchouc contains "8 proportionals nearly of carbon and 7 of hydrogen". The discarded ratio, 7.8, is in fact closest to that of polyisoprene (7.45). Even Faraday couldn't win them all!

By early 1826 Faraday had completed his researches in organic chemistry and Liebig was in process of setting up shop in Giessen. Faraday's manifest destiny lay elsewhere.

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34. Reference 4, p. 198.
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MICHAEL FARADAY AND THE ART AND SCIENCE OF CHEMICAL MANIPULATION

William B. Jensen, University of Cincinnati

Though a vast secondary literature now exists chronicling the life and achievements of Michael Faraday (figure 1), virtually none of it deals with his only full-length book, *Chemical Manipulation*, first published in 1827 (1). His numerous biographers mention only the fact of its publication, but tell us nothing of its contents and little of the circumstances surrounding its writing. Given the vast amount of important scientific work done by Faraday, this oversight is perhaps understandable. Unlike his famous *Diary* (2), the three volumes of his *Experimental Researches in Electricity* (3), and the companion volume of *Experimental Researches in Chemistry and Physics* (4), *Chemical Manipulation* records no significant scientific discovery. Unlike his famous juvenile lectures on the *Various Forces of Matter* (5) and the *Chemical History of a Candle* (6), or the lesser known *Lectures on the Non-metallic Elements* (7), it lacks accessibility and popular appeal. Yet, as already mentioned, it was the only book explicitly written by Faraday (Table 1) - the volumes of *Experimental Researches* were actually reprints of previously published scientific papers and all three of the juvenile lecture series were transcribed from stenographic notes and edited by others - *Forces of Matter* and the *Chemical History of a Candle* by William Crookes and the *Non-metallic Elements* by John Scoffern.

Nevertheless, it can be argued that *Chemical Manipulation* does merit closer examination, if for no other reason than it gives us valuable insight into the extent of Faraday's training

Table 1. Faraday's books.

Chemical Manipulation, 1827
Six Lectures on the Nonmetallic Elements, 1853
Experimental Researches in Electricity, 3 vols., 1839-1855
Experimental Researches in Chemistry and Physics, 1859
Six Lectures on the Various Forces of Matter, 1860
Six Lectures on the Chemical History of a Candle, 1861

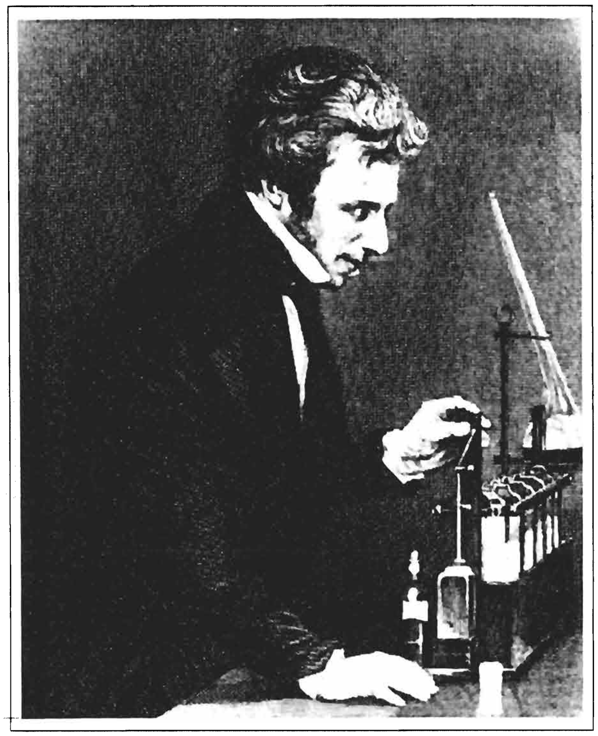


Figure 1. Michael Faraday

as a chemist and the minutiae of the laboratory environment in which he worked on a daily basis. I would like to approach this examination in four stages, starting with an analysis of the origins of the book and the laboratory milieu in which Faraday worked at the Royal Institution, followed by a brief survey of some of the book's predecessors, followed by a survey of its contents, and finally, by a brief look at some of its successors.

Faraday first entered the laboratory of the Royal Institution in the spring of 1813, at age 21, as Humphry Davy's laboratory assistant. After a 19-month leave of absence (October 1813-April 1815) to accompany Davy and his wife on a continential tour, he returned to the Royal Instititution as an assistant to William Brande, who had succeeded Davy as Professor of Chemistry after the latter's resignation in 1813. In 1821 Faraday was appointed, at age 29, as "Superintendent of the House and Laboratory" - a promotion which allowed him to marry Sarah Barnard - and in 1825 he became "Director of the Laboratory". It was only in 1834, at age 42, that he was finally appointed Fullerian Professor of Chemistry (8).

The institution in which Faraday found himself had been organized in 1799, largely at the instigation of the American expatriat, Count Rumford, and was located in a remodeled house at 21 Albermarle Street, London (the current front of the building with its stucco pillars was not added until 1838, see page 7 of this issue). As was typical of most laboratory design of the period, the architect in charge of the remodeling placed the chemical laboratory in the basement, where it occupied a position roughly corresponding to that of the original out-

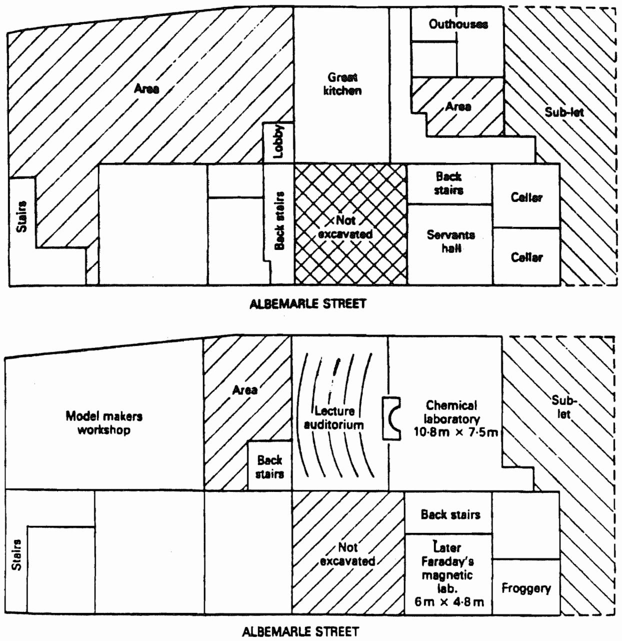


Figure 2. Floor plans of the basement of 23 Albemarle Street before and after being remodeled in 1799 (from reference 9).

houses (figures 2).

We have a rather good idea of what this original laboratory looked like, as William Brande included a view of it in the 1819 edition of his textbook, *A Manual of Chemistry* (figure 3), and Harriet Moore painted two water colors of it in 1852 - one showing Faraday at work by the large sandbath (figure 4) and the other showing Charles Anderson, a former Sergeant in the Royal Artillery, who had become Faraday's assistant in 1827 (figure 5) (8). The first of these water colors was later reproduced in the form of an etching as the frontispiece for the first volume of Bence Jones' 1869 biography of Faraday.

As can be seen in these views, one wall of the laboratory was open to a small lecture hall, roughly located on the site of the original kitchens and capable of accommodating about 120 persons. This was used by Brande to give his annual course of lectures on chemistry and physics (largely to local medical students) and was removed after Brande's retirement in 1852. It should not be confused with the large lecture hall on the ground floor (still extant) which is pictured in most representations of lectures at the Royal Institution, including the famous one of Faraday lecturing in front of the Prince Consort and his son, the Prince of Wales (see page 29 of this issue).

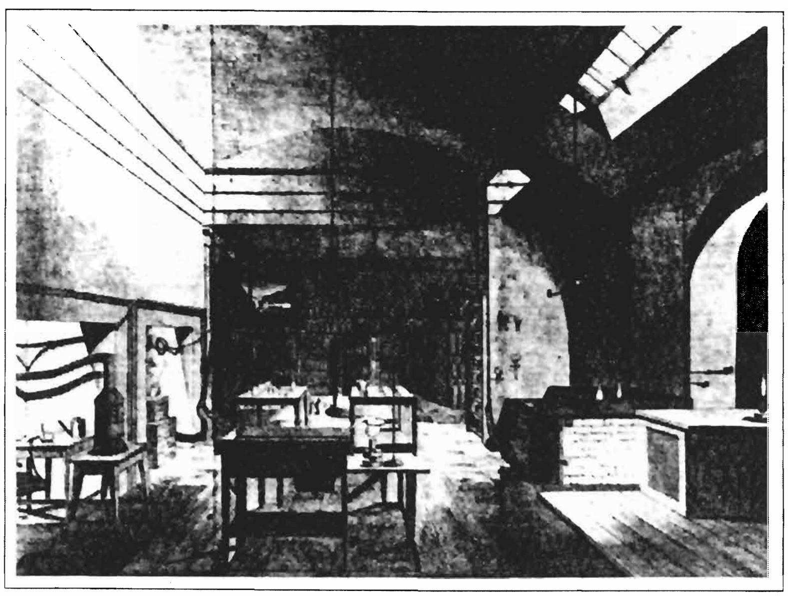


Figure 3. View of the chemical laboratory at the Royal Institution from the 1819 edition of William Brande's *Manual of Chemistry*.

The third important component of the basement laboratory complex was also documented by Harriet Moore in another water color done in 1852 (figure 6). This was the store room located in the former servant's quarters and entered through a door located under the back basement steps. By the time Moore did her painting, it had been converted into Faraday's magnet room. The entrance to this room is also clearly visible in the background in two of the views of the main laboratory (figures 3 and 4). For further details on the history of the laboratory, the reader should consult the superb study by Chilton and Coley (9).

This then was the environment in which Faraday received his training as a chemist and which he encapsulated in his volume on *Chemical Manipulation* in 1827, the year he turned 36. However, the precise reasons for writing the book are more difficult to come by. Agassi claims that it was based on lectures on practical laboratory technique which Faraday was required to give, in keeping with the Royal Institution's original educational mission, as part of his assigned duties as Superintendent of the Laboratory (10). These lectures were held in the small basement lecture hall adjacent to the laboratory to facilitate the presentation of practical demonstrations. Likewise, Bence Jones mentions a similar series of 12 lectures on laboratory technique that Faraday gave at the London Institution in Finsbury Circus (not to be confused with the Royal Institution) in 1827, the year the book appeared (11), and Sylvanus Thompson mentions a series of eight lectures on the same subject which Faraday gave at the Royal Institution in 1828 (12).

Based on this evidence, we may surmise that, whatever the exact details, the book was largely the product of Faraday's assigned teaching duties at the Royal Institution and not necessarily a labor of love. This supposition is further supported by Faraday's own testimony. Thus we know that he did not like lecturing on this subject, since he was later quite critical of the lecture series mentioned by Thompson (13):

The 8 lectures on the operations of the laboratory at the Royal Institution, April 1828, were not to my mind. There does not appear to be that opportunity of fixing the attention of the audience by a single clear, consistent and connected chain of reasoning which occurs when a principle or one particular application is made. I do not think the operations of the laboratory can be rendered useful and popular in lectures.

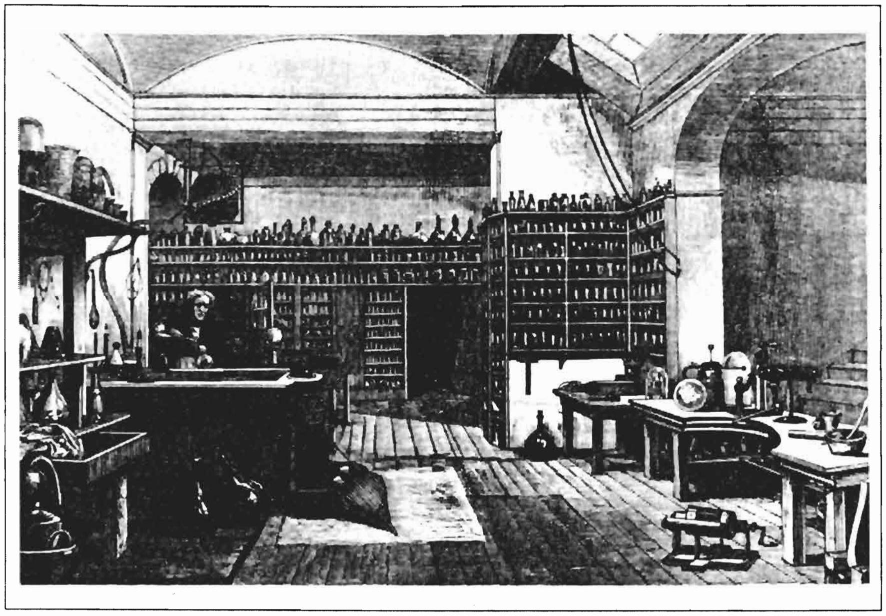


Figure 4. Faraday at work in the chemical laboratory of the Royal Institution. This etching is based on the 1852 water color by Harriet Moore. Note the lecture hall just visible through the arch to the right.

We also know that he did not particularly enjoy writing the book. At least this seems to be implied in a letter written to his friend, Edward Magrath, in July of 1826 (14):

I am writing away here & get on pretty well but it will be a more laborious job than I expected. I tire of writing day after day but have stuck to it pretty well this far.

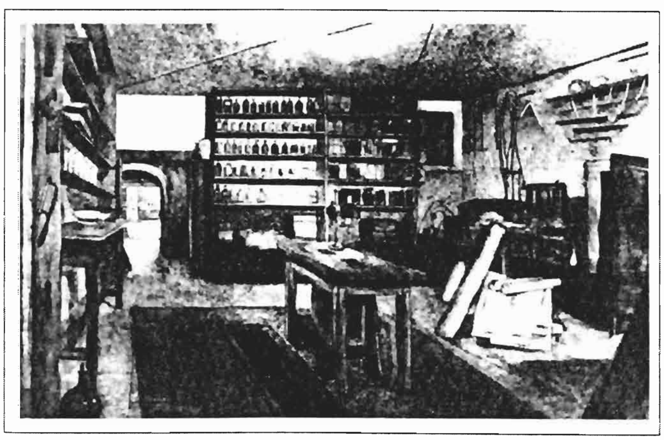


Figure 6. Harriet Moore's water color of Faraday's magnet room, formerly the storeroom. The entrance to this room is visible in both figure 3 and figure 4.

The completed volume (figure 7), published by William Phillips, the brother of Faraday's close friend, Richard Phillips, ran to 656 pages and, despite his apparent dislike for the subject, Faraday consented to produce a second edition three years later (15). This entailed a fair number of additions and deletions which, by balancing one another, kept the overall size of the book fairly constant (646 pages versus the original 656). A third edition was issued in 1842 but this was merely a reprint of the second edition with minor corrections (16). Though urged to do so, Faraday refused to produce further revisions (Table 2).

The first edition was also rapidly translated into French, appearing as a two-volume set in 1827 (17). This was followed by a German translation in 1828 (18) and a second German edition in 1832 (19). In 1831, the American physician and chemist, John Kearsley Mitchell, brought out an American edition of the second British edition. This contained a number

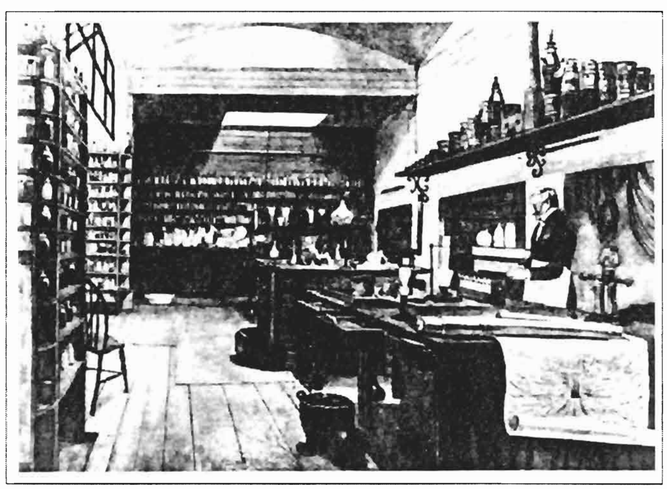


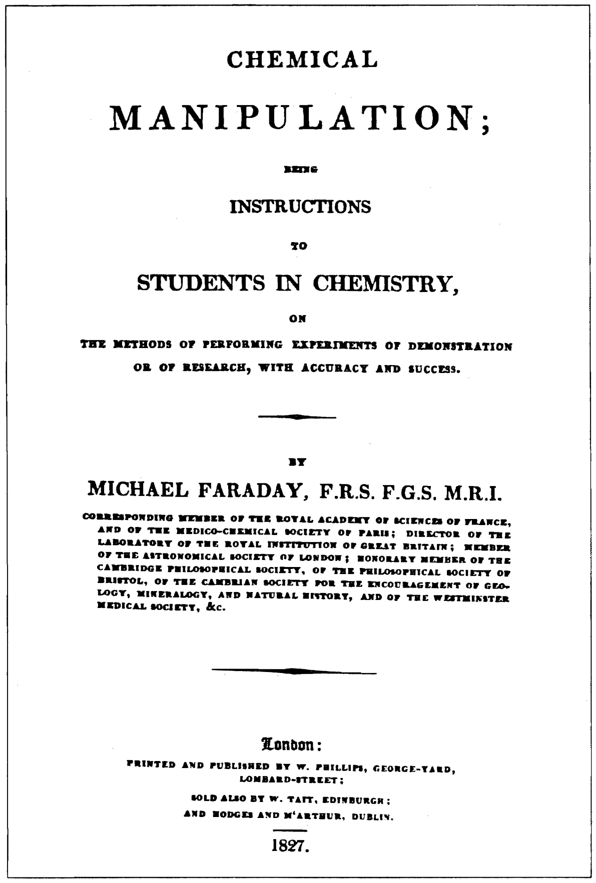
Figure 5. A second view by Harriet Moore of the chemical laboratory at the Royal Institution showing Faraday's assistant, Charles Anderson.

Table 2. Editions of *Chemical Manipulation.*

First British, London, 1827
First French, 2 vols., Paris, 1827
First German, Weimar, 1828
Second British, London, 1830
First American, Philadelphia, 1831
Second German, Weimar, 1832
Third British, London, 1842

of additions by Mitchell emphasizing American contributions (particularly those of Robert Hare) which Mitchell felt had been slighted by Faraday (20).

Chemical Manipulation was hardly the first book to deal with the subject of chemical apparatus and laboratory operations. Descriptions of equipment and common laboratory procedures, such as distillation, sublimation, filtration and digestion, had been an integral part of most chemical textbooks since the early 17th century. The prototype of this tradition was the famous *Alchymia* of Andreas Libavius, published in 1597 (21). Generally considered by historians to be the first explicitly didactic treatment of chemistry (actually of alchemy), the book contained more than 191 woodcuts (figure 8) devoted to the description of laboratory apparatus and laboratory design (22). The opinions of Libavius on the latter subject are perhaps best personified by his insistence that his plan for the ideal "Chemical Institute" include not only a chemical laboratory and library but a wine cellar.


CHEMICAL
MANIPULATION;
BEING
INSTRUCTIONS
TO
STUDENTS IN CHEMISTRY,
ON
THE METHODS OF PERFORMING EXPERIMENTS OF DEMONSTRATION OR OF RESEARCH, WITH ACCURACY AND SUCCESS.

BY
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London:
PRINTED AND PUBLISHED BY W. PHILLIPS, GEORGE-YARD, LOMBARD-STREET;
SOLD ALSO BY W. TAIT, EDINBURGH;
AND HODGES AND M'ARTHUR, DUBLIN.
1827.

Figure 7. Title page of the first edition of *Chemical Manipulation.*

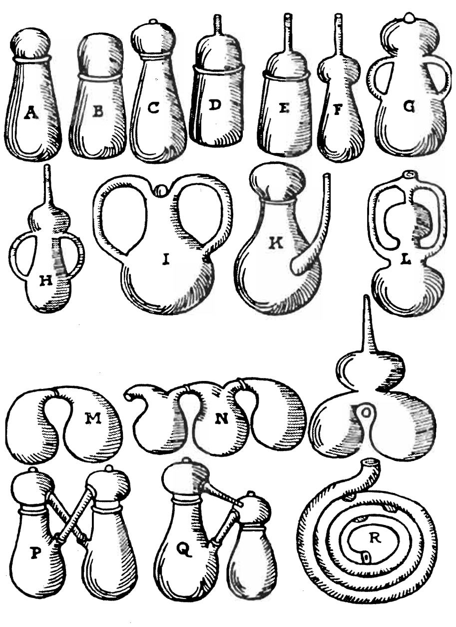


Figure 8. Typical late 16th-century chemical apparatus from Libavius' *Alchymia* of 1597.

