

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 Welter'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 Welter'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):

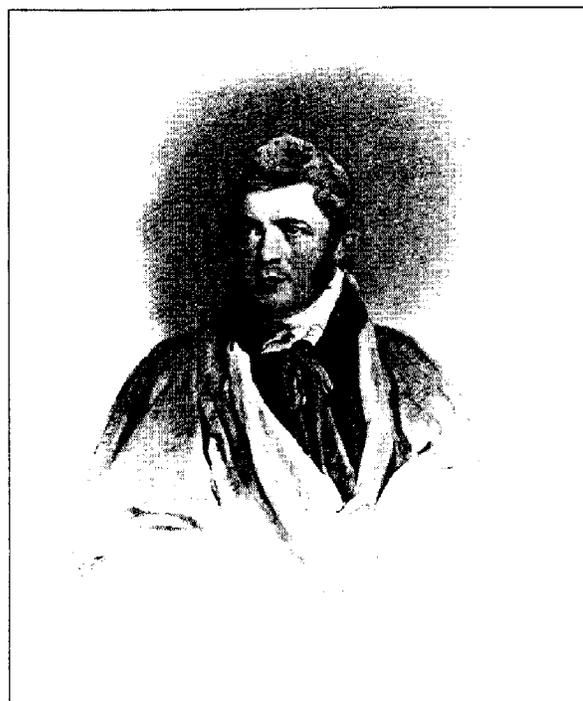
Of his friends and correspondents it may be said without the slightest exaggeration, that they formed no inconsiderable portion of the persons whose learning and genius have shed a lustre upon their country during the last twenty years, and this, not in one department only, but in several; and if he 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



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

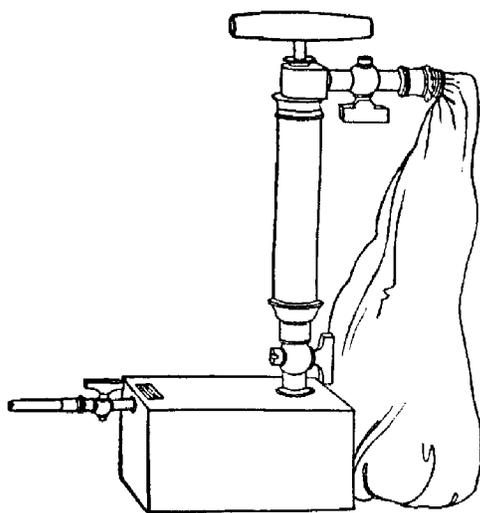
most striking characters belonging to the Cambridge life of that my early time. He was very eloquent: - I should say the most naturally eloquent man I have heard: that is, he gave to what he said all the charm that fluency, earnestness, and fine delivery could give, independent of its meaning and purport, 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.

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):

When Mr. Brande left London in August [1816], he gave the *Quar-*



The Newman-Brooke oxyhydrogen blowpipe.



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

*terly 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.

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 admitted, "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 sub-oxide 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

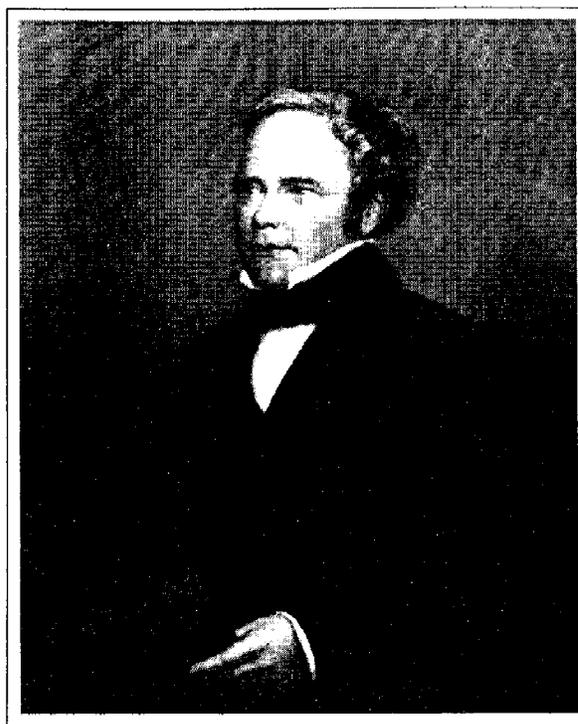
Sir - Mr. Brande is at present on the Continent but left directions with me before his departure for the management of the Journal.

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 Barytes* 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 Silix, 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 oxygen 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 extraordinarily (*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 likeregret* 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

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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.

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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, Descriptive Catalogue*, Science Museum, Her Majesty's Stationery Office, London, 1937, p. 62.

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## THE PATHWAY TO THE LAWS OF ELECTROLYSIS

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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 Grothuss

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