An overview of typical chemical apparatus in the late 17th century can be had from the plate in figure 9, which is taken from Johann Becher's *Tripus hermeticus* of 1680 (23). Most modern chemists are rather surprised at how many items in this plate are still to be found in today's laboratory, including the lab coat and apron depicted in boxes 62 and 63.

Moving to the end of the 18th century, we encounter yet another famous example of this textbook tradition in the form of Lavoisier's *Traité élémentaire de chimie* of 1789 (24). Though generally ignored by historians, the last third of this book, covering roughly 176 pages in the English translation of 1790, was devoted to chemical apparatus and operations. Indeed, the famous plates at the end of the book, based on Madame Lavoisier's original sketches, largely refer to this final section (figure 10). Lavoisier organized this part of his text around different classes of operations, such as grinding, manipulation of gases, distillation, etc. - an approach, as we will see, identical to that taken by Faraday in his own book 38 years later.

By the 18th century, not only textbooks, but entire monographs were devoted to the subject of chemical apparatus and laboratory operations. Some representative 18th and early

19th-century examples, extracted from Bolton's massive *Bibliography of Chemistry*, are given in Table 3 (25). Several of these appear to have been organized along the lines of dictionaries or encyclopedias, whereas others are probably thinly disguised catalogs for apparatus dealers (unhappily Bolton doesn't provide enough information to make this distinction).

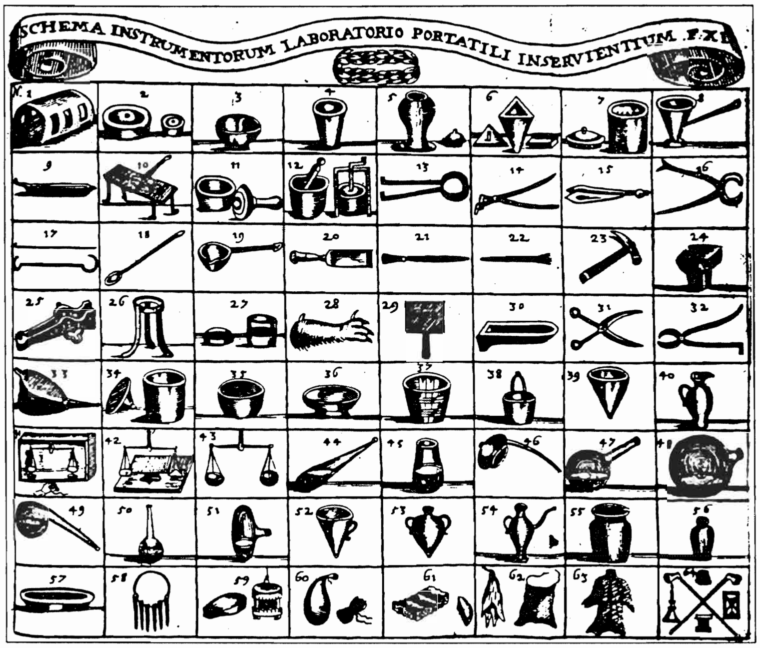


Figure 9. A plate of typical late 17th-century chemical apparatus from the 1680 edition of Becher's *Tripus hermeticus*.

As shown in Table 4, Faraday divided his own book into 24 sections (rather than chapters or lectures), organized, like Lavoisier's, around general classes of operations. As might be inferred from its length, the book is meticulous in its detail, often to the point of being tedious, but is redeemed by the fact that Faraday often draws on his personal experience and describes short cuts he has discovered, pitfalls he has learned to avoid, or inexpensive pieces of homemade apparatus he has devised. The book is illustrated by small line drawings, apparently done by Faraday himself, as they are identical in style to those appearing in his research papers. These are integrated within the printed text, rather than appearing as the beautifully etched plates typical of many of the book's predecessors.

Table 3. Some predecessors of *Chemical Manipulation.*

Year	Author	Title
1711	Hellwig	*Lexicon medio-chymicum ...*
1771	Hauboldus	*De usu instrumentorum physico-mathematicorum recte aestimando ...*
1783	Israel	*De chemicorum instrumentis mechanicis, errorum et dissensus fontibus ...*
1792	Geissler	*Beschreibung und Geschichte der neuesten und vorzüglichte Instrumente*
1821	Accum	*A Dictionary of the Apparatus and Instruments Employed in Operative and Experimental Chemistry, Exhibiting their Construction and the Method of Using Them to Greatest Advantage*
1824	Anon.	*An Explanatory Dictionary of the Apparatus and Instruments Employed in Various Operations of Philosophical and Experimental Chemistry*
1825	Anon.	*Eine Sammlung von Abbildungen und Beschreibungen der besten und neuesten Apparate zum Behuf der practischen und physikalischen Chemie*

It would be tedious and unnecessary to summarize each of Faraday's 24 topics, so I propose instead to single out a few instances where Faraday either described an interesting innovation or provided some insight into his personal philosophy of laboratory work, though it must be confessed that the latter instances merely reinforce what historians have already inferred from the study of his diary and published research papers.

The first topic of interest in this regard is Section 4 which deals with "Sources and Management of Heat". 17th- and 18th-century laboratory equipment was often quite bulky, being adapted from pharmaceutical apparatus designed to manufacture marketable quantities of products. Heating usually involved large brick ovens or smaller charcoal braziers. In fact, furnaces and ovens seemed to hold a particular fascination for earlier writers on chemical apparatus and Libavius devoted

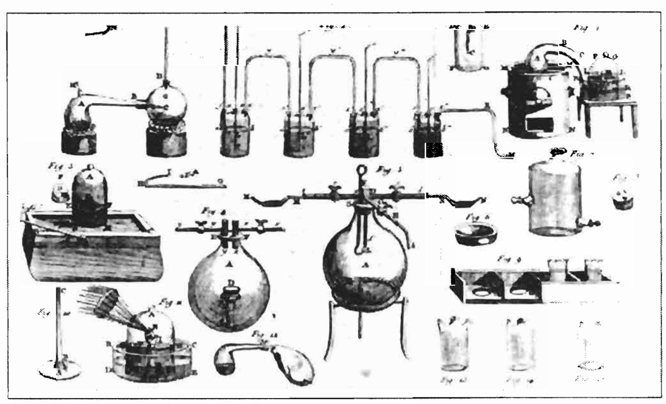


Figure 10. A plate of typical late 18th-century chemical apparatus from the 1789 edition of Lavoisier's *Traité élémentaire de chimie*.

Table 4. The contents of *Chemical Manipulation.*

1.	The Laboratory
2.	Balance; Weighing
3.	Measures; Measuring
4.	Sources and Management of Heat
5.	Comminution, Trituration, Mortars, Granulation
6.	Solution, Infusion, Digestion
7.	Distillation, Sublimation
8.	Precipitation
9.	Filtration, Decantation, Washing
10.	Crystallization
11.	Evaporation, Desiccation
12.	Coloured Tests, Neutralization
13.	Crucible Operations, Fusion, Reduction
14.	Furnace Tube Operations
15.	Pneumatic Manipulation or Management of Gases
16.	Tube Chemistry
17.	Electricity
18.	Lutes; Cements
19.	Bending, Blowing and Cutting of Glass
20.	Cleanliness and Cleansing
21.	General Rules for Young Experimenters
22.	Uses of Equivalents - Wollaston's Scale
23.	Miscellanea
24.	A Course of Inductive and Instructive Practices

114 of the 191 woodcuts in his 1597 treatise to this subject. Faraday, by contrast, described only six kinds of furnace, including the famous sandbath (figure 11) mentioned earlier. The reason for this, Faraday noted, was that (26):

... the character of chemical operations has changed so much as to render many of these contrivances useless, or of little importance ...

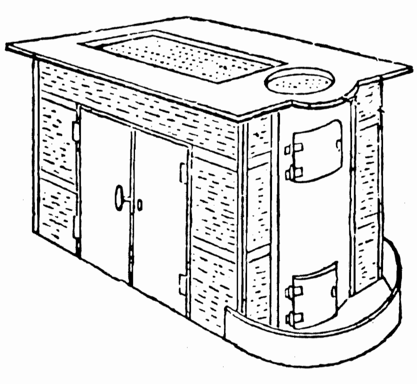


Figure 11. The large sandbath at the Royal Institution. This is visible in both figures 3 (right side) and figure 4 (left side).

The "character" to which Faraday was referring was the scale of the apparatus, which was becoming smaller. Indeed, as will become increasingly apparent, Faraday was strongly attracted to semi-microscale laboratory techniques and his favorite heat source was the spirit or alcohol lamp.

However, a second change in laboratory heat sources was also occurring about this time and is also prefigured in Faraday's book - the use of gas burners. In the first edition of *Chemical Manipulation* Faraday actually described his own experimental laboratory gas burner, consisting of an adjustable metal cone placed over the tip of a gas jet (figure 12). He further observed that (27):

The erection of gas works for public service is now so general in most large towns and in numerous private establishments, that the chemical gas lamp, which was a few years ago a mere curiosity, is now becoming a valuable and economical auxiliary to the establishment of the chemist.

These remarks, coupled with Faraday's description of his prototype burner, gave rise to a minor debate in the 1950s over the origins of the modern Bunsen burner. In his autobiography, the English chemist, Henry Roscoe, claimed that Bunsen was

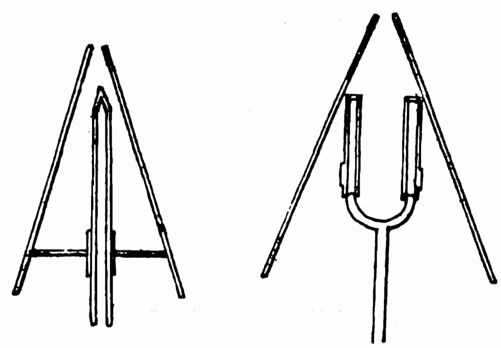


Figure 12. Two designs for laboratory gas burners proposed by Faraday in the first edition of *Chemical Manipulation.*

stimulated to develop his burner as a result of his attempts to improve a laboratory gas burner that Roscoe had brought with him to Heidelberg in 1853 and which was in common use in the chemical laboratories of London at the time (28). Many years later, Bunsen's biographer, Georg Lockemann, connecting Roscoe's comment with Faraday's description of his burner, concluded that the burner that Roscoe had brought from London was in fact Faraday's burner (29). However, in a detailed analysis published in 1955, Paul Dolch showed that the burner referred to by Roscoe was probably a "gauze burner" (figure 13) similar to the kind used by A. W. Hofmann and his students at the Royal College of Chemistry in the 1850s (30).

The term "gauze" referred to the fact that the gas-air mixture in these burners issued through a wire gauze before

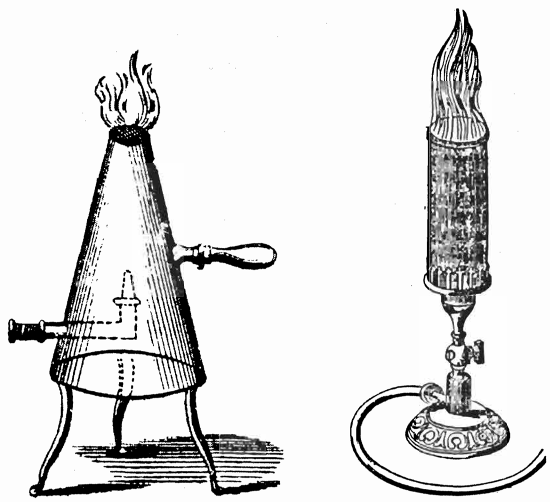


Figure 13. Gauze burners: left - Marcus (1855), right - Abel and Bloxam (1854) (46).

being lit, in order to prevent (in keeping with the principles of Davy's safety lamp) the danger of a flashback. Unfortunately, the resulting flame was very diffuse, not very hot (due to the large air to gas ratio used), and difficult to control. In addition, the gauze made the flame of the burner very uneven. Bunsen's innovation was to use gas under pressure which drew the air along with it so as to minimize the volume of the unlit gas-air mixture and thus minimize the danger of flashback while simultaneously giving an even, controllable flame that could be concentrated at a fixed point. In Faraday's design the gas and air mixed in the large volume under the metal cone and then issued from the opening at the top by virtue of their own "levity". No protective screen was used to prevent flashback. Though Faraday characterized his device as "promising" in the first edition of his book, he removed the description of his burner from the later editions, probably because he had discovered that it was dangerous.

A second item of interest in this section is Faraday's description of laboratory stands for supporting apparatus during the heating process. The most common commercial form at the time was the so-called "retort stand", consisting of rod with a circular base that was heavily weighted with iron or lead (figure 14). However, this had a tendency to tip over so it was occasionally necessary to use two stands (31):

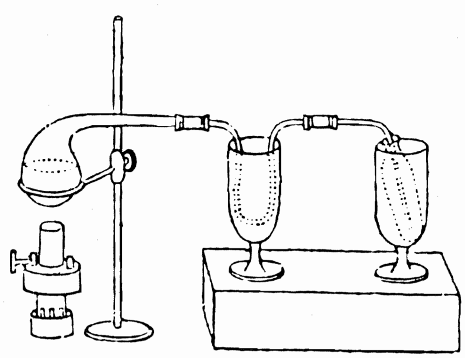


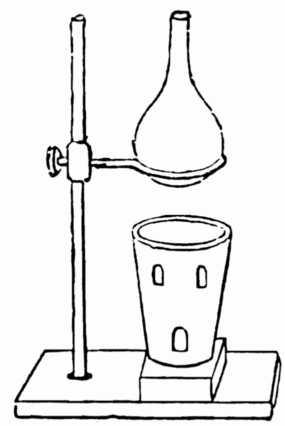
Figure 14. Drawing from *Chemical Manipulation* of a typical retort stand.

... on opposite sides, the ring of one being placed under the other, and the flask or basin on the upper most ... [or] it may now and then be necessary to put on a second ring in an opposite direction to the first, and to add weights for the purpose of equipoising the whole.

To remedy this defect, Faraday suggested an alternative (figure 15) (32):

It is much better to make the foot [of the stand] of stout board, about twelve inches in length, six inches in width, and an inch thick. The upright rod should be fixed about one inch and a half from one end of it, the lamp or furnace should be placed upon it, and the ring of course in a corresponding direction. Such an arrangement is perfectly steady, and cannot be overset by any weight which it is strong enough to bear.

Figure 15. A drawing from *Chemical Manipulation* of Faraday's homemade ring stand. The object sitting on the base is a homemade charcoal furnace constructed from a "blue pot".



What Faraday is describing in this passage is, of course, our modern day ring stand. However, since Faraday did not reference all of the details of his book, we cannot say with certainty that he was the first to suggest this solution. Nevertheless, his instructions for making the stand strongly imply that it was not commercially available at the time and that we are witnessing the approximate date of its birth. It is also interesting to note that, despite its apparent advantages, a fair amount of time was to pass before the ring stand completely displaced the older retort stand from the chemist's repertoire, and drawings of the undergraduate chemistry laboratory at the University of Cincinnati show that the students were still using the circular-based retort stands as late as 1890.

Perhaps the most innovative topic in *Chemical Manipulation*, for those interested in the evolution of laboratory equip-

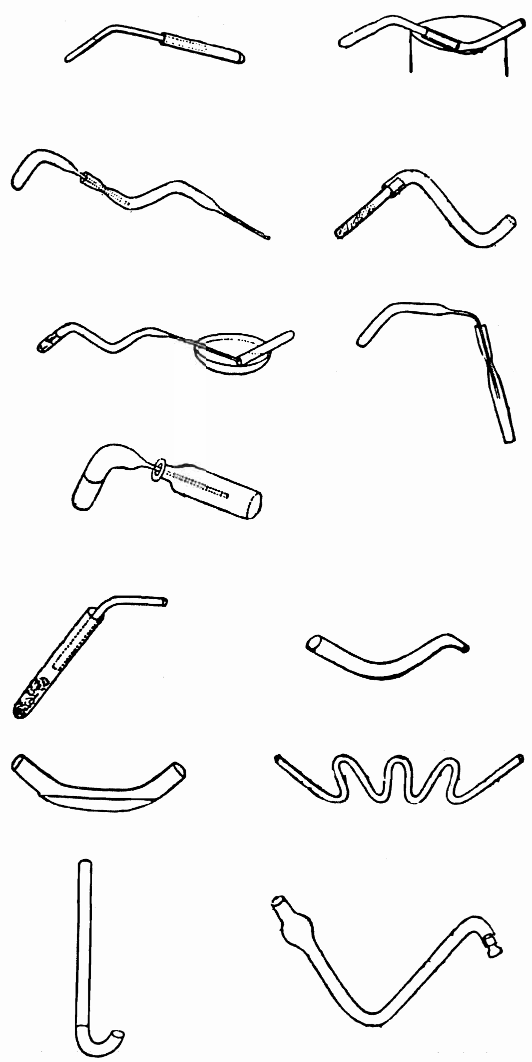


Figure 16. Examples of Faraday's tube apparatus for microscale laboratory work.

ment, is found in Section 16, which deals with what Faraday called "tube chemistry". In this section, Faraday's fascination with the miniaturization of chemical apparatus reveals itself in an elaborate array of devices - all of which are made from bent glass tubes - for heating, distilling, subliming and extracting small quantities of materials. Most of Faraday's drawings of these devices are collected together in figure 16. His use of sealed bent-glass tubes in the liquefaction of chlorine also falls into this category as does his suggestion that common test reactions using liquid solutions could be conveniently done in small glass tubes (33):

... closed at one end, of all diameters and lengths from one inch to five or six ... These tubes answer all the purposes of test-glasses, and in the small way precipitates are made, preserved, and washed very conveniently in them ... Those who frequently use them will find a tube-rack very convenient. It may be formed of two boards, one supported two or three inches above the other, and the upper pierced with holes to admit the tubes. Or a very simple one may be made of a board a foot in length and six inches in width, having a piece of coarse wire trellis about three inches above it, supported at the corners by upright pieces of strong wire. The apertures in the trellis serve to receive and retain the tubes.

Since one conducted qualitative "tests" in these tubes using characteristic color or precipitation reactions, they eventually became known as "test tubes" and Faraday's tube rack became the "test tube rack". On the other hand, as suggested by its shape (figure 17), the larger "test glass" mentioned by Faraday probably evolved from the wine glass. This is certainly implied by my study of the evolution of the chemistry set as I have found that the manuals to 18th-century chemistry sets make no mention of test tubes but invariably recommend that the owner use wine glasses to conduct test reactions which required no heating (34). As with the ring stand, we cannot definitely assert that Faraday was the inventor of the test tube and the test tube rack, but again the tenor of his descriptions strongly suggests that neither was commercially available at time and that one is witnessing the incipient stages of their introduction.

Faraday was not alone in his interest in semi-micro laboratory techniques. His fascination was shared by his older contemporary, William Hyde Wollaston, and in the latter's case was the subject of a famous anecdote (35). It is reported that on one occasion Wollaston was inopportuned by an overzealous visitor wishing to see the famous laboratory in which Wollaston had made so many important discoveries. At first Wollaston demurred but, when pressed by the visitor, finally ordered his footman to bring the laboratory out on a serving tray. Some additional predecessors and successors in this tradition, again extracted from Bolton, include a 1735 monograph by Shaw and Hauksbee entited *An Essay for Introducing a Portable Laboratory* and a 1830 volume by Schuster on *Kleiner chemischer Apparat.*

As John Stock's account in this issue shows, many of Faraday's qualitative experiments in electrochemistry were also carried out on a semi-micro scale using strips of paper impregnated with chemicals or small amounts of salts fused on pieces of glass or platinum by means of an alcohol lamp (36).

Insights into Faraday's philosophy of laboratory work may

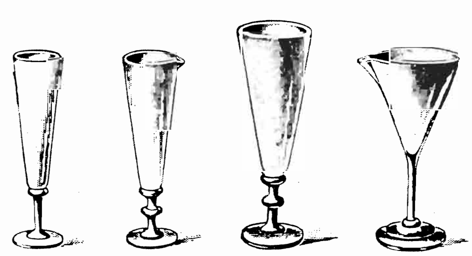


Figure 17. Test glasses.

be found in Section 20 on "Cleanliness and Cleansing", and especially in Section 21 on "Rules for Young Experimenters". In the latter section he emphasizes the necessity of making detailed notes in a timely fashion, a habit well documented in his diaries and other laboratory notebooks (37):

The laboratory note book, intended to receive the account of the results of experiments, should always be at hand, as should also pen and ink. All the results worthy of record should be entered at the time the experiments are made, whilst the things themselves are under the eye, and can be re-examined if doubt or difficulty arise. The practice of delaying to note until the end of a train of experiments or to the conclusion of the day, is a bad one, as it then becomes difficult to accurately remember the succession of events. There is a probability also that some important point which may suggest itself during the writing, cannot then be ascertained by reference to experiment, because of its occurrence to the mind at too late a period.

Faraday also comments on the virtues of a laboratory storage area (38):

Besides the working place another unconnected with the busy part of the laboratory, should be appointed, from which nothing is to be moved without the experimenter's direction. There are many occasions on which experiments or solutions are to be placed aside for a week or two, to be again resumed. These should be labelled, and put into a place which, from previous appointment, is considered as containing nothing that may be disturbed. In this way the experimenter will often avoid the disagreeable circumstance, of finding that what he intended to reserve for future examination, has been dismissed to the sink or the dust-hole.

and he records an even more important consequence of this long-term storage policy in the form of a quote from the 18th-century French chemist, Pierre Macquer (39):

When new researches and enquiries are made, the mixtures, results, and products of all the operations ought to be kept a long time well ticketed and noted. It frequently happens that at the end of some time these things present very singular phenomena, which would never have been suspected. There are many beautiful discoveries in chemistry which were made in this manner, and certainly a much greater number which have been lost because the products have been thrown away too hastily, or because they could not be recognized after the changes which happened to them.

On the other hand, the spirit of Section 20 is best conveyed by the title of one of the subsections, "The Sink and Its Accompaniments". In short, this section suggests a kind of prissy obsessiveness on Faraday's part that seems to be modeled more on the personality of Wollaston than of Davy. Indeed, the latter's rather slapdash laboratory technique may well have served as a compelling negative example for Faraday, who summarized his own philosophy on this subject at the end of Section 21 by again quoting Macquer (40):

Table 5. Some successors to *Chemical Manipulation.*

Date	Author	Title
1831	Berzelius	*Chemische Operationen und Gerätschaften*
1844	Robierre	*Traité des manipulations chimique*
1857	Williams	*Handbook of Chemical Manipulation*
1857	Morfit & Morfit	*Chemical and Pharmaceutical Manipulations*
1882	Rivière & Rivière	*Traité de manipulations de chimie*
1882	Weyde	*Anleitung zur Herstellung von physikalischen und chemischen Apparaten*
1883	Wisser	*Chemical Manipulations*
1885	de Walque	*Manuel de manipulations chimique ou de chimie opératoire*

These employments [i.e. maintaining laboratory cleanliness] are capable of cooling and retarding the progress of genius, and are tedious and disgusting; but they are nevertheless necessary ... We cannot depend too much on ourselves in these matters, however minute, on account of their consequences.

By contrast, one suspects that Davy was never one to patiently tolerate such impediments to the "progress of genius".

The concluding section of *Chemical Manipulation* outlines a course of practical laboratory exercises based on the book's contents and presumably gives us some of the flavor of the course taught by Faraday at the Royal Institution which had given rise to the book in the first place.

Just as Faraday's book was not the first to deal with the subject of chemical manipulation, so it wasn't the last. The titles of some of its successors, again extracted largely from Bolton, are listed in Table 5. The absence of later German, French and American editions of Faraday's book strongly suggests that it was rapidly displaced by these successors. Though the book received favorable reviews when it first appeared, most of these, as Ross has shown (41), were written by Faraday's close friends and one gets the impression that, while the book was respected (in large part because of Faraday's reputation as a research scientist), it was not as popular as some of its successors, many of which were lavishly illustrated (figure 18). This supposition is further supported by the fact that few if any tributes to the book are to be found in the autobiographical writings of chemists who received their education during this period. Indeed, the only mention I have been able to locate occurs in the autobiography of Sir Oliver Lodge, who was a physicist rather than a chemist, and by the time Lodge stumbled on the book, as a young boy in the 1860s, much of it was rather outdated (42):

... at some stage I studied his [Faraday's] book on *Chemical Manipulation*, admiring the deftness with which he evidently manipulated his apparatus, and the ingenuity which he showed in constructing all manner of little appliances that then had to be made on the spot, though now they can bought in a shop. Even such a thing as india-rubber tubing for joining quilled glass together was not then available, or was only then beginning. Faraday had to wrap his junction-pieces around the tubes and tie them with tread. When I tried to do chemical manipulations at home at odd times, Faraday's example represented an ideal which I never even approached.

By the 1830s the tradition of single-volume speciality books on chemical manipulation had largely displaced the less detailed accounts of chemical apparatus and operations typical of 17th and 18th-century textbooks and, as can be seen from the absence of later dates in Table 5, this tradition, in turn, began to decline in the 1880s. The speciality volume on chemical manipulation had originally been designed to train chemical amateurs in a period when formal university-level laboratory training was largely nonexistent, and in Faraday's case there is a certain element of the Samuel Smiles' "Self-Help" mentality which reflects Faraday's own route into science. By the 1880s, however, the situation had changed. Laboratory training was now part of an integrated course of instruction and the contents of the average manipulation book were now spread throughout the curriculum, from the introductory Freshman laboratory course, on the one hand, to advanced laboratory courses in organic and physical chemistry, on the other, though the Germans continued to generate multi-volume "Handbuch der Arbeitsmethoden" in advanced areas such as organic (43), inorganic (44) and biochemistry (45) well into the second decade of this century. With this change, the tradition of the single-volume introductory chemical manipulation book splintered into a profusion of laboratory manuals.

Our present day knowledge of the pervasive impact of Faraday's scientific legacy makes it very difficult to assess objectively the true significance of *Chemical Manipulation*. There is a tendency to hero worship in science as in other areas of human culture, and the compulsion to retrospectively clothe every aspect of a famous scientist's activities with the gloss of genius is a strong one. There is no doubt that *Chemical Manipulation* is a useful book that gives us valuable insight into the details of chemical laboratory practice in the early 19th century. But it is also certain that, however unbounded our admiration for Faraday, *Chemical Manipulation* is not, and was never intended to be, a classic of Western scientific thought.

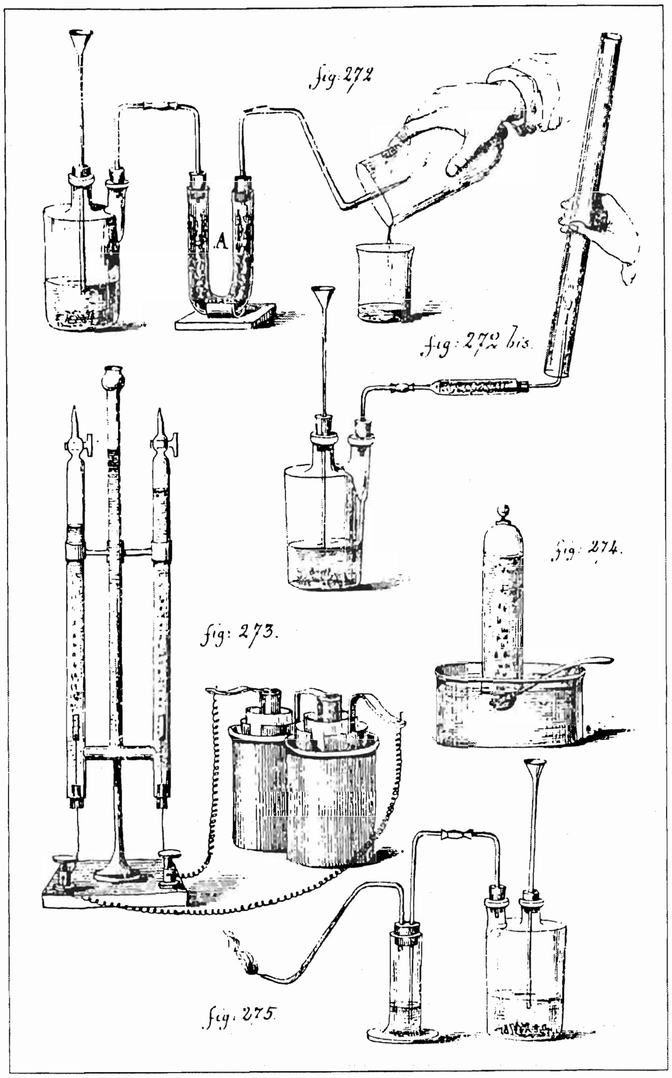


Figure 18. A typical plate from the 1882 edition of Rivière and Rivière's *Traité de manipulations de chimie*.

References and Notes

1. M. Faraday, *Chemical Manipulation; Being Instructions to Students in Chemistry on the Methods of Performing Experiments of Demonstration or of Research with Accuracy and Success*, W. Phillips, London, 1827. An earlier discussion of this book has been given by F. Bei, "Il *Chemical Manipulation* nell'opera di Michael Faraday", in F. Calascibetta and E. Torracca, eds., *Atti del II Convegno Nazionale di Storia e Fondamenti della Chimica*, Accademia Nazonale delle Scienze, Rome, 1988, pp. 215-223.

2. T. Martin, ed., *Faraday's Diary*, 7 Vols. + Index, Bell, London, 1932-1934.

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

4. M. Faraday, *Experimental Researches in Chemistry and Physics*, Taylor and Francis, London, 1859.

5. M. Faraday, *A Course of Six Lectures on the Forces of Matter and their Relation to Each Other*, Harper, New York, NY, 1860, edited by W. Crookes.

6. M. Faraday, *A Course of Six Lectures on the Chemical*

History of a Candle, Harper, New York, NY, 1861, edited by W. Crookes.

7. M. Faraday, *The Subject Matter of a Course of Six Lectures on the Non-Metallic Elements*, Longman, Brown, Green and Longmans, London, 1853, edited by J. Scoffern.

8. There seems to an incredible amount of variation among Faraday's biographers as to some of these dates. Davy's resignation as Professor of Chemistry is variously reported as 1812 and 1813. Based on the biography by T. E. Thorpe (*Humphry Davy, Poet and Philosopher*, Macmillan, New York, 1896, pp. 175-176), who quotes the minute book of the Royal Institution, I have chosen the latter. Likewise, 1813 and 1815 are variously given as the date when Brande was appointed to succeed Davy. Based on the study of Chilton and Coley (9), I have chosen 1813. Finally, Thompson (12) reports that Faraday was named "Superintendent of the House and Laboratory" in May of 1821 (p. 49), whereas H. Bence Jones (11) states that Faraday was appointed as "Director of the Laboratory" in February of 1825 (p. 346). I am assuming that these are two separate positions.

9. D. Chilton and N. G. Coley, "The Laboratories of the Royal Institution in the Nineteenth Century", *Ambix*, **1980**, *27*, 177-203.

10. J. Agassi, *Faraday as a Natural Philosopher*, University of Chicago, Chicago, 1971, p. 10. Dr. Derek Davenport has informed me that the manuscript lecture notes to the introductory chemistry course given by Brande and Faraday are still in existence in the library of the Royal Institution.

11. H. Bence Jones, *The Life and Letters of Faraday*, Vol. 1, 2nd ed., Longmans, Green, London, 1870, p. 354.

12. S. P. Thompson, *Michael Faraday, His Life and Work*, Macmillan, New York, 1898, p. 233-234, p. 101.

13. *Ibid.*, p. 233.

14. L. P. Williams, R. FitzGerald, and O. Stallybrass, eds., *The Selected Correspondence of Michael Faraday*, Vol. 1., Cambridge University Press, Cambridge, 1971, Letter 85, pp. 165-166.

15. M. Faraday, *Chemical Manipulation; Being Instructions to Students in Chemistry on the Methods of Performing Experiments of Demonstration or of Research with Accuracy and Success*, 2nd ed., Murray, London, 1830.

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21. A. Libavius, *Die Alchemie des Andreas Libavius*, Verlag Chemie, Weinheim, 1964. The first German translation of the 1597 Latin edition.

22. O. Hannaway, *The Chemists and the Word. The Didactic Origins of Chemistry*, Johns Hopkins, Baltimore, MD, 1975.

23. Discussed in F. Ferchl and A. Süssenguth, *A Pictorial History of Chemistry*, Heinemann, London, 1939. pp. 117-119. Süssenguth was the founder and director of the Chemical Section of the Deutsches Museum from 1905-1935 and this book, despite its misleading title, deals largely with the evolution of chemical apparatus. For a study of 18th century laboratory practice, see also J. Eklund, *The Incompleat Chymist*, Smithsonian, Washington, DC, 1975.

24. A. Lavoisier, *Elements of Chemistry*, Creech, Edinburgh, 1790, Part III and plates.

25. H. Bolton, *A Select Bibliography of Chemistry, 1492-1892*, Smithsonian, Washington, DC, 1893 and the *First Supplement*, Smithsonian, Washington, DC, 1899.

26. Reference 1, p. 84.

27. *Ibid.*, p. 107.

28. H. Roscoe, *Ein Leben der Arbeit*, Akademische Verlag, Leipzig, 1919, pp. 41-42.

29. G. Lockemann, *Robert Wilhelm Bunsen*, Wissenschaftliche Verlag, Stuttgart, 1949, pp. 123-124.

30. P. Dolch, "100 Jahre Bunsenbrenner - einer chemigeschichtlich Studie", *Österreich. Chem. Zeit.*, **1955**, *56*, 277-285. For a more accessible but much less thorough account, see M. Kohn, "Remarks on the History of Laboratory Burners", *J. Chem. Educ.*, **1950**, *27*, 514-516. Also of interest is T. E. Thorpe, "On the Theory of the Bunsen Lamp", *J. Chem. Soc.*, **1877**, *31*, 627-642. The author would like to thank Dr. Derek Davenport for bringing the last two references to his attention.

31. Reference 1, p. 178.

32. *Ibid.*, p. 178.

33. *Ibid.*, p. 391-392.

34. I am referring specifically to the set designed by the German chemist, Johann F. A. Göttling, (1789) and the set designed by the American chemist, James Woodhouse (1797). See W. B. Jensen, "Two Centuries of the Chemistry Set", to be published.

35. See F. S. Taylor, *An Illustated History of Science*, Praeger, New York, NY, 1968, p. 164.

36. J. T. Stock, "The Pathway to the Laws of Electrolysis", *Bull. Hist. Chem.*, **1991**, *11*, 86-92.

37. Reference 1, p. 546.

38. *Ibid.*, p. 549.

39. *Ibid.*, p. 551.

40. *Ibid.*, pp. 550-551.

41. S. Ross, "The Chemical Manipulator", *Bull. Hist. Chem.*, **1991**, *11*, 76-79.

42. O. Lodge, *Past Years, An Autobiography*, Scribners, New York, NY, 1932, p. 75.

43. J. Houben, *Die Methoden der organischen Chemie. Ein Handbuch für die Arbeiten in Laboratorium*, 3rd ed., 4 Vols., Theime, Leipzig, 1925.

44. A. Stähler, *Handbuch der Arbeitsmethoden in der anorganischen Chemie*, 5 Vols., Veit, Leipzig, 1913-1914.

45. E. Abderhalden, *Handbuch der Biochemischen Arbeitsmethoden*, 10 Vols., Uban and Schwarzenberg, Berlin, 1910-1919.

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THE CHEMICAL MANIPULATOR

Sydney Ross, Rensselaer Polytechnic Institute

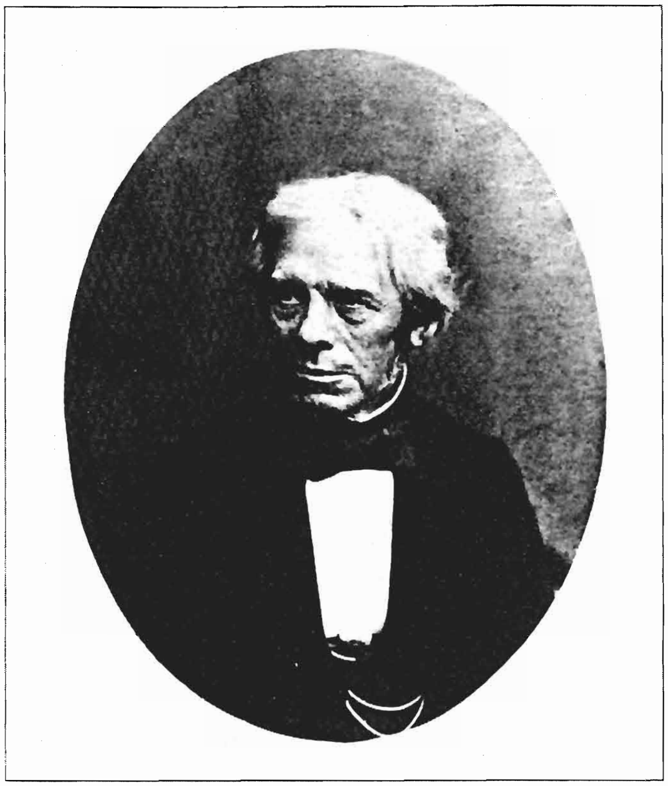
Faraday's *Chemical Manipulation* was published in 1827 (1). It was Faraday's first book; no doubt after the tedious work of its preparation he was proud to see it in print. He took a copy, divided it into three parts, placed a large (quarto) sheet of writing paper between each page, and rebound it into three volumes.

The binding was examined by Mrs. Fiona Anderson, bookbinder, who furnished me with the following technical description (2):

This quarto book has been made up from an octavo. The original book has been split into single sheets and these are tipped in between blank sheets. The blank sheets are made up from folio sheets of laid paper folded down the middle to form quarto sheets. The sections are made up from four folio sheets (making a quarto gathering of eight) and four printed pages. B = blank; P = printed. B/P/BB/P/BB/P/BB/P/B. The binding is 3/4 roan with green French shell marbled sides. Headbands are flat-silk over parchment strips. The sewing is on six sawn-in cords.

The three quarto volumes of Faraday's interleaved copy of *Chemical Manipulation* were part of the Honeyman collection of early works of science, which was sold at auction by Sotheby on 1983, when I acquired it. Sir George Porter (now Lord Porter), at that time the Director of the Royal Institution, told me that the Royal Institit
ion was the runner-up at the bidding, and that my final bid was successful only because it exceeded the limit he had assigned for the purchase. "But", he said, "I don't mind too much for after all we have the whole of Faraday's original manuscript of the book, so perhaps we can do without his later emendations." With such a wealth of Faraday material in its archives, the Royal Institution should not grudge a few crumbs to private collectors.

Clearly Faraday's intention was to use the interleaves as the repository of additional material, although most of it never appeared in any later edition. As it turned out his interest in the subject was destined to make way for more pressing concerns after his discovery of electromagnetic induction in 1831, when he became preoccupied with his experimental researches in electricity. So the ambitious provision of so many blank pages, if intended for the revision of *Chemical Manipulation*, had only limited fulfillment. A second and revised edition was published in 1830, but it contained only a few of the revisions included in Faraday's interleaved copy. Most of these notes are references to periodicals containing information on topics already discussed in the printed text, which often lacked acknowledgments to the source of the information, or they are references to articles that brought that information up to a later date.



Faraday in later life.

Obviously such an undertaking is open-ended and once commenced has to be kept up until the time when a new edition is in active preparation. After a promising start in which he probably copied his outstanding notes on to the interleaves, he seems to have abandoned the project (if he ever entertained it) of providing his readers with a complete set of references, for only a few of those written on his interleaves were included in later editions. The interleaves contain some 575 references. Far from including this large store of references in a later edition, Faraday actually reduced the number originally present from 78 foot-noted references in the first edition to 57 in the third. In the introduction, Faraday declared that his book was principally for beginners, and that he disclaimed any scientific character for it. He may well have thought that it was inappropriate to load it with references. In that case, the collection on the interleaves would have been entirely for his own private purposes, in a convenient form and location, perhaps in case he was ever challenged to back up a statement in his text with an external authority.

Faraday's intention to create a bibliography of articles on the various topics of chemical manipulation may well have been inspired by the example of Thomas Young, who in his *Lectures on Natural Philosophy* (1807) included a comprehensive bibliography of every topic treated in his lectures (3). These lectures had been delivered as a course at the Royal Institution, and so created a precedent that Faraday, who as a serious student appreciated such helps to self-study, would have considered worth emulating.

At the end of his book Faraday listed the reviews that it had received in various periodicals. Before giving examples of his other annotations on the interleaves, it will help to appreciate the character of *Chemical Manipulation* to cite some of these reviews. They were generally favorable, but that is not altogether surprising since, although published anonymously as was then the practice, internal evidence shows that they were written by personal friends of the author.

The *Philosophical Magazine* devoted eight pages to a review of *Chemical Manipulation*, but a large part was taken up by a long extract from the book itself, dealing with what Faraday called "tube chemistry," that is, the carrying out of chemical and physical operations in apparatus formed partly or altogether of glass tubing of about half an inch or less in diameter (4). Chemical operations can thus be carried out with great economy of materials; and indeed Faraday's tube chemistry is much like the microchemistry taught in our academic laboratories today. For physical operations, such as successive distillations or rectifications, a tube with several bends allows the volatile liquid to be evaporated in one U-bend by the application of heat, and condensed in the adjoining U-bend by the application of cold; and then by repeating the evaporations and condensations to drive each succeeding distillate to the next U-bend and finally to its last receiver. This and other physical operations in tubes of appropriate design are recounted in meticulous detail and with the utmost clarity by Faraday.

The anonymous reviewer was evidently himself a chemist. He wrote (5):

> In p. 172 excellent directions are given on the very simple subject of glass stirrers, to which however we would add one hint more; - they are extremely apt to roll from the experiment-table. Now this is easily prevented by softening them near the centre in the spirit-lamp, and then very slightly bending them.

Faraday included this useful tip in his second edition, and in so doing acknowledged his indebtedness to Mr. Phillips, thus incidentally identifying the reviewer as Richard Phillips (1778-1851), an old friend of Faraday and one of the editors of the *Philosophical Magazine*. Richard Phillips and Faraday had been members of an association of young men who sought mutual self-improvement by composing and reading essays to one another and then submitting to group criticism. Faraday was always grateful for and heedful of such criticism, which he deemed to be beneficial; but the practice of extending criticism to others on their invitation to do so tends to breed nit-picking and pedantry. Some trace of this disposition is apparent in Phillips' next remarks (6):

> There are however, two passages which contain (as we think) a figure of speech which we will not name, but which the author will probably guess at, and in a future edition alter.

The phrasing of this sentence suggests that Phillips had already conversed with Faraday and had rallied him in a friendly way on his stylistic misdemeanors. Phillips continued (7):

> The first [of these] occurs in the Introduction, p. iii. "There are also two parts in an experiment; first, it has to be devised" &c. Now as a thing does not exist until it is devised, we do not see how the devising of it is a "part" of it. In p. 174 we are informed that "the simplest step in the application of heat is to obtain a solution saturated when cold." To us it appears on the contrary that the obtaining of a cold solution is no step at all in the application of heat. - We observe also that the author uses the term "lute" in two and very different senses: first, in its proper sense, that of stopping the orifice between a retort and receiver; and secondly, in that of coating. Now *luting* a retort and *coating* one are two different operations.

Phillips goes on to take Faraday to task for crediting Lavoisier with the first use of oxygen as a means of increasing heat, whereas, as he shows by citing Priestley, the credit really belongs to the latter. But despite these strictures, Phillips concludes (8):

> After a careful perusal of the work, we strenuously recommend it, as containing the most complete and excellent instructions for conducting chemical experiments: there are few persons, however great their experience, who may not gain information in many important particulars; and, for ourselves, we beg, most unequivocally to acknowledge that we have acquired many useful and important hints, on subjects even of every-day occurrence.

The anonymous reviewer's praise of *Chemical Manipulation* had every appearance of a disinterested judgment, especially because of his previous rebukes of minor stylistic errors. But this was a skillful ruse to simulate impartiality: actually Richard Phillips was not entirely indifferent to the success of the book for, besides his close friendship with the author, he was also the younger brother of the publisher, William Phillips (1775-1828). When William Phillips died about a year later, the remaining unsold sheets were acquired by the publisher, John Murray, who issued them with a new title page. Murray also published Faraday's revised second and third editions.

Another review of *Chemical Manipulation* appeared in the *London Literary Gazette and Journal of the Belles Lettres, Arts, Sciences, &c* (9). The reviewer made a point of "calling the attention of our *juvenile* readers to the very valuable work before us". He pointed to the rapid advances being made in Britain in the physical sciences and the incalculable advantages that must result; but the sole advantage that he chose to mention was from "the avenues to science being thrown open to men in the ordinary and lower ranks of life". This comment is rather surprising since there are many other more obvious advantages of the rapid progress of science; but taken in conjunction with our knowledge of Faraday's humble social origins, it suggests that the reviewer was equally well aware of them, and therefore was either a personal friend or an acquaintance of Faraday. The reviewer returns to the same theme further on. That man may be said to confer a benefit on his country, he wrote, who places the various manipulations of chemical research "within the grasp of the middling (and may we add, the most industrious) classes of society". Promoting the same theme is the reviewer's commendation of Faraday for furnishing the chemical laboratory on the most economical scale. At the time great efforts were afoot to extend education beyond the privileged classes, as witness the various kinds of Mechanics' Magazines of the 1820s, the Society for the Dissemination of Useful Knowledge, the *Penny Cyclopaedia*, the journal *The Mechanic and Chemist* (1836-1842), the founding of the University of London, and of Mechanics' Institutes throughout the kingdom. While this movement almost certainly gave some occasion for the reviewer's remarks, yet his pointed use of Faraday's book to make mention of the opportunity for self-advancement provided by learning chemical manipulations suggests rather strongly that the example of its author was also in the reviewer's mind.

Table 1. Faraday's annotations for Section XVII: *Electricity*, §2. Voltaic Pile.

Pepys Galvanometer	Phil Mag	X. 38. XV. 94
Pepys Voltaic apparatus	Quar Journ	I. 193
Hare's Voltaic trough	Quar Journ	XVII. 378
Nobili's Galvanometer	Quar Journ	XX. 170
Wollaston's Elementary galvanic battery	Thom Annals	VI. 209
Power of Electrical batteries	Nich Journ 4to	II. 527
Hare's deflagrator	Phillips Annals	V. 129
Gold leaf test of Electro magnetism	Phillips Annals	VIII. 321
Poggendorffs galvanometer	Edin Phil Journ	V. 122
Marsh's apparatus	Trans Soc Arts	XLI. 47
Sturgeon's apparatus	Trans Soc Arts	XLIII. 37
Wilkinson's Galvanic trough	Phil Mag	XXIX. 243
Children's voltaic pile	Phil Mag	XXXIV. 26
Improved galvanic trough	Phil Mag	XLIV. 15
Children's voltaic battery	Phil Trans	CV. 363
Pepys voltaic conductor	Phil Mag	XLI. 15

Table 2. Faraday's annotations for Section XVIII: *Lutes - Cements*.

Lute	Saussure Essais sur l'Hygrométrie	§83
Good paste	Quar Jour	XV. 141
Cement	Nich Jour 4to	I. 355
Cement	Edin Phil Jour	XIII. 199
Cement	Tech Rep	I. 373. 412
Cement	Ure Dict	311
Cement	Phil Mag	XIV. 117
Cement for iron vessels	Phil Mag	IV. 216
Cement to exclude damp	Trans Soc Arts	XXIV. 81
Coating or luting	Ure Dicty	345
Lutes	Henry Chem	I. 9
Lutes	Tech Rep	II. 18
Lutes	Ure Dict	567
Lutes	Nich Jour	VI. 140
	or Phil Mag	XVI. 236
Lutes	Lavoisier	468
Lutes	Thenard Chem	IV. 294
Lutes and Cements	Aikins Dicty	I. 273
Preservation of bladder	Phillips Annals	I. 426

A third review of *Chemical Manipulation* appeared in the *Quarterly Journal of the Arts and Sciences* (10). This periodical was in effect the house organ of the Royal Institution, and was edited by William Brande with the assistance of Faraday. The review was long and laudatory, which was only to be expected. The reviewer was probably William Brande himself.

Let us return now to the contents of Faraday's interleaved copy of *Chemical Manipulation*. Tables 1 and 2 give some sample pages that show the range of topics that were included in the book. As can be seen, the references in the interleaved copy are grouped according to each Section of the text of *Chemical Manipulation*. Many of them refer to the statements in the text that lacked reference; others would have extended the information included in each Section. In the revised second and third editions, however, Faraday was at some pains to keep the book to a moderate size. In the Preface to the second edition, he wrote:

> I found it impossible to introduce any additional matter without displacing that which was more important; nor do I anticipate that I shall incur blame by withholding that which has not been tried, and, in my own judgment, is of less moment than that which experience has proved to be useful and desirable.

In consequence of this decision, many of the references in the interleaved copy bear no correspondence to anything in the text of the revised editions.

Yet Faraday continued to add references between the dates of the second and third editions, that is, between 1830 and 1842. One of these is in Section XIX, "Bending, Bowing and Cutting of Glass", which begins on page 522. It is listed as "Grinding of Glass" and refers to Silliman's Journal, **XVII,** page 345. The reference is to a paper by Elisha Mitchell, Professor of Chemistry, Mineralogy, &c. at the University of Carolina, entitled "On a Substitute for Welther's Tube of Safety, with Notices of Other Subjects" (11). This paper is interesting as it contains a reference to *Chemical Manipulation* and a practical suggestion on how to cut glass with a hot iron (11):

> Mr. Faraday has devoted four pages of his recent work on chemical manipulation, to an account of the methods of cutting glass with a hot iron. His directions are valuable to the young chemist, because they are drawn out into that minuteness of detail, which alone can make them of any use; and yet he has omitted one precaution, which I have found important in cutting large tubes, vials, etc. - that of not making the iron too hot. It should be heated to a redness barely visible in daylight. If in this state, it be caused to vibrate a few times around the tube, along the track where the division is to be made, and a drop of water put upon the spot, a simple fracture, without side flaws, will be obtained.

Faraday did not, however, include this tip in the third edition.

Another of these later references occurs in Section XIII, "Crucible Operations - Fusion - Reduction", which begins on page 281. The reference is to a paper entitled "On the Existence of Titanic Acid in Hessian Crucibles", by R. H. Brett and Golding Bird, published in *The Philosophical Magazine* in 1835 (12). Faraday noted on his interleaf:

> Dr. Wollaston told me in 1827-28 that Hessian crucibles contained Titanate and also that Cornish crucibles resembled them in that respect.

Again, Faraday did not carry this defense of Wollaston's priority into his third edition, although the fact that he entered it in an appropriate place in his interleaved copy of the text, indicates that at one time he had meant to do so.

One change, however, he deemed important for the third edition. It consisted of introducing the terms of his own coinage, "electrolyte" and "electrodes", into the section on voltaic electricity, instead of the terms he had used originally, namely, "imperfectly conducting matter" and "poles".

It may seem surprising that so creative a worker as Faraday should have employed himself in so routine task as combing the printed literature for references with the diligence that these annotations display. Nevertheless, a copy of the cumulative index to volumes 1-20, 1816-26, of the *Quarterly Journal of Science and the Arts*, published in 1826, in the possession of the Royal Institution, has added in manuscript on its title-page "Made by M. Faraday". Since the cumulative index was largely drawn from the separate indexes of each volume, it is likely that the recurrent task of making those was also undertaken by Faraday. If such were indeed the case, he would have had considerable experience in that kind of harmless drudgery, dating from the days when his position at the Royal Institution was still that of an assistant to William Brande.

References and Notes

1. M. Faraday, *Chemical Manipulation; Being Instructions to Students in Chemistry, on the Methods of Performing Experiments of Demonstration or of Research, with Accuracy and Success,* Phillips, London, 1827.
2. Private communication from Mrs. F. Anderson, 6 July 1989.
3. T. Young, *Lectures on Natural Philosophy and the Mechanical Arts*, Vol. 2, Johnson, London, 1807, pp. 87-520.
4. Anon., "Notices Respecting New Books", *Phil. Mag.*, **1827**, *2 (ser. 2)*, 58-66.
5. *Ibid.*, pp. 64-65.
6. *Ibid.*, p. 65
7. *Ibid.*, p. 65
8. *Ibid.*, pp. 65-66.
9. Anon., "Chemical Manipulation; &c", *Literary Gazette and Journal of the Belles Lettres, Arts, Sciences, &c*, **1827,** *22 (July 21)*, 472-473.
10. Anon., "Chemical Manipulation; &c", *Quart. J. Arts Sci.*, **1827**, *24*, 275-283.
11. E. Mitchell, "On a Substitute for Welther's Tube of Safety, with Notices of Other Subjects", *Amer. J. Sci. Arts*, **1830,** *17*, 345-350.
12. R. H. Brett and G. Bird, "On the Existence of Titanic Acid in Hessian Crucibles", *Phil. Mag.*, **1835**, *6 (ser. 3)*, 113-117.

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UNPUBLISHED LETTERS OF FARADAY AND OTHERS TO EDWARD DANIEL CLARKE

Sydney Ross, Rensselaer Polytechnic Institute

The letters printed here are part of a collection of autograph letters made by Edward Daniel Clarke (1769-1822) based on his own private correspondence. His biographer, William Otter, wrote (1):

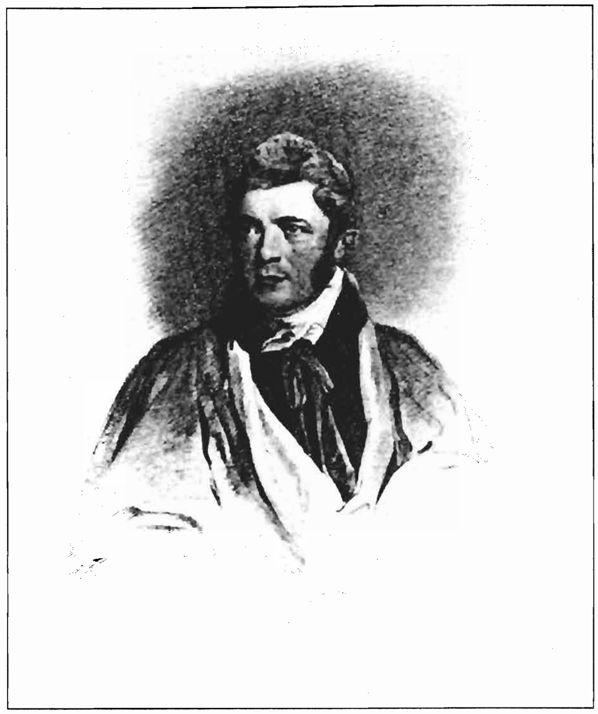
Of his friends and correspondents it may be said without the slightest exaggeration, that they formed no inconsiderable portion of the persons whose learning and genius have shed a lustre upon their country during the last twenty years, and this, not in one department only, but in several; and if he had shewn as much regard for his own letters, by taking copies of them, as he did for those of others, by preserving them, they would have constituted together a body of correspondence as interesting and instructive as any which has been presented to the public in our memory ... Besides the eminent names of Porson, Parr, and Burney, with Dr. Maltby and Dr. Butler, already mentioned, there appear in the departments of classical and philological literature, Mr. Payne Knight, Dr. Raine, Dr. Bloomfield, Professors Monk and Dobree, Dr. Kaye (Bishop of Bristol), Mr. Matthias, Mr. Weston, Archdeacon Wrangham, &c.; amongst persons distinguished by travel, or in the fine arts, Mr. John Hawkins, Mr. Malthus, Lord Byron, Mr. Walpole, Lord Aberdeen, Mr. Squire, Lord Valentia, Mr. Wilkins, Mr. Hobhouse, Mr. Banks, Mr. Burckhardt, Dr. Heber, Sir W. Gell, Mr. Hamilton, Major Rennel, Mr. Pennant, &c.; in chemistry, mineralogy, and natural history, Dr. Wollaston, whose letters are particularly kind and instructive, Mr. Tennant, Sir H. Davy, Mr. Wavel, Dr. Thomson, the mineralogical Professor at Aberdeen, Mr. Hailstone, Dr. Milner, Dean of Carlisle, Professor Kidd of Oxford, Mr. Holme, Mr. Lunn, Mr. Leslie, Dr. Brewster, Mr. Jameson, Sir W. Smith, Mr. Lambert, &c.; to these may be added, Mr. Edgeworth, Mr. Wilberforce, Dr. Nicholls, Arabic Professor at Oxford; amongst foreigners, Chevalier, Pallas, Haüy, Noezen, &c.

Many of these letters were sold at auction on 27 May 1842 but at least one substantial block of material was not sold at that time. The late Louis F. Gilbert of University College, London, owned a large collection of letters addressed to Clarke, which he had purchased from Thomas Thorp, bookseller. These are the letters mentioned by Otter as pertaining to chemistry, mineralogy, and natural history. They are bound into two large volumes, which were consigned to the auction room by Gilbert's widow and sold as lot 462 at Sotheby's on 19 July 1960, when they came into my possession.

We owe the preservation of these early letters of Faraday to Clarke's habit of retaining, as a part of his autograph collection, all letters addressed to him, which he then had bound together in chronological order, so that through the decades none became detached and separated. Few letters of Faraday are extant from this period, before his name was well known and even before the cult of collecting autographs had reached its later growth (2).

Edward Daniel Clarke, Faraday's correspondent in 1816, is well introduced in the following words of William Whewell (3):

When I was an undergraduate at Cambridge about 1813, I attended the mineralogical lectures of the celebrated Edward Daniel Clarke, then just returned from his travels which had extended from the Baltic to the Crimea and the Mediterranean. Certainly Clarke was one of the most striking characters belonging to the Cambridge life of that my early time. He was very eloquent: - I should say the most naturally eloquent man I have heard: that is, he gave to what he said all the charm that fluency, earnestness, and fine delivery could give, independent of its meaning and purport, which often could not bear a close examination. He was not an exact or profound man of science, but he had a good knowledge of what was doing in the world of science, and undaunted courage in endeavouring to take his share in it. He very nearly blew himself to pieces once or twice in his experiments with his oxyhydrogen blowpipe. He, on returning to the University after his travels, began to deliver a course of lectures on Mineralogy, which were very attractive, for in them he introduced stories and discussions about all that he had seen and heard of in the course of his travels. Among other things he spoke of meteoric stones. The celebrated mass of meteoric iron which Pallas had seen in Siberia and had described in his *Travels*, had then recently drawn general attention to the subject. Clarke had of course a theory on the subject of these stones. I do not know if anyone now maintains that theory. He held that as all substances can exist in a gaseous state, the components of these stones might occur, in a gaseous state, in the higher regions of the earth's atmosphere; might there, owing to some natural event or other, combine; of course with explosive violence, noise and fire, and might then fall to earth. I do not know if this theory made many converts; some of us certainly laughed at it; and one of my friends said that it seemed to him just as likely that the air should drop biscuits from time to time in the neighbourhood of a flour mill.

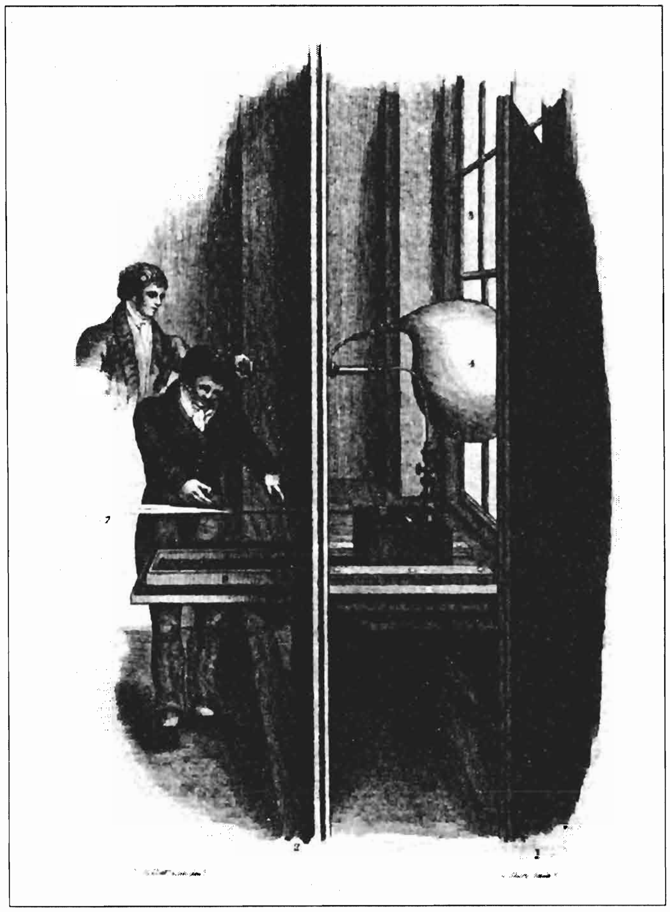


Edward Daniel Clarke
(From an engraving by H. Meyer)

Another of Clarke's undergraduate auditors was Adam Sedgwick, who testified that "he kept us awake", high praise indeed for any lecturer (4). Henry Gunning of Christ's College described him as one who "often suffered his imagination to run away with his judgment" and related several instances, among which is an anecdote of how Clarke, spying a picture, covered with dirt, in a shoemaker's shop, persuaded himself that it was a portrait of Shakespeare (5). He put it into a magnificent frame and exhibited it in the University library. On the first day it was exhibited upwards of 3000 persons came to see it and Clarke wrote a small pamphlet proving it to be an original Shakespeare. Later, however, he changed his mind and made a present of it to the shoemaker from whom he had purchased it. On another occasion he was greatly excited to discover a model of the *Flight into Egypt*, which he declared, after removing the dirt with which it was encrusted, to be covered in precious stones, especially the reins of the bridle, and to be very valuable. The stones were judged later to be of no value. Evidently Clarke imbued all his experiences with romantic qualities.

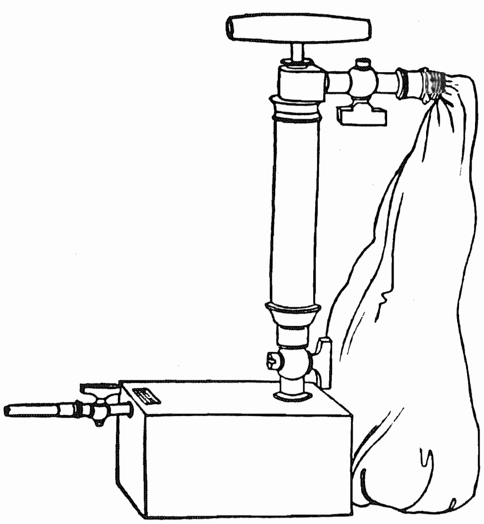
Clarke's results with the oxyhydrogen blowpipe led him to theorize that volcanic eruptions arise from the decomposition of water by geothermal heat and the subsequent pressurizing and recombination of its gaseous elements. Lord Spencer, expressing surprise at the noise and heat of the oxyhydrogen flame, remarked "It is like Etna." "Like Etna, m'Lord!" Clarke replied, "Why it is Etna itself!"

Clarke sent a written account of his first experiments with the oxyhydrogen blowpipe to a journal newly established at the Royal Institution, of which William Thomas Brande (1788-1866) was the editor and Faraday, as Brande's assistant, was factotum, or general dogsbody. Faraday wrote that (6):



Clarke's modification of the Newman-Brooke oxyhydrogen blowpipe, including a safety shield for protection from explosions (13).

> When Mr. Brande left London in August [1816], he gave the *Quarterly Journal* in charge to me; it has had very much of my time and care, and writing through it has been more abundant with me. It has, however, also been the means of giving me earlier information on some new objects of science.



The Newman-Brooke oxyhydrogen blowpipe.

Among the early information received by Faraday was Clarke's report of his experiments (7), but many of his conclusions were received with reservation, especially his claims to have reduced barium oxide by heat alone to elemental barium, which had a vitreous rather than a metallic appearance; and to have obtained a metal "of a greater degree of *metallic lustre* and *whiteness* than the purest silver" [the italics are Clarke's] from silica. This latter metal, he admited, "I have not been able yet to re-produce in a manner altogether satisfactory." Particularly offensive, however, to Davy and his circle of admirers at the Royal Institution must have been Clarke's presumption, or perhaps only naiveté, in naming the metal from silica *silicium*, thus implying that Davy's silicon was not elemental but a suboxide of Clarke's silicium, and his renaming Davy's barium as *plutonium* "because we owe it entirely to the *dominion of fire*:

according to *Cicero* there was a *temple* of this name, dedicated to the *God of Fire*, in *Lydia*" (7). The selection of the same name for element 94 has a different history. One of Seaborg's original team, Dr. Nicholas Kemmer, suggested that the use of planetary names, started by Klaproth with "uranium"(element 92), named in honor of the then newly discovered planet Uranus, should be continued. Outward from Uranus is Neptune, so element 93 should be named "neptunium". The next planet is Pluto, and so element 94 should be named "plutonium". That Pluto is the god of fire is a pleasing coincidence, but not the reason for the name chosen for element 94 (8).

The following letters from Faraday to Clarke have to do with the printing of Clarke's paper in *The Quarterly Journal of Literature, Science and the Arts*. The paper is entitled "Account of Some Experiments Made with Newman's Blow-pipe, by Inflaming a Highly Condensed Mixture of the Gaseous Constituents of Water". To Faraday was delegated the task of seeing this paper through the press but, as we see from his letters, he undertook, with all due respect, to engage the author in questions of chemistry. So well did he do this that Clarke came to consider him an authority and evidently addressed various queries to him, to which Faraday's fifth letter is a reply.

Faraday used little punctuation in his handwritten letters - to reproduce them in their original form in print would distract a reader and give a false impression of incoherence - for the purpose of this publication, therefore, occasional punctuation has been inserted:

FARADAY TO E. D. CLARKE
Royal Institution August 6th 1816

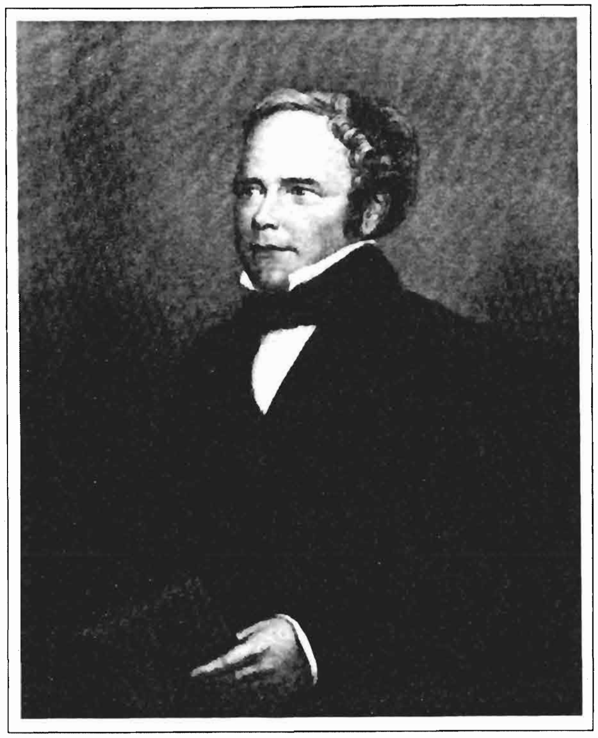
Sir - Mr. Brande is at present on the Continent but left directions with me before his departure for the management of the Journal.

The results obtained from the earths Barytes & Strontia independent of electrical powers must be interesting. From conversation with Mr. Newman I have presumed that the experiments are in extension of that first made by Sir H. Davy in which Oxygene & Hydrogen were burned from the new blowpipe.

I venture to return thanks on the part of Mr. Brande for any paper you may contribute to the Journal & promise that due attention shall be given to it.

I am Sir, With great respect, Your humble Srvt
M. Faraday.

John Newman (fl. 1816-1838) was an instrument maker with a shop at 7/8 Lisle Street, Leicester Square, London. He was the maker of the compressed-gas blowpipe, which he co-invented with Henry James Brooke (1771-1857) (12). The use of a mixture of hydrogen and oxygen, in a ratio of two to one by volume, as the combustible gas was Clarke's idea, though Faraday was soon to inform him that he was not the first to have tried it.



William Thomas Brande
(From an engraving by L. Wyon)

FARADAY TO E. D. CLARKE
Royal Institution August 8th 1816

Sir - I have been to the Printer to ask him the time he can allow for making up Copy and he says that three weeks are as much as he can spare; in which time Sir if you can favour us with a paper of so much interest as the experiments or rather results you so briefly relate, promise we shall be much indebted to you.

The printer is very willing & indeed prefers that you should yourself correct the press but we have no means except the Post by which to send the impression down. But if when you send the copy you also transmit other directions we shall strictly attend to them.

Mr. Newman appears to have been not sufficiently explicit in detailing to you the history of the experiment in which oxygene & hydrogene are burnt from his blowpipe. I presume that from the interest you must feel in your present series of experiments you will excuse me for giving a fuller account of it.

The merit of having first burned oxygene & hydrogene issuing in mixture from a common reservoir belongs to an unknown Native of Germany, who as far back as 3 years ago told Mr. Tatum of this City that he had burned a mixture of oxygene & hydrogene, propelled from a bladder through a long narrow tube, at the end of the tube with safety & without the inflammation passing up into the mass of gasses (9). He considered the security of the experiment as depending on a strong pressure given to the bladder. Whilst in conversation with Mr. Tatum & relating to him the singular experiment in which Sir H. Davy had introduced one of his lamps into a receiver filled with oxygene & hydrogene gasses in the most explosive proportions, he told me of the above circumstance but said he had never made the experiment. I afterwards mentioned it to Sir H. Davy because I consider it to depend on those very circumstances which insure the safety of his mining lamp. Mr. Newman's blowpipes were made for the first time about this period & Sir H. Davy immediately applied one of them to the performance of this experiment. I was present; it was made with my cautious request & succeeded perfectly. Platina was fused & a very intense heat obtained but nothing more was done with it.

This I have every reason to believe was the first time the experiment was done in England; at least no one had made it before in any way connected with the information I have just given. I myself first told Mr. Newman the result merely because it had been done with the substitution of his blow pipe for the original bladder. He informed me afterwards that he had mentioned it to you and that you wished to pursue it farther. I heard also of Dr. Wollaston's objections & of the communication that passed between you & Sir H. Davy.

Such is the history of the case. A German first conceived the experiment if he did not make it. Sir H. Davy first made it in England & you Sir have the merit of applying it so happily & to the obtaining such remarkable results. I shall this evening see Mr. Tatum & make further enquiries respecting the author of his information and if you are desirous transmit it to you on a future occasion.

I am Sir, with Great respect, Your vy humble servnt

M. Faraday.

Addressed to Revd. Dr. Clarke,
Trumpington Street, Cambridge

E. D. CLARKE TO FARADAY (10)
Cambridge, August 26, 1816

Dear Sir - While there is time I continue to add one discovery after another. Perhaps, if you have not sent my Ms to the Printer it will be better to return it that I may make the additions.

I have at this moment the *Metal of Strontia* before my eyes; shining with all the lustre and whiteness of highly burnished silver, although it was obtained so long ago as last Friday Morning from the *Earth.* It becomes covered with an earthy powder sometimes, but not always, when a stroke of the File discloses a fresh face of the Metal. The Metal of *Strontia* is, if anything, whiter and more like silver than that of *Barytes*.

You will please to observe that in reducing these *Earths* to the *metallic* state, they were not brought into contact with any metallic support, such as *Platinum*. I used Charcoal; and our Professor of Chemistry [James Cumming (1777-1861)] expressing a doubt whether Charcoal might not contain iron enough to cause such appearances, the Experiments were repeated without Charcoal; but the *Metals* were obtained as before. In short everything has been done which is necessary to demonstrate that these *Metals* of *Bartyes* and *Strontia* are severally derived from the *Earths* in their purest state, without the admission of any other *metallic* body whatsoever.

I have not yet succeeded with Silex, Alumina, Magnesia, and Lime further than by converting each of them into a Glass.

Yours truly E. D. Clarke

Addressed to Farraday [sic] / Royal Institution / Albermarle St / London

FARADAY TO E. D. CLARKE
Royal Institution August 27th [1816]

Sir - I send the paper by the Mail of this evening for your alterations. The Printer has composed a considerable part of it, which will however be altered according to the copy you will send back. He wishes for it as soon as possible. A drawing has been made of the blowpipe with its condensing syringe & the small tube and given to the *engraver on wood*. It will be placed at the head of the paper so that a reference to it in the body of the paper might be agreeable.

When you first mentioned the reduction of the earths Baryta and Strontia it was done so briefly as to allow of many doubts respecting the accuracy of the experiments & the results. I am glad these have been fully considered. Perhaps it would be worth while to state an experiment in which the metals have been converted into earths again. Indeed so singular is it that they should be at all permanent in the atmosphere that the world will require full proof that that is the case. Their action on water (particularly that of BPlutonium)(11). I should think would be very violent.

In your last letter you have said that you obtained the metals without the aid of charcoal but I suppose the reduction was effected not by the heat alone but with the aid of some combustible matter as oil, &c. The supposition relates merely to the reduction not the probable presence of any other metal.

I am Sir, With Great Respect, Your Humble Servt

M. Faraday

FARADAY TO E. D. CLARKE
Royal Institution August 29th 1816

Sir - I am very sorry that any confusion should arise in the return of your MS. I have been to the Office where it was booked (by Mr. Newman's lad) and they assure me it left town at the same time with the letter but account for the circumstance of its appearing to be missing by supposing that the men have delayed the delivery of the parcels for an hour or two.

For my own part I fancy it probable that they have sent it by the Coach, though directed for the Mail (a circumstance which I understand sometimes occurs) and I hope you have received it long since. They promise to write to their agent about it immediately, though they have no hesitation in saying that you have had it ere this. If you have not I should be glad to know immediately that the further necessary steps may be taken immediately.

Your discovery of a metal in Silica surprises me more than any thing you had before done, because of the strong presumptive proofs afforded by Sir H. Davy's experiments that the basis of Silica was not a metal but an inflammable substance analogous to Boron. It is very impertinent in me to suggest any thing but the great dissimilarity between silica and the other earths and the analogy between it & Boracic acid [i.e. boron trioxide], or rather between their bases when treated with the fluoric compounds, promises to open some very curious views in this department of chemical science, particularly if Silica is the oxide of a metal.

The interest of your experiments augments daily & will make your paper a valuable addition to our Journal.

Mr. Newman desires me to say he has read your Letter of the 24th.

I am Sir with much respect, your humble Servt

M. Faraday

FARADAY TO E. D. CLARKE
Royal Institution Sept 19th 1816

Sir - I have just received your letter of yesterday and hesitate not a moment in writing scarcely an answer to it but an acknowledgement of it. Indeed your Queries appear to be such as can only be answered by experimental investigations, for I am not aware of any information that can be quoted, i.e. drawn from ascertained knowledge, that can apply to them though, as my small stock of chemical science necessarily leaves untouched many important branches, it is very probable that answers to your queries may be known to some though unknown to me.

As however Sir I judge from the import of your communication you expect an answer from me, I shall venture a few observations on the subject. - It has been my intention for some time past to repeat some of your experiments but I have not yet procured a blow pipe from Mr. Newman and have therefore been obliged to defer them. Not having seen the experiments it is possible I may make a wrong judgement of them, for there are many little circumstances & changes which arise in the progress of an experiment which materially assist us in forming a conclusion.

Allowing that charcoal causes the vitrification of metals it is evident that it must be owing either to a change in the state of the metal, or to the decomposition of the metal, a vitrifiable body being left, or to a combination of the metal with some other substance forming a vitrifiable compound. Not having seen the experiments I have not sufficient means of judging, since effects may have been produced in them which are new; but reasoning from the habits of the metals as I have met with them I should not think that the pure metal was vitrified or decomposed by the powers you have applied to it and it then follows that it has combined with some thing. It strikes me indeed that you have formed a carburet & if that is the case that carburet may be vitrifiable, though the pure metal is not. I have often thought on the probable changes which charcoal might undergo at the heat you possess the means of procuring, if its combination with oxygene could be hindered. In some experiments made with the powerful voltaic apparatus here there were apparently evidences of its volatilization when acted on in vacuo and we can scarcely entertain a doubt of its fusibility at some temperature; and if we had never seen carbon as charcoal we should not have been much surprised at the idea of diamond forming with metals vitrifiable substances. Carbon has something peculiar in its combinations. It exists but in small proportions in steel yet causes a great change in the properties of the iron combined with it. It exists in extraordingly *(sic)* high proportion in plumbago yet still leaves it possessed of many metallic characters. I find nothing difficult in the idea of believing that it may form with the earthy metals a vitrifiable compound.

It is difficult to form an opinion on your second query without knowing every circumstance of the case and they can only be properly ascertained by the operator. The presence of extraneous substances, the vitrification before spoken of, the more or less perfect reduction & many other circumstances may be present & exert a very extensive influence. If the metal which presents those changeable appearances is capable of being vitrified without the addition of other substances than are present then there is strong reason to believe that the variable approaches to this state cause the appearances.

I must however Sir beg your pardon for troubling you with matter so unimportant and so far removed in its nature from [that] which you required but I can only present in excuse my ignorance of those particular facts and indeed of science in general.

I am Sir With great respect

Your Obedt. Humble Servt.

M. Faraday

Addressed to Revd. Dr. Clarke, Professor of Mineralogy, &c &c &c, Cambridge

A few letters from other correspondents are pertinent to the questions discussed in Faraday's letters:

W. T. BRANDE TO E. D. CLARKE
[Undated, but early 1817]

My dear Sir - I have just returned from the Continent or should have sent an earlier answer to your many valuable communications. I beg to thank you for having sent them to the Journal of this institution, and congratulate you on their importance.

I have succeeded in most of your methods of fusion, but have not yet been able to obtain Barium, or as you have named it "Plutonium", nor Strontium. The earth fuses, burns, and evaporates. You will therefore much oblige me if at your leisure you would give me such directions regarding *your* method as may enable me to attain the result desired. I hope you will prosecute your enquiries and extend them. My assistant Faraday has I hope acted conformably to your wishes in all that regards the proofs of your paper.

Your own copies will be forwarded in a few days.

Yours my dear Sir very faithfully

Willm. Thos. Brande. Royal Instn. Saturday

J. F. W. HERSCHEL TO E. D. CLARKE
[Undated]

Dear Sir - I have already perused, with anxious attention, the very extraordinary Statement of your amazing experiments in Brande's Journal. I am happy to see that the Berlin Socy has *distinguished* itself by its promptitude in indicating a sense of the importance of your discoveries and the noble ardour which in their prosecution has led you to defy more than ordinary danger.

Pardon me, if as a Member of the Royal Socy I express *something like regret* that their Transactions had not to announce to the Scientific world such wonderful results (though the public would have suffered by the delay) but the periodical work of an individual (however excellent) seems to me a vehicle unworthy [of the] magnitude of the discovery.

I write in the utmost haste - pray pardon me.

Yours very truly, J.F.W. Herschel

P.S. I have a very extraordinary expt. of Tully an optician in London to shew you if you can furnish me with a lens of pretty long focus, *and a sunny morning*.

HUMPHRY DAVY TO E. D. CLARKE
21 Queen's Square, Bath Oct. 28th [1816]

My dear Sir - I have spent the summer in the North of England principally amongst the coal mines enjoying the inexpressible pleasure of seeing my lamps everywhere employed in preserving the miners from danger. Your letter announcing your expts with Newman's blowpipe missed me in its first address & has since followed me south. I have this day received your second letter.

Had I seen your communication for the Quarterly Journal of Science before it was published I should certainly have considered it an act of friendship as well as duty to have begged of you to reconsider many parts of it & at all events to have altered the form in which certain results were announced.

Amongst the metals of the earths Barium or the metal of Barytes is the one which I obtained in the most unequivocal manner by the battery & in globules sufficiently large to examine. It does not bear even a momentary exposure to the free air & amalgamates readily with mercury.

You perhaps are not aware that Baryta has a strong attraction for oxygene, that it readily absorbs that principle & that the peroxide rapidly oxidates & dissolves platinum. I am strongly disposed to believe that the *metallic* films you obtained are from platinum that had been dissolved & revived & I am confirmed in this suspicion by what you say of the action of charcoal in occasioning a vitrification of the metals of the earths. The peroxide of Barium dissolves other metals as well as platinum. I should recommend it to you therefore to use no metallic substance as a stand for your earths.

Whilst I was writing your third letter was brought to me. I was just going to answer to your second that I was certain an explosion could not take place in a tube 1/80 of an inch in diameter & 3 inches long & was going to recommend to you to measure the aperture through which the explosion had passed.

With respect to Boron, this substance alloys with platinum & other metals. I once suspected that it was a metallic protoxide but certainly should never have conceived that it could have been revived in a stream of oxygene gas.

I can hardly suppose that the difference of 1/80 & 1/60 of an inch can make such a difference in the results. At least by diminishing the mass of matter an equal heat ought to be produced.

I can immediately give you a plan by which all possibility of danger is avoided & with which you may use *tubes* of any diameter you please. Have two compressing boxes made furnished with two stopcocks with diameters so arranged that one may deliver twice as much gas in a given time as the other - fill one with compressed hydrogene the other with compressed oxygene - let the two stopcocks terminate in a common duct or tube of fire glass.

You announce that the heat produced by the combustion of oxygene & hydrogene is stronger than that of the most powerful voltaic battery. I doubt this. It would require very accurate & minute expts of comparison to prove it.

I have no doubt that the blowpipe with oxygene & hydrogene will prove a useful instrument & the chemical world will have obligations to you for having shown the power of it. - Lady Davy's indisposition has brought me here. She continues unwell but I hope the Bath waters will effect a cure.

With respects to Mrs. Clarke in which Lady D joins me, Dear Sir, very sincerely yours, H. Davy.

I shall be glad to see your paper for the R. Society & will with great pleasure offer my suggestions upon it.

References and Notes

1. W. Otter, *The Life and Remains of Edward Daniel Clarke*, Vol. 2, London, 1825, pp. 271-2.

2. A. N. L. Munby, *The Cult of the Autograph Letter in England*, Athol Press, University of London, London, 1962, pp. 71-2.

3. I. Todhunter, *William Whewell, D. D., Master of Trinity College, Cambridge: An Account of His Writings with Selections from His Literary and Scientific Correspondence*, Vol. 2, Macmillan, London, 1876, p. 378.

4. J. W. Clark and T. McK. Hughes, *The Life and Letters of the Reverend Adam Sedgwick*, Cambridge University Press, Cambridge, 1890, Vol. 2, p. 349.

5. H. Gunning, *Reminiscences of the University, Town, and County of Cambridge, from the Year 1780*, Vol. 2, Bell, London, 1854, pp. 202-215.

6. Quoted in H. Bence Jones, *The Life and Letters of Faraday*, 2nd ed., Vol.1, Longmans, Green & Co., London, 1870, pp. 201-203.

7. E. D. Clarke, "Account of Some Experiments Made with Newman's Blow-pipe, by Inflaming a Highly Condensed Mixture of the Gaseous Constituents of Water", *Quart. J. Lit. Sci. Arts*, **1817**, *2*, 104-123.

8. G. Seaborg, *The Tranusranium Elements*, Yale, New Haven, CT, 1958, p. 7.

9. Faraday was not well-informed on the history of the oxyhydrogen blowpipe. For a fuller account see E. L. Gonzalez, "Bochard de Saron and the Oxyhydrogen Blowpipe", *Bull. Hist. Chem.*, **1989**, *4*, 11-15.

10. F. A. L. James, ed., *The Correspondence of Michael Faraday*, Vol. 1, The Institution of Electrical Engineers, London, 1991, p.145.

11. Faraday had started to write *Barium*, but changed the capital B to a capital P for *Plutonium*. The alteration is very evident.

12. H. I.[sic.] Brooke, "Description of a New Blow-pipe", *Ann. Phil.*, **1816**, *7*, 367 and J. Newman, *Quart. J. Sci. Art.*, **1816**, 1, 65-66. An example of Newman's blowpipe can be seen in the Science Museum, London, see A. Barclay, *Handbook of the Collections Illustrating Pure Chemistry, Part II, Decriptive Cataloque*, Science Museum, Her Majesty's Stationery Office, London, 1937, p. 62.

13. E.D. Clarke, *The Gas Blow-Pipe, or Art of Fusion by Burning the Gaseous Constituents of Water; giving the History of the Philosophical Apparatus so denominated; and the Proofs of Analogy in its Operations to the Nature of Volcanoes*, Cadell and Davies, London, 1819.

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THE PATHWAY TO THE LAWS OF ELECTROLYSIS

John T. Stock, University of Connecticut

Michael Faraday has a massive physical monument - the vast number of books, papers, and general articles that have surveyed virtually every aspect of his life and work. This paper is the result of looking at a limited but important aspect of this monument.

As a life member of the Royal Institution of Great Britain, I spent part of a sabbatical leave under the direction of Professor Ronald King. At that time, he was planning the Faraday Museum in the basement of the Institution. I had the opportunity of reading some of Faraday's manuscripts, giving me a feeling of looking over his shoulder as he planned his next experiment. To mark the 150th anniversary of the 1834 publication of the Second Law of Electrolysis, I set up a commemorative exhibit in one of our departmental wall cases. Included was a display that cyclically highlighted some of Faraday's contributions to chemistry (1).

Although the histories of both chemistry and electricity go back to ancient times, electrochemistry as we know it today did

Christian von Grotthuss

not begin until 1800, when Volta's account of the so-called "pile" was published (2). This device, and developments that rapidly followed, provided for the first time a source of continuous, reasonably steady, and comparatively large amounts of electricity. As Faraday was to point out later, the then well-known static or "common" electricity is characterized by high intensity but very little quantity. Nicholson read Volta's communication before its publication, with the result that a pile was constructed and used to prepare hydrogen and oxygen by the electro-decomposition of water (3). From this deceptively simple experiment sprang the vast and diverse field of electrochemistry (4).

Although the fact of the electro-decomposition of water was obvious, a satisfactory explanation of the mechanism involved was not, despite various efforts over several decades. In the long-studied area of "common" electricity, beliefs were in the existence of two forms of electricity, positive and negative; "like signs repel, unlike, attract"; and "action at a distance", governed by an inverse square law. These beliefs were the inheritance of early workers concerned with voltaic electricity. In attempting to explain electro-decomposition, this inheritance was largely a handicap.

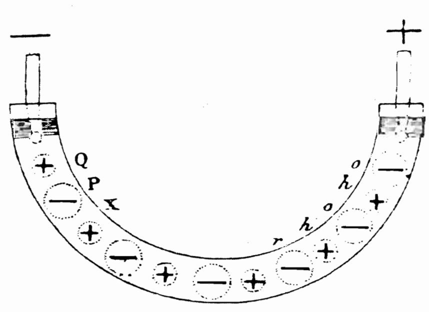
In 1801, Johann Wilhelm Ritter (1776-1810), a German physician, used V-shaped tubes to re-examine the electro-decomposition of water (5). This shape prevented transfer of matter from one pole to the other by convection or agitation. To explain the fact that gases appear only at the poles, he theorized that water plus positive electricity gives oxygen, whereas water plus negative electricity gives hydrogen.

Christian von Grotthuss (1785-1822) suggested that water molecules are polarized, becoming centers of attractive and repulsive forces which vary inversely as the squares of the distances from the respective poles (6). Thus the hydrogen and oxygen in a given molecule will be subject to attractive and repulsive forces, acting in opposite directions. Hydrogen and oxygen produced by the breakdown of this molecule would not escape, but would attack adjacent molecules. The production of the gases only at the poles could be explained by a chain-like abstraction mechanism.

The 1806 Bakerian Lecture, given to the Royal Society by Humphry Davy (1778-1829), was based on his own electrochemical experiments and ideas (7). His great contribution was to connect chemical affinity with electrical forces. Two of his experiments are shown in figure 1. Potassium sulfate solution was placed in each of two cups, with a moistened strip of asbestos as connector, as shown in (a) of figure 1 (8). A current was passed through the system for three days, then the contents of the cups were analyzed. The left cup contained sulfuric acid, the right, potash; Davy had achieved the complete separation of the components of the salt. The Grotthuss mechanism was obviously inadequate here; the "chain of molecules" must break for such completeness to be possible.

If the component acid and base existed even briefly in the solution rather than being generated at the electrodes, their detection in transit should be possible. Davy therefore set up the arrangement shown in (b) of figure1, the contents of the three vessels being as indicated (9). Moistened litmus paper strips X and Y were placed in contact with the asbestos connectors. On passing a current, the sulfuric acid moving towards the positive pole should redden strip X. This did not occur; instead, Y began to redden, and this effect slowly diffused into the central vessel. Apparently, ordinary chemical affinity had been suspended by the flow of electricity; acids could be passed through bases, and vice versa!

Although he had shown that the Grotthuss mechanism could not account for the complete decomposition of potassium sulfate, Davy wrote "In the cases of the separation of the



Grotthuss' chain mechanism for electroytic conduction.

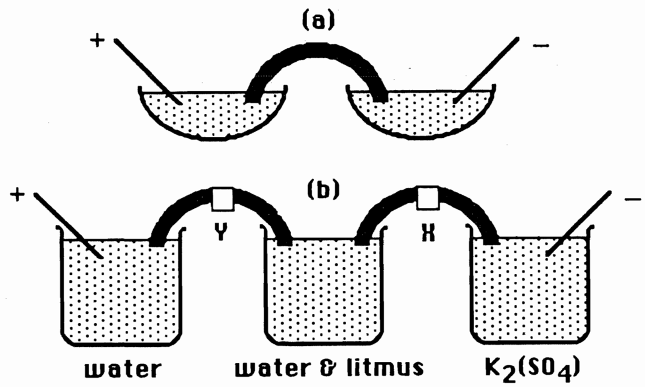


Figure 1. Davy's experiments with potassium sulfate solution: (a) the complete separation of acid and alkali (b) an attempt to detect the transit of sulfuric acid.

constituents of water, and of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions throughout the fluid" (10). When Faraday surveyed the suggestions of others as part of his own attack on the elucidation of the mechanism of electro-decomposition, he noted the lack of specificity of Davy's theory.

In 1814, during the European tour with Davy, Faraday met Auguste de la Rive (1801-1873), Professor of Physics at Geneva. Faraday corresponded with him for many years. De la Rive reconsidered electrochemical action during the 1820s (11-13). He postulated that the current flowing from the positive pole attacks the nearby molecules, grasping their hydrogen if water, or their base, if the molecules are salts. The oxygen or acid is left behind, while the positive current carries the substance with which it is united to the negative pole. This metal conductor cannot admit the transported substance, so hydrogen or base is released as the electricity enters the negative pole. The reverse current acts analogously on the oxygen or acid in the molecules near the negative pole. De la Rive did not accept a chain-type mechanism, believing the bulk of the liquid acted merely as a conductor. With the concept of positive and negative currents, only portions of which were involved in transporting matter, De la Rive's theory became very complicated (13).

When Faraday began his work on electro-decomposition, he was faced with theories which had about one real or implied common view: the poles acted at a distance upon the constituents of the substances being decomposed. Another problem was the apparent existence of various forms of electricity. Faraday was convinced that all forms were manifestations of a single identity. By exhaustive examination of the literature and his own extensive experiments, he was able to prove his conviction that electricity, "whatever may be its source, is identical in its nature" (14).

Decompositions could be brought about by the use of a voltaic pile. Faraday therefore examined claims that common electricity could produce similar decompositions. This form of electricity is noted for its ability to produce sparks. By a litmus-paper version of Cavendish's production of nitric acid by sparking in air, Faraday showed that the mere heat of an electric discharge could bring about a chemical reaction. He was therefore very careful to use spark-free conditions in his own experiments.

The arrangement for one of these is shown in figure 2. Three pieces each of litmus paper *ppp* and turmeric paper *nnn* were moistened with sodium sulfate solution and placed on a glass plate as litmus-turmeric pairs (turmeric is reddened by alkalis, litmus by acids) (15). Platinum wire conductors were bent so that they made point contact with the papers, as shown. Wire *m* was connected to a large frictional electrical machine, while wire *t* went to the "discharging train." This was a wire connected to a gas pipe or water pipe. Nowadays, we would say the wire *t* was grounded. Brief operation of the machine caused formation of acid at all point contacts on litmus and of alkali on turmeric paper.

Apart from demonstrating that common electricity and voltaic electricity produced the same chemical effects, this finding assured Faraday that he could use the high-intensity output of his machine whenever poor conductance of a system under investigation prevented the use of a battery. He had already begun to suspect that the poles in an electrochemical system have no mutual decomposing dependence. On occasion, he used his finger as one of the poles! Then came the prescient remark (16):

> When electro-decomposition takes place, there is great reason to believe that the quantity of matter decomposed is not proportionate to the intensity, but to the *quantity* of electricity passed.

Incidental to his attempts to demonstrate the reality of this belief, Faraday commented on the value of the galvanometer for measuring what we would now term the current strength in a circuit (17). The development of this instrument has a long and interesting history (18). Faraday mentions one advance, the introduction by Ritchie of a fine glass thread as the torsion element (19). More than half a century was to pass before C. V. Boys demonstrated the superiority of quartz threads over those of glass (20).

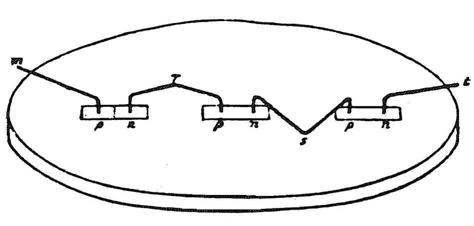
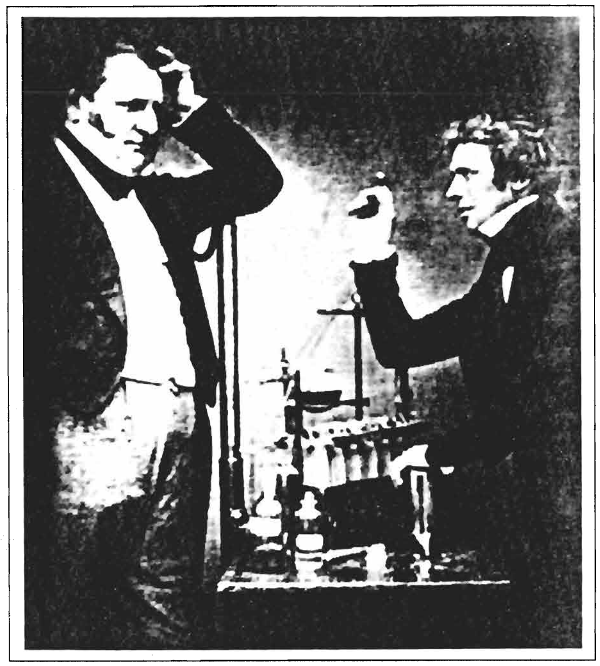


Figure 2. Multiple formation of acid and alkali by "common" electricity.



John Frederick Daniell (left) and Michael Faraday (right), circa 1843.

Faraday took four thicknesses of paper, equally moistened with a standard solution of potassium iodide, and placed them on a platinum spatula. A vertical platinum wire, 1/12 of an inch in diameter with a squared-off end, pressed on the paper sandwich, thus defining a definite area of contact. With a single platinum-zinc-dilute nitric acid cell as a source of electricity, the galvanometer in the circuit gave a steady deflection. By shifting the end of the wire from place to place on the test paper, the effect of varying the time of passage of the current could be observed as the extent of colorization due to the liberation of iodine. Faraday counted the beats of his watch as a means of timing. One finding was that, to match the effect produced by only an eight-beat period of voltaic current, he needed 30 turns of his frictional machine! A finding that 28 turns were insufficient probably indicates the attainable level of precision. Then comes the statement (21):

> It also follows that for this case of electrochemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force, is in direct proportion to the absolute quantity of electricity which passes.*

Here we have a statement of the First Law of Electrolysis, with a demonstrated precision of possibly about ten percent! An extensive treatment of the factors that led to the formulation of this law has been given by James (22).

Faraday had noted that "... the effects of decomposition would seem rather to depend upon a relief of the chemical affinity in one direction and an exaltation of it in the other" (23). Then the elements of a compound should separate and then combine with neighbouring particles, on the lines of the mechanism suggested by Grotthuss (6). Faraday thought that if a current could decompose a solid, then structural information might be obtained. He began to freeze solutions, aiming to trace and catch certain elements in their transit. He was surprised to find that even a thin film of ice interposed in the circuit stopped the flow of electricity even from a very powerful battery. However, a gold-leaf electrometer could be discharged through ice, which must therefore possess some small conducting power (24).

Realizing that the change in conducting power exhibited by the ice-water transition might also apply to other solid-liquid pairs, he began to study the electrochemistry of fused salts. Actually, Davy knew as early as 1801 that potassium nitrate, caustic potash and soda conduct electricity when melted, although, as Faraday indicated, he appeared to have forgotten this 11 years later (25).

Faraday began by fusing lead chloride and silver chloride on pieces of glass. He found that the melt conducted and electro-decomposition could be achieved. He then used a small V-shaped glass vessel, so that the decomposition products could be observed. The two compounds mentioned gave chlorine at the positive pole, metals at the other. Molten potassium nitrate or chlorate gave alkali, or even potassium at the negative pole, and gases such as oxygen at the other pole. Faraday used fusion on platinum when temperatures higher than possible with glass were required. He showed that many salts, oxides and sulfides became conductors when melted; in general, the liquids were much better conductors than water. The effect was not universal - sulfur, phosphorus, naphthalene, etc., remained non-conducting when fused.

At the end of his paper, dated 15 April 15 1833, Faraday summarized his results "... not without fearing that I may have omitted some important points" (26). Before he continued his fused-salt experiments, he returned to his suspicion that electro-decomposition did not necessarily depend upon the means by which the electricity entered or left the substance under investigation. He again used litmus and turmeric papers that were moistened with sodium sulfate solution. However, the papers, along with their point-contact wires, were placed on separate glass plates. String wetted with the same solution provided electrical connection between the two test papers. On turning the electrical machine, the production of acid and of alkali occurred, just as if the papers were in direct contact. This occured even if the string was 70 feet long! The supposition by others that both poles mutually "act at a distance" thus hardly seemed plausible; for a fixed quantity of electricity, the distance between the poles had no effect upon the amount of decomposition.

After several other experiments, Faraday used the arrangement shown at in (a) of figure 3 to produce decomposition

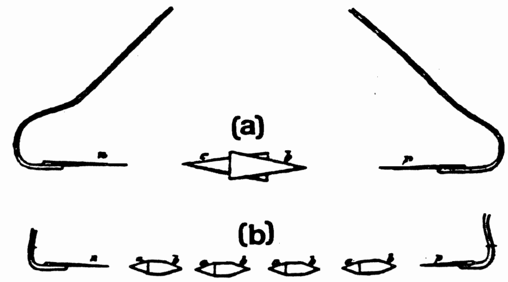


Figure 3. Electro-decomposition without contact with metallic poles: (a) single effect, (b) multiple effect.

when there was *no contact* with any metal poles (27). A triangular piece of litmus paper *a* was moistened with sodium sulfate solution and partially overlapped by a similarly moistened triangle of turmeric paper *b*. Needles *n* and *p* were supported on wax so that the gaps between the points and the tips of the papers were about half an inch. Needle *n* was grounded, while *p* joined to the electrical machine. On working this, the tips of both papers became reddened, indicating the evolution of both acid (litmus) and alkali (turmeric), despite the absence of any real poles. Faraday extended the demonstration by using four isolated pairs of strips, as shown in (b) of figure 3. All litmus tips indicated free acid, the turmeric tips, free alkali. Faraday concluded that the power which causes electrochemical decomposition appears to be exerted in the solution, and not at the poles (28).

Having shown that electrochemical decomposition could occur at an air-solution interface, Faraday demonstrated that such a decomposition could also occur at a water-solution junction (29). He had by now reached some important conclusions (30):

* Not a single fact supports the concept of "two electricities", i.e., positive and negative.

* There is no reason to consider the influence of the electric current as compound or complicated. This influence has not been resolved into simpler influences, and is best conceived as an "axis of power having contrary forces, exactly equal in amount, in contrary directions".

* The concept of rectilinear action between the poles is not necessary. Lines of action would be expected to diverge rapidly from point-contact poles in a liquid.

* Electrochemical decomposition is due to a weakening of the ordinary chemical affinity in one direction, and a strengthening of it in the opposite direction. Particles of opposite kinds will tend to pass in opposing courses. This effect is essentially dependent upon the "mutual chemical affinity" of these opposite species.

Like his predecessors, Faraday believed that the decomposition into oppositely-charged particles was caused by the passage of the electric current. The Arrhenius ionic theory, postulating the production of mobile ions by the mere act of dissolution of an electrolyte, introduced another way of thinking. However, the general acceptance of this theory, published in full in 1887 (31), was by no means instantaneous.

Faraday had accounted for the major effects of electrochemical decomposition:

* The products appear only at the poles, and are expelled, not drawn out by attraction.

* The transfer of elements is accounted for. Thus, in the passage of current between silver wires in fused silver chloride, the positive wire is eaten away, while the negative wire grows.

* The more the constituents of a substance have opposing chemical affinities, the more readily they separate in electrodecomposition. Davy's astonishing finding that acids could pass through alkalis, and the reverse, is actually the essential condition for the decomposition of a salt.

Planning to work quantitatively, Faraday began to construct a device that he termed a volta-electrometer. This was to be able to measure the "total amount" of electricity used in an experiment. (A galvanometer merely measures the current strength, or flow rate, at any given instant.) The idea was simple; let the current decompose acidulated water and measure the volume of hydrogen plus oxygen thus evolved.

He first used a graduated tube with long platinum poles sealed through the closed end. After filling with dilute acid, the tube was inverted in a cup of the same liquid and the poles were connected to a battery (32). Gas evolution occurred but, when the battery was disconnected, the volume of gas began to diminish and finally vanished. Faraday found that platinum that had been used as the positive pole in the decomposition of water could cause quite vigorous destruction of a previously-prepared 2:1 hydrogen-oxygen mixture. Platinum that had been used as the negative pole was inactive. At this stage, Faraday sidetracked to investigate this induced chemical reaction - what we would now term the heterogeneously catalyzed reaction of the two gases to form water.

Returning to the design of the volta-electrometer (Faraday later shortened the term to voltameter; the present-day term is coulometer), Faraday now knew that he must keep the positive pole out of any mixed-gas space (33). In one approach, hydrogen and oxygen are collected in separate graduated tubes. In another version, only one of the gases is collected, while the other escapes.

He then thought of a simple double-plate configuration, diagrammed in figure 4; the plates remain totally submerged and cannot affect the collected mixed gases. The plates can be close together, thus lessening the electrical resistance of the device. Faraday described three versions of this design.

He then carefully examined the variables that might control the performance. For absolute measurements the collection of hydrogen only, and correction of its volume to standard conditions, are recommended.

Now beginning to use his new (our present) terminology, Faraday defined "primary products" of electro-decomposition as those which remain unaltered when they are evolved. Examples are hydrogen and oxygen from water, or acid and alkali (both compounds!) from sodium sulfate solution. "Secondary products" occur when the separating substances are changed at the "electrodes". Thus evolving oxygen can attack a carbon "anode", giving rise to carbon dioxide. At this juncture Faraday came to a conclusion that would have important consequences for the rest of his experimental program (33):

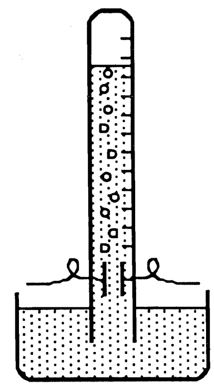


Figure 4. A double-plate volta-electrometer.

> ... when aqueous metallic salts are decomposed by the current, the metals evolved at the *cathode*, though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

It was for this reason that Faraday decided to use fused salts as "electrolytes" in his quantitative studies, thus avoiding any ambiguities that might arise from the use of aqueous systems (34).

Figure 5 shows the arrangement used to investigate tin protochloride (tin(II) chloride). The cathode, a platinum wire coiled into a knob at one end, was weighed and then sealed into a glass tube so that the knob was at the bottom. The salt was then introduced and heated to melt it. After the introduction of a platinum wire anode, the cathode was connected to a volta-electrometer and battery power was applied. Volatile "bichloride of tin" (tin(IV) chloride) was produced at the anode, while the tin liberated at the cathode formed an alloy with platinum that was liquid at the fusion temperature. After collection of a suitable volume of gas in the volta-electrometer, the anode was removed from the melt, which was then allowed to solidify.

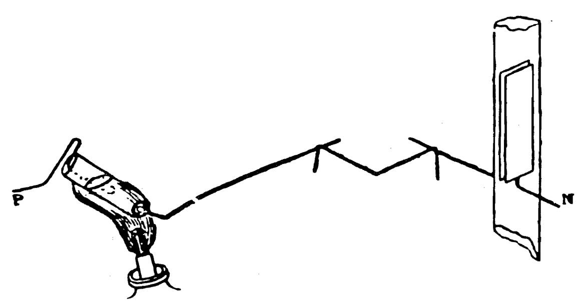


Figure 5. Determination of the electrochemical equivalent of tin.

The vessel was broken open and, after removal of salt and glass from the cathode, this was reweighed to obtain the weight of tin deposited.

From the results of four experiments, Faraday found an average value of 58.53 for the electrochemical equivalent of tin. The value of the chemical equivalent that he accepted was 57.9. Faraday gives the data concerning one of his experiments on tin protochloride, as well as the method of calculation. It is strange that the many glowing accounts concerning Faraday's work on the laws of electrolysis say little about his apparent lack of appreciation of "significant figures". However, a teacher in a grammar school has made the comment: "It is not only our pupils who claim five figure accuracy from three figure measurements" (35).

In the electrolysis of fused lead chloride, Faraday found that some platinum, dissolved from the anode, was cathodically deposited along with the lead. He therefore changed to a graphite anode. The mean of three experiments gave 100.85 as the electrochemical equivalent of lead. A similar experiment with lead borate gave 101.29, "which is so near to 103.5 (the "chemical" value) as to show that the action of the current had been definite."

Having passed the same current through protochloride of tin, lead chloride, and water, Faraday remarked (34):

> It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen being *definite in quantity* and electrochemical equivalents to each other.

Here is an implied statement of the Second Law of Electrolysis. No data are given; if the accuracy was much the same as in the separate measurements for tin and lead, Faraday had proved experimentally that this law holds to within a few percent.

In the electrolysis of fused silver chloride between silver electrodes, the anode dissolves and silver is deposited on the cathode. When attempts were made to perform this experiment quantitatively, the crystalline nature of the deposit gave problems. Faraday was more successful with fused lead chloride, finding that the loss in weight at the lead anode was equal to the gain at the cathode. The experiment gave 101.5 as the electrochemical equivalent of lead. Similar "metal transference" experiments with lead iodide and tin protochloride gave values of 103.5 and 59 for lead and tin, respectively.

Faraday, actually determined the electrochemical equivalent of zinc by use of aqueous media (36). However, it seems that his principal aim was to show that "the electricity which decomposes, and that which is evolved by the decomposition of a certain quantity of matter, are alike". The method involved the spontaneous anodic dissolution of zinc to cause displacement of hydrogen at the cathode.

The apparatus is diagrammed in figure 6. Dilute sulfuric acid was left overnight after the addition of a small piece of zinc. In this preconditioning step, dissolved air was expelled by liberated hydrogen. A gas jar was entirely filled with this acid and inverted in a basin containing the same liquid. Amalgamated zinc plates A and B (amalgamation inhibits direct attack by the acid), were weighed and introduced as shown. Then platinum plate C was introduced, so that it touched plate A. Hydrogen rose only from the platinum plate. After 10 to 12 minutes, plates A and B were withdrawn, rinsed, dried, and reweighed. The hydrogen was transferred to a water trough for volume measurement. Faraday turned his measured volume of hydrogen, 12.5 cu. in., into a corrected volume of 12.15453 cu. in. With logic so devastating in other respects, it is indeed surprising that Faraday did not sense the implication of his "expansion of figures." However, the value, 32.31, that he found for the electrochemical equivalent of zinc agrees closely with the then accepted chemical equivalent, 32.5.

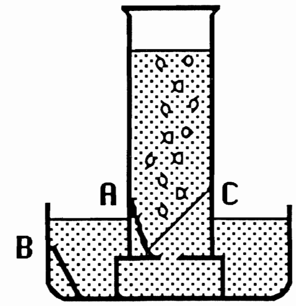


Figure 6. Determination of the electrochemical equivalent of zinc by internal electrolysis.

By 1834, Faraday had placed electrolysis on a sound quantitative basis. He then turned to a consideration of the absolute quantity of electricity associated with an atom of matter and to an examination of the rival "metal contact" and "chemical" theories of the action of the voltaic pile. These stories are beyond the scope of the present paper.

With the advantage of hindsight, we can see that Faraday paid a high price for his belief that metal deposition from aqueous solutions is a secondary process, and therefore possibly subject to ambiguity. He turned to the much more difficult fused-compound electrolyses, becoming a pioneer in this important field. It is ironic that deposition of silver from aqueous silver nitrate solution was later shown to be so precise that the procedure was used for many years to define the international ampere.

Faraday is unique in having two units named for him. These are the "farad", the unit of capacitance, and the "Faraday", the unit of electrochemical action. No doubt Faraday would have been pleased to learn that, when the latter unit was redetermined at the National Bureau of Standards, the value, precise to about 1 part in 50,000, depended upon the loss in weight of a silver anode when a known amount of electricity was passed through the perchloric acid electrolyte (37).

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FROM ELECTROCHEMICAL EQUIVALENCY TO A MOLE OF ELECTRONS: THE EVOLUTION OF THE FARADAY

Marcy Hamby Towns and Derek A. Davenport, Purdue University

In the 1988 edition of *Quantities, Units and Symbols in Physical Chemistry*, (1) we find the following recommended values for the Avogadro constant (L or N_A), the elementary charge (e), and the Faraday constant (F):

$$N_A = 6.0221367(36) \times 10^{23}\ \text{mol}^{-1}$$
$$e = 1.60217733(49) \times 10^{-19}\ \text{C}$$
$$F = 9.6485309(29) \times 10^{4}\ \text{C mol}^{-1}$$

Simple multiplication of the first two of these yields, with suitably arcane adjustments of limits of error, the third, i.e.

$$N_A e = F$$

Further examination reveals that the recommended values for both N_A and e are independent of any electrochemical measurement (2). It would seem that the long and fruitful marriage of electrochemistry and the Faraday has come to an amicable parting of the ways, a parting endorsed by the units of C mol^{-1}. A brief history of the "Faraday" will be given in terms of a concept (however named), a value (however measured) and a name (by whomever dubbed).

Faraday's establishment of the law(s) of electrolysis - "electrochemical equivalents coincide, and are the same with ordinary chemical equivalents" - has been widely studied (3-8). What has seldom been remarked is the sparsity of examples and the semiquantitative nature of much of the data upon which his great quantitative generalization was based. In Faraday's table of relative electrochemical equivalents of some 60 anions and cations (including "quinia", "cinchona" and "morphia"!) less than ten were substantiated by direct electrochemical means; the remainder are "chemical results of other philosophers in whom I could repose more confidence, as to these points, than in myself" (9). He adds:

I may be allowed to express a hope, that the endeavour will always be to make it a table of *real*, and not *hypothetical*, electrochemical equivalents; for we shall else overrun the facts, and lose all sight and consciousness of the knowledge lying directly in our path.

In the prefiguring of this passage in the *Diary* we find the more admonitory: "I must keep my researches really *Experimental* and not let them deserve any where the character of *hypothetical imaginations*" (10).

As to precision, the *Diary* gives the values 59.805, 56.833, 57.9 and 59.57 for the relative electrochemical equivalent of tin (11). (Faraday was charmingly cavalier when it came to significant figures.) In the published paper he states (12):

It is not often I have obtained an accordance in numbers [with the accepted chemical equivalent] so near as that I have just quoted ... The average of the four experiments gave 58.53 as the electrochemical equivalent of tin.

Similarly for lead one finds such varied values as 105.11, 97.26, 101.29, 93.17 and 80.51 (13). Admittedly the experimental difficulties of working with molten salts were large but as a later worker in the field somewhat ruefully remarked (14):

The experiments upon which he based his law of electrolysis are an interesting illustration of the keen insight which led Faraday to enunciate a general law upon what seems today to be very meagre and inaccurate data.

"On the Absolute Quantity of Electricity Associated with the Particles or Atoms of Matter" - so runs the heading for the concluding section of Faraday's magisterial Seventh Series of *Experimental Researches in Electricity*. The opening paragraph might seem to raise our Whiggish hopes (15):

The theory of definite electrolytical or electrochemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain: and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity.

John Frederick Daniell (left) and Michael Faraday (right), circa 1843.

However, Faraday, like many of his contemporaries, was at best a reluctant atomist, a view he passionately believed to be fraught with hypothetical, if not quite horrible, imaginings. One wonders what he would make of recent work on "electrochemistry at single-molecule sites" (16). A later passage finds him turning aside atomistic temptation (17):

The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity, is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

Faraday's electrochemical researches, with their accompanying nomenclature, quickly found their way into some of the more adventurous textbooks of the time (18, 19) but for over 30 years no one probed much more deeply into their ultimate significance than had Faraday himself.

The closest, and incomparably the most rewarding, reading of Faraday's *Experimental Researches in Electricity* was that of James Clerk Maxwell. As befits one of the founding fathers of kinetic molecular theory Maxwell was reasonably comfortable with the concept of a molecule while remaining something of an agnostic on the reality of Daltonian atoms. In 1873 (the year that saw the publication of his *A Treatise on Electricity and Magnetism*) Maxwell gave a lecture on "Molecules" at the Bradford meeting of the British Association for the Advancement of Science (20). After paying tribute to his predecessors - "The lecture in which Democritus explained the atomic theory to his fellow citizens of Abdera realized, not in golden opinions only, but in golden talents, a sum hardly equalled even in America" - Maxwell goes on to propound the conventional wisdom of physicists of his time (20):

Every substance, simple or compound, has its own molecule. If this molecule be divided, its parts are molecules of a different substance or substances from that of which the whole is a molecule. An atom, if there is such a thing, must be a molecule of an elementary substance. Since, therefore, every molecule is not an atom, but every atom is a molecule, I shall use the word molecule as the more general term.

Later in the lecture he turns to electrolysis but does not pursue the question of a molecular charge (20):

We have no time to do more than mention that most wonderful molecular motion which is called electrolysis. Here is an electric current passing through acidulated water, and causing oxygen to appear at one electrode and hydrogen at the other. In the space between, the water is perfectly calm, and yet two opposite currents of oxygen and of hydrogen must be passing through it ... Electrolysis, therefore, is a kind of diffusion assisted by electromotive force.

The reasons are not far to seek (20):

There is another set of quantities which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate, as in the second, but is only as yet of the nature of a probable conjecture. These are the absolute mass of a molecule, its absolute diameter, and the number of molecules in a cubic centimeter.

In the *Treatise* Maxwell addresses the question of molecular charge directly in the short chapter on "Electrolysis" (21):

Of all electrical phenomena electrolysis appears the most likely to furnish us with a real insight into the true nature of the electric current, because we find currents of ordinary matter and currents of electricity forming essential parts of the same phenomenon. It is probably for this very reason that, in the present imperfectly formed state of our ideas about electricity, the theories of electrolysis are so unsatisfactory.

Maxwell makes the characteristic point that:

... the ordinary chemical equivalents, however, are the *mere* numerical ratios in which the substances combine, whereas the electrochemical equivalents are quantities of matter of a determinate magnitude, depending on the definition of the unit of electricity.

Ah, the physicist's "mere"! He continues:

It is therefore extremely natural to suppose that the currents of the ions are convection currents of electricity, and, in particular, that every molecule of the cation is charged with a certain fixed quantity of positive electricity, which is the same for the molecules of all cations, and that every molecule of the anion is charged with an equal quantity of negative electricity.

Maxwell, still an adherent of the "two fluid" theory, then issues the caution (21):

But if we go on, and assume that the molecules of the ions within the electrolyte are actually charged with certain definite quantities of electricity, positive and negative, so that the electrolytic current is simply a current of convection, we find that this tempting hypothesis leads us into very difficult ground ... If, instead of a single molecule, we consider an assemblage of molecules constituting an electrochemical equivalent of the ion, then the total charge of all the molecules is, as we have seen, one unit of electricity, positive or negative.

We do not as yet know how many molecules there are in an electrochemical equivalent of any substance, but the molecular theory of chemistry, which is corroborated by many physical considerations, supposes that the number of molecules in an electrochemical equivalent is the same for all substances. We may therefore, in molecular speculations, assume that the number of molecules in an electrochemical equivalent is N, a number unknown at present, but which we may hereafter find means to determine.

Each molecule, therefore, on being liberated from the state of combination, parts with a charge whose magnitude is 1/N, and is positive for the cation and negative for the anion. This definite quantity of electricity we shall call the molecular charge. If it were known it would be the most natural unit of electricity.

Maxwell's speculations are leading us close to macroscopic/microscopic concept of the Faraday.

G. Johnstone Stoney is today best remembered for suggesting the name "electron" for the elementary charge in 1894. Twenty years earlier he had read a paper "On the Physical Units of Nature" at the Belfast meeting of the British Association for the Advancement of Science. This rather idiosyncratic paper was republished in 1881, the year of Helmholtz's famous Faraday Lecture (22). It provides an interesting historical background to the subject of SI units. Having first defined "lengthine", "massine", "timine", and "forcine" Stoney continues (22):

> e_1, the electromagnetic electrine, or the electromagnetic unit quantity of electricity in the metric series, is that quantity of each of the two kinds of electricity which must be discharged every second in opposite directions along a wire in order to maintain in it the metric unit current - this currentine or unit current being defined as the current which must exist in a wire a metre long in order that it may exert a force of a hyper-decigramme on ponderable matter at a metre distance charged with a unit of magnetism ...

So far all rather academic but later we find (22):

> And, finally, Nature presents us, in the phenomenon of electrolysis, with a single definite quantity of electricity which is independent of the particular bodies acted on. To make this clear I shall express "Faraday's Law" in the following terms, which, as I shall show, will give it precision, viz.: *For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases.* This definite quantity of electricity I shall call E_1. If we make this our unit quantity of electricity, we shall probably have made a very important step in our study of molecular phenomena.

Crucially, Stoney goes on to estimate E_1 using Loschmidt's, his own, and Thomson's estimates of the size of atoms/molecules and hence of the approximate number of atoms/molecules in a macroscopic "amount of substance". His estimate is within an order of magnitude of today's value. In short, Stoney was the first to interpret a macroscopic electrochemical equivalent (of hydrogen) in terms of a microscopic charge (positive or negative) carried by an approximately known number of microscopic particles. This seems to us the essence of the concept of the "Faraday".

In his 1894 paper "Of the 'Electron', or Atom of Electricity" (23) Stoney juxtaposes the second of the above quotations to the more famous statement by Helmholtz made in his Faraday Lecture of 1881. The circumstances of this lecture are well known. More so Helmholtz's statement (24):

> Now the most startling result, perhaps, of Faraday's law is this: If we accept the hypothesis that the elementary substances are composed of atoms we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about on the electrolytic liquid each atom remains united with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive power, and then the atoms give off their electric charges and become electrically neutral.

Hermann von Helmholtz

For our purposes little need be added to what has already been written. Nowhere does Helmholtz estimate explicitly the elementary (later the electronic) charge. Sir Henry Roscoe was in the chair on the occasion of Helmholtz's lecture and his concluding remarks include the passage (25):

> But our lecturer has gone further, for upon Faraday's well-known law of electrolysis he has founded a new electro-chemical theory, which reveals to us chemists, conclusions of the utmost importance. He tells us as the results of the application of the modern theory of electricity to Faraday's great experimental law, that the atom of every chemical element is always united with a definite invarying quantity of electricity. Moreover - and this is most important - that this definite amount of electricity attached to each atom stands in close connection with the combining power of the atom which modern chemistry terms quantivalence. For if the amount of electricity belonging to the monad atom be taken as the unit, then that of the dyad atom is two, of the triad atom three, and so on.

The future historian of the atomic theory was clearly pleased by this marriage of Dalton's atoms and Faraday's laws. Even though F was not yet the Faraday, N was not yet Avogadro's Constant, and the electron was not yet discovered, the macroscopic/microscopic essence of F was now established.

By its nature, the electrochemical equivalent is a charge-to-mass ratio and it is not surprising that it played a key role in Thomson's elucidation of the nature of the electron in 1897 and in Rutherford's identification of the alpha particle in 1905. Thomson gave a Friday Evening Discourse at the Royal Institution on 30 April 1897. Its title was "Cathode Rays"(26). After establishing a value of 1.6 x 10^{-7} for the mass-to-charge ratio of the electron, Thomson concludes his lecture with the passage (26):

This is very small compared with the value 10^{-4} for the ratio of the mass of an atom of hydrogen to the charge carried by it. If the result stood by itself we might think that it was probable that e was greater than the atomic charge of [the] atom rather than that m was less than the mass of a hydrogen atom. Taken, however, in conjunction with Lenard's results for the absorption of the cathode rays, these numbers seem to favour the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen.

It is interesting to notice that the value of e/m, which we have found from the cathode rays, is of the same order as the value 10^{-7} deduced by Zeeman from his experiments on the effect of a magnetic field on the period of the sodium light.

In Churchill's phrase, this was the electrochemical equivalent's finest hour. Mention of the Zeeman Effect brings to mind that an unsuccessful search for this effect was the subject of Faraday's last experiment (27).

Almost exactly four years later, in another Friday Evening Discourse, Thomson showed how the charge on a single electron and the value of the electrochemical equivalent yielded a satisfactory value for Loschmidt's Number (Avogadro's Constant had not yet been so named) without the "not entirely satisfactory" assumptions of Kelvin, Stoney and Loschmidt (28).

Shortly afterwards Rutherford was to use similar arguments in pinning down the nature of the alpha particle (29):

It is now necessary to consider what deductions can be drawn from the observed value of e/m found for the α particle. The value of e/m for the hydrogen ion in the electrolysis of water is known to be very nearly 10^4. The hydrogen ion is supposed to be the hydrogen atom with a positive charge, so that the value of e/m for the hydrogen atom is 10^4. The observed value of e/m for the α particle is 5.1 x 10^3, or, in round numbers, one half of that of the hydrogen atom. The density of helium has been found to be 1.98 times that of hydrogen, and from observations of the velocity of sound in helium, it has been deduced that helium is a monatomic gas. From this it is concluded that the helium atom has an atomic weight 3.96. If a helium atom carries the same charge as the hydrogen ion, the value of e/m for the helium atom should consequently be about 2.5 x 10^3. If we assume that the α particle carries the same charge as the hydrogen ion, the mass of the α particle is twice that of the hydrogen atom. We are here unfortunately confronted with several possibilities between which it is difficult to make a definite decision.

The value of e/m for the α particle may be explained on the assumptions that the α particle is (1) a molecule of hydrogen carrying the ionic charge of hydrogen; (2) a helium atom carrying twice the ionic charge of hydrogen; or (3) one-half of the helium atom carrying a single ionic charge.

With typical aplomb, Rutherford comes out firmly for the second option.

We must now turn to a short history of the experimental determination of the numerical value of the electrochemical equivalent. Near the close of his life Faraday purchased the first one ohm wire-wound resistance standard offered for sale by the Committee of the British Association for Electrical Resistance Standards (30). For most of his active research life he had had to be content with relative effects, e.g., relative electrochemical equivalents, and with semi-quantitative measurements based on ingenious *ad hoc* standards. The problem is well-illustrated in one of Faraday's most memorable metaphors (31):

One grain of water, acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three-quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire 1/104 of an inch in thickness, red-hot, in the air during the whole time; and if interrupted anywhere by charcoal points, will produce a very brilliant and constant star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in the beautiful experiments of Mr. Wheatstone, and to what I have said elsewhere on the relation of common and voltaic electricity, it will not be too much to say that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolyzation, it has only separated the elements of a single grain of water.

The establishment of international units in electric science was effected by one of the earliest, greatest and most influential of international collaborations in science (32, 33). There were two essential components to the task: (a) relating the various electrical units to the more fundamental units of mass, length and time, e.g., resistance has the dimensions of a velocity, (b) developing practical and transportable standards incorporating these fundamental units. Following theoretical contributions of Gauss and of Weber and prompted by the "progress and extension of the electric telegraph", a particularly important role was played by the Committee of Electrical Standards of the British Association for the Advancement of Science. The original committee of 1861 consisted of Williamson, Wheatstone, Thomson (Kelvin) and Jenkin. They were shortly joined by Siemens, Maxwell and Joule. All of these illuminati were working members and it is scarcely surprising that progress was rapid. The choice of the (as yet un-named) ohm as the first target of opportunity was dictated partly by the importance of resistance measurements in telegraphy and partly by the realization that the unit could be manifested in a simple material standard such as a specified column of mercury that could then be matched with conveniently transportable wire-wound resistors. International agreement was ratified in 1881.

The next step - the establishment of units and standards for current/quantity and/or for electromotive force - was more complex. As Rayleigh was later to state in his classic 1884 paper "On the Electrochemical Equivalent of Silver, and on the Absolute Electromotive Force of CLARK Cells" (34):

> The complete solution of the problem of absolute electrical measurement involves, however, a second determination, similar in kind, but quite independent of the first. In addition to resistance, we require to know some other electrical quantity, such as current or electromotive force. So far as we are aware, all the methods employed for this purpose define, in the first instance, an electrical current; but as a current cannot, like a resistance, be embodied in any material standard for future use, the result of the measurement must be recorded in terms of some effect. Thus, several observers have determined the quantity of silver deposited, or the quantity of water decomposed, by the passage of a known current for a known time. In this case the definition relates not so much to electric current as to electric quantity.

Rayleigh had inherited a tradition (and even some requisite equipment) for advancing electrical standards from his predecessor as Cavendish Professor, James Clerk Maxwell. His experiments, in which he was aided by Mrs. Sidgwick, were carried out in the same room where, 15 years later, his successor, J. J. Thomson, was to discover the electron (35).

Rayleigh's was by no means the first determination of the electrochemical equivalent of silver but it set a standard (in several senses) for thoroughness and exquisite attention to detail that lasted until the middle of the 20th century. It is not difficult to recognize the experimental skills that were later to enable Rayleigh to sniff out the presence of argon in the atmosphere from a less than one half of one per cent discrepancy in the density of nitrogen (36). As R. J. Strutt proudly points out in his biography of his father, Rayleigh's value for the electrochemical equivalent of silver (0.00111794 g/ampere-second corresponding to F = 96488) stood the test of time extraordinarily well. In 1893 it was to become the basis of the international ampere.

Many others were to attempt to refine Rayleigh's value. In a paper titled "The Universally Exact Application of Faraday's Law", T. W. Richards showed that (37):

> ... a galvanic current deposits essentially the same amount of silver from a solution of argentic nitrate in other [sodium and potassium] nitrates at 250°C as it does from an aqueous solution at 25°C, within 0.005 per cent. Taken in connection with previous work of Richards, Collins, and Heimrod, this result shows that Faraday's law is not a mere approximation, but is rather to be ranked among the most precise and general of the laws of nature.

However, Richard's value for the electrochemical equivalent of silver differed significantly from that of Rayleigh.

In spite of all the experimental ingenuity subsequently expended on the silver voltameter (or silver coulometer as Richards preferred to call it), nagging discrepancies remained. As a consequence, alternate chemical systems were investigated. The first of these was the iodine coulometer perfected by Washburn and Bates (39). This obviates the weighing of silver deposits (possibly containing occluded liquid) and has the further advantage of internal referencing since the reactions at the anode and cathode can be monitored by identical analytical methods:

$$2e^- + I_3^-(aq) \rightarrow 3I^-(aq)$$

$$3I^-(aq) \rightarrow I_3^-(aq) + 2e^-$$

Differences of 0.02% in the value of the Faraday calculated from the iodine and the silver voltameter remained, though many years later it was shown that these differences could be largely reconciled (39). Other systems studied included benzoic and oxalic acids (40), and 4-aminopyridine (41) coulometers. A major advance in precision was also achieved when the silver coulometer was changed from the silver-deposition to the silver-dissolution mode.

As we shall see, the various electrochemical determinations of the Faraday gradually converged over the years. Increasingly, however, they were challenged by non-electrochemical methods. Given the simple identity F = Ne, it is obvious the knowledge of any two quantities can be used to calculate the third. This relationship was first implicitly employed as we have seen by Stoney and was later used by J. J. Thomson to show that it yielded a plausible value for N, or rather for the Loschmidt Number. With the progress of X-ray crystallography, increasingly accurate values for N became available and the presently accepted value cited at the beginning of this article is based on this method (42). Precision measurements of the absolute charge on the electron by Millikan and others followed a more chequered path (43,44). As a consequence, in 1949 Sommer and Hipple still felt justified in claiming (45):

> The value of the Faraday has been determined by a physical method ... This measurement is particularly significant because this new method is entirely different from the usual electrochemical derivation.

The method measures the Faraday directly and involves determinations of the proton rest mass, the gyromagnetic ratio of the proton, and the proton magnetic moment in nuclear magnetons. In a 1968 summary paper, Zielen gave the comparative values shown in Table 1 for the electrochemical Faraday (46).

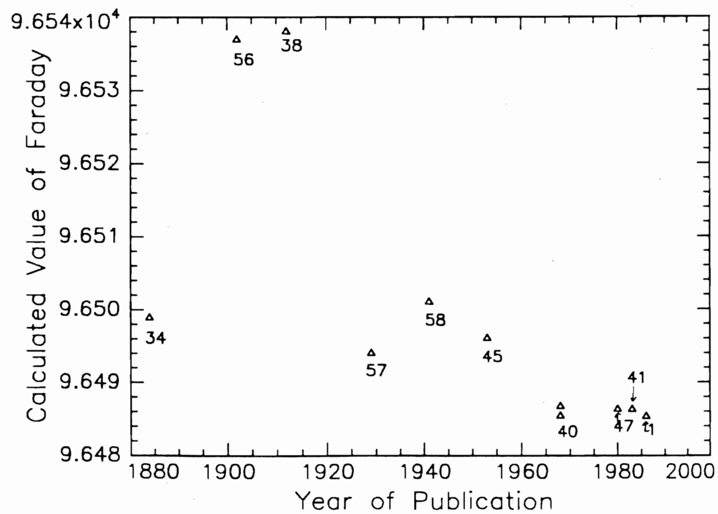
Since then the electrochemical methods have been made

Table 1. Comparataive values of the Faraday in coulombs/equivalent.

*	Silver dissolution coulometer	96,486.82 ± 0.66
*	Iodine coulometer (new or recalculated)	96,486.5 ± 2.3
*	Iodine coulometer (old)	96,490.7 ± 1.9
*	Oxalate	96,481.6 ± 3
*	Electromagnetic	96,487.6 ± 1.3

vastly more precise to yield 96,486.00 ± 0.10 (47). In addition the 4-amino pyridine coulometer of Diehl *et alia* yields 96,486.05 ± 0.72 (48). Problems of interpretation and correlation still remain as is apparent from the following wry comment by Diehl (49):

> The Craig silver dissolution value was the accepted value from 1960 on, as recalculated successively, for the shift of the atomic weight scale to carbon-12 for two changes in the definition of the ampere, for a determination of the isotope ratio in the silver used, for a change in the definition of the volt, and for a more generous statistical treatment than Craig gave his own data. The physicists interested in the values of the various fundamental constants, given successively better values for various physical quantities, obtained the significantly lower value. This discrepancy, some 20 ppm, is some four times greater than the estimated uncertainty in the electrochemical value and ten times the estimated uncertainty in the calculated value. So confident had the physicists become by 1973 that they felt it necessary to reject the Craig electrochemical value outright as "being subject to some serious error". It came, then, as a source of astonishment to them when the Iowa State University (ISU) value based on coulometric titrations of 4-aminopyridine was advanced in 1974, agreeing in most pleasant and surprising fashion with the Craig value (49).



Values of the Faraday over the years. The year, value and reference number for the points on the graph are **1884**: 96,498.9 (34); **1902**: 96,536.9 (56); **1912**: 96,538 (38); **1929**: 96,494 (57); **1941**: 96,501 (58); **1953**: 96,496 (45); **1968**: 96,486.7 (40); **1968**: 96,485.4 (40); **1980**: 96,486.33 (47);**1983**: 96,486.05 (41), **1986**: 96,485.309 (1).

Today both physical and chemical methods seem to be ineluctably and asymptotically approaching the "true" value, a value that appears astonishingly close to that put forward by Rayleigh in 1884. One is reminded of T. S. Eliot's lines:

We shall not cease from exploration
And the end of all our exploring
Will be to arrive where we started
And know the place for the first time.

When did the value for the electrochemical equivalent (of silver) become known as the Faraday? Later than one might expect it seems. The name "ohm" for the unit of electrical resistance was adopted in 1862. Five years later the unit of electrical capacitance was dubbed the "farad". "Volt", "coulomb", and "ampere" were adopted at the First International Electrical Congress held in Paris in 1881. Others had been quick to capitalize on the Faraday name. In *A Practical Treatise on the Medical and Surgical Uses of Electricity*, the second edition of which was published in 1875, we find index entries for: "Faradism", "Faradization", "Farado-contractality", "Farado-electrolyzation", "Farado- puncture or Electropuncture with the Faradic current (not much used)", and "Farado-susceptibility"(50). As is typical with that most faddish of professions, few of these pseudo-treatments and effects survive.

It is probably largely a coincidence that the words "mole", "Avogadro's Constant" and "Faraday" all entered the scientific literature during the ten years following the discovery of the electron. The term "mole" was introduced by Wilhelm Ostwald in the 1900 edition of his *Grundlinien der anorganischen Chemie* (51). The concept and name are introduced almost in passing in a section titled "The Molar Weight of Hydrogen Peroxide". Understandably there is no mention of the associated (in our eyes) Avogadro Constant or Loschmidt number, for Ostwald was at that time the most visible apostate from atomic theory; indeed the suggestion has been made that the coinage of the word mole was a consequence of this apostasy (52). In this connection it is of interest to read Ostwald's Faraday Lecture to the Chemical Society (53) delivered "in the Theatre of the Royal Institution on Tuesday, April 19th 1904". It expresses a profound scepticism concerning the existence of atoms. One wonders if the ghost of Faraday murmured his approval.

The use of the term "Faraday" for the electrochemical equivalent seems also to have arisen in Germany. In 1904 we find Lehfeldt writing in the opening chapter of his *Electrochemistry* (54):

This fundamental quantity of electricity, which occurs constantly in all writings on electro-chemistry, is called by the Germans a "faraday", a term which we in England may very well adopt.

The name soon took hold in England and elsewhere.

All threads of our story seem to come together in Jean Perrin's classic paper of 1909, "Mouvement Brownien et Réalité Moléculaire" (55):

Any two gram-molecules contain the same number of molecules. This invariable number N is a universal constant, which may appropriately be designated Avogadro's Constant.

... lastly, if the name faraday is given to the quantity F of electricity (96,550 coulombs) which passes in the decomposition of 1 gram-molecule of hydrochloric acid, it is known that the decomposition of any other gram-molecule is accompanied by the passage of a whole number of faradays, and, in consequence, that any ion carries a whole number of times the charge on the hydrogen ion. This charge *e* thus also appears as indivisible, and constitutes the atom of electricity or the electron (Helmholtz).

It is easy to obtain this universal constant if either of the constants, N or α [i.e., 3R/2N], is known. Since the gram-atom of hydrogen in the ionic state, that is to say N atoms of hydrogen, carries one faraday, then necessarily, Ne = F ...

All that remained was to improve the accuracy and precision with which N and/or e and/or F was known.

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A BIOGRAPHICAL CHECKLIST

The following is a checklist of biographies of Faraday. For teachers and students looking for a brief, accessible introduction, Thomas (1991) is highly recommended. For a more detailed biography, Williams (1965) is still the standard and is currently available in an inexpensive paperback reprint. Though long out of print, the volumes by Bence Jones (1869) and Thompson (1898) are also very worthwhile provided one is lucky enough to come across a copy.

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A FARADAY TIMETABLE

Year	*Faraday's Life*	*Concurrent Political and Chemical Events*
1790		* Publication of the English translation of Lavoisier's *Traité élémentaire de chimie*.
1791	* Born on 22 September in Newington Butts near London.	* Formulation of the metric system.
1792		* Publication of first volume of Richter's *Anfangsgründe der Stöchyometrie*.
1794		* Execution of Lavoisier.
1799		* Royal Institution (RI) founded by Count Rumford; discovery of voltaic pile; death of Black.
1801		* Humphry Davy and Thomas Young receive appointments at the RI; Dalton formulates his law of partial pressures; discovery of vanadium, tantalum, and niobium.
1803		* Publication of Berthollet's *Essai de statique chimique*; Berzelius and Hisinger study the electroysis of salts; the Louisiana Purchase.

1804		* Death of Priestley.
1805	* Begins bookbinder's apprenticeship.	* Grotthuss mechanism of electrolysis; Battle of Trafalgar.
1806		* First use of coal and oil gas for street illumination.
1807		* Davy isolates potassium and sodium.
1808		* Davy isolates calcium, barium, and strontium as metals, Berzelius as amalgams; Dalton publishes the atomic theory in his *New System of Chemistry*, Part I.
1809		* Gay-Lussac establishes law of combining volumes; Davy establishes elemental nature of chlorine; death of Fourcroy.
1810	* Gives first lecture to the City Philosophical Society.	* Death of Cavendish.
1811		* Avogadro states his hypotheses; discovery of iodine.
1812	* Attends Davy's lectures at RI; ends apprenticeship.	* Berzelius introduces dualistic theory.
1813	* Joins RI, begins European tour with Davy and Lady Davy; assists Davy in investigation of newly discovered iodine.	
1814	* Tours Italy, Switzerland, Bavaria and again France.	* Death of Rumford in Paris; Berzelius' first table of atomic weights; British burn "White" House.
1815	* Returns to England; promoted to Assistant and Superintendent of the Laboratory at RI; assists Davy in invention of safety lamp.	* Battle of Waterloo; Fresnel introduces "transverse wave theory of light".
1817	* First independent paper on "Native Caustic Lime".	* Discovery of lithium and cadmium; publication of 1st edition of Gmelin's *Handbuch*.
1818	* Begins protracted work with Stodart on steel and its alloys.	* Dulong and Petit propose their law of atomic heats.
1820	* Prepares C_2Cl_6 and C_2Cl_4.	* Oersted and Ampère demonstrate connection between electricity and magnetism.
1821	* Marries Sarah Barnard; demonstrates electromagnetic rotation.	
1822	* Oersted visits Faraday.	* Fourier publishes *Théorie analytique de la chaleur*; Berzelius begins his *Jahres-Bericht*; death of Berthollet.
1823	* Liquefies chlorine and other gases.	* Berzelius isolates silicon.
1824	* Elected to the Royal Society.	* Carnot introduces his thermodynamic cycle; Liebig sets up teaching laboratory at Giessen.
1825	* Isolates and characterizes bicarburet of hydrogen (benzene), begins five-year study of optical glass.	* Berzelius isolates titanium; Oersted isolates aluminum.
1826	* Inaugurates Christmas Lectures "adapted to a juvenile audience"; gives first Evening Discourse on "Caoutchouc."	* Discovery of bromine; Davy's final lecture "On the Relation of Electrical and Chemical Changes"; Dumas method for measuring vapor densities.
1827	* Publication of *Chemical Manipulation*	* Ohm publishes his law.
1828		* Wöhler converts ammonium cyanate to urea; death of Wollaston.
1829	* Appointed to Professorship at Royal Military Academy.	* Death of Davy in Geneva on 29 May; Döbereiner's first paper on chemical triads; discovery of thorium; Graham's law of diffusion; death of Vauquelin.
1830		* Publication of Lyell's *Principles of Geology*; Berzelius coins the term isomerism.
1831	* Publication of "Experimental Researches in Electricity [First Series]"; discovers electromagnetic induction.	* British Association for The Advancement of Science founded; north magnetic pole located.

1833	* Publication of "On the Identity of Electricity Derived from Different Sources".	* Babbage develops difference engine; Gauss proposes absolute electrical and magnetic units.
1834	* Publication of law(s) of electrolysis; correspondence with William Whewell on electrochemical nomenclature; studies catalysis; appointed Fullerian Professor of Chemistry at RI.	* McCormick patents his reaper; British Association recommends adoption of Berzelius' chemical symbolism, Dumas formulates his law of substitution.
1835	* Studies conduction of electricity by gases.	
1836	* Dielectric constant, permittivity, Faraday cage experiment.	* Republic of Texas established; invention of the Daniell cell.
1837	* Lectures "On Induction"; introduces lines of force.	* Deere introduces steel plow; beginnings of the electric telegraph; Victorian era begins.
1838	* Begins to experience severe health problems and loss of memory.	
1839	* Publication of first volume of *Experimental Researches in Electricity*.	* Daguerre describes his photographic process; discovery of lanthanum.
1841	* Extended recuperation in Switzerland.	* First meeting of The Chemical Society (of London); Fox Talbot introduces photographic negative/positive process.
1842		* Grove describes first fuel cell; Mayer states first law of thermodynamics; invention of the Bunsen cell.
1843		* Joule reports on conservation of energy and mechanical equivalent of heat.
1844	* Publication of second volume of *Experimental Researches in Electricity*.	* Death of Dalton; discovery of ruthenium.
1845	* Publishes "On the Liquefaction and Solidification of ... gases"; studies the "Faraday Effect" and diamagnetism.	* Founding of the Royal College of Chemistry; Schönbein discovers gun cotton.
1846	* Publishes "Thoughts on Ray Vibrations."	
1848	* Studies magnetic anisotropy.	* Publication of the *Communist Manifesto* and Mill's *Principles of Political Economy*; death of Berzelius; Pasteur discovers molecular asymmetry.
1849	* Fails to establish link between gravity and electricity.	
1850		* Death of Gay-Lussac; Wilhelmy's study of the rate of the hydrolysis of sugar; Graham distinguishes colloids and crytalloids.
1851	* Demonstrates paramagnetism of gaseous oxygen to RI audience.	* Kelvin reconciles the work of Carnot and Joule.
1852		* Frankland anticipates the concept of chemical valence.
1853	* Publication of *Lectures on the Non-metallic Elements*.	* Tyndall appointed Professor of Natural Philosophy at RI; death of Laurent.
1854	* Publishes "Observations on Mental Education".	* Clausius introduces the entropy function but not the term.
1855	* Publication of third volume of *Experimental Researches in Electricity*.	* Invention of the dichromate cell.
1856		* Perkin synthesizes mauve; death of Gerhardt.
1857	* Writes last major paper on colloidal metal systems; this is also the subject of his last Bakerian Lecture.	* Death of Thénard.
1858	* Receives life-tenancy of house at Hampton Court Palace.	* Couper and Kekulé propose quadrivalence and catenation of carbon; Cannizzaro rationalizes atomic weights in his *Sunto*.
1859	* Publication of *Experimental Researches in Chemistry and Physics*.	* Publication of Darwin's *On the Origin of Species*; Bunsen and Kirchhoff study spectra.

1860	* Publication of *Various Forces of Nature.*	* Karlsruhe Conference; discovery of cesium.
1861	* Publication of *The Chemical History of a Candle*; offers his resignation to the Managers of the RI.	* The Emancipation Edict frees Russian serfs; start of American Civil War; discovery of rubidium and thallium; Solvay Process.
1862	* Performs last experiment seeking effect of magnetic field on flame spectra; last Friday Evening Discourse; moves permanently to Hampton Court.	* August W. Hofmann lectures at RI on "Mauve and Magenta."
1864		* Publication of Meyer's *Die modern Theoriern der Chemie*; Guldberg and Waage formulate the law of mass action.
1865		* Publication of Hofmann's *Modern Chemistry;* Newlands publishes his law of octaves; Clausius proposes the term entropy; Kekulé proposes his benzene structure.
1867	* Death on 25 August.	* Marx publishes first volume of *Das Kapital.*

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